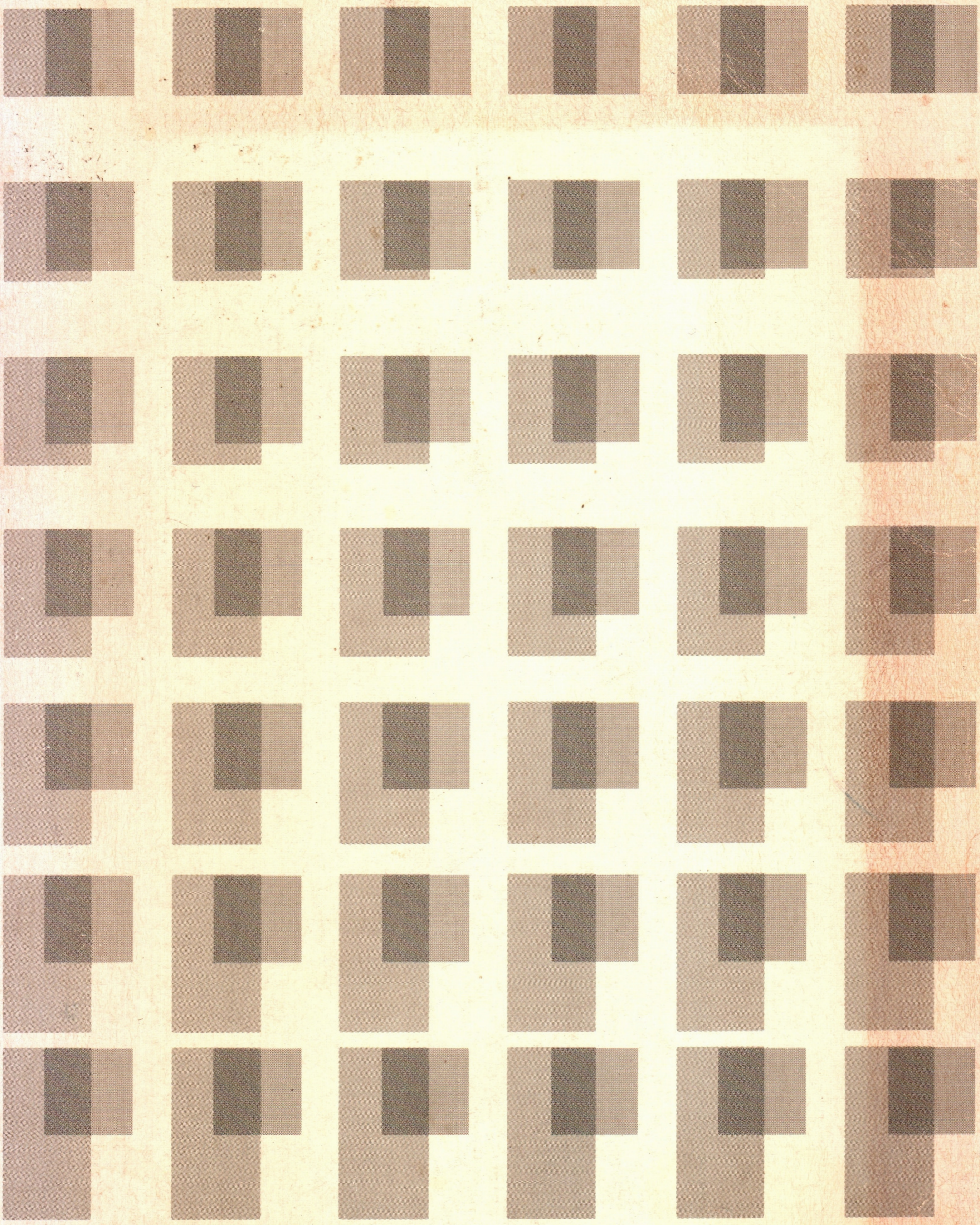
Nicholas Georgescu-Roegen

The Entropy Law and the Economic Process



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Contents

Appendices

Tables

(Figures are on pages 154, 155, 379, 408, 416.)

Preface

The Entropy Law is still surrounded by many conceptual difficulties and equally numerous controversies. But this is not the reason why most natural scientists would agree that it occupies a unique position among all laws of matter. Sir Arthur Eddington even maintained that the position is "supreme." The important fact is that the discovery of the Entropy Law brought the downfall of the mechanistic dogma of Classical physics which held that everything which happens in any phenomenal domain whatsoever consists of locomotion alone and, hence, there is no irrevocable change in nature. It is precisely because this law proclaims the existence of such a change that before too long some students perceived its intimate connection with the phenomena peculiar to living structures. By now, no one would deny that the economy of biological processes is governed by the Entropy Law, not by the laws of mechanics.

The thought that the economic process, too, must be intimately connected with the Entropy Law is the origin of the inquiry that forms the subject of this book. To examine the numerous aspects of this connection has taken me—and will take the reader—in many fields beyond the boundary of economics. For this reason I felt that the task of introducing the topic of this book had to be left to a special chapter.

Here I may say that, precisely because of the special nature of the subject, working on this book has confirmed an old notion of mine, namely, that practically all works we usually call our own represent only a few scoops of originality added on top of a mountain of knowledge received from others. Going over the galley proofs was for me an occasion as no other to realize how immense is my debt to my teachers and also how numerous they are. It prompted me to seize upon this opportunity to express my gratitude to them by the dedication of this volume.

Many teachers will not have their name carved inside the pantheon of the great minds, even though many will be no less highly revered. Foremost in this category (and in my heart) are my parents—my father, who taught me to read, write, and calculate and who planted in me the seed of intellectual curiosity, and my mother, who, by her living example, taught me the value of hard work. Gheorghe Rădulescu, my elementary school teacher in a small town of old Romania, fostered with great skill my early mathematical inclinations by teaching us how to solve "tricky" problems which, as I learned later, are usually solved by algebra. From the long list of the inspiring and devoted teachers I had at Lyceum Mânăstirea Dealu I may mention Grigore Zapan and Gh. I. Dumitrescu, who with tremendous love for their profession guided my first steps in higher mathematics. I think that my share of good luck at the university level also was above the average. I studied with scholars whose names now occupy a place of honor in the history of science: Traian Lalescu, Octav Onicescu, and G. Țițeica (in Bucharest), Albert Aftalion, Émile Borel, Georges Darmois, and Maurice Fréchet (in Paris), and E. B. Wilson (in the U.S.A.). But two of my teachers had the most decisive influence on my scientific orientation: Karl Pearson, the man of broad knowledge who single-handedly laid the foundations of the science of statistics, and Joseph A. Schumpeter, whose unique vision of the economic process combined in a harmonious manner quantitative with qualitative evolutionary analysis.

Needless to say, one should consider as his teachers also those from whom he learned in any other way, chiefly through their writings. Like everyone else, I also learned a great deal from my professional colleagues (many things even from my students). From their number, which is legion, I cannot resist singling out two of my fellow economists (and econometricians)—Wassily W. Leontief and Paul A. Samuelson.

The reader does not need any hint to realize that a book of this nature cannot be written as a research project with a definite timetable. The ideas contained in it were worked out in my mind over many years (as many as twenty, I believe) and in various circumstances—sometimes while lecturing, sometimes while working in the garden. Some of these ideas have already appeared in print, mostly in the introductory essay to my *Analytical Economics*.

During all these years, Vanderbilt University has given me encouragement and provided me with facilities for work, many of which came from the Graduate Program in Economic Development. For all this I am

especially and variously indebted to my colleagues George W. Stocking, Rendigs Fels, Anthony M. Tang, and James S. Worley. A research grant from the National Science Foundation has enabled me to devote half of my teaching time during one and a half years to bringing this work to its present form. During this last phase I was assisted by Messrs. Aly Alp Ercelawn and Ibrahim Eriş.

I am grateful also to the Syndics of Harvard University Press for having considered it worthwhile to have the introductory essay of my *Analytical Economics* expanded and completed in the present volume.

My final thought of gratitude is for my wife, who has been a patient, attentive reader, a sympathetic but constructive critic, and a tireless proofreader, and who has provided me with a home atmosphere conducive to study and work.

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July 1970

NOTE

"AE" in the footnotes stands for my *Analytical Economics: Issues and Problems*, published by Harvard University Press in 1966.

The Entropy Law and the Economic Process

Introduction

No science has been criticized by its own servants as openly and constantly as economics. The motives of dissatisfaction are many, but the most important pertains to the fiction of *homo oeconomicus*. The complaint is that this fiction strips man's behavior of every cultural propensity, which is tantamount to saying that in his economic life man acts mechanically. This is why the shortcoming is ordinarily exposed as the mechanistic outlook of modern economics. The criticism is irrefutable. However, the mechanistic sin of economic science is much deeper than this criticism implies. For the sin is still there even if we look at the economic process from the purely physical viewpoint only. The whole truth is that economics, in the way this discipline is now generally professed, is mechanistic in the same strong sense in which we generally believe only Classical mechanics to be.

In this sense Classical mechanics is mechanistic because it can neither account for the existence of enduring qualitative changes in nature nor accept this existence as an independent fact. Mechanics knows only locomotion, and locomotion is both reversible and qualityless. The same drawback was built into modern economics by its founders, who, on the testimony of Jevons and Walras, had no greater aspiration than to create an economic science after the exact pattern of mechanics. A most eloquent proof of how staunch the enthusiasm for mechanics was among the early architects is provided by Irving Fisher, who went to the trouble of building a very intricate apparatus just for demonstrating the purely mechanical nature of consumer behavior.[1]

[1] Irving Fisher, *Mathematical Investigations in the Theory of Value and Prices* (New Haven, 1925), pp. 38 f and *passim*. The work was first published in 1892.

And these architects succeeded so well with their grand plan that the conception of the economic process as a mechanical analogue has ever since dominated economic thought completely. In this representation, the economic process neither induces any qualitative change nor is affected by the qualitative change of the environment into which it is anchored. It is an isolated, self-contained and ahistorical process—a circular flow between production and consumption with no outlets and no inlets, as the elementary textbooks depict it. Economists do speak occasionally of natural resources. Yet the fact remains that, search as one may, in none of the numerous economic models in existence is there a variable standing for nature's perennial contribution. The contact some of these models have with the natural environment is confined to Ricardian land, which is expressly defined as a factor immune to any qualitative change. We could very well refer to it simply as "space." But let no one be mistaken about the extent of the mechanistic sin: Karl Marx's diagrams of economic reproduction do not include even this colorless coordinate. So, if we may use a topical slogan for a trenchant description of the situation, both main streams of economic thought view the economic process as a "no deposit, no return" affair in relation to nature.

The intriguing ease with which Neoclassical economists left natural resources out of their own representation of the economic process may not be unrelated to Marx's dogma that everything nature offers us is gratis. A more plausible explanation of this ease and especially of the absence of any noticeable attempt at challenging the omission is that the "no deposit, no return" analogue befits the businessman's view of economic life. For if one looks only at money, all he can see is that money just passes from one hand to another: except by a regrettable accident, it never gets out of the economic process. Perhaps the absence of any difficulty in securing raw materials by those countries where modern economics grew and flourished was yet another reason for economists to remain blind to this crucial economic factor. Not even the wars the same nations fought for the control of the world's natural resources awoke economists from their slumber.[2]

All in all, the wholesale attachment of almost every economist of the last one hundred years to the mechanistic dogma is still a historical puzzle. Once, it is true, physicists, mathematicians, and philosophers were one in singing the apotheosis of mechanics as the highest triumph of human reason. But by the time Jevons and Walras began laying the cornerstones

[2] To top all the intriguing facts of this history: not more than six years before Jevons published his pathbreaking *Lectures*, he wrote a highly interesting analysis of the consequences for Great Britain of a speedy depletion of her coal reserves. W. Stanley Jevons, *The Coal Question*, ed. A. W. Flux (3rd edn., London, 1906), originally published in 1865, was Jevons' first major work in economics.

of modern economics, a spectacular revolution in physics had already brought the downfall of the mechanistic dogma both in the natural sciences and in philosophy. And the curious fact is that none of the architects of "the mechanics of utility and self-interest" and even none of the latter-day model builders seem to have been aware at any time of this downfall. Otherwise, one could not understand why they have clung to the mechanistic framework with the fervor with which they have. Even an economist of Frank H. Knight's philosophical finesse not long ago referred to mechanics as "the sister science" of economics.[3]

Revolution is a fairly recurrent state in physics. The revolution that interests us here began with the physicists' acknowledging the elementary fact that heat always moves by itself in one direction only, from the hotter to the colder body. This led to the recognition that there are phenomena which cannot be reduced to locomotion and hence explained by mechanics. A new branch of physics, thermodynamics, then came into being and a new law, the Entropy Law, took its place alongside—rather opposite to—the laws of Newtonian mechanics.

From the viewpoint of economic science, however, the importance of this revolution exceeds the fact that it ended the supremacy of the mechanistic epistemology in physics. The significant fact for the economist is that the new science of thermodynamics began as a physics of economic value and, basically, can still be regarded as such. The Entropy Law itself emerges as the most economic in nature of all natural laws. It is in the perspective of these developments in the primary science of matter that the fundamentally nonmechanistic nature of the economic process fully reveals itself. As I have argued in the introductory essay of my *Analytical Economics*, only an analysis of the intimate relationship between the Entropy Law and the economic process can bring to the surface those decisively qualitative aspects of this process for which the mechanical analogue of modern economics has no room. The object of that essay—to examine this relationship with a view to filling a conspicuous lacuna of the economic discipline—will be pursued in this volume with greater detail and in more varied directions.

The fact that a natural law is involved in every aspect of man's behavior is so common that we would not expect the study of the influence of the Entropy Law on man's economic actions to present any unusual complications. Yet manifold avenues open up almost as soon as one begins to tackle the problem. What is more, these avenues lead beyond the boundary

[3] Frank H. Knight, *The Ethics of Competition* (New York, 1935), p. 85.

not only of economics but of the social sciences as well. And if one endeavors to explore them however cursorily, one discovers that issues which are generally considered to be specific to economics (or to the social sciences) spring up even in some areas of the natural sciences. Any searcher would find it hard to close his eyes to such an exciting vista and proceed undisturbed with his ordinary business.

It goes without saying that to undertake a project of this nature requires venturing into territories other than one's own, into fields in which one is not qualified to speak. The most one can do in this situation is to build on the writings of the consecrated authorities in every alien field and, for the reader's sake, to suppress no reference to any source (notwithstanding the current literary wisdom to minimize the number of footnotes or even to do away with them altogether). Even so, one runs some substantial risks. Yet the project is definitely worth undertaking. It reveals that the relationship between the economic process and the Entropy Law is only an aspect of a more general fact, namely, that this law is the basis of the *economy* of life at all levels. There are also some epistemological object lessons to be learned from the same analysis, all converging to one general conclusion which should interest every scientist and philosopher, not only the student of life phenomena (as the economist is). This conclusion is that in actuality only locomotion is qualityless and ahistorical: everything else is Change in the fullest sense of the word.

To some, the term "entropy" may seem esoteric. Once it was, but now it is becoming increasingly popular in one field after another. What should now give us reason for concern in meeting the term is the fact that its meaning varies substantially, at times even within the same domain of intellectual endeavor. In *Webster's Collegiate Dictionary* alone we find four distinct entries under "entropy." In part, this situation reflects the most unusual history of the Entropy Law, continuously punctuated by celebrated controversies, not all dead yet. In view of the confusion which has accumulated in some quarters, a preliminary survey to contrast the main meanings of "entropy" may prove useful even for the reader already familiar with some of them.

There is, first, the original meaning with which "entropy" was introduced more than one hundred years ago by the German physicist Rudolf Clausius. This meaning is grounded in a bedrock of physical facts. All other meanings constitute a separate category that stands in opposition to it. These are related in a purely formal way to a simple algebraic formula which is the cloak under which "entropy" is now becoming familiar to an increasing number of social scientists. Just recently, the term—with such a formal meaning—was brought within the economist's field of vision by the invitation to include a special "theory of information" in his tool box.[4]

The physical concept is generally judged to be quite intricate. If we take the word of some specialists, not even all physicists have a perfectly clear understanding of what this concept exactly means. Its technical details are, indeed, overwhelming. And even a dictionary definition suffices to turn one's intellectual curiosity away: "a measure of the unavailable energy in a closed thermodynamic sytem so related to the state of the system that a change in the measure varies with change in the ratio of the increment of heat taken in to the absolute temperature at which it is absorbed."[5] All this does not alter the fact that the nature of most thermodynamic phenomena is so simple that the layman may grasp the concept of entropy in its broad lines without much difficulty.

Let us take the case of an old-fashioned railway engine in which the heat of the burning coal flows into the boiler and, through the escaping steam, from the boiler into the atmosphere. One obvious result of this process is some mechanical work: the train has moved from one station to another. But the process involves other undeniable changes as well. To wit, the coal has been transformed into ashes. Yet one thing is certain: the total quantity of matter and energy has not been altered. That is the dictate of the Law of the Conservation of Matter and Energy—which is the First Law of Thermodynamics and which, we should stress, *is not* in contradiction with any of the laws of mechanics. The conclusion can only be that the change undergone by matter and energy must be a *qualitative* change.

At the beginning, the chemical energy of the coal is *free*, in the sense that it is *available* to us for producing some mechanical work. In the process, however, the free energy loses this quality, bit by bit. Ultimately, it always dissipates completely into the whole system where it becomes *bound* energy, that is, energy which we can no longer use for the same purpose. To be sure, the complete picture is more involved. And in fact, the merit of the introduction of entropy as a new variable of state lies precisely in the analytical simplification and unification achieved thereby. Even so, the other, more intuitive concepts of free and bound energies have never lost their transparent significance. For, in a broad yet substantive perspective, entropy is an index of the relative amount of bound energy in an isolated structure or, more precisely, of how evenly the energy is distributed in such a structure. In other words, *high* entropy means a structure in which most or all energy is bound, and *low* entropy, a structure in which the opposite is true.

The common fact that heat always flows by itself from the hotter to the

[4] H. Theil has devoted a whole volume to expounding this particular idea. See his *Economics and Information Theory* (Chicago, 1967).

[5] *Webster's Seventh New Collegiate Dictionary.*

colder body, never in reverse, came to be generalized by the Entropy Law, which is the Second Law of Thermodynamics and which *is* in contradiction with the principles of Classical mechanics. Its complete enunciation is incredibly simple. All it says is that the entropy of the universe (or of an isolated structure) increases constantly and, I should like to add, irrevocably. We may say instead that in the universe there is a *continuous* and *irrevocable* qualitative degradation of free into bound energy. Nowadays, however, one is more likely to come across a modern interpretation of this degradation as a continuous turning of *order* into *disorder*. The idea is based on the observation that free energy is an ordered structure, while bound energy is a chaotic, disordered distribution.

In rounding out this picture, we should note that the full meaning of the Entropy Law is not that the qualitative degradation occurs only in connection with mechanical work performed consciously by some intelligent beings. As exemplified by the sun's energy, the entropic degradation goes on by itself regardless of whether or not the free energy is used for the production of mechanical work. So, the free energy of a piece of coal will eventually degrade into useless energy even if the piece is left in its lode.

There are some good reasons why I stress (here as well as in some chapters of this volume) the irrevocability of the entropic process. One reason interests the economist in particular. If the entropic process were not irrevocable, i.e., if the energy of a piece of coal or of uranium could be used over and over again ad infinitum, scarcity would hardly exist in man's life. Up to a certain level, even an increase in population would not create scarcity: mankind would simply have to use the existing stocks more frequently. Another reason is of more general interest. It concerns one of man's weaknesses, namely, our reluctance to recognize our limitations in relation to space, to time, and to matter and energy. It is because of this weakness that, even though no one would go so far as to maintain that it is possible to heat the boiler with some ashes, the idea that we may defeat the Entropy Law by bootlegging low entropy with the aid of some ingenious device has its periodical fits of fashion. Alternatively, man is prone to believe that there must exist some form of energy with a self-perpetuating power.[6]

It must be admitted, though, that the layman is misled into believing in entropy bootlegging by what physicists preach through the new science known as statistical mechanics but more adequately described as statistical thermodynamics. The very existence of this discipline is a reflection of the fact that, in spite of all evidence, man's mind still clings with the

[6] As Jevons reports (*Coal Question*, pp. 106 f), in his own time many thought that electricity has such a power. My personal experience suggests that some economists (at least) now believe that atomic energy fits the case.

tenacity of blind despair to the idea of an actuality consisting of locomotion and nothing else. A symptom of this idiosyncrasy was Ludwig Boltzmann's tragic struggle to sell a thermodynamic science based on a hybrid foundation in which the rigidity of mechanical laws is interwoven with the uncertainty specific to the notion of probability. Boltzmann took his life in bitterness over the mounting criticism of his idea. But after his death, the same human idiosyncrasy induced almost everyone to trample over all logical flaws exposed by that criticism so that Boltzmann's idea might become a recognized branch of physics. According to this new discipline, a pile of ashes may very well become capable of heating the boiler. Also, a corpse may resuscitate to lead a second life in exactly the reversed order of the first. Only, the probabilities of such events are fantastically small. If we have not yet witnessed such "miracles"—the advocates of statistical mechanics contend—it is only because we have not been watching a sufficiently large number of piles of ashes or corpses.

In contrast with Classical thermodynamics, even a summary discussion of statistical thermodynamics cannot do without numerous technical points, some of them highly technical. Boltzmann's main premise, however, has to be brought into the picture even at this stage. This premise is that, aside from a factor representing a physical constant, the entropy of an isolated gas of N molecules is given by the formula

$$\text{Entropy} = S = \ln W, \tag{1}$$

where

$$W = \frac{N!}{N_1!\,N_2!\cdots N_s!} \tag{2}$$

and the N_i's represent the distribution of the gas molecules among the s possible states. And since the combinatorial coefficient W is a familiar sight in the calculus of probabilities, relation (1) has been translated as "entropy is equal to the thermodynamic probability."

In this way, Boltzmann's approach opened the door to an almost endless series of interpretations of what entropy means and, concomitantly, to different formal definitions of the term. Some of the disciples of this approach have gone so far as to deny that the Entropy Law expresses a natural law. Instead, they maintain, it reflects only the difficulty of the human mind in describing a state which involves an increasing number of details. Certainly, these are muddled waters in which any user of the term "entropy" should navigate carefully.

If we take formula (1) as a formal definition of entropy, we may bring this concept into any situation with which W can be associated in some way or another. For a telling example, let us consider five distinct points in a plane. If we put $N = 5$, $N_1 = 2$, and $N_2 = 3$, then W gives the maximum number of distinct straight lines determined by these points. We may therefore speak of

$$S = \log_{10} \frac{5!}{2!\,3!} = 1 \tag{3}$$

as the "entropy of a pentagon." This shows how easy it is to concoct meanings of "entropy" that are wholly vacuous.

However, the emergence of (1) in problems connected with the transmission of sequences of signals (or symbols) is a normal event that should not surprise us: if the number of distinct signals is s, then W is the number of distinct sequences of length N in which each i-th symbol enters N_i times. What should surprise us is that S has been equated with the *amount of information* contained in such a sequence. According to this equation, if we take, say, Newton's *Principia Mathematica* and scramble its letters and symbols, the result still represents the same amount of information! Even more perplexing is a subsequent argument by which the total information is identified with *negentropy* (i.e., the negative value of *physical* entropy).

The concept of entropy has even penetrated into domains in which there is no room for combinatorial analysis and, hence, for W. This is due to the fact that the most popular definition of the concept as a "measure" of the amount of information is given by a special transformation of (1). The definition is[7]

$$E = -\sum f_i \log f_i \tag{4}$$

where $f_i > 0$ for every i and $\sum f_i = 1$.

This expression has several interesting properties which account for the attraction it has exercised on many minds. But its most interesting feature is that we can apply it to any percental distribution—say, the distribution of a country's exports by destinations or of personal incomes by income brackets. It is by such a complicated metamorphosis, of which not all users of the term "entropy" may be aware, that we have come to speak of the *amount of information* of almost any statistical data. And we march on, without even noticing that this terminological mess compels us to say, for instance, that for a country in which income is more equally distributed the statistics of income distribution contains a greater amount of information![8]

[7] This transformation assumes that every N_i is large enough for $N_i!$ to be approximated by Stirling's formula. This formula is reproduced in Appendix G, note 29, in this volume.

[8] This statement follows from the fact that the property unmistakably reflected by E is the degree of evenness (indirectly, the degree of concentration) of the distribution described by the f_i's. Cf. Appendix B in this volume.

The code of Humpty Dumpty—which allows one to use a word with any meaning one wishes—is much too often invoked as a supreme authority on terminological prerogative. But nobody seems to have protested that ordinarily the only consequence of this prerogative is confusion. An advertising tendency may have been the father to denoting the numerical value of expressions such as (1) or (4) by "amount of information." Be this as it may, this terminological choice is probably the most unfortunate in the history of science.

One can now see why it is imperative to emphasize that the position taken in the present study is that in the physical world there is a coordinate which corresponds to Clausius' concept of entropy and which is not reducible to locomotion, much less to probability or to some subjective element. Another way of saying the same thing is that the Entropy Law is neither a theorem deducible from the principles of Classical mechanics nor a reflection of some of man's imperfections or illusions. On the contrary, it is as independent a law as, for example, the law of universal attraction, and just as inexorable. The entropic phenomenon of a piece of coal burning irrevocably into ashes is neither a flow of probability from a lower to a higher value, nor an increase in the onlooker's ignorance, nor man's illusion of temporal succession.

As we shall gradually come to realize in the course of this volume, the position occupied by the Entropy Law among all other laws of nature is unique from numerous viewpoints. And this fact accounts for the wealth of questions and issues that overwhelm any student interested in assessing the importance of the Entropy Law beyond the strictly physical domain.

No one would deny that entropy, together with its associated concepts of free and bound energies, is a much more mysterious notion than locomotion. The only way man can consciously act on the material environment is by pushing or pulling, even when he starts a fire. But this limitation is no reason for clinging to the idea that the entropic process must be reducible to locomotion. Monism has long since ceased to be the password in science. Even the argument that science must be free of any contradiction is no longer commanding. Physics itself now teaches us that we must not insist on molding actuality into a noncontradictory framework. Just as we are advised by Niels Bohr's Principle of Complementarity that we must accept as a brute fact that the electron behaves both as a wave and as a particle—concepts irreducible to one another—so must we at present reconcile ourselves to the existence of thermodynamic and mechanical phenomena side by side, albeit in opposition.

From the epistemological viewpoint, the Entropy Law may be regarded as the greatest transformation ever suffered by physics. It marks the recognition by that science—the most trusted of all sciences of nature—that there is qualitative change in the universe.[9] Still more important is the fact that the irrevocability proclaimed by that law sets on a solid footing the commonsense distinction between locomotion and true happening. According to this distinction, only that which cannot be brought back by reverse steps to a previous state represents true happening. What "happening" thus means is best exemplified by the life of an organism or the evolution of a species (as distinct from mere mutational changes, which *are* reversible). This opposition between true happening and locomotion is likely to be censured as an anthropomorphic idea. In fact, positivistic purists have denounced thermodynamics itself as an anthropomorphic amalgam. One writ contends that even Time is only man's illusion, and hence there is no sense in speaking of reversibility or irreversibility of natural phenomena. On the other hand, there is no denying that it was the importance which the distinction between free and bound energy has for man's economy of mechanical power that set thermodynamics going. Yet it would be utterly wrong to maintain that only thermodynamics is in this situation. Locomotion, particle, wave, and equation, for example, are concepts no less anthropomorphic than the two faces of entropy, the two qualities of energy. The only difference is that of all sciences of inert matter thermodynamics is the nearest to man's skin—literally, not figuratively.

We know that people can live even if deprived of sight, or of hearing, or of the sense of smell or taste. But we know of no one able to live without the feeling of the entropy flow, that is, of that feeling which under various forms regulates the activities directly related with the maintenance of the physical organism. In the case of a mammal this feeling includes not only the sensations of cold and warm, but also the pangs of hunger and the contentment after a meal, the feeling of being tired and that of being rested, and many others of the same kind.[10] Things are not stretched therefore if one argues that the entropic feeling, in its conscious and unconscious manifestations, is the fundamental aspect of life from amoeba to man.

Be this as it may, the fact is that the material basis of life is an entropic process. As Erwin Schrödinger crystallized this idea, any life-bearing structure maintains itself in a quasi-steady state by sucking low entropy from the environment and transforming it into higher entropy. Some

[9] By now this notion is no longer a rarity in the science of elementary matter. The two presently contending speculations in cosmology speak even of creation—one by arguing that the universe was created by a Big Bang, the other, that matter is continuously created and annihilated.

[10] On the basis of the above definition, one should expect that the "senses" of taste and smell cannot be absent at the same time.

writers—the French philosopher Henri Bergson, in particular—even contended that life actually opposes the trend of qualitative degradation to which inert matter is subject. Think of the nucleus of some primeval strain of amoeba which may still be around in its original pattern. No inert structure of as many molecules can boast the same tour de force—to resist the disrupting work of the Entropy Law for perhaps as long as two billion years.

The thought that life may be "characterized by a capacity for evading this law"—once generally denounced as sheer obscurantism—is now endorsed by almost every authority in physicochemistry.[11] It is nonetheless true that, if expressed in this terse form, the thought may easily be distorted. A living being can evade the entropic degradation of its own structure only. It cannot prevent the increase of the entropy of the whole system, consisting of its structure and its environment. On the contrary, from all we can tell now, the presence of life causes the entropy of a system to increase faster than it otherwise would.

The truth of the last point is especially evident in the case of the human species. Actually, hardly anything need be added now to make us see also that the economic struggle is only about low entropy and that the nature of the economic process viewed as a whole is purely entropic. Yet, among the economists of distinction, only Alfred Marshall intuited that biology, not mechanics, is the true Mecca of the economist. And even though Marshall's antimechanistic proclivities were reflected mainly in his famous biological analogies, we must impute to them his salient discovery of the irreversibility of long-run supply schedules. Unfortunately, Marshall's teaching caused no lasting imprint and the fact that irreversibility is a general feature of all economic laws received no attention.

Lacking Marshall's understanding, economists have seen no point in following the developments in biology and have thus missed many fertile ideas. This is the case with the highly interesting way in which Alfred J. Lotka, a physical biologist, explained why the economic process is a continuation of the biological one. In the last process—Lotka pointed out—man, like any other living creature, uses only his *endosomatic* instruments, i.e., the instruments that are part of each individual organism by birth. In the economic process man uses also *exosomatic* instruments—knives, hammers, boats, engines, etc., which he produces himself. Lotka's framework will help us understand why only the human species is subject to an irreducible social conflict.

A peculiar feature of the determinative powers of the Entropy Law is

[11] The above quotation from Sir James Jeans, *The New Background of Science* (New York, 1934), p. 280, is one among numerous such endorsements.

responsible for the fact that the relationship between this law and the domain of life phenomena is yet deeper than the facts just mentioned reveal. Geometry (conceived in its etymological sense), astronomy, and Classical mechanics accustomed us to the power of science to determine "exactly" where and when a definite event will take place. Later, quantum phenomena taught us to be content with the weaker position in which scientific laws determine merely the probability of an occurrence. But the Entropy Law constitutes a singular case. It determines neither *when* (by clock-time) the entropy of a closed system will reach a certain level nor exactly *what* will happen.[12] In spite of this drawback (and contrary to what some have contended), the Entropy Law is not idle: it does determine the general direction of the entropic process of any isolated system.

But the drawback acquires a momentous importance in connection with the fact that the only other thermodynamic law to bear upon an entropic process is the Law of the Conservation of Matter and Energy.[13] This means that all we can say about such a process is that, as time goes by, its total energy remains constant while the distribution of this energy becomes more even. The thermodynamic principles, therefore, leave some substantial freedom to the actual path and the time schedule of an entropic process. According to the position taken in this study about the nature of thermodynamic phenomena, this freedom is not to be confused with random uncertainty. We may refer to it as the *entropic indeterminateness*.

This is an extremely important feature of actuality. For without the entropic indeterminateness it would not be possible for a living creature to maintain its entropy constant. Nor would it be possible for man to "reverse" entropy from high to low, as in the production of steel from iron ore and coal. Above all, it would be impossible for the living forms to go after environmental low entropy and use it in manners as strikingly diverse as that of a bacterium, a lobster, a butterfly, a tumbleweed, a *Homo sapiens*, and so on down the potentially limitless list. We must, however, recognize that this indeterminateness by itself does not ensure the existence of the infinitude of forms and functions displayed by the organic domain. In point of fact, it does not even ensure the existence of any living being whatsoever. The existence of life-bearing structures is a primary fact that must be postulated, just as we do for other "mysterious" components of actuality—say, space or matter.

But even with this postulate we cannot explain why the room left by the

[12] The first point follows directly from the simple enunciation of the law, the second from the fact that the entropy is only an average index of the distribution of the total energy within a system.

[13] In addition to the two laws already mentioned, there is only one other fundamental law of thermodynamics, Nernst's Law, which in essence says that the minimum of entropy is not achievable in actuality.

entropic indeterminateness is filled with numberless species and varieties instead of one single form. For the material structure of any living being must obey not only the laws of thermodynamics but also every other law of inert matter. And if we look beyond thermodynamics we see, first, that Classical mechanics leaves nothing indeterminate, and second, that the freedom allowed by quantum mechanics is limited only to random, not to permanent, variations. It would seem, therefore, that the variability of living creatures is still a puzzle. Yet the puzzle has a solution, which is provided by a fundamental, albeit unremarked, principle: *the emergence of novelty by combination.*

The meaning of this principle is as simple as it is unmistakable. Most of the properties of water, for example, are not deducible by some universal principles from the elemental properties of its components, oxygen and hydrogen; with respect to the latter properties, the former are therefore novel. The principle is at work everywhere with a degree of diversity that increases constantly from the physics of the atom in the inorganic field to the social forms in the superorganic domain. In view of all this, the oft quoted statement that "living organisms are the greatly magnified expressions of the molecules that compose them"[14] appears as one of the most inept slogans of the aggressive scholarship for which this half of the century will pass down into history. If the statement were true, then also a molecule should be only the expression of the elementary particles that compose it, and a society the expression of the biological organisms of its members. Telescoping all this, we reach the conclusion that societies, organisms, molecules, and atoms are only the expressions of elementary particles. But then, one should not study biomolecules either. One should study only elementary particles by themselves!

Of course, we should study molecules, not only those of organisms but wherever we find them. But, at the same time, we should not fail to see that, because of the novelty created by combination, the properties of molecules qua molecules cannot enable us to know how organisms, too, behave or, more generally, how a molecule will behave in relation to any other molecule. For one of the numerous topical examples: did the study of thalidomide by itself at the molecular level enable us to foresee the novelties produced by that substance in contact with every kind of molecule of the human organism? Science is not served if we do not recognize

[14] The original statement is in George Wald, "Phylogeny and Ontogeny at the Molecular Level," in *Proceedings of the Fifth International Congress of Biochemistry*, vol. III, *Evolutionary Biochemistry*, ed. A. I. Oparin (New York, 1963), p. 12. I should hasten to add that perhaps Wald himself does not embrace it wholly. Witness, as one example, his statement that "It is the bargain that the whole organism strikes with its environment, in competition with its neighbors, that decides its fate; and that fate is then shared by its molecules, including its genetic DNA." *Ibid.*, p. 13.

that the properties of an electron (or of any of the manifold elementary particles) must include every property of a material structure, inert or living. The basis of knowledge cannot be reduced to either the whole alone or to the parts by themselves.[15] The biologist must study molecules and cells and organisms, just as the economist must study the economic units and the entire economies.

Even though the relevance of the two principles just outlined—the entropic indeterminateness and the novelty by combination—is far greater for the world of life phenomena than for that of mere matter, we must not forget that their roots are in the last phenomenal domain. It is all the more interesting, therefore, that these principles inevitably invite us to take a new look at some other issues which are generally regarded as spuriously generated by the biologists and social scientists of the so-called romantic school.

One such issue is the myth that science is measurement, that beyond the limits of theory there is no knowing at all. "Theory" is here taken in its discriminating meaning: a filing of all descriptive propositions within a domain in such a way that every proposition is derived by Logic (in the narrow, Aristotelian sense) from a few propositions which form the logical foundation of that science. Such a separation of all propositions into "postulates" and "theorems" obviously requires that they should be amenable to logical sifting. And the rub is that Logic can handle only a very restricted class of concepts, to which I shall refer as *arithmomorphic* for the good reason that every one of them is as discretely distinct as a single number in relation to the infinity of all others. Most of our thoughts, however, are concerned with forms and qualities. And practically every form (say, a leaf) and every quality (say, being reasonable) are *dialectical* concepts, i.e., such that each concept and its opposite overlap over a contourless penumbra of varying breadth.

The book of the universe simply is not written as Galileo claimed—only "in the language of mathematics, and its characters are triangles, circles, and other geometrical figures."[16] In the book of physics itself we find the most edifying dialectical concept of all: probability. And no book about the phenomena of life can dispense with such basic yet dialectical concepts as species, want, industry, workable competition, democracy, and so on. It would be, I maintain, the acme of absurdity to decree that no such book be written at all or, if it is written, that it simply disseminates nonsense.

[15] Wald's statement quoted in the preceding note illustrates this point splendidly.
[16] Galileo Galilei, *Il Saggiatore*, in *The Controversy on the Comets*, trs. S. Drake and C. D. O'Malley (Philadelphia, 1960), p. 184.

Lest this position is misinterpreted again by some casual reader, let me repeat that my point is *not* that arithmetization of science is undesirable. Whenever arithmetization can be worked out, its merits are above all words of praise. My point is that wholesale arithmetization is impossible, that there is valid knowledge even without arithmetization, and that mock arithmetization is dangerous if peddled as genuine.

Let us also note that arithmetization alone does not warrant that a theoretical edifice is apt and suitable. As evidenced by chemistry—a science in which most attributes are quantifiable, hence, arithmomorphic—novelty by combination constitutes an even greater blow to the creed "no science without theory." A theoretical edifice of chemistry would have to consist of an enormous foundation supporting a small superstructure and would thus be utterly futile. For the only *raison d'être* of theory is economy of thought, and this economy requires, on the contrary, an immense superstructure resting on a minute foundation.

Still another issue that becomes immediately salient against the background sketched so far is that of determinism, which interests us here because of its bearing upon the power of science to predict and manipulate.

For some time now, physicists have been telling us that an atom of radium explodes, not when something causes it to do so, but when it likes. However, the complete story is that the frequency of explosions has a *dialectical* stability and this stability enables us to predict at least the behavior of radium in bulk. The point is that the strongest limitation to our power to predict comes from the entropic indeterminateness and, especially, from the emergence of novelty by combination. These are the most important reasons why our prehensions of nature cannot be reduced to the efficient cause as we know it from Aristotle.

In the case of novelty by combination (of contemporaneous or consecutive elements), things simply happen, without either a *causa efficiens* or a *causa finalis*. What is more, the most numerous and basic elements of our knowledge belong to this category. Their truth can be justified by repeated observations, not by ratiocination, nor by relating them to a purpose. Naturally, an intelligent being who has never witnessed oxygen and hydrogen combining into a substance having the properties of water would regard that reaction as somewhat of a mystery after he is confronted with it once only. By the same token, evolution appears so mysterious to us only because man is denied the power of observing other planets being born, evolving, and dying away. And it is because of this denial that no social scientist can possibly predict through what kinds of social organizations mankind will pass in its future. To be sure, our knowledge constantly advances, but at any one time it can encompass only part of the Whole. Moreover, this advance is such that multifarious new questions grow out of every solved problem.

In this situation, we must not insist on asking always "why." For some problems we may achieve a greater insight if we ask "for what purpose." Even biologists bent on avoiding anything that might smack of vitalism admit that there is some advantage in classifying some biological phenomena as quasi finalistic. But this verbalist legerdemain may do only for other species than man. Man knows (and by the most direct way) that a *causa finalis*, not a *causa efficiens*, makes him work for an academic degree or save for old age. To deny that man, in his deliberate actions, is animated by a purpose would be a flight from truth. The recurrent writer who announces that his purpose is to prove that the concept of purpose is a bogey constitutes—as Whitehead amusingly observed—a highly interesting subject of study.

Actually, the sorry plight of the student of a contemporary society may be mitigated only by an empathic interpretation of its propensities and its mood, a task that cannot be delegated to any instrument. Only a human mind can find out what other men feel and what their purposes are. And only in this way can a student determine at least the broad direction of the immediate social trend.

The verdict is indisputable: no social science can subserve the art of government as efficaciously as physics does the art of space travel, for example. Nevertheless, some social scientists simply refuse to reconcile themselves to this verdict and, apparently in despair, have come out with a curious proposal: to devise means which will compel people to behave the way "we" want, so that "our" predictions will always come true. The project, in which we recognize the continual striving for a "rational" society beginning with Plato's, cannot succeed (not even under physical coercion, for a long time) simply because of its blatant *petitio principii:* the first prerequisite of any plan is that the behavior of the material involved should be completely predictable, at least for some appreciable period.

But aggressive scholarship will never run out of new plans for the "betterment of mankind." Since the difficulties of making an *old* society behave as we want it can no longer be concealed, why not produce a *new* society according to our own "rational" plans? Some molecular biologists even assure us that our ability to produce "Einsteins from cuttings" is around the corner. But they close their eyes to many elementary obstacles, among which are the supercosmic dimensions of some aspects of the problem and the novelty by combination. Most interesting of all, they do not even seem to suspect that a society made only of geniuses, nay, of people fit only for an intellectual occupation, could not live even for one day. On the other hand, if the man-made society includes also a "productive" class, the inevitable social conflict between the two classes will stop that society from being "rational" (unless the same biological wizards can remodel the human species after the genetic pattern of the social insects).

Many an economist has indirectly alluded to the First Law of Thermodynamics by noting that man can produce neither matter nor energy. But even Irving Fisher—who was first a pupil of J. Willard Gibbs, one of the founders of statistical thermodynamics—did not perceive that the Entropy Law is still more important for the economic process. One of the pioneers of econometrics, Harold T. Davis, seems to be alone in seeking to establish a formal similarity between the fundamental thermodynamic equations and some equations used in economic models. He considered the budget equations of macroanalysis and suggested that the utility of money represents economic entropy.[17] But as J. H. C. Lisman noted later in commenting on Davis' solitary attempt,[18] none of the variables used in the mathematical economic models seems to play the same role as entropy in thermodynamics. In the light of the ideas developed in the preceding pages, this conclusion is inevitable: in a mechanical analogue nothing could correspond to the concept that opposes thermodynamics to mechanics.

Instead of looking for a thermodynamic homology in the usual mathematical systems of economics, we may now try to represent the economic process by a new system of equations patterned after that of thermodynamics. In principle, we can indeed write the equations of any given production or consumption process (if not in all technical details at least in a global form). Next, we may either assemble all these equations into a gigantic system or aggregate them into a more manageable one. But to write any set of the initial equations, we must know the exact nature of the individual process to which it refers. And the rub is that in the long run or even in the not too long run the economic (as well as the biological) process is inevitably dominated by a qualitative change which cannot be known in advance. Life must rely on novel mutations if it is to continue its existence in an environment which it changes continuously and irrevocably. So, no system of equations can describe the development of an evolutionary process. If it were not so, biologists (who have long since put thermodynamics to good work) would have already come out with a vast system to represent the course of the biological process until doomsday.

The representation of a *given* production or consumption process by its

[17] Harold T. Davis, *The Theory of Econometrics* (Bloomington, 1941), pp. 171–176.

[18] J. H. C. Lisman, "Econometrics and Thermodynamics: A Remark on Davis' Theory of Budgets," *Econometrica*, XVII (1949), 59–62.

thermodynamic system may aid an engineer, perhaps a management expert as well, in deciding which process may be more efficient in entropic terms. But the way in which the acknowledgment of the entropic nature of the economic process may enlighten the economist as a student of man is not through a mathematical system which reduces everything to entropy. Man, we should not forget, struggles for entropy but not for just any form of it. No man can use the low entropy of poisonous mushrooms and not all men struggle for that contained in seaweed or beetles.

Nor does the intimate connection between the Entropy Law and the economic process aid us in managing a *given* economy better. What it does is, in my opinion, much more important. By improving and broadening our understanding of the economic process, it may teach to anyone willing to listen what aims are better for the economy of mankind.

The simple fact that from the purely physical viewpoint the economic process is not a mechanical analogue forces upon us a thorny question of fundamental importance for science in general. What is "process" and how can we represent it analytically? The answer uncovers some unsuspected omissions in both Neoclassical and Marxist analyses of production. It also enables us to arrive at an equation of value (we should rather say "quasi equation") against which we can project, compare, and evaluate all doctrines of value propounded so far. This equation settles some points of the controversy-torn problem of value.

Since the economic process materially consists of a transformation of low entropy into high entropy, i.e., into waste, and since this transformation is irrevocable, natural resources must necessarily represent one part of the notion of economic value. And because the economic process is not automatic, but willed, the services of all agents, human or material, also belong to the same facet of that notion. For the other facet, we should note that it would be utterly absurd to think that the economic process exists only for producing waste. The irrefutable conclusion is that the true product of that process is an immaterial flux, the enjoyment of life. This flux constitutes the second facet of economic value. Labor, through its drudgery, only tends to diminish the intensity of this flux, just as a higher rate of consumption tends to increase it.

And paradoxical though it may seem, it is the Entropy Law, a law of elementary matter, that leaves us no choice but to recognize the role of the cultural tradition in the economic process. The dissipation of energy, as that law proclaims, goes on automatically everywhere. This is precisely why the entropy reversal as seen in every line of production bears the indelible hallmark of purposive activity. And the way this activity is planned and performed certainly depends upon the cultural matrix of the society in question. There is no other way to account for the intriguing differences between some developed nations endowed with a poor environment, on the one hand, and some underdeveloped ones surrounded by an abundance of natural riches. The exosomatic evolution works its way through the cultural tradition, not only through technological knowledge.

The Entropy Law does not help an economist to say what precisely will happen tomorrow, next year, or a few years hence. Like the aging of an organism, the working of the Entropy Law through the economic process is relatively slow but it never ceases. So, its effect makes itself visible only by accumulation over long periods. Thousands of years of sheep grazing elapsed before the exhaustion of the soil in the steppes of Eurasia led to the Great Migration. The Entropy Law enables us to perceive that a development of the same nature and of far greater consequences is running its full course now. Because of the pressure of population on agricultural land the area of which cannot be appreciably increased, man can no longer share the agricultural low entropy with his traditional companions of work, the beasts of burden. This fact is the most important reason why mechanization of agriculture must spread into one corner of the world after another, at least for a long time to come.

The Entropy Law also brings to the fore some fundamental yet ignored aspects of the two problems that now preoccupy the governed, the governments, and practically every scientist: pollution and the continuous increase of population.

It is natural that the appearance of pollution should have taken by surprise an economic science which has delighted in playing around with all kinds of mechanistic models. Curiously, even after the event economics gives no signs of acknowledging the role of natural resources in the economic process. Economists still do not seem to realize that, since the product of the economic process is waste, waste is an inevitable result of that process and *ceteris paribus* increases in greater proportion than the intensity of economic activity. That is why at this time pollution does not plague Tibet or Afghanistan, for instance. Had economics recognized the entropic nature of the economic process, it might have been able to warn its co-workers for the betterment of mankind—the technological sciences—that "bigger and better" washing machines, automobiles, and superjets must lead to "bigger and better" pollution. When contemporary scientists gather in symposia for finding a solution to the impasse, they do little besides blaming their predecessors for too aggressive a scholarship and too narrow a foresight. The future being for us as unpredictable as it is, one may only wonder what the future scientists will have to say about the aggressiveness and the foresight of the present generation.

The most extremist views of the literary group of Vanderbilt Fugitives, many of whom decried the effects of modern technology on the pastoral life of the countryside, would simply pale in comparison with those professed now by some members of the rising class of pollution experts. Other members seem to think that, on the contrary, mankind can simply get rid of pollution without any cost in low entropy provided we use only pollutionless industrial techniques—an idea that betrays the belief in the possibility of bootlegging entropy of which I spoke earlier. The problem of pollution is one of very, very long run and intimately connected with the way mankind is going to make use of the low entropy within its reach. It is this last problem that is the true problem of population.

It is fashionable nowadays to indulge in estimating how large a population our earth can support. Some estimates are as low as five billions, others as high as forty-five billions.[19] However, given the entropic nature of the economic process by which the human species maintains itself, this is not the proper way to look at the problem of population. Perhaps the earth can support even forty-five billion people, but certainly not ad infinitum. We should therefore ask "*how long* can the earth maintain a population of forty-five billion people?" And if the answer is, say, one thousand years, we still have to ask "what will happen *thereafter*?" All this shows that even the concept of optimum population conceived as an ecologically determined coordinate has only an artificial value.

There are even some dangers for the human species in narrowing the problem of population to how large a population can be maintained by A.D. 2000 or at any other time. The issue of population extends beyond A.D. 2000. Moreover, to have a maximum population at all times is definitely not in the interest of our species. The population problem, stripped of all value considerations, concerns not the parochial maximum, but the maximum of life quantity that can be supported by man's natural dowry until its complete exhaustion. For the occasion, life quantity may be simply defined as the sum of the years lived by *all* individuals, present and future.[20] Man's natural dowry, as we all know, consists of two essentially distinct elements: (1) the *stock* of low entropy on or within the globe, and (2) the *flow* of solar energy, which slowly but steadily diminishes in intensity with the entropic degradation of the sun. But the crucial point for the popula-

[19] To my knowledge, forty-five billions is the highest figure ever mentioned as a possible size of the world population. Its propounder is Colin Clark. See his "Agricultural Productivity in Relation to Population," in *Man and His Future*, ed. G. Wolstenholme (Boston, 1963), p. 35.

[20] It may be well to note that this total is independent, first, of when each individual lives, and second, of whether the same number of years are lived by one or several individuals. What individual average life span is optimal constitutes one of the many subsidiary issues.

tion problem as well as for any reasonable speculations about the future exosomatic evolution of mankind is the relative importance of these two elements. For, as surprising as it may seem, the entire stock of natural resources is not worth more than a few days of sunlight!

If we abstract from other causes that may knell the death bell of the human species, it is clear that natural resources represent the limitative factor as concerns the life span of that species. Man's existence is now irrevocably tied to the use of exosomatic instruments and hence to the use of natural resources just as it is tied to the use of his lungs and of air in breathing, for example. We need no elaborated argument to see that the maximum of life quantity requires the minimum rate of natural resources depletion. By using these resources too quickly, man throws away that part of solar energy that will still be reaching the earth for a long time after he has departed. And everything man has done during the last two hundred years or so puts him in the position of a fantastic spendthrift. There can be no doubt about it: any use of the natural resources for the satisfaction of nonvital needs means a smaller quantity of life in the future.[21] If we understand well the problem, the best use of our iron resources is to produce plows or harrows as they are needed, not Rolls Royces, not even agricultural tractors.

The realization of these truths will not make man willing to become less impatient and less prone to hollow wants. Only the direst necessity can constrain him to behave differently. But the truth may make us foresee and understand the possibility that mankind may find itself again in the situation in which it will find it advantageous to use beasts of burden because they work on solar energy instead of the earth's resources. It also exposes the futility of the human pride that overcame some scholars on learning that by A.D. 2000 we may be able to feed people with proteins derived from crude oil and thus solve the population problem completely and forever. Highly probable though this conversion is, we can rest assured that sometime, perhaps sooner than one may think, man will have to reorient his technology in the opposite direction—to obtain gasoline from corn, if he will still be around and using internal combustion engines. In a different way than in the past, man will have to return to the idea that his existence is a free gift of the sun.

[21] The distinction between vital and nonvital needs—I hasten to admit with pleasure—is a dialectical one. Certainly, to plow a corn field is a vital need, but to drive a Rolls Royce, not.

CHAPTER I *Science: A Brief Evolutionary Analysis*

1. The Genesis of Science. We can look at science from several viewpoints, for science is "a many splendored thing." However, science has not been in all places and at all times as we know it today. Nor has its modern form come to us by fiat as some specific commandments revealed in the shortness of a single blink to all men in every part of the globe. Science had a genesis and an evolution in the sense in which these terms are used in biology. The more we ponder how science has radically changed over the last three or four centuries, the more obvious it becomes that science is a living organism. This being so, we should not be surprised that every attempt to define it by one single trait has failed.

To proceed systematically, I shall search first for the reason why science came to be, that is, for its *causa efficiens* (in the Aristotelian sense). From what we can infer, this cause was the instinct of exploring the environment, an instinct man shares with all other animals. Here and there, some tribes came to realize, first, that knowledge gives controlling power over the environment (unfortunately, over men as well) and consequently makes life easier for him who possesses it; and second, that learning what others already know is far more economical than acquiring this knowledge by one's own experience. It was then that man began to value the aggregate knowledge of all individuals in the community and feel the need of storing and preserving this knowledge from generation to generation. Science, in its first form, came thus into being.

It is clear then that the *causa materialis* (again, in the Aristotelian sense) of science is stored communal knowledge, that is, the body of all descriptive propositions available to any member of a community and believed to be true according to the criteria of validity prevailing at the time of reference. To take this equation as a definition of science would be patently inept. On the other hand, we must agree that the equation is valid for all times and places, from the earliest cultures to those of today. Furthermore, the point disposes of the view that science is the opposite of description. On the contrary, science cannot exist without description.[1]

Furthermore, the equation set forth in the preceding paragraph applies not only to sciences of fact—like physics or political science, for instance—but also to sciences of essence, i.e., to mathematics and Logic.[2] Indeed, "p implies q and q implies r yields p implies r" is just as much a descriptive proposition as "an acid and a base yield a salt." Both propositions represent *gained* knowledge and, hence, their meaning is apt to change as this knowledge increases. By now we know that sciences of essence too have the privilege of discovering that not all swans are white. Bernhard Bolzano was perfectly right in cautioning us, more than a hundred years ago, that many a fresh discovery remained to be made in logic.[3] Only the knowledge at which every individual *inevitably* arrives—such as "I am not you," for instance—does not change with time. Nor do such propositions form the *causa materialis* of a science.

2. Evolution by Mutations. As already intimated, the animal instinct of learning did not suffice for a community to develop science: the community had also to develop the utilitarian instinct to an appreciable degree so as to become conscious of the utility of storing all communal knowledge. There are examples of tribes which have survived to modern times and which have not developed science precisely because of their weak utilitarian instinct. This deficiency is responsible also for other cultural patterns that are common to these communities and which seem to us equally puzzling. We must observe also that the survival of scienceless communities to our own time is due exclusively to their accidental isolation from others. For, otherwise, natural selection—as any Darwinist will instruct us—would have seen to their history's ultimately being brought to an end by the onslaught of other tribes which could put science

[1] Cf. P. W. Bridgman, *The Logic of Modern Physics* (New York, 1928), p. 175.

[2] That fact as well as essence constitute the object of description and, hence, of science represents the viewpoint of Edmund Husserl, *Ideas: General Introduction to Pure Phenomenology* (New York, 1931), pp. 61 ff. The difference is that, instead of describing facts, the sciences of essence describe the manners in which the human mind apprehends, classifies, and relates facts. Or, if you wish, mathematics studies objects stripped of all particular qualities and Logic studies propositions stripped of all factual content.

[3] Bernhard Bolzano, *Paradoxes of the Infinite* (New Haven, 1950), p. 42. See also P. W. Bridgman, *The Intelligent Individual and Society* (New York, 1938), p. 98.

in the service of war. History shows that even differences in the level of factual knowledge play a paramount, if not decisive, role in the struggle between human societies. One can hardly doubt that had the European nations not acquired a vastly superior amount of factual knowledge in comparison with the rest of the world European colonialism would not have come about. In all probability China or India would have colonized the world, including Europe, if the Asian civilizations had first achieved this superiority.

Though the causes that could account for the birth of science seem to be the same everywhere, the evolution of science did not follow everywhere the same pattern. We may, with Veblen, impute the subsequent expansion and transformation of primitive science to the instinct of *idle curiosity*. But if we do, we must also admit that this instinct is not an innate one, as the instinct of learning is. This admission seems inevitable in view of the entirely different evolution of science in various parts of the world. The instinct of idle curiosity undoubtedly represents a later accidental mutation, which like any successful mutation was gradually diffused to larger and larger groups.

3. Memory: The Earliest Store of Knowledge. The problem of storing and preserving knowledge soon led to the profession of scholars and to the institution of teaching. As long as the list of descriptive propositions remained relatively short, memorizing it provided the easiest mode of storage. This mode was also perfect in the sense that it permitted almost instantaneous access to every bit of extant knowledge. For a long time, therefore, good memory was the only required ability of a scholar; it thus came to be regarded as one of the most valuable virtues of a people.[4]

Ultimately a point was reached when the memory of even the ablest individual could no longer serve as a filing cabinet for the growing amount of knowledge. Nonhuman cabinets had to be invented lest knowledge be lost. The impasse was resolved fortunately by the invention of writing and papyri. But as knowledge still continued to expand, a new and most troublesome problem arose: how to file countless propositions so as to find the one needed without having to search through the whole cabinet. Though we do not find the problem stated in these precise terms, we can nevertheless understand that the need must have continuously irritated the minds of the learned. They first fell upon the idea of taxonomic filing, as witnessed by the earliest codes of moral or legal conduct. However, good taxonomic filing in turn requires a readily applicable criterion, such as the chronological order for filing historical facts. At least one—probably

[4] Plato, *Phaedrus*, 274–275, relates that a famous Egyptian king deplored the invention of writing because it would induce people to pay less attention to the training of memory.

the only one—of the known cultures, namely that of Ancient Greece, thus came to talk about classification and to debate its delicate issues hotly. Plato, for instance, argued that dichotomy is the rational principle of classification. Aristotle strongly disagreed with this, rightly pointing out that in most cases dichotomy is "either impossible or futile."[5]

Classification as a filing system has survived to this very day for the simple reason that we still have to file a great deal of our factual knowledge taxonomically. This is true not only for biology but also for the highest realm of physics: physicists are now preoccupied with classifying the ever growing number of different intra-atomic particles.[6] It seems that the commandment formulated by Georges Cuvier, "*nommer, classer, décrire,*" has a lasting value, even though the three commands cannot always be executed separately or in that order. Unfortunately, the basic issues of classification too have survived unresolved and still torment the scholarly world from the biological taxonomist to the logician. For indeed, most logical paradoxes—from that of "the Cretan who says that all Cretans are liars" to Russell's "class of all classes"—grow out of classification.[7]

4. From Taxonomic to Logical Filing. The search for a universal principle of classification caused the Greek philosophers to inquire into the nature of notions and their relationship. Out of these intellectual labors, Logic was born. This marked the end of a prolonged and diffused process. Logical proofs of geometrical propositions were used as far back as the beginning of the sixth century B.C. Yet even Plato, Aristotle's teacher, had no idea of syllogism. He did talk about scientific propositions following from some basic truths, but a clear picture of the logical edifice of knowledge did not appear before Aristotle.[8] And the important fact is that even Aristotle himself was inspired by some *Elements of Geometry* which existed in his time and have come down to us in highly polished form from the hands of Euclid.[9] Time and again, the coming into being of a thing—in this instance the first theoretical science—preceded its conceptual description.

It goes without saying that the theoretical edifice of geometry—in its etymological meaning—was not erected in one day. And since no one had a definite idea of what the final result was going to be, its bricklayers must have been guided by other purposes. The abstract thinkers, in the characteristic tradition of Greek thought, were searching for some First Principle.

[5] Plato, *Sophist*, 219, 253, *Statesman*, *passim*; Aristotle, *De Partibus Animalium*, I. 2–4.

[6] Cf. Louis de Broglie, *New Perspectives in Physics* (New York, 1962), p. 35; David Bohm, *Causality and Chance in Modern Physics* (London, 1957), pp. 122 f.

[7] The above point is admirably brought into focus by Henri Poincaré, *Mathematics and Science: Last Essays* (New York, 1963), pp. 45–55.

[8] Plato, *Republic*, VII. 533; Aristotle, *Analytica Posteriora*, I. 1–6.

[9] Cf. W. D. Ross, *Aristotle* (3rd edn., London, 1937), p. 44.

On the other hand, we may plausibly surmise that the *arpedonapts*, the land surveyors in ancient Egypt, must have sooner or later observed that once one can remember, for instance, that

A. *The sum of the angles in a triangle is two right angles,*

one need not memorize also the proposition

B. *The sum of the angles in a convex quadrangle is four right angles.*

Thus *arpedonapts* came to use, however unawares, the logical algorithm long before the first *Elements of Geometry* was written, simply because the device saved them memorizing efforts. Without this economical aspect, the logical algorithm would have in all probability remained a notion as esoteric as the First Cause, for example.

Today the relationship between the logical algorithm and theoretical science seems simple. By logical sorting, all propositions, $P_1, P_2, \ldots, P_n$, already established in any particular field of knowledge can be separated into two classes (α) and (β), such that

(1) *every β-proposition follows logically from some α-propositions,* and
(2) *no α-proposition follows from some other α-propositions.*[10]

This logical sorting represents the inner mechanism by which a scientific theory is constructed and maintained. Theoretical science, therefore, is a catalog which lists known propositions in a logical—as distinct from taxonomic or lexicographic—order. In other words, we have a first equation

"Theoretical science" = "Logically ordered description."

Actually, the logical economy does not always stop here. Often some speculative propositions are "thought up" and added to (α) with a view of shifting as many α-propositions to (β). Thus, (α) is replaced by (ω), the latter having the same properties and the same relation with the new (β) as (α) has had. The only difference is that (ω) contains some unobservable propositions, i.e., some first principles. But this does not affect the validity of the equation written above.[11]

5. Theoretical Science and Economy of Thought. By filing knowledge logically we do not increase it; we only carry the economic advantage of the logical algorithm to its utmost limits. Clearly, the ω-propositions of any individual science contain, explicitly or implicitly, the entire extant

[10] For (β) not to be a null set, the propositions $P_1, P_2, \ldots, P_n$ must not be entirely circular. For instance, our factual knowledge should not consist only of: Lightning is light; Light is electricity; Electricity is lightning. This necessity may account for the traditional aversion men of science display for circular arguments.

[11] Since in current usage "theoretical science" and, especially, "theory" have very elastic meanings, to spare the reader possible difficulties later on I want to stress the point that throughout the book "theoretical science" is used in the sense defined by that equation.

knowledge in a particular domain. Strictly speaking, therefore, to store all that is already known in a domain we need only to memorize (ω), i.e., what we currently call the logical foundation of the respective science. To be sure, a scholar normally memorizes some β-propositions as well but only because he finds it convenient to have immediate access to those propositions most frequently needed in the daily exercise of his profession. The highly important point is that, although the volume of factual information has expanded continuously, its cumber has mattered less each day precisely because of the growing number of the domains—however fragmentary—that have been brought under the power of theoretical understanding. As P. B. Medawar nicely put it, "in all sciences we are progressively relieved of the burden of singular instances, the tyranny of the particular. We need no longer record the fall of every apple."[12]

The Greek philosophers may appear to have been preoccupied with ethereally abstract issues and pragmatically idle problems. But in the deep waters of their intellectual struggle there was the need for a classification of knowledge in a form that could be grasped by one individual mind. The heroes of the battle might not have been aware of the economic implications of this need, nor always of the need itself, just as no one seems to have paid any attention to the immense economy brought about by the change from ideographic to alphabetical writings, either when the change happened or, still less, before the event. Generally speaking, needs generated by evolution guide us silently, as it were; seldom, if ever, are we aware of their influence upon our complex activity (or even of their existence). Only after a very long time do we realize why we labored and what we searched for. Only after the event can we say with Oswald Spengler that "a task that historic necessity has set *will* be accomplished with the individual or against him."[13]

It is not surprising, therefore, that the economic aspect of theoretical science remained completely unnoticed until 1872 when Ernst Mach first argued that science "is experience, arranged in economical order."[14]

[12] Quoted in *The Listener*, May 18, 1967, p. 647.

[13] Oswald Spengler, *The Decline of the West* (2 vols., New York, 1928), II, 507. Parenthetically, but apropos, one may speculate that the present space programs might at some distant future prove to have corresponded to the need of taking care of an exploding population.

[14] Ernst Mach, *Popular Scientific Lectures* (Chicago, 1895), p. 197 and *passim*. See also his *The Science of Mechanics* (La Salle, Ill., 1942), pp. 578–596. The same idea was elaborated with much greater insight by Karl Pearson, *The Grammar of Science* (Everyman's Library edn., London, 1937), pp. 21, 32, and *passim*.

Mach, however, made little if anything out of *logical* order. Rather, he emphasized the disburdening of memory through numerical tables and mathematical symbolism. However, ephemerides existed long before mechanics became a theoretical science; and the multiplication table has always been only a mnemonic. The economy of thought yielded by tables and symbols should be attributed to the invention of writing rather than to anything else.

To speak of the economy of thought achieved through theoretical science we must first show that memorizing is a costlier intellectual effort than ratiocination. Certainly, this does not seem true for an overwhelming majority of humans: even university students in appreciable numbers prefer descriptive courses where knowledge being presented taxonomically has to be memorized rather than logically sorted. Besides, memory and ratiocination are abilities that training can improve; training, on the other hand, may place the accent on one or the other depending upon the cultural tradition. For years on end the memory of Chinese and Japanese students has been subject to a continuous training unparalleled in the West; this will continue as long as they have to learn by heart thousands of ideographic characters. Yet, in the end, even Chinese and Japanese scholars had to succumb to the pressure on memory. Nowadays no one, however narrow is his chosen field, can dream of memorizing the vast amount of factual knowledge, just as no one can hope to reach the moon in an ordinary balloon. By memorizing only a part of factual knowledge one can succeed as a craftsman, but certainly not as a scholar.

But in evolution nothing is general or definitive. Thinking beings from other solar systems may have their brains so constructed that for them memory is a relatively free factor; for such beings theoretical science might very well be uneconomical. On the other hand, we must expect even the structure of science on this planet to change with time. We can already catch some glimpses, however faint, of its next mutation after the electronic brains will have completely taken over memorizing, remembering everything learned, sorting, and calculating—operations which they can perform with fantastic speed and on a far larger scale than the human mind.

We must not, however, take it for granted that the human mind freed from these chores will necessarily be able to exercise its creative prerogatives—to discover regularities of fact and fiction, to think up new concepts by which to synthesize apparently diversified facts into a single logical foundation, to formulate and prove propositions valid for an infinity of situations, and so on—with greater ease and efficiency.[15] From what is happening around us at present, the human mind seems rather to sink

[15] More on this in Chapter III, Section 10, below. But one widespread fallacy concerning the superiority of computers had better be mentioned now. The brain's memory capacity, scarce though it is (as I have said), is relatively far larger than the computer's. If nevertheless we have the opposite impression, it is only because we ignore how much the brain has to memorize so that the individual should not be lost in his own life as a newcomer in a metropolis. A man has to remember, at least for a while, even what he had for breakfast and how it tasted. This is why his brain is so constructed that it finds it silly to memorize a whole table of logarithms. Not so the computer.

under the immense tide of results produced with electronic speed by the army of computing machines. Is it not already true—at least in the field of economics—that practically no synthetic work has been achieved with the thousands of Ph.D. dissertations which, since the advent of the computer centers, have dealt with still another particular? It is hard to see how even an Adam Smith or a Karl Marx could find their way in such a jungle of so-called analyzed facts. And, as revealed recently by a reputed research director, the situation may not be different across the fence: a prominent organization suddenly realized that "their large program of 'applied' research has proved ineffective in advancing their field of responsibility [and decided] to inaugurate a multimillion-dollar program of 'basic' research."[16] It thus appears that the computer, just by being there, induces each one of us to record the fall of still another apple. Besides, even these records tend to become increasingly spurious, for the easy access to a computer center leads many students to pay even less attention than before to the appropriateness of the statistical tools used in testing their particular models.[17]

But the greatest danger of an age in which dubbing the fall of every apple with a colossal system of equations passes for the hallmark of scholarship and a good day's work means to transfer one's problem to a computer is that even the potential Newtons are discouraged from becoming interested in synthesis. The observation of Ruth Davis that nowadays there is more effort going on in the improvement of computers than in the improvement of the human mind must have been ill received by the other members of the Washington Symposium on Computer Augmentation of Human Reasoning.[18] The temper of this age is not disposed to listen to truths such as this. The result is that science seems now involved in a vicious circle. Dearth of synthetical work causes us to believe that we have not analyzed a sufficient number of facts. And by providing more particular facts, we only render the task of synthesis increasingly more difficult. Think of it: the journal *Chemical Abstracts* for the year 1949 (when the cumber must have been less crushing than today) contained not less than 70,000 papers which reported on 220,000 scientific items. Pauling estimates that in all natural sciences together the number of

[16] J. R. Pierce, "When Is Research the Answer," *Science*, March 8, 1968, p. 1079. Pierce, who seems to be a strong believer in the supremacy of "applied" research, blames the natural scientists of that organization (which he does not name) for failing to do "their daily work well and thoughtfully." But, as I have just suggested, even a Newton might be unable to arrive at any synthesis under the flood of particulars.

[17] Cf. my paper "Further Thoughts on Corrado Gini's *Delusioni dell' econometria*," *Metron*, XXV (1966), 265–279.

[18] *Symposium on Computer Augmentation of Human Reasoning*, eds. Margo A. Sass and W. D. Wilkinson (Washington, 1965), p. 152.

"new scientific facts" reported each year may amount to one million![19] We can foresee some rejection convulsions, therefore, before science has absorbed completely the transplant of its own creation—the automata of all kinds.

6. Significant Differences between East and West. Facts described in a logical order form a texture entirely different from that of taxonomic description. We are therefore justified in saying that with Euclid's *Elements* the *causa materialis* of geometry underwent a radical transformation; from a more or less amorphous aggregate of propositions it acquired an *anatomic* structure. Geometry itself emerged as a living organism with its own physiology and teleology, as I shall presently explain. And this true mutation represents not only the most valuable contribution of the Greek civilization to human thought but also a momentous landmark in the evolution of mankind comparable only to the discovery of speech or writing.

Looking back at the developments of Greek thought one is tempted to conclude that the emergence of theoretical science was a normal, almost necessary, upshot of Logic. One could not be more mistaken. Both Indian and Chinese civilizations arrived at a logic of their own—in some respects even more refined than Aristotle's[20]—but neither came to realize its utility for classifying factual knowledge. As a result, science in the East never went beyond the taxonomic phase. Greek culture, therefore, must have had some peculiar feature which the East lacked: otherwise we could not account for the difference in the development of science in the East and the West.

It is not difficult to convince ourselves that the distinctive birthmark of Greek philosophy is the belief in a First Cause of a nondivine nature. As early as the beginning of the sixth century B.C., Thales, the scholar of many and diverse talents, taught that "water is the material cause of all things."[21] To discover the First Cause one must sooner or later come to inquire for the proximate cause. And indeed, only one generation after Thales, we find Anaximander explaining in a quite modern vein that the earth "stays where it is [held by nothing] because of its equal distance from everything."[22]

[19] Linus Pauling, "The Significance of Chemistry," *Frontiers in Science: A Survey*, ed. E. Hutchings, Jr. (New York, 1958), pp. 279 f.

[20] For instance, Oriental logic required that the premise of the syllogism should include an example so as to eliminate vacuous truth: "Where there is smoke, there is fire, as in the kitchen." (See Chan Wing-tsit, "The Spirit of Oriental Philosophy," in *Philosophy: East and West*, ed. Charles A. Moore, Princeton, 1944, p. 162.) However, the logic of the East developed mainly along highly dialectical lines. (Chan Wing-tsit, "The Story of Chinese Philosophy," in the same volume, pp. 41 ff.)

[21] J. Burnet, *Early Greek Philosophy* (4th edn., London, 1930), p. 47.

[22] *Ibid.*, p. 64.

Other civilizations may have arrived at the notions of cause and effect, but only that of Ancient Greece struck, and almost from the outset, on the idea of causality as a two-way algorithm: except the First Cause, everything has a cause as well as an effect. However, because of their paramount interest in the First Cause, the Greek thinkers focused their attention on cause rather than on effect.[23] As we know, Aristotle turned around the notion of cause until he discovered four forms of it.[24]

To search for the proximate cause, at least, came to be regarded as one of the noblest activities of the mind, second only to the search for the First Cause. Remembering facts—the Greeks held—is half-knowledge, i.e., mere *opinion*; true knowledge, i.e., *understanding*, includes also knowing *causa rerum*. This view had already gained such a strong ground by Plato's time that Aristotle had no difficulty in setting it up as an unquestionable dogma.[25] There is no exaggeration in saying that the distinctive feature of Greek thought was its obsessive preoccupation with "why?"

But this obsession still does not suffice by itself to explain the marriage of Logic and science in Greek thought. The marriage was possible because of one peculiar confusion: between "the why" and "the logical ground," that is, between *causa efficiens* and *causa formalis*. The symptom is obvious in Aristotle's bringing them together in his four types of causes,[26] and even more so in our using "explanation" in two distinct senses, each related to one of the *causae* just mentioned.[27] Had Logic by chance been applied first to constructing a theoretical science in a different field from geometry—where things neither move nor change, but merely are—the war now fought between logical positivists and realists would have very likely exploded soon after the first *Elements*.

As partakers of the Western mind we are apt to believe that causality represents, if not an *a priori* form in Kant's sense, at least one of the earliest notions inevitably grasped by the primeval man.[28] Yet the brute fact is that in contrast to Greek civilization the ancient cultures of Asia never developed the idea of causality.[29] It was thus impossible for them

[23] The first formulation on record of the principle of causality (by Leukippos, middle of the fifth century B.C.) speaks for itself: "Naught happens for nothing, but everything from a ground and of necessity." *Ibid.*, p. 340.

[24] *Physics*, II. 3; *Metaphysics*, I. 3, 10; V. 2.

[25] Cf. Plato, *Meno*, 81–86, *Theaetetus*, 201 ff; Aristotle, *Analytica Posteriora*, 78^a 23, 88^b 30, 94^a 20, *Physics*, 194^b 15.

[26] Indeed, Aristotle begins section iii of *Physics*, II, with the remark that there are "many senses [in which] 'because' may answer 'why.'" See also *Metaphysics*, 1013^b 3–4.

[27] Cf. John Stuart Mill, *A System of Logic* (8th edn., New York, 1874), pp. 332 ff.

[28] E.g., Mach, *Lectures*, p. 190.

[29] Cf. Junjiro Takakusu, "Buddhism as a Philosophy of 'Thusness,'" in *Philosophy: East and West*, already cited, pp. 74 ff.

to link the logical syllogism with the causal algorithm and organize factual knowledge theoretically. However, we cannot blame only the absence of theoretical science for the very well-known fact that over the last two millennia or so factual knowledge in the East progressed little, if at all, despite the substantial advance it had over the West at the outset.[30] Other factors as well counted heavily in the balance.

While in Greece philosophers were searching for the First Cause, in India, for instance, they were bending their efforts to discover the Absolute Essence behind the veil of Māyā. While the Greeks believed that truth is reached by ratiocination, the Indians held that truth is revealed through contemplation. Now, contemplation has some unquestionable merits: without it we could not arrive even at pure description, nor strike upon the interpretative fictions of modern science. But a purely contemplative mind, even if it may see things that "have completely eluded us,"[31] can hardly observe systematically the happenings of all sorts in the domain of natural phenomena; still less can such a mind think of devising and carrying out experiments. With the contemplative bent of the intellectual elite, the growth of factual knowledge remained in the East dependent solely upon the accidental discoveries made by the craftsman, whose mind is ordinarily less qualified than the scholar's to observe and evaluate.[32]

7. Theoretical Science: A Continuous Source of Experimental Suggestions. The last remarks raise a new question: had the Eastern scholars not shrunk from observing ordinary natural phenomena, could their factual knowledge have grown as much as that of the West? In other words, is theoretical science not only a more convenient storage of knowledge but also a more efficient instrument than crude empiricism in expanding knowledge? On this we often hear two contradictory views: that most revolutionary discoveries have been made accidentally, and that theory frees us from depending on accidental discoveries.

[30] Notice of the difference is not new: Cf. G. W. F. Hegel, *Lectures on the Philosophy of History* (London, 1888), pp. 141 ff.

[31] Cf. William Ernest Hocking, "Value of the Comparative Study of Philosophy," in *Philosophy: East and West*, p. 3.

[32] As I have already remarked, without theoretical science the storing of knowledge with easy access relies exclusively on good memory. This is not unrelated to the survival of ideographic writing in the Far East. We should also note that this survival constitutes a tremendous intellectual handicap: ideographic writing narrows the number of actual scholars and, further, wastes much of their intellectual energy. Nowadays it prohibits the use of typewriters and linotype machines, a far more general loss for the communities involved.

In the case of China the survival may have some justification: the multiplicity of dialects within an otherwise unitary culture. But the survival in Japan of a hybrid writing that defies any systematic rule whatever constitutes a puzzle which appears all the more intriguing in view of the development miracle achieved by the Japanese economy.

On closer examination, however, it is seen that the notion of entirely new factual knowledge completely divorced from accident is a contradiction in terms. Yet this does not mean that there is no way by which the probability of "lucky" accidents might be increased. Clearly, the advice "do not wait for accidents to happen but cause them by continuous experimenting" is excellent, but in following it we are bound to be confronted sooner or later with the problem of what experiment to undertake next. Were we entirely to depend upon imagination to furnish us with new experimenting suggestions, we would not be able to comply with the advice, for imagination is moody and often bears no fruit for long stretches of time. Is there a way out?

Let us observe that although the work of imagination is indispensable in the process of logical discovery, it is not so at all times. Logic, in all its forms, has some automatic rules which can keep the process moving for quite long strides without any outside aid. As this has been put, more often than not the tip of the pen displays an intelligence greater than the writer's. Besides, the road of logical inquiry always branches into so many directions that it is highly improbable that all ratiocinating processes should stall simultaneously because all have reached the stage where imagination is needed to start them running again. Consequently, new propositions can be derived from the logical foundation of a science without interruption. One physiological function of theoretical science is precisely the continuous derivation of *new* propositions, i.e., of propositions not already included in (β). As a result, laboratories are never short of new ideas to be tested experimentally and no total eclipse of the sun, for instance, occurs without immense experimental stir. It is clear then that the second economic advantage of theoretical science consists in the fact that experimental resources are always fully employed. And if a scholar is thus kept busy all day long, he might find even something for which he was not looking at all. A famous example is A. H. Becquerel, who discovered radioactivity while he was looking for a phenomenon of fluorescence in which he, wrongly, believed.

8. Theoretical Science and the Analytical Habit. At this juncture it is important to observe that the greatest strides in knowledge are made when a logically derived proposition is refuted by experiment, not when it is confirmed. We must then ask the pertinent question whether the full employment of experimental resources in testing logically derived propositions enhances the chance of such a lucky accident, i.e., of a real discovery. It is rather strange that the dogma of the rationality of reality, intended exclusively for proving the superiority of theoretical science, cannot help us in answering the question in the affirmative. For if reality is rational, the nearer science gets to it the greater is the probability that a logically derived proposition shall pass the experimental test. On the other hand, if we argue that the facts suffice to answer the question affirmatively, then we must forcibly conclude that reality is antirational, not merely irrational. The problem is extremely delicate.

It was the Eleatics who first propounded that "the thing that can be thought and that for the sake of which the thought exists is the same."[33] The dogma reached its apogee in the rationalist philosophy of the eighteenth century, when it could with immense pride invoke the new theoretical science, Newtonian mechanics, in its own support. But we know equally well that with almost every great discovery of the last hundred years rationalism has received a decisive blow.

Indeed, if reality is rational there can be no logical contradiction between any two factual propositions; in particular, the logical foundation of a science must be not only nonredundant—as warranted by the logical algorithm by which it is constructed—but also noncontradictory. However, contradictions have come up periodically in physics. To mention the most eloquent ones: (1) though in mechanics motion is indifferent to direction, heat moves only from the hotter to the colder body; (2) the electron appears at times as a localized particle, at others, as a wave filling the whole space.[34] Even Einstein, who categorically refused to renege the rationalist dogma, had to admit that "for the time being, . . . we do not possess any general theoretical basis for physics, which can be regarded as its logical foundation."[35] And since "for the time being" seemed to perpetuate itself, Niels Bohr proposed a new epistemological tenet known as the Principle of Complementarity: "Only the totality of the phenomena exhausts the possible information about the objects."[36] For example, two theories of the electron, a corpuscular and a wave theory—mutually contradictory but each noncontradictory within itself—must be accepted side by side: which one to use depends upon the particular phenomenon observed. Or as Bridgman put it more directly, "the only sort of theory possible is a partial theory of limited aspects of the whole."[37]

[33] From a fragment of *Parmenides* in Burnet, *Early Greek Philosophy*, p. 176.

[34] For further details see, for instance, R. E. Peierls, *The Laws of Nature* (London, 1957), pp. 152, 182, 246, and *passim*.

[35] Albert Einstein, *Out of My Later Years* (New York, 1950), p. 110; also p. 71. Max Planck, in *The Universe in the Light of Modern Physics* (New York, 1931), p. 94, is more categorical: relativity theory and quantum mechanics "are even antagonistic." For a fascinating account of some of the dramatis personae involved in this "conflict," see Broglie, *New Perspectives*, pp. 138–155.

[36] Niels Bohr, *Atomic Physics and Human Knowledge* (New York, 1958), pp. 40, 90. See also Werner Heisenberg, *Physics and Philosophy: The Revolution in Modern Science* (New York, 1958), pp. 162 f.

[37] P. W. Bridgman, *The Nature of Physical Theory* (Princeton, 1936), p. 118.

Thoughts such as these were long ago foreshadowed by Kant's teachings that "*the understanding does not draw its laws (a priori) from nature, but prescribes them to nature*," by which he meant that it is we who endow nature with rationality so that our rational minds may grasp it.[38] There is no wonder then that the more we learn about the behavior of nature, the more we discover how irrational nature is. The miracle—as Louis de Broglie observed—is that even a compartmental and limited concordance between natural phenomena and our mental representation of them is nevertheless possible.[39] By contrast, one should expect rationality to be the very scaffold of any science of man. Yet the Principle of Complementarity—as I argued some years ago—seems to be applicable also in economics: in most cases, no single theory suffices to explain an event.[40]

The interesting fact is that even those men of science who repudiate the rationalist dogma behave like many atheists: they reject the gospel but follow its teachings. Regardless of their metaphysical beliefs, all thus strive to arrange facts in logical order. The origins of this mental habit, for habit it is, go back to the time of the first theoretical science, that is, to the first *Elements of Geometry*. The way it came about is familiar to us from the attitude of a housewife after using a labor-saving gadget for the first time: men of science, too, after having experienced the economic advantages of theoretical science, refuse to do without it. By tasting knowledge in the theoretical form only once, the human mind becomes infected with an incurable virus, as it were, which produces an irresistible craving for logical order. This is why, whenever a Spencerian tragedy—a theory killed by a fact—takes place, the minds of the scholarly world know no rest until a new logical foundation is laid out.

Though economy of thought is the reason why the human mind acquired the analytical habit, this habit in turn has a very important economic role. Thanks to this habit, experimenting ceases to be a routine procedure by which the factual truth-value of logically derived propositions is established. By stimulating the imagination of the experimenting scholar, the analytical habit is alone responsible for the fact that theoretical experimenting is far luckier than mere experimenting. In the experimental undertaking, as Pasteur once observed, chance favors only the prepared minds.

[38] Immanuel Kant, *Prolegomena to Any Future Metaphysics That Will Be Able to Present Itself as a Science* (Manchester, 1953), p. 82.

[39] Louis de Broglie, *Physics and Microphysics* (London, 1955), pp. 208 f.

[40] Cf. my article "Economic Theory and Agrarian Economics" (1960) reprinted in AE, pp. 361 f; also H. S. Ferns, *Britain and Argentina in the Nineteenth Century* (Oxford, 1960), p. 436. But since man is by definition rational, the complementarity may in this case be spurious and reflect only the complexity of the forms in which man can be rational—a point to which I shall return in the last chapter of this book.

Moreover, the analytical mind creates what it craves for: logical order. During the centuries that elapsed between Euclid and Newton, it slowly created patches of logically ordered knowledge, gradually increased the area of each patch, and ultimately united some in a single unit: theoretical mechanics. As I have said, whenever a theory is destroyed the analytical mind immediately sets out to rebuild a new logical foundation on the ashes of the old. The most important work in this reconstruction is the building of entirely new concepts. These concepts then open up new experimenting grounds, thus extending the fields upon which we harvest new factual knowledge. Thanks to the analytical habit, every Spencerian tragedy is followed by a scientific bonanza. On the other hand, as Einstein cautioned us, we should not believe that this habit consists only of pure logical thinking: it requires above all "intuition, resting on *sympathetic* understanding of experience,"[41] and—I may add—a consummate intellectual phantasy. Logic helps us only present thought already thought out, but it does not help us think up thoughts.[42]

From the preceding analysis it follows that the immense difference between East and West in the progress of factual knowledge constitutes no evidence in support of a rational reality. It does prove, however, that theoretical science is thus far the most successful device for learning reality *given the scarcity pattern of the basic faculties of the human mind.*

9. Theoretical Science: A Living Organism. The main thesis developed in this chapter is that theoretical science is a living organism precisely because it emerged from an amorphous structure—the taxonomic science—just as life emerged from inert matter. Further, as life did not appear everywhere there was matter, so theoretical science did not grow wherever taxonomic science existed: its genesis was a historical accident. The analogy extends still further. Recalling that "science is what scientists do," we can regard theoretical science as a purposive mechanism that reproduces, grows, and preserves itself. It reproduces itself because any "forgotten" proposition can be rediscovered by ratiocination from the logical foundation. It grows because from the same foundation new propositions are continuously derived, many of which are found factually true. It also preserves its essence because when destructive contradiction invades its body a series of factors is automatically set in motion to get rid of the intruder.

To sum up: *Anatomically*, theoretical science is logically ordered knowl-

[41] Albert Einstein, *Ideas and Opinions* (New York, 1954), p. 226, also p. 271. My italics.

[42] A well-documented and highly penetrating discussion of this problem is offered by Jacques Hadamard, *An Essay on the Psychology of Invention in the Mathematical Field* (Princeton, 1945).

edge. A mere catalog of facts, as we say nowadays, is no more science than the materials in a lumber yard are a house. *Physiologically*, it is a continuous secretion of experimental suggestions which are tested and organically integrated into the science's anatomy. In other words, theoretical science continuously creates new facts from old facts, but its growth is organic, not accretionary. Its anabolism is an extremely complex process which at times may even alter the anatomic structure. We call this process "explanation" even when we cry out "science does not *explain* anything."[43] *Teleologically*, theoretical science is an organism in search of new knowledge.

Some claim that the purpose of science is prediction. This is the practical man's viewpoint even when it is endorsed by such scholars as Benedetto Croce or Frank Knight.[44] Neo-Machians go even further. Just as Mach focused his attention on economy of thought without regard for the special role of logical order, they claim that practical success is all that counts; understanding is irrelevant. No doubt, if science had no utility for the practical man, who acts on the basis of predictions, scientists would now be playing their little game only in private clubs, like the chess enthusiasts. However, even though prediction is the touchstone of scientific knowledge—"in practice man must prove the truth," as Marx said[45]—the purpose of science in general is not prediction, but knowledge for its own sake. Beginning with Pythagoras' school, science ceased to serve exclusively the needs of business and has remained always ahead of these.[46] The practical man may find it hard to imagine that what animates science is a delight of the analytical habit and idle curiosity; hence, he might never realize what is the source of his greatest fortune. The only thing that excites the true scholar is the delight in adding a few bars to an unfinished symphony or, if he happens to believe in the ontological order of nature, in uncovering another articulation of that order. His interest in a problem vanishes completely the very moment he has solved it.[47]

Others say that science is experimenting. As far as theoretical science at least is concerned, this view confuses the whole organism with one of

[43] Alfred J. Lotka, *Elements of Physical Biology* (Baltimore, 1925), p. 389.

[44] Frank H. Knight, *The Ethics of Competition* (New York, 1935), pp. 109 f.

[45] F. Engels, *Ludwig Feuerbach and the Outcome of Classical German Philosophy* (London, 1947), p. 76.

[46] Burnet, *Early Greek Philosophy*, p. 99; P. W. Bridgman, *Reflections of a Physicist* (2nd edn., New York, 1955), pp. 348 f. What might happen to this relation in the very immediate future is a matter of speculation. But the contention of F. Engels, in *On Historical Materialism* (New York, 1940), p. 14, that science did not exist before the bourgeois society because only this society could not live without it, is a far cry from the truth.

[47] Hadamard, *Psychology of Invention*, p. 60; H. Poincaré, *The Foundations of Science* (Lancaster, Pa., 1946), pp. 366 f.

its physiological functions. Those who commit this error usually proclaim that "Bacon [is science's] St. John the Baptist."[48] Naturally, they also blame Aristotle's philosophy of knowledge with its emphasis on Logic for the marasmus of science until Francis Bacon's time. Facts have never been more ignored. To begin with, Aristotle never denied the importance of experience; one eloquent quotation will suffice: "If at any future time [new facts] are ascertained, then credence must be given rather to observation than to theories and to theories only if what they affirm agrees with the observed facts."[49] In relation to the time in which he lived he was one of the greatest experimenters and keenest observers. As Darwin judged, Linnaeus and Cuvier are "mere schoolboys to old Aristotle."[50] His teachings should not be blamed for what Scholasticism did with them. Finally, mechanics was already moving fast on Aristotelian theoretical tracks at the time Bacon's works appeared. Without the analytical habit which had been kept alive by Euclid's *Elements* and Aristotle's writings, Kepler, Galileo, and Newton, as well as all the great men of science that came later, would have had to join the Sino-Indians in contemplative and casual observation of nature.[51] To the extent to which we may turn history around in thought, we may reason that without the peculiar love the Greeks had for Understanding,[52] our knowledge would not by far have reached its present level; nor would modern civilization be what it is today. For better or for worse, we have not yet discovered one single problem of Understanding that the Greek philosophers did not formulate.

[48] J. S. Huxley, "Science, Natural and Social," *Scientific Monthly*, L (1940), 5.

[49] *De Generatione Animalium*, 760^{b} 30–33. Also *Metaphysics*, 981^{a}.

[50] See Ross, *Aristotle*, pp. 112–114.

[51] Cf. Alfred North Whitehead, *Process and Reality: An Essay in Cosmology* (New York, 1929), p. 7.

[52] Cf. Plato, *Republic*, V. 435–436. Also W. T. Stace, *A Critical History of Greek Philosophy* (London, 1934), pp. 17 f; Cyril Bailey, *The Greek Atomists and Epicurus* (Oxford, 1928), p. 5.

CHAPTER II *Science, Arithmomorphism, and Dialectics*

1. "No Science without Theory." Theoretical science having the marvelous qualities just described, we can easily understand the sanguine hopes raised by Newton's success in transforming mechanics into such a science. At last, some two thousand years after Euclid's *Elements*, Newton's *Principia Mathematica* proved that theoretical science can grow in other domains besides geometry, and equally well. But sanguine hopes are sanguine hopes: thoughts on the matter, especially of those fascinated most by the powers of Logic, became prey to the confusion between "some fields" and "all fields." In the end almost everybody interpreted the evidence as proof that knowledge in *all* fields can be cast into a theoretical mold. Especially after the astounding discovery of Neptune "at the tip of Leverrier's pen," spirits ran high in all disciplines, and one scientist after another announced his intention of becoming the Newton of his own science. François Magendie aspired to place even physiology "on the same sure footing" as mechanics.[1] "Thus the confusion of tongues"—as one economist lamented—"was propagated from science to science."[2]

On the whole, the scientific temper has not changed much. To be sure, the position that mechanics constitutes the only road leading to divine knowledge—as Laplace argued in his magnificent apotheosis[3]—has been

[1] J. M. D. Olmsted and E. H. Olmsted, *Claude Bernard and the Experimental Method in Medicine* (New York, 1952), p. 23.

[2] S. Bauer, quoted in J. S. Gambs, *Beyond Supply and Demand* (New York, 1946), p. 29n. My translation.

[3] P. S. Laplace, *A Philosophical Essay on Probabilities* (New York, 1902), p. 4.

officially abandoned by almost every special science. Curiously, the move was not caused by the recognition of the failures following the adoption of this position outside physics, but induced by the fact that physics itself had to reject it.[4] In place of "all sciences must imitate mechanics," the battle cry of the scholarly army is now "no science without theory." But the change is rather skin deep, for by "theory" they usually mean a logical file of knowledge as exemplified only by geometry and mechanics.[5]

No other science illustrates better than economics the impact of the enthusiasm for mechanistic epistemology upon its evolution. Does the transforming of economics into "a physico-mathematical science" require a measure of utility which escapes us? "*Eh bien!*"—exclaimed Walras characteristically—"this difficulty is not insurmountable. Let us suppose that this measure exists, and we shall be able to give an exact and mathematical account" of the influence of utility on prices, etc.[6] Unfortunately, this uncritical attitude has ever since constituted the distinct flavor of mathematical economics. In view of the fact that theoretical science is a living organism, it would not be exaggerating to say that this attitude is tantamount to planning a fish hatchery in a moist flower bed.

Jevons showed some concern over whether the new environment—the economic field—would contain the basic elements necessary for the theoretical organism to grow and survive. Indeed, before declaring his intention to rebuild economics as "*the mechanics of utility and self-interest,*" he took pains to point out that in the domain of economic phenomena there is plenty of quantitative "moisture" in "the private-account books, the great ledgers of merchants and bankers and public offices, the share lists, price lists, bank returns, monetary intelligence, Custom-house and other Government returns."[7] But Jevons, like many others after him, failed to go on to explain how ordinary statistical data could be substituted for the variables of his mechanical equations. By merely expressing the hope that statistics might become "more complete and accurate . . . so that the formulae could be endowed with exact meaning,"[8] Jevons set an often-followed pattern for avoiding the issue.

Certainly, after it was discovered that theoretical science can function properly in another domain besides geometry, scientists would have been

[4] See chapter "The Decline of the Mechanical View" in A. Einstein and L. Infeld, *The Evolution in Physics* (New York, 1938).

[5] The point has been repeatedly recognized by numerous scholars: e.g., Max Planck, *Scientific Autobiography and Other Papers* (New York, 1949), p. 152.

[6] Léon Walras, *Éléments d'économie politique pure* (3rd edn., Lausanne, 1896), p. 97. My translation.

[7] W. Stanley Jevons, *The Theory of Political Economy* (4th edn., London, 1924), p. 21 and p. 11.

[8] *Ibid.*, p. 21.

derelict if they had failed to try out "a fish hatchery in a flower bed." For trying, though not sufficient, is as absolutely necessary for the advancement of knowledge as it is for biological evolution. This is why we cannot cease to admire men like Jevons and Walras, or numerous others who even in physics hurried to adopt a new viewpoint without first testing their ground.[9] But our admiration for such unique feats does not justify persistence in a direction that trying has proved barren. Nor do we serve the interest of science by glossing over the impossibility of reducing the economic process to mechanical equations. In this respect, a significant symptom is the fact that Carl Menger is placed by almost every historian on a lower pedestal than either Walras or Jevons only because he was more conservative in treating the same problem, the subjective basis of value.[10] Moreover, in spite of the fact that no economy, not even that of a Robinson Crusoe, has been so far described by a Walrasian system in the same way in which the solar system has been described by a Lagrange system of mechanical equations, there are voices claiming that economics "has gone through its Newtonian revolution": only the other social sciences are still awaiting their Galileo or Pasteur.[11] Alfred North Whitehead's complaint that "the self-confidence of learned people is the comic tragedy of [our] civilization"[12] may be unsavory but does not seem entirely unfounded.

Opposition to Walras' and Jevons' claim that "economics, if it is to be a science at all, must be a mathematical science,"[13] has not failed to manifest itself. But, in my opinion, during the ensuing controversies swords have not been crossed over the crucial issue. For I believe that what social sciences, nay, all sciences need is not so much a new Galileo or a new Newton as a new Aristotle who would prescribe new rules for handling those notions that Logic cannot deal with.

This is not an extravagant vision. For no matter how much we may preen ourselves nowadays upon our latest scientific achievements, the evolution of human thought has not come to a stop. To think that we have even approached the end is either utter arrogance or mortifying pessimism. We cannot therefore write off the possibility of striking one

[9] Cf. P. W. Bridgman, *Reflections of a Physicist* (2nd edn., New York, 1955), p. 355.

[10] E.g., K. Wicksell, *Value, Capital and Rent* (London, 1954), p. 53; Joseph A. Schumpeter, *History of Economic Analysis* (New York, 1954), p. 918. Among the few exceptions: Frank H. Knight, "Marginal Utility Economics," *Encyclopaedia of the Social Sciences* (New York, 1931), V, 363; George J. Stigler, *Production and Distribution Theories* (New York, 1948), p. 134.

[11] Karl R. Popper, *The Poverty of Historicism* (Boston, 1957), p. 60 and note.

[12] Alfred North Whitehead, *Science and Philosophy* (New York, 1948), p. 103.

[13] Jevons, *Theory*, p. 3.

day upon the proper mutant idea that would lead to an anatomy of science capable of thriving equally well in natural as in social sciences. On rare occasions we find this hope more clearly expressed with the extremely pertinent remark that in such a unifying science physics will be "swallowed up" by biology, not the reverse.[14] Or, as Whitehead put it more sharply, "murder is a prerequisite for the absorption of biology into physics."[15] A historical precedent already exists: physicists and scientific philosophers had for a long time denied that "scientific" laws exist outside physics and chemistry, because only there did we find rigidly binding relations. Today they work hard to convince everybody that on the contrary the laws of nature are not rigid but stochastic and that the rigid law is only a limiting, hence highly special, case of the stochastic law. Somehow they usually fail to point out that the latter type of law is not a native of physical science but of the life sciences.

The history of human thought, therefore, teaches us that nothing can be extravagant in relation to what thought might discover or where. It is all the more necessary for us to recognize fully the source as well as the nature of our difficulty *at present*.

2. *Theoretical Science versus Science*. The first condition an environment must satisfy in order to sustain the life of a certain organism is to contain the chemical elements found in the anatomy of that organism. If it does not, we need not go any further. Let us, therefore, begin our inquiry by a "chemical" analysis of the anatomy of theoretical science.

As I have pointed out, the *causa materialis* of science, not only of theoretical science, consists of descriptive propositions. I have further explained that the distinctive feature of theoretical science is its logically ordered anatomy. Whoever is willing to look at the brute facts and accept some of their unpleasantness will agree that in some phenomenal domains an overwhelming majority of descriptive propositions do not possess the "chemical" properties required by logical ordering.

I can hardly overemphasize the fact that Logic, understood in its current Aristotelian sense, is capable of dealing only with one distinct class of propositions, such as

A. *The hypotenuse is greater than the leg*,

[14] Cf. J. S. Haldane, *The Sciences and Philosophy* (New York, 1929), p. 211. Also Erwin Schrödinger, *What Is Life?* (Cambridge, Eng., 1944), pp. 68 f; R. E. Peierls, *The Laws of Nature* (London, 1957), p. 277; L. von Bertalanffy, *Problems of Life* (New York, 1952), p. 153. Quite recently, G. P. Thomson, a Nobel laureate, ended his address at the Semicentennial lectures of Rice Institute (1962) by saying that "the future of physics lies with biology."

[15] Alfred North Whitehead, "Time, Space, and Material," in *Problems of Science and Philosophy*, Aristotelian Society, suppl. vol. 2, 1919, p. 45. See also Chapter V, Section 1, below.

but it is largely impotent when it comes to propositions such as

B. *Culturally determined wants are higher than biological wants,*

or

C. *Woodrow Wilson had a decisive influence upon the Versailles Peace Treaty.*

A logician would hardly deny this difference. But many, especially the logical positivists, would argue that propositions such as B or C are meaningless and, hence, the difference does not prove at all the limitation of Logic. This position is clearly explained by Max Black: *red* being a vague concept, the question "Is this color red?" has scarcely any meaning.[16] However, the use of the term "meaningless" for propositions that Logic cannot handle is a clever artifice for begging a vital question.

At bottom, the issue is whether knowledge is authentic only if it can be unified into a theory. In other words, is theoretical science the only form of scientific knowledge? The issue resolves into several questions: the first is what accounts for Logic's impotence to deal with "meaningless" propositions.

3. Numbers and Arithmomorphic Concepts. The boundaries of every science of fact are moving penumbras. Physics mingles with chemistry, chemistry with biology, economics with political science and sociology, and so on. There exists a physical chemistry, a biochemistry, and even a political economy in spite of our unwillingness to speak of it. Only the domain of Logic—conceived as *Principia Mathematica*—is limited by rigidly set and sharply drawn boundaries. The reason for this is that *discrete* distinction constitutes the very essence of Logic: perforce, discrete distinction must apply to Logic's own boundaries.

The elementary basis of *discrete* distinction is the distinction between two written symbols: between "m" and "n," "3" and "8," "excerpt" and "except," and so on. As these illustrations show, good symbolism requires perfect legibility of writing; otherwise we might not be able to distinguish without the shadow of a doubt between the members of the same pair. By the same token, spoken symbolism requires distinct pronunciation, without lisping or mumbling.

There is one and only one reason why we use symbols: to represent concepts visually or audibly so that these may be communicated from one

[16] Max Black, *The Nature of Mathematics* (New York, 1935), p. 100n. This position is frequently used to dodge basic questions, such as "Can a machine think?" See Chapter III, Section 10, below.

mind to another.[17] Whether in general reasoning or in Logic (i.e., formal logic), we deal with symbols qua representatives of *extant* concepts. Even in mathematics, where numbers and all other concepts are as distinct from one another as the symbols used to represent them, the position that numbers are nothing but "signs" has met with tremendous opposition.[18] Yet we do not go, it seems, so far as to realize (or to admit if we realize) that the fundamental principle upon which Logic rests is that *the property of discrete distinction should cover not only symbols but concepts as well.*

As long as this principle is regarded as normative no one could possibly quarrel over it. On the contrary, no one could deny the immense advantages derived from following the norm whenever possible. But it is often presented as a general law of thought. A more glaring example of Whitehead's "fallacy of misplaced concreteness" than such a position would be hard to find. To support it some have gone so far as to maintain that we can think but in words. If this were true, then thoughts would become a "symbol" of the words, a most fantastic reversal of the relationship between means and ends. Although the absurdity has been repeatedly exposed, it still survives under the skin of logical positivism.[19] Pareto did not first coin the word "ophelimity" and then think of the concept. Besides, thought is so fluid that even the weaker claim, namely, that we can coin a word for every thought, is absurd. "The Fallacy of the Perfect Dictionary"[20] is plain: even a perfect dictionary is molecular while thought is continuous in the most absolute sense. Plain also are the reason for and the meaning of the remark that "in symbols truth is darkened and veiled by the sensuous element."[21]

Since any particular real number constitutes the most elementary example of a discretely distinct concept, I propose to call any such concept *arithmomorphic*. Indeed, despite the use of the term "continuum" for the set of all real numbers, within the continuum every real number retains

[17] This limitation follows the usual line, which ignores tactile symbolism: taps on the shoulder, handshakes, etc. Braille and especially the case of Helen Keller prove that tactile symbolism can be as discretely distinct and as efficient as the other two. Its only shortcoming is the impossibility of transmission at a distance.

[18] Cf. the Introduction by P. E. B. Jourdain to Georg Cantor, *Contributions to the Founding of the Theory of Transfinite Numbers* (New York, n.d.), pp. 20, 69 f; R. L. Wilder, *Introduction to the Foundations of Mathematics* (New York, 1956), ch. x and *passim*.

[19] For a discussion of the psychological evidence against the equation "thought = word," see Jacques Hadamard, *An Essay on the Psychology of Invention in the Mathematical Field* (Princeton, 1945), pp. 66 ff. For what it might be worth, as one who is multilingual I can vouch that I seldom think in any language, except just before expressing my thoughts orally or in writing.

[20] Alfred North Whitehead, *Modes of Thought* (New York, 1938), p. 235. See also P. W. Bridgman, *The Intelligent Individual and Society* (New York, 1938), pp. 69 f.

[21] G. W. F. Hegel, *Hegel's Science of Logic* (2 vols., London, 1951), I, 231.

a *distinct individuality* in all respects identical to that of an integer within the sequence of natural numbers. The number π, for instance, is discretely distinct from any other number, be it 3.141592653589793 or 10^{100}. So is the concept of "circle" from "10^{100}-gon" or from "square," and "electron" from "proton." In Logic "is" and "is not," "belongs" and "does not belong," "some" and "all," too, are *discretely* distinct.

Every arithmomorphic concept stands by itself in the same specific manner in which every "Ego" stands by itself perfectly conscious of its absolute differentiation from all other "Egos." This is, no doubt, the reason why our minds crave arithmomorphic concepts, which are as translucent as the feeling of one's own existence. Arithmomorphic concepts, to put it more directly, *do not overlap*. It is this peculiar (and restrictive) property of the material with which Logic can work that accounts for its tremendous efficiency: without this property we could neither compute, nor syllogize, nor construct a theoretical science. But, as happens with all powers, that of Logic too is limited by its own ground.

4. Dialectical Concepts. The antinomy between One and Many with which Plato, in particular, struggled is well known. One of its roots resides in the fact that the quality of discrete distinction does not necessarily pass from the arithmomorphic concept to its concrete denotations. There are, however, cases where the transfer operates. Four pencils are an "even number" of pencils; a concrete triangle is not a "square." Nor is there any great difficulty in deciding that Louis XIV constitutes a denotation of "king." But we can never be absolutely sure whether a concrete quadrangle is a "square."[22] In the world of ideas "square" is One, but in the world of the senses it is Many.[23]

On the other hand, if we are apt to debate endlessly whether a particular country is a "democracy" it is above all because the concept itself appears as Many, that is, it is not discretely distinct. If this is true, all the more the concrete cannot be One. A vast number of concepts belong to this very category; among them are the most vital concepts for human judgments, like "good," "justice," "likelihood," "want," etc. They have no arithmomorphic boundaries; instead, *they are surrounded by a penumbra within which they overlap with their opposites.*

At a particular historical moment a nation may be both a "democracy" and a "nondemocracy," just as there is an age when a man is both "young" and "old." Biologists have lately realized that even "life" has no arithmomorphic boundary: there are some crystal-viruses that con-

[22] Strangely, logicians do not argue that because of this fact "square" is a *vague* concept and "Is this quadrangle a square?" has no meaning.

[23] Plato, *Phaedrus*, 265D and, especially, *Republic*, VI. 507.

stitute a penumbra between living and dead matter.[24] Any particular want, as I have argued along well-trodden but abandoned trails, imperceptibly slides into other wants.[25] Careful thinkers do not hide that even in mathematics "the use of good judgment in determining when a statement form is acceptable in defining a class seems to be unavoidable."[26]

It goes without saying that to the category of concepts just illustrated we cannot apply the fundamental law of Logic, the Principle of Contradiction: "B cannot be both A and non-A." On the contrary, we must accept that, *in certain instances* at least, "B is both A and non-A" is the case. Since the latter principle is one cornerstone of Hegel's Dialectics, I propose to refer to the concepts that may violate the Principle of Contradiction as *dialectical*.[27]

In order to make it clear what we understand by dialectical concept, two points need special emphasis.

First, the impossibility mentioned earlier of deciding whether a concrete quadrangle is "square" has its roots in the imperfection of our senses and of their extensions, the measuring instruments. A *perfect* instrument would remove it. On the other hand, the difficulty of deciding whether a particular country is a democracy has nothing to do—as I shall explain in detail presently—with the imperfection of our sensory organs. It arises from another "imperfection," namely, that of our thought, which cannot always reduce an apprehended notion to an arithmomorphic concept. Of course, one may suggest that in this case too the difficulty would not exist for a *perfect* mind. However, the analogy does not seem to hold. For while the notion of a perfect measuring instrument is sufficiently clear (and moreover indispensable even for explaining the indeterminacy in physical measurements), the notion of a perfect mind is at most a verbal concoction. There is no direct bridge between an imperfect and the perfect measuring instrument. By the same token, the imperfect mind cannot know how a perfect mind would actually operate. It would itself become perfect the moment it knew how.

The second point is that a dialectical concept—in my sense—does not

[24] On the arithmomorphic definition of life, see Alfred J. Lotka, *Elements of Physical Biology* (Baltimore, 1925), chap. i and p. 218n.

[25] My essay entitled "Choice, Expectations and Measurability" (1954), reprinted in AE.

[26] L. M. Graves, *The Theory of Functions of Real Variables* (New York, 1946), p. 7. Also Henri Poincaré, *The Foundations of Science* (Lancaster, Pa., 1946), p. 479.

[27] The connection between dialectical concepts thus defined and Hegelian logic is not confined to this principle. However, even though the line followed by the present argument is inspired by Hegel's logic, it does not follow Hegel in all respects. We have been warned, and on good reasons, that one may ignore Hegel at tremendous risks. To follow Hegel only in part might very well be the greatest risk of all; yet I have no choice but to take this risk.

overlap with its opposite *throughout the entire range of denotations*. To wit, in most cases we can decide whether a thing, or a particular concept, represents a living organism or lifeless matter. If this were not so, then certainly dialectical concepts would be not only useless but also harmful. Though they are not *discretely distinct*, dialectical concepts are nevertheless *distinct*. The difference is this. A penumbra separates a dialectical concept from its opposite. In the case of an arithmomorphic concept the separation consists of a void: *tertium non datur*—there is no third case. The extremely important point is that the separating penumbra itself is a dialectical concept. Indeed, if the penumbra of A had arithmomorphic boundaries, then we could readily construct an arithmomorphic structure consisting of three discretely distinct notions: "proper A," "proper non-A," and "indifferent A." The procedure is most familiar to the student of consumer's choice where we take it for granted that between "preference" and "nonpreference" there *must* be "indifference."[28]

Undoubtedly, a penumbra surrounded by another penumbra confronts us with an infinite regress. But there is no point in condemning dialectical concepts because of this aspect: in the end the dialectical infinite regress resolves itself just as the infinite regress of Achilles running after the tortoise comes to an actual end. As Schumpeter rightly protested, "there is no sense in our case in asking: 'Where does that type [of entrepreneur] begin then?' and then to exclaim: 'This is no type at all!'"[29] Should we also refuse to recognize and study virtue and vice just because there is no sharp division—as Hume, among many, observed[30]—between these two opposing qualities of the human spirit? Far from being a deadly sin, the infinite regress of the dialectical penumbra constitutes the salient merit of the dialectical concepts: as we shall see, it reflects the most essential aspect of Change.

5. Platonic Traditions in Modern Thought. To solve the perplexing problem of One and Many, Plato taught that ideas live in a world of their own, "the upper-world," where each retains "a permanent individuality" and, moreover, remains "the same and unchanging."[31] Things of the "lower-world" partake of these ideas, that is, resemble them.[32] The pivot

[28] Cf. my essay "Choice, Expectations and Measurability" (1954), reprinted in AE.

[29] Joseph A. Schumpeter, *The Theory of Economic Development* (Cambridge, Mass., 1949), p. 82n.

[30] David Hume, *Writings on Economics*, ed., E. Rotwein (London, 1955), p. 19.

[31] *Phaedo*, 78, *Philebus*, 15. Plato's doctrine that ideas are "fixed patterns" permeates all his Dialogues. For just a few additional references, *Parmenides*, 129 ff, *Cratylus*, 439–440.

[32] *Phaedo*, 100 ff. It is significant that although Plato (*Phaedo*, 104) illustrates the discrete distinction of ideas by referring to integral numbers, he never discusses the problem why some things partake fully and others only partly of ideas.

of Plato's epistemology is that we are born with a latent knowledge of all ideas—as Kant was to argue later about some notions—because our immortal soul has visited their world some time in the past. Every one of us, therefore, can learn ideas by reminiscence.[33]

Plato's extreme idealism can hardly stir open applause nowadays. Yet his mystical explanation of how ideas are revealed to us in their purest form underlies many modern thoughts on "clear thinking." The Platonic tenet that only a privileged few are acquainted with ideas but cannot describe them publicly, is manifest, for example, in Darwin's position that "species" is that form which is so classified by "the opinion of naturalists having sound judgment and wide experience."[34] Even more Platonic in essence is the frequently heard view that "constitutional law" has one and only one definition: it is the law pronounced as such by the U.S. Supreme Court if and when in a case brought before it the Court is explicitly asked for a ruling on this point.

There can be no doubt about the *fact* that a consummate naturalist or a Supreme Court justice is far more qualified than the average individual for dealing with the problem of species or constitutional law. But that is not what the upholders of this sort of definition usually mean: they claim that the definitions are operational and, hence, dispose of the greatest enemy of clear thought—vagueness. It is obvious, however, that the claim is specious: the result of the defining operation is not One but Many.[35] Not only is the operation extremely cumbersome, even wholly impractical at times, but the definition offers no enlightenment to the student. Before anyone becomes an authority on evolution, and even thereafter, he needs to know what "fitness" means without waiting until natural selection will have eliminated the unfit. Science cannot be satisfied with the idea that the only way to find out whether a mushroom is poisonous is to eat it.

Sociology and political science, in particular, abound in examples of another form of disguised Platonic rationale. For instance, arguments often proceed, however unawares, from the position that the pure idea of "democracy" is represented by one particular country—usually the writer's: all other countries only partake of this idea in varying degrees.

Plato's *Dialogues* leave no doubt that he was perfectly aware of the fact that we know concepts either by definition or by intuition. He realized that since definition constitutes a public description, anyone may

[33] *Meno*, 81–82, *Phaedo*, 73 ff, *Phaedrus*, 249–250.

[34] Charles Darwin, *The Origin of Species* (6th edn., London, 1898), p. 34.

[35] As Charles Darwin himself observes in a different place, *The Descent of Man* (2nd edn., New York, n.d.), p. 190: Thirteen eminent naturalists differed so widely as to divide the human species into as few as two and as many as sixty-three races!

learn to know a concept by definition. He also realized that we can get acquainted with some concepts only by direct apprehension supplemented by Socratic analysis.[36] Plato's difficulty comes from his belief *that regardless of their formation all concepts are arithmomorphic,* that "everything resembles a number," as his good friend Xenocrates was to teach later. One Dialogue after another proves that although Plato was bothered by the difficulties of definition in the case of many concepts, he never doubted that in the end all concepts can be defined. Very likely, Plato—like many after him—indiscriminately extrapolated the past: since all defined concepts have at one time been concepts by intuition, all present concepts by intuition must necessarily become concepts by definition.

The issue may be illustrated by one of our previous examples. Should we strive for an arithmomorphic concept of "democracy," we would soon discover that no democratic country fits the concept: not Switzerland, because Swiss women have no voting right; not the United States, because it has no popular referendum; not the United Kingdom, because the Parliament cannot meet without the solemn approval of the King, and so on down the line. The penumbra that separates "democracy" from "autocracy" is indeed very wide. As a result, "even the dictatorship of Hitler in National-Socialist Germany had democratic features, and in the democracy of the United States we find certain dictatorial elements."[37] But this does not mean that Hitlerite Germany and the United States must be thrown together in the same conceptual pot, any more than the existence of a penumbra of viruses renders the distinction between "man" and "stone" senseless.

Furthermore, the efforts to define democracy are thwarted by a more general and more convincing kind of difficulty than that just mentioned. Since "democracy" undoubtedly implies the right to vote but not for all ages, its definition must necessarily specify the *proper* limit of the voting age. Let us assume that we agree upon L being this limit. The natural question of why $L-\epsilon$ is not as good a limit fully reveals the impossibility of taking care of all the imponderables of "democracy" by an arithmomorphic concept.

Of "democracy" as well as of "good," "want," etc., we can say what St. Augustine in essence said of Time: if you know nothing about it I cannot tell you what it is, but if you know even vaguely what it means let us talk about it.[38]

[36] *Republic,* VI. 511. In all probability, it was this sort of analysis that Plato meant by "dialectics," but he never clarified this term.

[37] Max Rheinstein in the "Introduction" to Max Weber, *On Law in Economy and Society* (Cambridge, Mass., 1954), p. xxxvi.

[38] Saint Augustine, *Confessions,* XI. 17.

6. Dialectical Concepts and Science. No philosophical school, I think, would nowadays deny the existence of dialectical concepts as they have been defined above. But opinions as to their relationship to science and to knowledge in general vary between two extremes.

At one end we find every form of positivism proclaiming that whatever the purpose and uses of dialectical concepts, these concepts are antagonistic to science: knowledge proper exists only to the extent to which it is expressed in arithmomorphic concepts. The position recalls that of the Catholic Church: holy thought can be expressed only in Latin.

At the other end there are the Hegelians of all strains maintaining that knowledge is attained only with the aid of dialectical notions in the strict Hegelian sense, i.e., notions to which the principle "A is non-A" applies *always*.

There is, though, some definite asymmetry between the two opposing schools: no Hegelian—Hegel included—has ever denied either the unique ease with which thought handles arithmomorphic concepts or their tremendous usefulness.[39] For these concepts possess a built-in device against most kinds of errors of thought that dialectical concepts do not have. Because of this difference we are apt to associate dialectical concepts with loose thinking, even if we do not profess logical positivism. The by now famous expression "the muddled waters of Hegelian dialectics" speaks for itself. Moreover, the use of the antidialectical weapon has come to be the easiest way for disposing of someone else's argument.[40] Yet the highly significant fact is that no one has been able to present an argument against dialectical concepts *without incessant recourse to them!*

We are badly mistaken if we believe that the presence of such terms as "only if" or "nothing but" in a sentence clears it of all "dialectical nonsense." As an eloquent example, we may take the sentence "A proposition has meaning only if it is verifiable," and the sentence "When we speak of verifiability we mean *logical* possibility of verification, and nothing but this,"[41] which together form the creed of the Vienna posi-

[39] That Hegel's philosophy has been made responsible for almost every ideological abuse and variously denounced as "pure nonsense [that] had previously been known only in madhouses" or as "a monument to German stupidity" need not concern us. (Will Durant, in *The Story of Philosophy*, New York, 1953, p. 221, gives E. Caird, *Hegel*, London, 1883, as the source of these opinions; all my efforts to locate the quotation have been in vain.) But I must point out that the often-heard accusation that Hegel denied the great usefulness of mathematics or theoretical science is absolutely baseless: see *The Logic of Hegel*, tr. W. Wallace (2nd edn., London, 1904), p. 187.

[40] Precisely because I wish to show that the sin is not confined to the rank and file, I shall mention that Knight within a single article denounces the concept of instinct as arbitrary and unscientific but uses the concept of want freely. Frank H. Knight, *The Ethics of Competition* (New York, 1935), p. 29 and *passim*.

[41] Moritz Schlick, "Meaning and Verification," *Philosophical Review*, XLV (1936), 344, 349.

tivism. If one is not a positivist, perhaps he would admit that there is some sense in these tenets, despite the criticism he may have to offer. But if one is a full-fledged positivist, he must also claim that "the dividing line between logical possibility and impossibility of verification is *absolutely sharp and distinct*; there is no gradual transition between meaning and nonsense."[42] Hence, for the two previous propositions to have a meaning, we need to describe "the logical possibility of [their] verification" in an absolutely sharp and distinct manner. To my knowledge, no one has yet offered such a description. Positivism does not seem to realize at all that the concept of verifiability—or that the position that "the meaning of a proposition is the method of its verification"[43]—is covered by a dialectical penumbra in spite of the apparent rigor of the sentences used in the argument. Of course, one can easily give examples of pure nonsense—"my friend died the day after tomorrow" is used by Moritz Schlick—or of pure arithmomorphic sense. However—as I have argued earlier—this does not dispose of a dialectical penumbra of graded differences of clearness between the two extreme cases. I hope the reader will not take offense at the unavoidable conclusion that most of the time all of us talk some nonsense, that is, express our thoughts in dialectical terms with no clear-cut meaning.

Some of the books written by the very writers who—like Bertrand Russell or Bridgman, for example—have looked upon combatting vagueness in science as a point of highest intellectual honor, constitute the most convincing proof that correct reasoning with dialectical concepts is not impossible.[44] In connection with this thesis of mine and in relation to the positivist viewpoint mentioned earlier (Section 2) that "this color is red" is a meaningless proposition, let me refer the reader to one of the most appreciated articles of Bertrand Russell: "Not only are we aware of particular yellows, but if we have seen a sufficient number of yellows and have sufficient intelligence, we are aware of the universal *yellow*; this universal is the subject in such judgments as 'yellow differs from blue' or 'yellow resembles blue less than green does.' And the universal yellow is the predicate in such judgments as 'this is yellow.'"[45] Although a positivist would certainly make no sense of this chain of dialectical concepts, this is what I would call dialectical reasoning at its best (with the risk of making Mr. Bertrand Russell feel offended thereby). And the important

[42] *Ibid.*, 352. My italics.

[43] *Ibid.*, 341.

[44] E.g., Bertrand Russell, *Principles of Social Reconstruction* (London, 1916), and P. W. Bridgman, *The Intelligent Individual and Society*.

[45] Bertrand Russell, *Mysticism and Logic* (New York, 1929), p. 212. [Concerning the next remark in the text, the reader should know that Bertrand Russell was alive when this volume went to press.]

fact is that such a reasoning is a far more delicate operation than syllogizing with arithmomorphic concepts. As I shall argue later on (Chapter III, Section 10), it constitutes the most important quality that differentiates the human mind from any mechanical brain.

Long ago, Blaise Pascal pointed out the difference between these two types of reasoning as well as their correlation with two distinct qualities of our intellect: *l'esprit géométrique* and *l'esprit de finesse*.[46] To blame dialectical concepts for any muddled thinking is, therefore, tantamount to blaming the artist's colors for what the artless—and even the talented at times—might do with them. As to the artful use of dialectical concepts by sophists of all strains, we have been long since instructed by Socrates on the difference between "the mere art of disputation and true dialectics."[47]

Now, both *l'esprit géométrique* and *l'esprit de finesse* are acquired (or developed) through proper training and exposure to as large a sample of ideas as possible. And we cannot possibly deny that social scientists generally possess enough *esprit de finesse* to interpret correctly the proposition "democracy allows for an equitable satisfaction of individual wants" and to reason correctly with similar propositions where almost every term is a dialectical concept. (And if some social scientists do not possess enough *esprit de finesse* for the job, God help them!) The feat is not by any means extraordinary. As Bridgman once observed, "little Johnnie and I myself know perfectly well what I want when I tell him to be good, although neither of us could describe exactly what we meant under cross-examination."[48]

The position that dialectical concepts should be barred from science because they would infest it with muddled thinking is, therefore, a flight of fancy—unfortunately, not an innocuous one. For it has bred another kind of muddle that now plagues large sectors of social sciences: arithmomania. To cite a few cases from economics alone. The complex notion of economic development has been reduced to a number, the income per capita. The dialectical spectrum of human wants (perhaps the most important element of the economic process) has long since been covered under the colorless numerical concept of "utility" for which, moreover, nobody has yet been able to provide an actual procedure of measurement.

7. *Probability: An Illustration of Hegelian Dialectics.* Nothing could illustrate the argument of the foregoing section more sharply than the concept of probability, now common to all special sciences. There are, as we all know, a multiplicity of largely antagonistic "doctrines," each claiming

[46] *Pensées*, 1–2, in Blaise Pascal, *Oeuvres complètes*, ed. J. Chevalier (Paris, 1954), pp. 1091 ff.

[47] Plato, *Philebus*, 17; more on this in *Theaetus*, 167–168.

[48] Bridgman, *Intelligent Individual and Society*, p. 72; also pp. 56 ff.

that only its own approach leads to what probability means (or, rather, should mean). The claim should not surprise us. Without going into lengthy details here, let me observe that, antagonistic though these doctrines are, they all have in fact the same objective: to order expectations with the aid of some numerical coefficient which each doctrine calls "probability." Expectation, however, is a complex state of the human mind involving two distinct elements: E, that part of the individual's knowledge of which he is aware at the time of the expectation, and P, an assertive proposition about a fact or an event usually, but not necessarily, uncertain. Symbolically, the expectation of an individual, I, may then be represented by $\mathscr{E}(I, E, P)$.[49]

In one group of doctrines—the Personalistic and the Subjectivistic, as they are called—attention is focused on I and probability is defined as the "degree of belief" the individual has in the fact or the event asserted by P. Another category leaves out I as well as that part of E that is not language and defines probability as a measure of the "truth" expressed by P, in fact, as a coefficient computed according to some (largely arbitrary) syntactical recipe. Needless to add, none of these doctrines is free from assumptions that fly in the face of elementary facts; some amount to little more than an exercise, however delightful, in empty axiomatization.

The only doctrines that should retain our attention here are those which may be called "Objectivistic" because they define probability independently of I (and of the kind of language used by I). In my formalization of expectation, the objective coefficient of probability—whenever it can be determined according to the rules provided and is also known by I—is part of E. The important point is that the ordering of the individual's expectations is a consequence of the arithmetical ordering of probabilities, not vice versa (as is the case in the Subjectivistic or Personalistic doctrines).[50]

The contest in the objectivistic approach is between the Laplacean and the Frequentist doctrines. The main criticism against the Laplacean definition of probability as the ratio between the number of favorable cases and that of all cases, "all cases being equally probable," cannot be refuted on Logical grounds: the definition *is* circular. The criticism, I contend, is idle because objective probability is basically a dialectical notion in the Hegelian sense. Indeed, the Frequentist definition, too, is circular if formulated properly.

[49] See my article "The Nature of Expectation and Uncertainty" (1958), reprinted in AE, where I presented a general critique of the main doctrines of probabilities. In addition to the present section, Chapter VI and Appendix F in the present volume contain some further thoughts on this topic.

[50] More on this in Appendix F.

In the Frequentist doctrine the probability of an event is defined by the *mathematical* relation

$$(\mathrm{D}_1) \qquad p = \lim f_n \quad \text{for } n \to \infty,$$

where f_n is the relative frequency of the event in the first n observations of an infinite sequence of observations under invariable conditions.[51] Although the domain of application of probability is thereby greatly restricted, the doctrine has a double merit—it relates the concept directly to observed facts and to a number. For this reason, it won an overwhelming acceptance from the outset and was embraced wholeheartedly by all statisticians. With modern physics taking the position that phenomena at the level of the elementary particles are governed only by probabilistic laws which reflect an irreducible random factor in nature, *not our ignorance of some hidden variables*,[52] the Frequentist doctrine set a claim to epistemological completeness.

"Probabilities are as real as masses," said H. Margenau.[53] Yet the truth is rather the reverse: masses are as real as probabilities. Indeed, anything we can now say about masses depends on what we can say about probabilities. "The mass of the mu meson is 200 times that of the electron," for instance, is a proposition that involves the probability of an observation showing that the mass of a mu meson is, say, 195 times that of an electron. The verification of propositions about probabilities is, therefore, the only fundamental issue. Everything else depends upon this verification.

[51] Obviously, the statement does not imply that *all* expectations are thereby ordered; for some P's the probabilities may not exist according to the rules provided or may not be part of E.

[52] Uncertainty in quantum physics "is not due to our human ignorance: it is *objectively uncertain* when [a particular] atom will disintegrate." F. Waismann, "The Decline and Fall of Causality," *Turning Points in Physics*, ed. R. J. Blin-Stoyle, *et al.* (Amsterdam, 1959), p. 141. A theorem proved in 1932 by J. von Neumann (*Mathematical Foundations of Quantum Mechanics*, Princeton, 1955, pp. 323–325), according to which the present laws of quantum mechanics are incompatible with the thought that they may hide some causal variables, fostered the belief that these laws represent a definitive limit to a causal explanation of the behavior of elementary matter (cf. Louis de Broglie, *Physics and Microphysics*, London, 1955, pp. 195–205). However, because it implies that there can be no breakthrough at the subquantum level, Neumann's "dead-end" theorem should have been suspect from the outset. Actually, Broglie—who first saluted it enthusiastically—found a loophole in the whole argument (Broglie, *New Perspectives in Physics*, New York, 1962, pp. 99–102). For a more comprehensive discussion of the point, see David Bohm, *Causality and Chance in Modern Physics* (London, 1957), pp. 95–97.

The opposite idea, that chance reflects only our inability to solve the inextricable system of equations that govern phenomena or to know all the factors involved, is associated with the name of Henri Poincaré (*The Foundations of Science*, pp. 159 f, 395 f).

[53] H. Margenau, *Open Vistas: Philosophical Perspectives of Modern Science* (New Haven, Conn., 1961), p. 183n.

Recalling a point made earlier, let us ask a positivist which method of verification we should use, according to his philosophy, for the familiar proposition "the probability of this coin to show heads is 1/2." He could not possibly answer that we should perform an infinite number of observations, as required by the definition (D_1), for in that case he would implicitly admit that there are propositions that cannot be verified and, hence, cannot be classified as either sense or nonsense. Nor could he tell us to perform "a *sufficiently large number* of observations," because, among other things, he would be caught flagrante delicto of using a dialectical concept! But let us see what the typical answer tells us in essence: "If a coin were thrown a thousand times and the head came up 490 times, we would regard this as supporting the hypothesis that the probability of its coming up is 1/2 . . .; but if it came up only 400 times we would normally reject the hypothesis . . . We proceed in this manner because . . . there is a tacit acceptance of *some* degree of allowable deviation, however *vaguely* we may formulate it to ourselves."[54] The dialectical cat is thus let out of the positivist bag.

To be sure, the calculus of probabilities provides us with a number which statisticians call degree of confidence in a hypothesis. But what particular degree of confidence draws "the absolutely sharp and distinct line" between a verified and a false proposition is a question that has to be answered before positivists can make good their claim about the absolute distinction between "meaning and nonsense." This is not all, however. From the fundamental theorems of probability calculus—which are endorsed by the Frequentist doctrine, too—it follows that a coin that has been verified to be fair with an immensely great degree of confidence might nevertheless show only "heads" for an infinite number of times.[55] The Frequentist definition, therefore, harbors a contradiction of Logic. And this contradiction will stay with us as long as we refuse to recognize that since any proposition on probability—understood as a physical coordinate—turns to probability for its verification, the definition of probability has to be circular.

The basic fault of the Frequentist doctrine resides in not seeing that, *viewed as a whole, a sequence of observations is a random event just as a single observation is.* And just as a single observation may be erratic, so may a sequence be. Therefore, we must allow for the fact that in some cases f_n may have a limit different from p or no limit at all. But we need to add that the probability of a sequence for which f_n tends toward p is very

[54] Ernest Nagel, "The Meaning of Probability," *Journal of American Statistical Association,* XXXI (1936), 22. "Vaguely" italicized by me.

[55] For the apparent contradiction between this statement and the fact that the probability of such an occurrence is "zero," see Appendix A, para. 1 and 13.

large, and the longer the sequence is, the larger is this probability. More exactly, this probability tends to unity as n tends toward infinity. Let us therefore posit the following:

If E is a random event, there exists a number p such that for any positive numbers ϵ and δ there is an integer N such that

$$1 > \mathrm{Prob}\,[|f_n - p| < \epsilon] > 1 - \delta \qquad (\mathrm{D}_2)$$

for any $n > N$.

A few things should be made clear about this proposition. First, the condition that the middle term in (D_2) should be smaller than unity is indispensable. Only in this way can we acknowledge that erratic sequences must occur at times. Second, the proposition must be viewed as a law of nature for which we may reserve the name of the Law of Large Numbers and, thus, put an end to the confusion ordinarily surrounding the nature of this law. Third, we may regard the proposition as a definition of physical probability. We need only observe that this definition includes both the beginning and the end of a complete thought, in order to see that there is no way of conceiving probability other than in the purest Hegelian sense. If probability is the ultimate element of nature, then forcibly its definition must rest on probability.

And if the centuries-old struggle with the problem of finding an *analytical* definition of probability has produced only endless controversies between the various doctrines, it is, in my opinion, because too little attention has been paid to the singular notion of random. For the dialectical root, in fact, lies in this notion: probability is only an arithmetical aspect of it.

That the notion of random involves an irreducible contradiction is beyond question. To begin with, random order must be so irregular as to exclude any possibility of representing it by an analytical formula. This is the essence of the highly interesting observation of Borel that the human mind is incapable of imitating the hazard.[56] But long ago Joseph Bertrand queried, "How dare we speak of the laws of hazard? Is not hazard the antithesis of any law whatsoever?"[57] The answer to Bertrand's query is that random does not mean wild haphazard, that is, complete absence of order. The opposition between the thesis of random's irregularity and the antithesis of random's peculiar order finds its synthesis in the notion of

[56] Émile Borel, "Sur l'imitation du hasard," *Comptes Rendus, Académie des Sciences*, CCIV (1937), 203–205. However, Borel's claim (Émile Borel, "Les probabilités" *Encyclopédie Française*, I, 1. 96-4) that he has offered a demonstration of that impossibility is spurious.

[57] Joseph Bertrand, *Calcul des probabilités* (Paris, 1889), p. vi. My translation.

probability. From this comes the circularity of the definition of probability, whether in the Laplacean or the Frequentist form.

Another upshot of the preceding argument is that the opposition between Poincaré's view—that random is a residual of the imperfection or incompleteness of our knowledge—and the tenet of modern physics—that random is an intrinsic aspect of the mode of being of things in general—is fictitious. As was often the case with him, Poincaré did offer some startling demonstrations of how statistical permanences may arise from causal relationships if very small variations in the initial conditions of the system (say, a roulette wheel) produce appreciably different outcomes.[58] All these proofs assume, however, that the initial conditions are subject to some, not necessarily known, probabilistic law. That is, Poincaré did not create random from a purely causal structure. And in rounding up the justification of his position that random is connected with ignorance, Poincaré explained that the information supplied by probabilities "will not cease to be true upon the day when these [fortuitous] phenomena shall be better known."[59] The statement lays bare the crux of the whole matter. For let us suppose that one day we discover some subquantum phenomena that will enable us to predict which atom(s) of radium shall disintegrate next. We shall still have to explain why the disintegration of atoms left to themselves follows a random order.

We can understand then why, ever since the dawn of statistical mechanics, physicists have shown a marked weakness for the idea that random can be generated by a system governed by causal relationships *alone*. Among the numerous attempts to justify this idea, that of David Bohm provides a good instructive ground. In explaining that if automobile accidents are unpredictable it is only because we can neither ascertain nor take account *ex ante* of all the numerous factors that *ex post* explain each individual accident, Bohm merely follows Poincaré.[60] The same is true of his position that the world is governed by infinitely many laws and, hence, there is always an infinity of laws or factors that remain beyond the reach of science.[61] What he tries to build on this foundation, however, is not always clear. He says that "the assumption that the laws of nature constitute an infinite series of smaller and smaller steps that approach what is in essence a mechanistic limit is just as arbitrary and unprovable as is the assumption of a finite set of laws permitting an exhaustive

[58] Henri Poincaré, *Calcul des probabilités* (Paris, 1912), pp. 146–152, and Poincaré, *Foundations of Science*, pp. 403–406.

[59] Poincaré, *Foundations of Science*, p. 396.

[60] Bohm, *Causality and Chance*, pp. 2 f, 21 ff. See also D. Bohm and W. Schützer, "The General Statistical Problem in Physics and the Theory of Probability," *Nuovo Cimento*, Suppl. Series X, II (1955), 1006–1008.

[61] Bohm, *Causality and Chance*, *passim*.

treatment of the whole of nature."[62] This statement proves that Bohm implicitly recognizes the existence of an irreducible random *residual*. But then his subsequent statement that randomness is the result of the fact that the infinitely many factors "left out of any such system of [finite] theory are in general undergoing some kind of a random fluctuation,"[63] is puzzling. For if we assume a random residual, two conclusions follow immediately: first, the additional assumption of infinitely many laws governing the same phenomenon is no longer necessary to explain random, and second, regardless of how many factors are left out of account the deviations of the observed from the "theoretical" values are not purely random errors. The reason why Bohm brings in the infinity of laws is that he wants to justify the last proposition on the "well-known theorem [according to which] the effects of chance fluctuations tend to cancel out."[64]. However, the famous theorem has power over actuality if and only if each effect is produced by a *random* cause, which must be defined independently of the theorem. And if each cause is subject to random, again we do not need an infinity of them for explaining random. Like many other writers on probability, Bohm seems to confuse here an abstract mathematical theorem with the actual behavior of nature. This confusion is neatly obvious in his claim (so dear to many physicists) that a "determinate law" always generates random provided that the mechanism governed by it is such that extremely small variations in the initial conditions produce appreciably different results. As is the case in similar arguments by others, what he proves in fact is an ergodic geometrical theorem which, needless to say, is in antithetical opposition to the idea of random.[65]

Curiously, the authors who set out to prove the reducibility of random to causality usually raise a corner of the veil that covers the fallacy of their formal arguments. Thus Bohm seems to ignore that mechanics alone cannot justify the proposition that, because of the symmetry of the die

[62] *Ibid.*, p. 134. In this connection, we may recall the opposite belief shared by many statisticians in the applied fields who, explicitly or implicitly, think that if the regression function would include all "nonspecified factors" a perfect correlation (i.e., a totally determined relationship) would obtain. The thought is that the product

$$1 - R^2_{1.23\ldots n} = (1 - r^2_{12})(1 - r^2_{13.2})\cdots(1 - r^2_{1n.23\ldots n-1}),$$

where R and the r's are the standard notations for correlation coefficients, must tend toward zero for $n \to \infty$ since it decreases with every successive factor, $1 - r^2_{1n.23\ldots n-1} < 1$. However, on purely mathematical grounds, the limit of that product need not be zero.

[63] *Ibid.*, p. 141; also Bohm and Schützer, p. 1008

[64] Bohm, p. 23.

[65] Bohm and Schützer, pp. 1024 ff. More on this issue in Chapter VI, Section 3, below.

and the complexity of the hand's motion, "in the long run and in the average, these fluctuations [of the outcome] favor no particular face."[66] On the other hand, to take this proposition as an independent basis for random and probability is to go back to old man Laplace and his subjective Principle of Insufficient Reason.

The only way out is to face the fact that in the mode of being of the actual world there is an order which, because of its dialectical nature in the Hegelian sense, cannot be represented by an analytical (strictly causal) formula. Like the pain which cannot exist either without the needle or without the sentient being, random is a relational element. The two opposing views on random, about which we have spoken here, are the two ends of one and the same bridge between human understanding and the actual world.

[66] Bohm and Schützer, p. 1011. The most convincing counter example of this thesis is the fact that even though the sequence of Monte Carlo "random" numbers is constructed by a procedure that satisfies both the condition of instability and of statistical trends, the sequence in the end hits a constant run. The situation is entirely analogous to Poincaré's famous slip in asserting that the third decimal digits in a logarithm table form a random sequence (Poincaré, *Foundations of Science*, pp. 161 f).

CHAPTER III *Change, Quality, and Thought*

1. Science and Change. As explained in the opening chapter, Greek philosophy began by asking what causes things to change. But the recognition of Change soon raised the most formidable question of epistemology. How is knowledge possible if things continuously change, if "you cannot step twice into the *same* rivers," as the obscure Herakleitos maintained?[1] Ever since, we have been struggling with the issue of what is *same* in a world in flux. What is "same"—as Mach asked—in a sodium vapor which, as its temperature increases, turns from violet to yellow? Or, as Bridgman asked, what is "same" in a tumbler of water that continuously evaporates?[2] Numerous are the unrelenting attempts at answering this question by arithmomorphic schemes (and we shall presently examine the most ambitious one, by Bertrand Russell). David Bohm's observation, that at each instant of time a thing has "an enormous (in fact infinite) number of aspects which are in common with those that it had a short time ago,"[3] shows why the question raised by Mach is far from idle; yet it does not answer it. Many present things have an infinite number of aspects common to each one and also common to many things of a short while ago. So we still do not know which of all possible pairings

[1] Fragment 41 in J. Burnet, *Early Greek Philosophy* (4th edn., London, 1930), p. 136. My italics.

[2] Ernst Mach, *Popular Scientific Lectures* (Chicago, 1895), p. 202; P. W. Bridgman, *The Logic of Modern Physics* (New York, 1928), p. 35. Bridgman adds that even $2 + 2 = 4$ collapses if applied to "spheres of a gas which expand and interpenetrate."

[3] David Bohm, *Causality and Chance in Modern Physics* (London, 1957), p. 157.

of one past and one present thing represent the "same thing." And if we rest satisfied with the argument of the continuation in time of a particular quality of the *things* observed, then we must necessarily accept as perfectly scientific also the procedure by which Lamaism decides who is the *same* Dalai Lama through death and birth.

On the other hand, if there were no Change at all, that is, if things have been and will always be as they are, all science would be reduced to a sort of geometry: *ubi materia, ibi geometria*—where there is matter, there is geometry—as Kepler thought.

The knot was cut but not untied by the distinction, introduced quite early, between change of nature and change of place.[4] And since, as Aristotle was to express it straightforwardly, "place is neither a part nor a quality of things,"[5] it appeared expedient to resolve that all Change is locomotion, change of nature being only appearance. To avoid any reference whatever to quality, the ancient atomistic doctrine originated by Leukippos held that Change consists only of the locomotion of atomic particles of a *uniform* and *everlasting* matter. The first systematic criticism of monistic atomism came from Aristotle, who opposed to it the doctrine of matter and form. This led him to analyze Change into change (1) of place, (2) of quantity (related to change by generation or annihilation), and (3) of quality.[6] Though we have ever since abided by this analysis in principle, the attitude of science toward Change has had a most uneven history.

To begin with, atomism suffered a total eclipse for some two thousand years until Dalton revived it at the beginning of the last century. It then gradually came to rule over almost every chapter of physics. However, the recent discoveries of one intra-atomic particle after another, all qualitatively different, have deprived monistic atomism of all its epistemological merit. Quality, being now recognized as a primary attribute of elementary matter, is no longer reducible to locomotion. For the time being, one point of Aristotle's doctrine is thus vindicated.

For quite a while change by generation and annihilation lingered in Scholastic speculations. But after the various principles of conservation discovered by physics during the last hundred years, we became convinced that this type of change was buried for good. Only cosmologists continued to talk about the creation of the universe. However, the idea that matter is continuously created and annihilated in every corner of the universe has recently acquired increasing support from a number of physicists. If it turns out to be a helpful hypothesis, then it may not only revolutionize

[4] See, for instance, Plato, *Parmenides*, 138.

[5] Aristotle, *Physics*, 209^{b} 26–27, 221^{a} 1.

[6] *Physics*, 190^{a} 33–37, 260^{a} 27–29.

cosmology but also solve the greatest mystery of physics, that of gravitation.[7] The universe also will become more intelligible because its laws will then become truly invariant with respect to Time. It is thus quite possible that we shall return to Aristotle's views and reconsider the modern axiom that "the energy concept without conservation is meaningless."[8] In fact, we already know that the concept of entropy (which will occupy us in the subsequent chapters) is not meaningless, in spite of the fact that the very nature of entropy is to increase continuously.

Qualitative change has never ceased to be a central theme of the life sciences. But, time and again, the admiration produced by the operational successes of physics in almost every direction—in spite of its decision to ignore Change—misled us into thinking that science cannot study Change. Social scientists, especially, continue to pay fervent lip service to this principle.[9] In spite of all these professions and the repeated arguments in their support, we may as well recognize that the highest ambition of any science is to discover the laws of whatever Change is manifest in its phenomenal domain.

That is the most challenging task of science. Contrary to what many scientists maintain,[10] the maturity of any science is not measured by the extent to which it has been able to construct a mechanical representation of its special phenomenal domain. Instead, as David Bohm (a physicist!) argues, the maturity must be judged by the ability to "consider the processes in which things *have become* what they are, starting out from what they once *were* and in which they continue to change and to become something else again in the future."[11] To do this, even astrophysicists, not only biologists, have to fall back on Aristotle's doctrine of Change, of matter *and* form. Creation in the "Big Bang" hypothesis—explains Gamow—means "making something shapely out of shapelessness."[12] It is not therefore superfluous to raise a huge question mark in relation to the prevalent temper in economics to ignore qualitative change and even to belittle all preoccupations with it despite the fact that in the economic domain Change is even more the soul of what happens than in astrophysics.

2. *Change and Dialectical Concepts.* The undeniably difficult problem

[7] This hypothesis will be discussed in some detail later on (Chapter VIII). Here I may only note that the idea that matter is continuously created and annihilated had already been revived in other circles. See, for instance, Henri Bergson, *Creative Evolution* (New York, 1913), pp. 246n, 368 f.

[8] Bridgman, *Logic of Modern Physics*, p. 127.

[9] E.g., Frank H. Knight, *The Ethics of Competition* (New York, 1935), p. 21.

[10] E.g., Henri Poincaré, *Mathematics and Science: Last Essays* (New York, 1963), p. 8.

[11] Bohm, *Causality and Chance in Modern Physics*, p. 15.

[12] G. Gamow, *The Creation of the Universe* (New York, 1952), p. vii.

of describing qualitative change stems from one root: qualitative change eludes arithmomorphic schematization. The leitmotiv of Hegel's philosophy, "wherever there is movement, wherever there is life, wherever anything is carried into effect in the actual world, there Dialectic is at work,"[13] is apt to be unpalatable to a mind seasoned by mechanistic philosophy. Yet the fact remains that Change is the fountainhead of all dialectical concepts. "Democracy," "feudalism," "monopolistic competition," for instance, are dialectical concepts because political and economic organizations are continuously evolving. The same applies to "living organism": biological life consists of a continuous and insinuating transformation of inert into living matter. What makes "want" a dialectical concept is that the means of want satisfaction can change with time and place: the human species would have long since vanished had our wants been rigid like a number. Finally, "species" is dialectical because every species "includes the unknown element of a distinct act of creation."[14]

The reason that compelled Plato to exclude all *qualitative change* from his world of arithmomorphic ideas is obvious. The issue of whether motion too is excluded from this world is not discussed by Plato. But we can be almost certain that he had no intention—for there was no need for it—of conceiving that world as *motionless*. He thus implicitly recognized that an arithmomorphic structure is incompatible with qualitative change but not with locomotion, even though he admitted that Change consists of either.[15] As a result, Plato was as puzzled as the generation before him by Zeno's paradoxes, and could not crack them.

Through his paradoxes Zeno aimed to expose the flaws of the Pythagorean doctrine of Many as opposed to Parmenides' doctrine of One. The Arrow Paradox, in particular, intends to prove that even locomotion is incompatible with a molecular (i.e., arithmomorphic) structure of Space and Time. For, to reinterpret Zeno, if at any given instant the arrow is in some *discretely distinct* place, how can it move to another such place? Some argue that the paradox is resolved by defining locomotion as a relation between a time-variable and a space-coordinate.[16] The fact that this "mathematical" solution is good enough for physics is beyond question. However, in one respect the paradox is simpler, while in another more intricate, than this solution suggests.

It is simpler, because all that Zeno does is to ignore the qualitative

[13] G. W. F. Hegel, *The Logic of Hegel*, tr. W. Wallace (2nd edn., London, 1904), p. 148. One page earlier he says that "the Dialectical principle constitutes the life and soul of scientific progress."

[14] Charles Darwin, *The Origin of Species* (6th edn., London, 1898), p. 30.

[15] Plato, *Parmenides*, 139.

[16] E.g., Bertrand Russell, *The Principles of Mathematics* (Cambridge, Eng., 1903), chap. liv.

difference between "to be in a place" and "to move through a place," i.e., between "rest" and "motion." The error committed intentionally by Zeno and unconsciously by all who deluded themselves in believing they had defeated the paradox by simple mathematical formulae was to ignore the fact that if we refer only to an instant of time it is impossible to distinguish between "rest" and "motion." A perfectly instantaneous photograph of an automobile cannot possibly disclose whether the automobile was standing or moving. To know which was the case we need to observe the automobile over a time interval, however short. Without duration, even "rest" has no meaning. As Aristotle countered Zeno's paradox, both motion and rest "must occupy time."[17] And to listen to Louis de Broglie, whose persistent preoccupation with the opposition between particle and wave in microphysics is unique among physicists, there is a subtlety in that paradox which is revealed only by modern physics: "That which is in a point cannot be in motion or evolve; what moves and evolves cannot be in any point."[18]

So the paradox is more intricate than its mathematical solution leads us to believe, in that it discloses the perplexities surrounding the idea that Space and Time are not *continuous wholes* but mere *multiplicity of indivisible points.* As has been repeatedly pointed out by many mathematical authorities, these issues are still with us in spite of the splendid achievements of Dedekind, Weierstrass, and Cantor in connection with the arithmetical continuum.[19] No doubt, what these famous mathematicians mainly sought was a mathematical formalization of the intuitive continuum. Dedekind, in particular, constantly referred his argument to the intuitive aspects of the line continuum.[20] But Bertrand Russell's claim, still heard now and then, that "no other continuity [other than that of the arithmetical continuum] is involved in space and time,"[21] lacks any basis. The truth is that the proposition that there exists a one-to-one correspondence between the real numbers and the points on a line is either an axiom or a mathematical definition of line.[22]

[17] Aristotle, *Physics*, 234^b 8–9.

[18] Louis de Broglie, *Physics and Microphysics* (London, 1955), p. 126.

[19] E.g., Hermann Weyl, *Das Kontinuum* (Leipzig, 1918), p. 16; Henri Poincaré, *The Foundations of Science* (Lancaster, Pa., 1946), pp. 51 f.

[20] R. Dedekind, *Essays on the Theory of Numbers* (Chicago, 1924), pp. 6–12. Actually, until Weierstrass the irrational number had no other basis in mathematics than the geometrical representation inherited from the Pythagoreans. Cf. G. Cantor, *Contributions to the Founding of the Theory of Transfinite Numbers* (New York, n.d.), p. 17.

[21] Russell, *Principles of Mathematics*, p. 260.

[22] See G. D. Birkhoff, "A Set of Postulates for Plane Geometry, Based on Scale and Protractor," *Annals of Mathematics*, XXXIII (1932), 329, and Cantor, *Contributions*, p. 30.

Developments in mathematics—later than Russell's statement quoted above—prove that Aristotle's tenet, point is the limit of line not *part* of it,[23] is far from groundless.

In the first place, the modern theory of measure is a belated admission that at least the tenet is not concerned with a pseudo problem. Still more telling is Ernst Zermelo's famous theorem that the arithmetical continuum can be well ordered, which means that every real number has an immediate successor. Even though this immediate neighbor cannot be *named*, the proof of its existence bears on a point made earlier, namely, that a number has a perfectly isolated individuality.[24] Whatever properties the arithmetical continuum might have, its structure is still that of beads on a string, but *without the string*.

There is no need whatsoever, I believe, to insist on the point that each element of the arithmetical continuum is exactly like a bead, "an individual thing absolutely distinct from the others and, moreover, absolutely indivisible."[25] The point is now a mathematical commonplace. On its basis, Henri Bergson even made the penetrating observation that "to posit the impenetrability of matter simply means to recognize the solidarity of the notions of number and space, to enunciate a property of numbers rather than of matter"—and at least one famous physicist went along with the idea wholeheartedly.[26] But the metaphor involving "the string"—by which I wish to convey the idea that the beads of the arithmetical continuum are packed next to each other as those of an ordinary necklace—might call for additional elaboration. Since this elaboration cannot possibly shun technicalities, it is relegated to Appendix A.

3. The Intuitive versus the Arithmetical Continuum. There is real

[23] Aristotle, *Physics*, 231ª 25–29.

[24] Modern logicians have acquired a rather peculiar habit: each time a paradox springs up they legislate new rules outlawing one of the steps by which the paradox is reached. Clearly, the procedure means nothing less than the hara-kiri of reason. (Cf. H. Weyl, *Philsophy of Mathematics and Natural Science*, Princeton, 1949, p. 50. See also the sharp criticism by Henri Poincaré, *Foundations*, pp. 472 ff, esp. p. 485.) In any case, it does not *resolve* the paradox; it merely *shelves* it. As to Zermelo's theorem, the proposal is to outlaw choosing a member of a set without actually naming it. To use the highly instructive analogy of Bertrand Russell, *Introduction to Mathematical Philosophy* (New York, 1930), p. 126, though it would be legal to choose the left boot from a pair of boots, choosing one sock out of a pair of identical socks would be an illegal operation. I completely fail to see why the latter choice would be improper in a domain like mathematics where far more bizarre operations are performed all the time. Is not marrying nobody to nobody—as in the mapping of the null set onto itself—a most bizarre idea?

[25] Poincaré, *Mathematics and Science: Last Essays*, p. 30.

[26] Henri Bergson, *Essais sur les données immédiates de la conscience* (Geneva, 1945), p. 77; Louis de Broglie, *Physique et microphysique* (Paris, 1947), p. 208. (I must refer the reader to the French original of Broglie's work because in the English translation the chapter on Bergson has been altered beyond recognition.)

wisdom in the advice that science should not borrow words from the common vernacular for labeling its newly created concepts. If a word has been in use for a long time, there is a resilient stickiness between that word and its old connotations. Some of these connotations, although entirely foreign to the new concept, may slide into our mental image of it and cause us to lose sight of the difference between the new and the old concepts. Late in life, the architect of modern utilitarianism, Jeremy Bentham, lamented that "*utility* was an unfortunately chosen word" for the special concept he had in mind for the new political science:[27] economic analysis is still paying a heavy price for that unfortunate choice. Similarly, the use of the word "continuum" for denoting an aggregate of isolated, discretely distinct, elements is no doubt responsible for the frequent assertions that there is no other kind of continuity. Yet, before the arithmetical continuum acquired its legal status in mathematics, the notion denoted by continuum was intended to express an intuitive property of Time, Space, and Nature itself—that of being *seamless* wholes.

The dictum that "things that are in one world are not divided nor cut off from one another with a hatchet" goes back to the ancient Anaxagoras.[28] In contrast to the arithmetical continuum, the world continuum has no holes, not even joints where a good carver could, as Plato thought, separate one species from another.[29] Numbers more than anything else are artificial slits cut by us into this whole. Of course, given any whole we can make as many slits into it as we please. But the converse claim, implicit in arithmetical positivism, that the whole can be reconstructed from the slits alone rests on the thinnest air.

The familiar objection of the Logical absolutism, that nobody has yet been able to supply a formal definition of the intuitive continuum, stands on a solid fact which, nevertheless, is no proof against the validity of that concept. The intuitive continuum belongs to that special category of concepts about which we can discourse with each other without being able to define them. All-out for Logic though he was (he made the most illustrious attempt at reducing Arithmetic to Logic), G. Frege warned Logical absolutists that for what is "logically simple [fundamental], a definition is not possible; there is nothing for it but to lead the reader or hearer, by means of hints, to understand the words as is intended."[30] This category is the natural habitat of all dialectical concepts, which all

[27] Jeremy Bentham, *The Works of Jeremy Bentham*, ed. J. Bowring (11 vols., Edinburgh, 1838–1843), X, 582.

[28] Anaxagoras, Fragment 8, in Burnet, *Early Greek Philosophy*, p. 259.

[29] Plato, *Phaedrus*, 265.

[30] *Translations from the Philosophical Writings of Gottlob Frege*, eds. P. Geach and M. Black (Oxford, 1960), p. 43.

descend in fact from that of the intuitive continuum. Change itself is inconceivable without this continuum.

One might also interject that under the dialectical cloak some humbugs could be introduced into science. Many humbugs have actually been thus introduced. But many others have entered science clad in a splendid Logical mantle. Whether we like it or not, we have no philosopher's stone for touching quickly a humbug. As to the intuitive continuum, it would be the crown of ineptitude (or of intellectual arrogance) to claim that the human mind has constructed the arithmetical continuum by a pure whim of fancy, without being guided by any preexisting conceptual form. If this were so, then we would have in it the greatest miracle since Creation: a concept that came from nowhere and out of nothing and happens nevertheless to fit so well the geometrical space and the time of physical sciences. The historical facts dispense us from paying attention to this puzzle, for they reveal that there is no such miracle. The arithmetical continuum is the product of a hard struggle with the problem of how to portray the intuitive continuum so as to render the greatest possible part of its content accessible to mathematical analysis. "At the time when the theory of the arithmetic continuum was developed, the only conception of the continuum which was extant was that of the continuum as given by intuition"—vouches a mathematician of great repute.[31] The problem of portraying the intuitive continuum did not cease to preoccupy the minds of mathematicians even after the arithmetical continuum was constructed in the form known to us now. There followed many interesting attempts at incorporating into an expanded arithmetical structure some features of the intuitive continuum that had been left outside the arithmetical continuum.[32] It does not do, therefore, to argue that the notion of the intuitive continuum became a humbug after it had served the purpose of leading to the arithmetical continuum. There is no analogy whatsoever between the case of the intuitive continuum and, say, the old belief that any continuous function has a derivative. That belief involved an error of Logic, the unwarranted identification of two different mathematical constructs. On the other hand, the issue of the intuitive continuum being an epistemological one, no conceivable test exists by which to settle it.

We can say, however, that the impossibility of defining formally the intuitive continuum is a logical consequence of the opposition between the essential property of numbers to be distinctly discrete and the characteristic property of the intuitive continuum to consist of dialectically overlapping elements leaving no holes. Perhaps the most adequate way

[31] E. W. Hobson, *The Theory of Functions of a Real Variable and the Theory of Fourier's Series* (2 vols., New York, 1957), I, 53.

[32] See Appendix A in this volume.

yet to express this peculiar structure is the suggestion of Poincaré: in the intuitive continuum, $A = B$, $B = C$ is perfectly compatible with $C > A$.[33] By this he meant that even though A is indistinguishable from B and B from C, C may be distinguished from A—a property that recalls my own notion of the dialectical penumbra separating A from its opposite. We must, however, guard ourselves against the deadly sin of attributing to the signs in these relations their arithmetical meanings and then submitting them to an analysis by the rules of Logic. Only by doing precisely this could Borel reach the conclusion that Poincaré's idea is absurd.[34] The point is that, whatever novel ideas the future may offer for expanding the present concept of the arithmetical continuum, it does not appear possible to resolve the clash which at present exists between the reality constructed with numbers and our intuition. There is no way of making sense of Poincaré's idea from the arithmomorphic standpoint. In an analogous manner, our intuition rebels at some results of arithmetical analysis. An excellent example is provided by Cantor's ternary set. This set is a subset of the interval (0, 1) and, although it has the same power (i.e., as many points) as that interval, its measure is zero. The oddity is that it would thus seem possible to remove an enormous number of material points from a ruler without affecting its material content and structure at all.[35]

To conclude, however, that in this clash between our intuition and the edifice constructed with numbers our intuition alone errs is the highest form of non sequitur. One of the present crises in microphysics, for instance, stems precisely from the fact that the current theories assume that the elementary particles are mere points in a three-dimensional arithmetical continuum.[36] And even though our present conception is that nature is made of indivisible quanta, the same conception rejects the idea that things can be "cut off from each other." As Niels Bohr insisted, the object and the physicist's instrument form an indivisible whole: at least one quantum overlaps both. Actually, the object, the instrument, and the observer form an indivisible whole, since there must be at least one quantum in the instrument and the observer.

[33] Poincaré, *The Foundations of Science*, p. 46.

[34] Émile Borel, *Probability and Certainty* (New York, 1963), pp. 106–109. Borel's argument is that from $A = B$, $B = C$, $A < C$, it must necessarily follow that $A < B < C$. For if either $C < B$ or $B < A$, then either $A < B$ or $B < C$ respectively; one of the premises would thus be contradicted. Strangely, Borel did not realize that his sin was duofold: as is easily seen, his argument is not sound even according to the rules of Logic. A formal counter example is available from my analysis of the threshold in choice in the article "The Pure Theory of Consumer's Behavior" (1936), reprinted in AE, pp. 158 f.

[35] Émile Borel, *Les paradoxes de l'infini* (2nd ed., Paris, 1946), pp. 189–191.

[36] David Bohm, *Causality and Chance in Modern Physics*, pp. 121–123. Naturally, the crisis is that in the paper-and-pencil operations there appear infinite energies.

4. Instants of Time and Duration. Concerning the issue just debated, one should not pass lightly over the fact that none other than a coauthor of *Principia Mathematica*, Alfred North Whitehead, has erected his entire philosophical system on the essential difference between the continuum of the world and that of arithmetical analysis. That he centered his argument on Time is natural: Time is the very origin of the intuitive continuum. But the essence of Whitehead's philosophical position is not altogether new. Before him, and perhaps even with greater insistence, Henri Bergson saw in the peculiar nature of Time the answer to many philosophical problems, especially that of consciousness and evolution.

Aristotle, again, was first to argue that time is not made of point-instants succeeding each other like the points on a line.[37] The message has been variously echoed throughout the subsequent centuries. In modern times, it has been revived not only by philosophers, such as Henri Bergson or Whitehead, but also by prominent physicists: the "now" of our experience is not the point of separation in mathematics.[38] Bergson and Whitehead, however, go much further and explain why the difference matters in science in spite of the fact that physics has fared splendidly without any overt concern for the intuitive continuum. As Whitehead admits,[39] they both insist that the ultimate fact of nature is Change. Whether we prefer to use instead the word "happening," "event," or "process," Change requires time to be effected or to be apprehended. Nature at an instant or state of change at an instant are most forbidding abstractions. To begin with, there is no answer to the question "what becomes of velocity, at an instant?" Even "iron at an instant" is unintelligible without the temporal character of an event. "The notion of an instant of time, *conceived as a primary simple fact*, is nonsense."[40] The ultimate facts of nature vanish completely as we reach the abstract concept of point of Time. An instant has an arithmomorphic structure and, hence, is indifferent to "whether or no there be any other instant."[41]

[37] Aristotle, *Physics*, 231^b 6–10, 234^a 23.

[38] P. W. Bridgman, *The Intelligent Individual and Society* (New York, 1938), p. 107.

[39] Alfred North Whitehead, *The Concept of Nature* (Cambridge, Eng., 1930), p. 54.

[40] Alfred North Whitehead, *Modes of Thought* (New York, 1938), pp. 199, 207 (my italics); *An Enquiry Concerning the Principles of Natural Knowledge* (2nd edn., Cambridge, Eng., 1925), p. 23. The same ideas occur as a leitmotiv in all Whitehead's philosophical works, although they are more clearly stated in the early ones. See his *Enquiry*, pp. 1–8, and his "Time, Space, and Material," *Problems of Science and Philosophy*, Aristotelian Society, Supp. vol. II, 1919. See also Erwin Schrödinger, *Science, Theory and Man* (New York, Dover Publications, 1957), p. 62.

For Bergson's approach, see Henri Bergson, *Time and Free Will* (3rd edn., London, 1913), and his *Creative Evolution*.

[41] Whitehead, *Modes of Thought*, pp. 199 f; Whitehead, "Time, Space, and Material," p. 45.

The ultimate fact of nature, Bergson's becoming or Whitehead's event, includes a *duration* with a temporal extension.[42] But "the immediate duration is not clearly marked out for our apprehension." It is rather "a wavering breadth" between the recalled past and the anticipated future. Thus, the time in which we apprehend nature is not "a simple linear series of durationless instants with certain mathematical properties of serial [arithmetic] continuity,"[43] but a *sui generis* seriation of durations. Durations have neither minimum nor maximum extension. Moreover, they do not follow each other externally, but each one passes into others because events themselves "interfuse." No duration is discretely distinct from its predecessor or its successor, any more than an event can be completely isolated from others: "an isolated event is not an event."[44] Durations overlap durations and events overlap events in a peculiar complexity, which Whitehead attempted with relative success to analyze through the concept of extensive abstraction and abstractive classes.[45] However, everything he says in "vague" words leaves no doubt that both "duration" and "event" as conceived by Whitehead are concepts surrounded by dialectical penumbras, in our sense.[46] The same conclusion emerges, though less pointedly, from Bergson's writings: "That which is given, that which is real, is something intermediate between divided extension [the time interval] and pure inextension [the instant]."[47]

In a nutshell, the position of Whitehead and Bergson is that Time is filled with events that endure and overlap in a dialectical succession.

[42] Bergson, *Time and Free Will*, pp. 98 ff; Bergson, *Creative Evolution*, pp. 1–7; Whitehead, "Time, Space, and Material," pp. 45 f; Whitehead, *Enquiry*, chap. ix.

[43] Whitehead, *Concept of Nature*, p. 69 and *passim*; Whitehead, "Time, Space, and Material," p. 44; Bergson, *Creative Evolution*, pp. 21 f. Also P. W. Bridgman, *The Nature of Physical Theory* (Princeton, 1936), p. 31.

[44] Whitehead, *Concept of Nature*, p. 142.

[45] Whitehead, *Enquiry*, Part III. In my opinion, his analysis represents rather a *simile*, for in the end his operations of extensions, intersection, etc., imply discrete distinction, as is obvious from the diagrammatical analysis on his pp. 103, 105. In line with Whitehead's position, C. D. Broad, *Examination of McTaggart's Philosophy* (2 vols., Cambridge, Eng., 1933–1938), vol. II, part I, 284, rightly points out that specious presents (i.e., durations) are not adjoining. They must overlap; for otherwise presentness would be subject to "repeated sudden jumps." But, just like Whitehead, he was unable to describe the overlapping by a graph free from arithmomorphic "jumps" (*ibid.*, pp. 285–288).

[46] See the following works by Whitehead: "Time, Space, and Material," p. 51; *Enquiry*, p. 4 and *passim*; *Concept of Nature*, pp. 55, 59, 72 f, 75; *Process and Reality: An Essay in Cosmology* (New York, 1929), p. 491; *Science and the Modern World* (New York, 1939), pp. 151, 183 ff.

[47] Henri Bergson, *Matter and Memory* (London, 1913), p. 326 and *passim*. This dialectic feature of time is admirably expressed by F. H. Bradley, *Appearance and Reality* (2nd edn., Oxford, 1930), p. 52. "Time . . . must be made, and yet cannot be made, of [discretely distinct] pieces."

Above all, Time is not a sequence, however dense, of durationless instants representable by numbers. The reason why this simplistic scheme exercises nevertheless such a great fascination even on some professional philosophers is that we all have the tendency to think of instants rather than duration in relation to Time. Whether as physicists in a laboratory or as ordinary people going about our business, we are preoccupied primarily with coincidences—the coincidences of a clock's hand with one of the points on the dial. "It is half past three and he has not shown up yet," or "I was just leaving when the telephone rang," are typical of our way to notice Time. We rarely pay conscious attention to the flux of Time, and even when we do, more often than not we again refer to coincidences.

When we observe motion we also focus our attention on coincidences, the passage of the moving body through some place or other. And as Bergson observes, we thus imagine that the moving body "*might* stop there; and even when it does not stop there, [we] incline to consider its passage as an arrest, though infinitely short, because [we] must have at least the time to think of it."[48] This is how we get the illusion—against which Zeno aimed his paradoxes—that motion consists of a sequence (dense, to be sure) of rests. Nothing need be added to bring to the surface the full incongruity of the entirely equivalent position that Time is nothing but a dense sequence of durationless instants.

A philosopher should know better than to consider the Bergson-Whitehead position refuted by the indisputable fact that kinematics can operate with an arithmetical time.[49] True, all that physical sciences need most of the time is coincidences, clock readings. Also, a physicist may very well argue that $s = vt$ is shorthand for $\Delta s = v\Delta t$. But even in classical physics this explanation does not always work: one illustration is provided by the phenomenon of surface stress.[50] Speaking from his authoritative knowledge of microphysics, Broglie argues that if Bergson's critique of durationless time and motionless movement sins at all "it is rather by an excess of cautiousness."[51] He refers to Heisenberg's Principle of Indeterminacy, according to which the errors of observation, Δx and Δp, of the position and the momentum of a particle are subject to the inequality $\Delta x \times \Delta p \geq h$ (where h is Planck's constant). Hence, "if one tries to localize a particle by measurement or observation, in some point of the space, he will obtain only its position and have no knowledge whatsoever

[48] Bergson, *Matter and Memory*, p. 247.

[49] A. Grünbaum, "Are 'Infinity Machines' Paradoxical?" *Science*, January 26, 1968, p. 398, commits another indiscretion as well: without citing a single text in support, he attributes to Whitehead the idea that durations succeed each other not in a dialectical continuity but "in the manner of a discrete sequence."

[50] Whitehead, *Enquiry*, pp. 2 f.

[51] Louis de Broglie, *Physique et microphysique*, pp. 201 f.

about its movement."[52] Actually, what distinguishes modern from classical physics is the developments that go against the notion of an event at an instant of time and occurring in a dimensionless point of space. A while ago I mentioned the crisis caused by the reduction of elementary particles to points. Equally instructive is the fact that quantum phenomena beyond certain limits of smallness (10^{-13} cm for distance and 10^{-15} sec for time) present such bewildering aspects that we may safely judge that the very notions of Space and Time dwindle away as we try to push our extrapolation of objects and events to dimensionless limits.[53]

With regard to the opposition between Change and arithmomorphic structure, Whitehead's position is essentially the same as Hegel's. Perhaps in no other place does Hegel state his thought on the matter more clearly than in the following passage: "Number is just that entirely inactive, inert, and indifferent characteristic in which every movement and relational process is extinguished."[54] The statement has generally been criticized as Hegelian obscurantism and anti-scientism. Yet, as I have already intimated, Hegel did not intend to prove anything more than Whitehead, who maintained that no science can "claim to be founded upon observation" if it insists that the ultimate facts of nature "are to be found at durationless instants of time."[55] Whitehead only had the benefit of a far greater knowledge in mathematics and sciences of fact than was available in Hegel's time.

5. A Logistic Solution. Even though the onus of proof rests with him who affirms the operationality of an idea, no one among those who claim that Change can be completely described by means of arithmomorphic concepts seems to have shown how this can be done in all instances. (Merely to point at physics would be obviously insufficient even if physics were a model of perfection in this respect.) To my knowledge, there is only one exception, which thus is all the more instructive. In an *oeuvre de jeunesse*, Bertrand Russell asserted that any qualitative change can be represented as a relation between a time variable and the *truth-value*

[52] *Ibid.*, p. 201. My translations. Also Louis de Broglie, *New Perspectives in Physics* (New York, 1962), p. 9. (In the first edition of this essay I used "Principle of Indeterminacy" instead of the consecrated "Principle of Uncertainty," not because I wanted to stick new labels on old bottles—a practice which is far from my conception of scholarship—but because I believed that for a nonspecialist the first term describes better the meaning of Heisenberg's law. Subsequently, I have discovered that also some physicists consider the same term more appropriate. Cf. David Bohm, *Causality and Chance in Modern Physics*, p. 85n).

[53] Cf. Bohm, *ibid.*, pp. 105, 155.

[54] G. W. F. Hegel, *The Phenomenology of Mind* (2nd edn., New York, 1931), p. 317.

[55] Whitehead, *Enquiry*, p. 2, and *Concept of Nature*, p. 57.

of a set of propositions "concerning the *same* entity."[56] The assertion raises several questions.[57]

Perhaps we ought to ask first what "*same*" is in such a complex changing structure; however, it appears expedient to beg this question for a while. Therefore, let E denote "the same entity." To take the simplest possible case of a continuous change, what Russell further means is that (1) for every value of the time variable t, there is one proposition "E is $A(t)$" that is true, and (2) this very proposition is false for any other value of the time variable. Obviously, the set of all propositions "E is $A(t)$" and, hence, the set $[A(t)]$ have the power of continuum. There are now two alternatives.

First, $[A(t)]$ represents a range of a *quantified* quality. In this case $A(t)$ is a number, and Russell's solution is no better but no worse than the mathematical representation of locomotion. Its operationality, however, is confined to the domain of measurable qualities.

The second alternative, upon which the matter hinges, is the case where $A(t_1)$ and $A(t_2)$ for $t_1 \neq t_2$ represent two distinct pure qualities, like "feudalism" and "capitalism," for example. In this case, Russell's solution is purely formal, nay, vacuous. On paper, we can easily write that E is $A(t)$ at the time t, but if $A(t)$ is a pure quality it must be defined independently of the fact that it is an attribute of E at t. Obviously, to say that on January 1, 1963, the United States economic system is "the United States economic system on January 1, 1963" is the quintessence of empty talk. What we need is a proposition in which $A(t)$ is replaced by, say, "free enterprise under government vigilance." If $A(t)$ is a pure quality, i.e., if it cannot be represented by a number, then the representation of continuous change by Russell's scheme runs against a most elementary stumbling block: any vocabulary is a finite set of symbols. We may grant, at most, that the structure of vocabulary is that of a countable infinity, but certainly it does not have the power of continuum. Russell's proposal thus breaks down before we can ask any question of the sort that a logistic philosopher would dismiss as "metaphysical."

6. What Is Sameness? There are indeed other issues which cannot be pinpointed by the simple illustration I have used in the preceding discussion. The most relevant case of qualitative change is where for any value of t there is more than one true proposition concerning E. To take the simplest case, Russell's scheme tells us only this: given $t \neq t'$, there

[56] Russell, *Principles of Mathematics*, p. 469. My italics.

[57] A famous criticism of Russell's idea is that of J. M. E. McTaggart, *The Nature of Existence* (2 vols., Cambridge, Eng., 1927), II, ch. xxxiii, on the ground that time, itself, is not real (cf. Chapter V, Section 5, below). My own objections, which follow here, come from a different direction.

exists a pair of propositions, "E is A" and "E is B," true at t and false at t', and another pair, "E is C" and "E is D," true at t' and false at t. Nothing is said about whether the pairs are *ordered* or not. But without the condition that they are ordered, the scheme is inadequate for describing even a quantitative change. For what would become of any physical law if the observer were unable to ascertain which member in each pair, (A, B) and (C, D), represents, say, gas pressure and which represents temperature? To order each pair by using the Axiom of Choice would not do, even if we regard the axiom as perfectly legitimate. Therefore, if the scheme is to be operational at all, it must include from the outset the condition that one member of every pair, such as (A, B), belongs to a set $[P_1(t)]$, and the other to another set $[P_2(t)]$. Clearly, this additional information corresponds to the fact that the observer must know beforehand whether or not two attributes observed at two different times belong to the *same* qualitative range. An operational Russell's scheme, therefore, requires the concept of *sameness* not only in relation to E but to each attribute as well. To establish *sameness* of attribute we need to know what "*same* quality" is. Therefore, Russell's exercise in formal logic does not do away with what intuition posits; on the contrary, on closer examination it is seen that it cannot function without what it purports to destroy.[58]

Perhaps nothing illustrates more aptly the staggering issues raised by "sameness" than one of Bridgman's remarks. With the discovery of relativity in physics, it is perfectly possible that two observers traveling in different directions through space may record a signal from a third source as two different facts. For instance, one observer may see "a flash of yellow light" while the other may only feel "a glow of heat on his finger." How can they be sure then that they have reported the same event, since they cannot turn to simultaneity in the absence of absolute time?[59] Bridgman's point is that even relativity physics presupposes sameness in some absolute sense although it fails to show how it could be established.

[58] The fallacy of believing that the weapon of pure logic suffices by itself to kill any creature of intuition is not uncommon. An instance of this fallacy is discussed in the author's paper, "The End of the Probability Syllogism?" *Philosophical Studies*, February 1954, pp. 31 f. An additional example is the refutation of historical laws on "*strictly logical reasons*." (Karl R. Popper, *The Poverty of Historicism*, Boston, 1957, pp. ix–xi.) The very first premise of Popper's argument, "the course of human history is strongly influenced by the growth of human knowledge," is plainly a historical law. That is, the conclusion that a set of propositions is empty is derived from a proposition belonging to the very same set! We should observe, however, that in a new footnote (*The Logic of Scientific Discovery*, New York, 1959, p. 279n2) Popper takes a more softened line, somewhat agnostic.

[59] Bridgman, *Nature of Physical Theory*, p. 77, and especially his *Reflections of a Physicist* (2nd edn., New York, 1955), p. 318 ff.

The upshot is that we have to recognize once and for all that sameness is an internal affair of a single mind, whether an individual one or one embracing several individual minds. We have gone too far, it appears, in believing that natural phenomena can be reduced to signal registrations alone and hence that mind has no direct role in the process of observation. Mind, on the contrary, is as indispensable an *instrument* of observation as any physical contrivance. The point is of paramount importance for social sciences, and I shall return to it later on.

On the philosophical problem of "sameness," one can only say that it is as thorny as it is old. How thorny it is can be shown in a brief summary of Whitehead's ideas on the subject. According to Whitehead, we apprehend nature in terms of *uniform* objects and *unique* events, the former being ingredients of the latter. "Objects are elements in nature which do not pass." Because they are "out of time," they "can 'be again,'" so that we can say "Hullo, there goes Cleopatra's Needle again." Events, on the contrary, once passed "are passed, and can never be again." At most, we may recognize that one event is *analogous* to another.[60] One cannot help feeling that this dualist view is far from settling the issue, and that "analogous events" stand in the same relation to one another as two objects recognized as being the same. Moreover, one is still baffled by the question of whether any object, such as Cleopatra's Needle, is really out of time so that thousands of years from now we could still say "there it goes again." And if we think in millions of years, we should doubt whether the universe itself is "out of time." Besides, in describing nature we are interested as much in *uniform* objects as in *analogous* events. That is, keeping within Whitehead's system, we know that science is equally concerned with whether we can say "there goes *another* 'King of England' again" and whether we can say "there goes *another* 'coronation' again." Actually, science may even dispense with objects, but not with events. The electron, for instance, "cannot be identified, it lacks 'sameness.'"[61] We cannot therefore say "there goes the same electron again," but only that "there goes another electron-event again."

But then, why should we distinguish between object, i.e., Being, and event, i.e., Becoming? In the end, we verify what we have known of old, that dualism is full of snags. The only way out is to recognize that the distinction between object and event is not discrete but dialectical, and

[60] Whitehead, *Concept of Nature*, pp. 35, 77 f, 143 ff, 169 ff; Whitehead, *Enquiry*, pp. 61 ff, 167 f.

[61] Schrödinger, *Science, Theory and Man*, p. 194; Bridgman, *Intelligent Individual and Society*, pp. 32 f; Louis de Broglie, *Continu et discontinu en physique moderne* (Paris, 1941), p. 75.

probably this is Whitehead's message too.[62] Any further discussion of this point, however, would involve us too deeply in pure Hegelian Dialectics.

7. *How Numerous Are Qualities?* The existence of quality in the world as seen by man would pose no problem whatsoever for science if the number of qualities were finite. Until recently, physics could afford to ignore the problem precisely because the number of qualitatively different particles seemed to be finite, actually very small. But with the latest discoveries, no limit to the number of new particles appears in sight, so that some physicists now recognize that there is "no choice but to consider the consequences [for the orientation of physics] of the assumption that the number of such significant qualities is not limited."[63] The qualitative infinity of nature, however, raises (or revives) a quite difficult problem. In my first discussion of Russell's formalization of change (Section 5), I have shown that an impasse is reached because words are not as numerous as pure qualities. But perhaps the impasse might be cleared by using numbers instead of words for labeling qualities. An example of such a *continuous* catalogue is readily available: each color in the *visual* spectrum can be identified by the wave length of the equivalent unmixed color. As is almost needless to add, such a cataloguing does not necessarily imply the measurability of the range of the qualities involved. However, the question whether the cataloguing is possible forms a prerequisite to that of measurability, although for some reason or other the point has not been recognized, at least in economics, until recently. Clearly, there is no reason why the cardinal power of all the qualities we can think of even in a simple set-up should not exceed that of the arithmetical continuum. On the contrary, as I have argued in relation to individual expectations and preferences,[64] there are good reasons for the view that real numbers are not always sufficient for cataloguing a set of qualities. In other words, the manifold of our thoughts differs from the arithmetical continuum not only by its indivisible continuity but also by its dimensionality.[65] As we say in mathematics, the continuum of the real number system forms only a *simple* infinity.

The suggestion, natural at this juncture, of using more than one real number, i.e., a vector, for labeling qualities would still not reduce quality to number. For, as set theory teaches us, no matter how many coordinates we add, no set of vectors can transcend simple infinity in power. There is

[62] Cf. Whitehead, *Concept of Nature*, pp. 166 f.

[63] David Bohm, *Causality and Chance in Modern Physics*, p. 134, also pp. 123, 133–136.

[64] See "Choice, Expectations and Measurability" (1954), reprinted in AE.

[65] I am not at all sure that these two aspects do not boil down to a single one.

an intimate connection between this mathematical proposition and the well-known difficulties of biological classification.

It was Linnaeus who first struck upon the idea of using a two-word name for each species, the first word representing the genus, the second the species within the genus. By now all naturalists agree that any taxonomical term, in spite of its two-dimensionality, does not cover one immutable, arithmomorphic form but a dialectical penumbra of forms. The fact that they still use Linnaeus' *binary* system clearly indicates that the manifold of biological species is in essence more complex than simple, linear infinity. The problem of biological classification therefore is not equivalent to that illustrated by the continuous cataloguing of colors, and hence the predicament of naturalists would not come to an end even if a numerical vector would be used for labeling species.

One naturalist after another has intuitively apprehended that—as Yves Delage put it—"whatever we may do we will never be able to account for all affinities of living beings by classifying them in classes, orders, families, etc."[66] Many have argued that this is because in the domain of living organisms only *form* (shape) counts and shape is a fluid concept that resists any attempt at classification.[67] Some have simply asserted that form cannot be identified by number.[68] Even Edmund Husserl, though educated as a mathematician, thought the point to be obvious: "The most perfect geometry"—he asserts—cannot help the student to express in precise concepts "that which in so plain, so understanding, and so entirely suitable a way he expresses in the words: notched, indented, lens-shaped, umbelliform, and the like—simple concepts which are *essentially and not accidentally inexact*, and are *therefore* also unmathematical."[69] Yet a simple proposition of the theory of cardinal numbers vindicates the gist of all these intuitive claims. It is the proposition that the next higher cardinal number mathematics has been able to construct after that of the arithmetical continuum is represented by the set of all functions of a real variable, i.e., by a set of forms. Clearly, then, *forms cannot be numbered*.

8. The Continuity of Qualities. The peculiar nature of most qualitative structures leads to a somewhat similar sort of difficulty in connection

[66] Quoted in G. G. Simpson, "The Principles of Classification and a Classification of Mammals," *Bulletin of the American Museum of Natural History*, LXXXV (1945), 19, except that I have translated Delage's words into English.

[67] E.g., Theodosius Dobzhansky, *Evolution, Genetics, and Man* (New York, 1955), chap. x, and especially the eloquent picture on p. 183.

[68] E.g., P. B. Medawar, *The Uniqueness of the Individual* (New York, 1958), pp. 117 ff.

[69] Edmund Husserl, *Ideas: General Introduction to Pure Phenomenology* (New York, 1931), p. 208. Italics are Husserl's.

with their ordering. I can best illustrate this difficulty by an example from my own work. Thirty years ago, as I was trying to unravel the various thoughts underlying early and contemporary writings on utility and to map them out as transparent "postulates," I became convinced of the logical necessity of settling first of all one issue, that with which Postulate A of one of my early papers is concerned.[70] This postulate states that given a preferential set $[C_\alpha]$—where α is a real number and $[C_\alpha]$ is preferentially ordered so that C_α is preferred to C_β if $\alpha > \beta$—and C not belonging to $[C_\alpha]$, there exists an i such that C and C_i are indifferent combinations. At the time, the postulate bothered me; intuitively I felt that the accuracy of human choice cannot be compared with that of a perfect instrument, but I was unable to construct a *formal* example to convince myself as well as the few colleagues with whom I discussed the matter that Postulate A can be negated. The most I could do was to introduce a stochastic factor in choice—which, I believe, was a quite new idea. But this still did not settle my doubts, nor those of my colleagues, about my Postulate A.

In retrospect, the objections of my colleagues and my inability—due to a deficiency in my mathematical knowledge—to meet these objections are highly instructive and also apropos. My critics generally felt that Postulate A is entirely superfluous: some argued that it is impossible to pass from nonpreference to preference without effectively reaching a stage of indifference;[71] others held that since $[C_\alpha]$ is continuous there is no room in it for other items, not even for one. An example which I offered as a basis for discussion was too clumsy for everyone concerned: a hypothetical wine lover who always prefers more to less wine but has nevertheless a very slight preference for red wine, so that between two equal quantities of wine he prefers the red. I denoted by y_r and z_w the quantities of red and white wine respectively, but as I came to write $x_r > x_w$, I invited the objection that "x is x." Today the connection between the example and the old notion of a hierarchy of wants may seem obvious, but I was unable to clarify my own thoughts on the matter until much later, after I came across an objection raised by a reviewer to one of Harold Jeffrey's propositions. Learning then for the first time of lexicographic ordering, I was able to solve my problem.[72] However, my initial difficulties with the example of the wine lover bear upon a point I wish to make now.

[70] See my essays, "The Pure Theory of Consumer's Behavior" (1936) and "Choice, Expectations and Measurability" (1954), reprinted in AE.

[71] From recent discussions I learned that even mathematicians are apt to raise this objection.

[72] "Choice, Expectations and Measurability" (1954), in AE. Perhaps this bit of personal history suffices to show how indispensable to the student of economics is a substantial familiarity with every branch of mathematics.

Either set, $[y_r]$ or $[z_w]$, taken by itself, is continuous in the mathematical sense. Consequently, no brutal offense is committed by regarding, say, $[x_r]$ as the arithmomorphic representation of the preference continuum in case only red wine is available. However, if both red and white wine are introduced into the picture the arithmomorphic representation of the wine lover's preference suddenly becomes discontinuous: in the corresponding lexicographic ordering (with respect to the *subscript*) there is no element between x_w and x_r, or alternatively, x_r is the immediate successor of x_w. On the other hand, there is no reason why preference itself should become discontinuous because of qualitative variations in the object of preference. To argue that preference is discontinuous because its arithmomorphic simile is so, is tantamount to denying the three-dimensionality of material objects on the ground that their photographs have only two dimensions. The point is that an arithmomorphic simile of a qualitative continuum displays spurious seams that are due to a peculiar property of the medium chosen for representing that continuum. The more complex the qualitative range thus formalized, the greater the number of such artificial seams. For the variety of quality is continuous in a sense that cannot be faithfully mirrored by a mathematical multiplicity.

9. A Critique of Arithmomorphism. Like all inventions, that of the arithmomorphic concept too has its good and its bad features. On the one hand, it has speeded the advance of knowledge in the domain of inert matter; it has also helped us detect numerous errors in our thinking, even in our mathematical thinking. Thanks to Logic and mathematics in the ultimate instance, man has been able to free himself of most animistic superstitions in interpreting the wonders of nature. On the other hand, because an arithmomorphic concept has absolutely no relation to life, to *anima*, we have been led to regard it as the only sound expression of knowledge. As a result, for the last two hundred years we have bent all our efforts to enthrone a superstition as dangerous as the animism of old: that of the Almighty Arithmomorphic Concept. Nowadays, one would risk being quietly excommunicated from the modern *Akademia* if he denounced this modern superstition too strongly. The temper of our century has thus come to conform to one of Plato's adages: "He who never looks for numbers in anything, will not himself be looked for in the number of the famous men."[73] That this attitude has also some unfortunate consequences becomes obvious to anyone willing to drop the arithmomorphic superstition for a while: today there is little, if any, inducement to study Change unless it concerns a measurable attribute. Very plausibly, evolution would still be a largely mysterious affair had

[73] Plato, *Philebus*, 17.

Darwin been born a hundred years later. The same applies to Marx and, at least, to his analysis of society. With his creative mind, the twentieth-century Marx would have probably turned out to be the greatest econometrician of all times.

Denunciations of the arithmomorphic superstition, rare though they are, have come not only from old-fashioned or modern Hegelians, but recently also from some of the highest priests of science, occasionally even from exegetes of logical positivism. Among the Nobel laureates, at least P. W. Bridgman, Erwin Schrödinger, and Werner Heisenberg have cautioned us that it is the arithmomorphic concept (indirectly, Logic and mathematics), not our knowledge of natural phenomena, that is deficient.[74] Ludwig Wittgenstein, a most glaring example in this respect, recognizes "the bewitchment of our understanding by the means of our [rigidly interpreted] language."[75] The arithmomorphic rigidity of logical terms and symbols ends by giving us mental cramps. We can almost hear Hegel speaking of "the dead bones of Logic" and of "the battle of Reason . . . to break up the rigidity to which the Understanding has reduced everything."[76] But even Hegel had his predecessors: long before him Pascal had pointed out that "reasoning is not made of *barbara* and *baralipton*."[77] The temper of an age, however, is a peculiarly solid phenomenon which advertises only what it likes and marches on undisturbed by the self-criticism voiced by a minority. In a way, this is only natural: as long as there is plenty of gold dust in rivers why should one waste time in felling timber for gold-mine galleries?

There can be no doubt that all arguments against the sufficiency of arithmomorphic concepts have their roots in that "mass of unanalyzed prejudice which Kantians call 'intuition,'"[78] and hence would not exist without it. Yet, even those who, like Russell, scorn intuition for the sake of justifying a philosophical flight of fancy, could not possibly apprehend or think—or even argue against the Kantian prejudice—without this unanalyzed function of the intellect. The tragedy of any strain of positivism is that in order to argue out its case it must lean heavily on something which according to its own teaching is only a

[74] Bridgman, *Logic of Modern Physics*, p. 62, and *Nature of Physical Theory*, p. 113; Erwin Schrödinger, *What Is Life?* (Cambridge, Eng., 1944), p. 1; Werner Heisenberg, *Physics and Philosophy: The Revolution in Modern Science* (New York, 1958), pp. 85 ff.

[75] L. Wittgenstein, *Philosophical Investigations* (New York, 1953), I, 109. My translation.

[76] *The Logic of Hegel*, p. 67.

[77] Blaise Pascal, "De l'esprit géométrique et de l'art de persuader," in *Oeuvres complètes*, ed. J. Chevalier (Paris, 1954), p. 602.

[78] Russell, *Principles of Mathematics*, p. 260.

shadow. For an excellent illustration in point we may turn to a popular treatise which aims to prove that if "no possible sense-experience" can determine the truth or falsehood of a nontautological proposition then "it is metaphysical, . . . neither true nor false but literally senseless."[79] After reading this statement on the first page of the preface one cannot help wondering in what way the rest of the book can support it if the statement is true—as its author claims. Certainly, the subsequent argument has no relation whatever to sense-experience—except, of course, the visual perception of black letters, nay, spots on a white background.

The frequent diatribes against this or that particular dialectical concept are guilty of the same sin. Cornelius Muller, for example, preaches the abolition of the concept of community. The reasoning is that since "the several examples of one class of communities are not identical and [since] two adjacent classes of communities are not distinct from one another . . . the word has no meaning."[80] But the argument is obviously self-destroying, for the meaning of the premise is negated by its own conclusion. We have not learned, it seems, everything from the legendary Cretan liar of the ancient sophist school.

The propounders of the views such as those just mentioned—or this author for that matter—would not go to the trouble of discussing the issues of dialectical concepts if we thought that these issues have no bearing upon scientific orientation. It is therefore not surprising that Muller, who argues that there are no "real entities"—whatever this might mean—unless we can distinguish them in the same way we distinguish one carbon isotope from another, begins his attack on "community" by asking "Is there a mechanistic theory that . . . conforms to the true nature of communities?"[81] The moral is plain: social sciences and biology should cling to the universality of mechanics, that is, to a retrograde position long since abandoned even by physics.

Unfortunately for everyone concerned, life phenomena are not as simple as that, for not all their aspects are as pellucid as an arithmomorphic concept. Without dialectical concepts life sciences could not fulfill their task. As I have argued earlier, there is no way of defining "democracy" or "competition," for instance, so as to comply with Muller's criterion of real entity. The most we can do for a greater precision is to distinguish species within each genus, as in biology: "American democracy," "British democracy," "monopolistic competition," "workable competition," etc. Let us observe that even the familiar and apparently simple notion of

[79] A. J. Ayer, *Language, Truth and Logic* (2nd edn., New York, 1946), p. 31.

[80] Cornelius H. Muller, "Science and Philosophy of the Community Concept," *American Scientist*, XLVI (1958), 307 f.

[81] *Ibid.*, 298.

the struggle for existence has many shades of meaning "which pass into each other"[82] and, hence, is dialectical. Finally, let us observe that the only proof of evolution is the dialectical relation of species in their phylogenetic classification. Should we one day succeed in constructing an arithmomorphic concept of species (or of something equivalent), that very day biology will have to return to the pre-Lamarckian views: species were created immutable and by fiat. A self-identical species, a self-identical community, anything self-identical, cannot account for biological or social evolution: "self-identity has no life."[83] More explicitly, no process of change can be completely decomposed into arithmomorphic parts, themselves devoid of change.[84] And it is because society and its organization are in constant flux that genuine justice cannot mean rigid interpretation of the words in the written laws. Only bitter and unnecessary conflict, as Bridgman correctly observed, can result from ignoring the dialectical nature of "duty" and using the term as if it has the "sharpness and uniqueness of a mathematical concept."[85]

Robert Mayer's outcry that "a single number has more true and permanent value than a vast library of hypotheses" was perfectly in place. He spoke as a physicist addressing physicists, and hence there was no need for him to add, "provided that that number helps us describe reality adequately." Omissions such as this one have allowed similar statements by the greatest authorities in science to be interpreted as applying to *any* number. The fascination of our intellect by number is not easily conquered. It is responsible also for the fact that Galileo's advice to astronomers and physicists has been transformed into a definition of essence: "science is measurement." The consequences of these gratifying generalizations have not always been fortunate.

Planck, for example, observed that by exaggerating the value of measure we might completely lose touch with the real object. Of the many examples that could illustrate the point, one is particularly piquant. From as far back as we can go in history a man's degree of aging has been measured by his age. Because of this biologists have simply thought little, if at all, of aging. So, recently they suddenly discovered "an unsolved problem of biology": age may be an average measure of aging, but aging is something entirely different from growing old in years.[86] A still more piquant example is the fact that we keep on measuring

[82] Darwin, *Origin of Species*, p. 46.

[83] G. W. F. Hegel, *Hegel's Science of Logic* (2 vols., London, 1951), II, 68.

[84] Whitehead, *Modes of Thought*, pp. 131 f. See also *Hegel's Science of Logic*, II, 251 f.

[85] Bridgman, *Intelligent Individual and Society*, p. 116.

[86] See Medawar, *The Uniqueness of the Individual*, chap. ii.

"intelligence" by the familiar I.Q. but we do not known exactly what is being measured.[87]

Undoubtedly, for the sciences concerned with phenomena almost devoid of form and quality, measure usually means expanded knowledge. In physics, which has quite appropriately been defined as the quantitative knowledge of nature, there is no great harm if measurement is regarded as an end in itself. But in other fields the same attitude may lead to empty theorizing, at the very least. The advice "look for number" is wise only if it is not interpreted as meaning "you must find a number in everything." *We do not have* to represent beliefs by numbers just because our mind feels similarly embarrassed if it has to predict the outcome of a coin-tossing or the political conditions in France ten years from now. The two events are not instances of the same phenomenon. A measure for all uncertainty situations, even though a number, has absolutely no scientific value, for it can be obtained only by an intentionally mutilated representation of reality. We hear people almost every day speaking of "calculated risk," but no one yet can tell us how he calculated it so that we could check his calculations. "Calculated risk" if taken literally is a mere parade of mathematical terms.[88]

It was under the influence of the idea "there is a number in everything" that we have jumped to the conclusion "where there is 'more' and 'less' there is also quantity," and thus enslaved our thoughts to what I have called "the ordinalist's error"—which is to hold that wherever there is ordering there also is measure, an ordinal one at least.

10. Thought and "Thought." At first and for long ages, animism was man's scientific faith: everything that moves, from clouds and rivers to living creatures, moves because it has a soul like that of which man is directly aware. Slowly, inanimate things were eliminated from the category *anima*. As far as we know, it was Leonardo da Vinci who, in one of his numerous unpublished notes and essays, first dissented from animism. In a vein that admirably matches the present machinistic faith, he proclaimed that "a bird is an instrument working according to mathematical law, which instrument it is within the capacity of man to reproduce with all its movements" (*Macchine per volare*, n.d.). The time seems to have been ripe for dissenting. For shortly thereafter and independently, the Spanish physician and philosopher Gómez Pereira (*Antoniana Margarita*, 1554), using his medical knowledge, expounded the general

[87] For a succint discussion of this point, see J. P. Guilford "Intelligence Has Three Facets," *Science*, May 10, 1968, pp. 615–618, as well as the brief remarks in the next section, below.

[88] For the argument I have offered against the measurability of even documented belief, see my articles "Choice, Expectations and Measurability" (1954) and, especially, "The Nature of Expectation and Uncertainty" (1958), reprinted in AE.

thesis that all life-bearing structures consist of movements only: excepting man, they all are automata without soul.[89] Because this reversed doctrine frees us from recognizing many a mystery of nature, it has ever since exercised an immense fascination for the human mind even though it obviously foreshadowed the ultimate negation of mind itself.[90] Hence, the periodic ebullient fashions to which it has given rise.

One hundred years after Pereira, Descartes lent to the doctrine his great authority by arguing that "the living body is a machine . . . not more, not less than the movements of a clock or of any other automaton" (*De l'Homme*, 1664). After another hundred years, Julien de La Mettrie (*L'Homme Machine*, 1748) pushed further the theme and supported it by a host of sophisticated details. The following century, Charles Babbage, with characteristic British practicalism, moved toward applying the doctrine to facts by attempting to construct an Analytical Engine.[91] After still another hundred years, there came the current fashion with its fervent belief that only some temporary imperfections of the present state of the arts stand in our way of constructing a machine that can "compete with men in all purely intellectual fields"—as the British logician, A. M. Turing, assuredly proclaims.[92]

It is generally argued that what makes the claim valid this time is the modern discoveries and innovations in electronics.[93] The truth is that the

[89] See J. M. Guardia, "Philosophes Espagnols: Gómez Pereira," *Revue philosophique de la France et de l'Étranger*, XXVIII (1889), 270–291, 382–407, 607–634.

[90] To emphasize this negation G. Ryle, *The Concept of Mind* (London, 1949), pp. 15 ff, spoke of the Mind as "the Ghost in the Machine," or "the Horse in the Railway Engine." (Actually, the metaphor belongs to a German novelist and had already been mentioned by Max Planck, *The New Science*, New York, 1959, p. 82.) Interesting also is Ryle's conclusion in "The Physical Basis of Mind: A Philosophers' Symposium," *The Physical Basis of Mind*, ed. P. Laslett (Oxford, 1952), pp. 75–79, namely, that "'Mind' and 'Matter' are echoes from the hustings of philosophy and prejudice the solutions of all problems posed in terms of them." For a penetrating, albeit impassioned, rebuttal see A. Koestler, *The Ghost in the Machine* (New York, 1967).

[91] See B. V. Bowden, "A Brief History of Computation," *Faster than Thought: A Symposium on Digital Computing Machines*, ed. B. V. Bowden (London, 1953), pp. 3–31.

[92] A. M. Turing, "Computing Machinery and Intelligence," *Mind*, LIX (1950), 460.

[93] E.g., W. R. Ashby, "Design for a Brain," *Electronic Engineering*, XX (1948), 379, argues that, before electronics, machines were mechanical, but now they have a richer meaning because of the feedback, an idea propounded also by Norbert Wiener, *Cybernetics or Control and Communication in the Animal and the Machine* (2nd edn., New York, 1961). Yet the feedback does not belong only to electronics. The principle of virtual displacements used by classical mechanics in analyzing the stability of equilibrium (later imported into economics) clearly implies the feedback. Formally, there is no difference between Ashby's homeostat (his pp. 380 f) and a system of a number of balls left to themselves inside a bowl; both will go back to their "equilibrium" if disturbed within reasonable bounds.

belief in the validity of the claim has been fed and continues to be fed by the growing cult of the Almighty Arithmomorphic Concept. Indeed, the general blueprint of the modern automata, a path-breaking article by Turing, preceded by at least five years the first electronic computer.[94] Turing himself insists that the real basis of a computer's performance is its mechanistic blueprint: electronics only speeds up the operation.[95]

The criterion for verifying the claim has also been set up first by Turing. As we should expect from a professional logician, Turing starts by denying any meaning to the question "Can a machine think?" unless "think" and "machine" are defined unambiguously. So he replaces it by another question "in relatively unambiguous words": Can a machine imitate the human mind? Specifically, would a human interrogator communicating with a concealed interlocutor only by typed messages guess wrongly the nature of that interlocutor as often in case it is a machine as in case it is a human?[96] The "test" is unquestionably reasonable. It has been used in answering numberless questions such as "Can this Californian wine be taken for a certain St. Emilion from France?" The question is whether the test allows for all differences to be taken into account (except, of course, the difference of "labels"). The test for the two wines, for instance, should not exclude the difference of color. Pretending to be colorblind in order to argue that those who insist upon their seeing something that cannot be reduced to colorless tone are either blind to tone or have metaphysical hallucinations will never work. But this is precisely what is done by those who claim that machines think.

Turing's artificial specimen of the imitation dialogue between a human and a computer is patently designed to foster belief. Naturally, it has become a quite popular trade article. For an excerpt:

> Interrogator: Would you say Mr. Pickwick reminded you of Christmas?
> Witness: In a way.
> Interrogator: Yet Christmas is a winter's day, and I do not think Mr. Pickwick would mind the comparison.
> Witness: I don't think you're serious. By a winter's day one means a typical winter's day, rather than a special one like Christmas.[97]

[94] A. M. Turing, "On Computable Numbers, with an Application to the Entscheidungsproblem," *Proceedings of the London Mathematical Society*, Ser. 2, XLII (1936), 230–265, and "A Correction," *ibid.*, XLIII (1937), 544–546.

[95] Turing, "Computing Machinery," p. 439.

[96] *Ibid.*, pp. 433–435. Time and again, a logical positivist does not seem to be aware of his predicament. Turing does not object that "a human interlocutor," too, is an ambiguous term. Does it include a Newton or an Einstein? If not, where do we stop? Also, he seems indifferent to the obvious boomerang of his position as he comes to admitting that even for a machine "it is difficult to frame the definitions so as to satisfy [the listed] conditions," which—it should be noted—are formulated in dialectical terms!

[97] *Ibid.*, p. 446.

Turing even claims that the specimen illustrates the yet unrealized potentialities not of a complex but of a purely digital computer. But he shuns any unambiguous definition of the test dialogue, even some Socratic elaboration on its nature. His article does not lead one to believe that there may be some restriction as to what the dialogue may involve. True, Turing reflects in passing that, possibly, one should not expect the machine to answer questions such as "What do you think of Picasso?"—which does not seem an essentially different kind of question from that pertaining to Mr. Pickwick. There is no doubt, though, that one can ask the interlocutor whether "it" plays tick-tack-toe, NIM, checkers, etc., until one gets the answer "No." The question "Are you willing to start learning *now* how to play it?" will then provide an infallible acid test. For a human is programmed to start learning *anything at any time*. By contrast, a machine programmed to learn, say, checkers already knows how to play it: if it does not know, it is *ipso facto* not programmed to learn it either. So, the answer of the machine will perforce give up the show there and then. I also contend that the same result will obtain (with less assurance, however) by asking the interlocutor to do the questioning for a change. For to think up some simple yet highly interesting questions is on the whole far more difficult (even for a human mind) than to formulate complex technical questions by scanning the memorized material.

One may interject that a Turing universal machine, i.e., a machine that can perform *any* operation performed by *any* other machine, would be immune to the above acid tests. The rub is that such a machine exists only on paper, for its blueprint requires that its capacity for instructions should be limitless.[98]

When everything is said and done, it is seen that all proofs of the "computer performance = the human thought" involve the eternal verbal swindle. "Thinking" is only what computers do (or may do on paper), not what the primitive computer, the human brain, does in fact.[99] As we have seen in Turing's case, the justification offered is that "intelligence" or "thinking" are ambiguous notions in their general acceptance. The equation thus becomes a tautology in the game. Curiously, in the

[98] The theoretical interest of infinite capacity computers is not to be denied. *Ibid.*, pp. 438 f.

[99] Even the more careful writers on the subject are not always free from this inversion sin. For example, we read in John von Neumann, "The General and Logical Theory of Automata," *Cerebral Mechanisms in Behavior: The Hixon Symposium*, ed. L. A. Jeffress (New York, 1951), p. 10: "I shall consider the living organisms as if they were purely digital automata." Few are the specialists in this field who, like W. S. McCulloch and W. Pitts, "A Logical Calculus of the Ideas Immanent in Nervous Activity," *Bulletin of Mathematical Biophysics*, V (1943), 117, warn the reader that they do not conceive "the formal equivalence to be a factual explanation. *Per contra!*"

finale it emerges again with its old ambiguous meaning, this time followed by Q.E.D. in the boldest possible face. The point is admirably illustrated by W. R. Ashby's "intelligence-amplifiers" for which he argues that "intelligence" consists only of the faculty of selecting a particular element out of a set of alternatives.[100] This view does not alter the brute fact that the human mind includes many other intellectual faculties. Nor does it turn every selecting operation into an intellectual feat. Although a river sieves out sand from pebbles, it would be absurd to endow it with intelligence (unless we return to early animism).

True, ever since the beginning of our century, when Alfred Binet first raised the question, psychologists have sought in vain an arithmomorphic definition of intelligence in simpler terms. "There is no generally accepted meaning of the term."[101] And there is none because intelligence, like most manifestations of life, is a dialectical notion. The penumbra surrounding it may be signalled by examples at will. As one feels uncertain about the usage of a word, one reaches for the dictionary and reads the explanation. One also reaches for the rail and grasps it if one feels he is losing his balance on a staircase. Which action, if any, is a manifestation of intelligence?

As a whole, however, the general picture of intelligence as studied by psychologists does not differ from that corresponding to the "vulgar" meaning. It includes all activities from that of memorizing, remembering, calculating, and ratiocinating, to thinking up new concepts and synthesizing diverse elements into unified abstractions, recognizing analogies of form and content by abstracting from the details of the particular, reasoning with dialectical concepts, and composing artistic works. It is against this incomplete (I am sure) list of activities that we must judge the claims that computers not only "possess" intelligence, even amplified intelligence, but also provide the only efficient means for studying how the human brain solves problems.[102]

Most curiously, no such extravagant claims have accompanied other

[100] W. R. Ashby, "Design for an Intelligence-Amplifier," *Automata Studies*, eds. C. E. Shannon and J. McCarthy (Princeton, 1956), pp. 220, 233.

[101] W. C. Halstead, "Brain and Intelligence," *Cerebral Mechanisms in Behavior*, pp. 246, 251; see also J. P. Guilford, "Intelligence Has Three Facets," pp. 615–620.

[102] And note, such claims are not always made by tyros. Witness the works (already cited) by A. M. Turing, Norbert Wiener, and John von Neumann, as well as H. A. Simon, "The Control of the Mind by Reality: Human Cognition and Problem Solving" and the interventions by the same author in the panel on "Restriction of the Mind," *Man and Civilization: Control of the Mind*, eds. S. M. Farber and R. H. L. Wilson (New York, 1961), pp. 219–232, 281–285, as well as H. A. Simon and A. Newell, "Heuristic Problem Solving by Computer," *Symposium on Computer Augmentation of Human Reasoning*, eds. Margo A. Sass and W. D. Wilkinson (Washington, 1965), pp. 25–35.

inventions made by man, equally marvelous though many were when introduced. A telescope with an adapted camera can "see" one thousand times farther and better than the human eye; yet nobody claimed that it possesses all the qualities of the human eye or that ophthalmologists should turn to such a contraption in order to study how the human eye functions in every respect. The immense usefulness of a jet plane is not in the least affected by the fact that it does not flap its wings, lay eggs, and hatch them—as birds do. By the same token, the equally immense usefulness of computers needs no sales talk. A computer has calculated the first one hundred thousand decimals of π in a little less than nine hours, a job estimated to require 30,000 years for a man with a calculator to accomplish.[103] This is no reason for presenting the computers as intelligent giants with an I.Q. of one million whose intelligence "exceeds that of its designer."[104] And if in an ordinary test a computer showed an I.Q. of one million, nay, of one thousand, it would only attest that, as I have argued earlier, focusing on measure we have lost sight of what is measured. The computer does transcend some of the intellectual limitations of its designer, but not his intelligence in the relevant meaning of the term.[105]

For obvious reasons, the arguments in support of the dual equation, "computer = human brain," can hardly resort to verbal swindle and define "brain" *ad hoc*. Instead, they resort to paper-and-pencil models based on "convenient" assumptions or analogies, completely ignoring what neurohistologists, neurophysiologists, neuropsychologists, and psychiatrists tell us about the unanswered questions about the brain. Even Neumann's early speculation that the brain is a mixed system of an analog and a digital computer is well off the mark. And, a strong believer though he was in the potentiality of automata, Neumann had to admit in the end that "in fact, the 'digital method' . . . may be entirely alien to the nervous system [of any animal]."[106] The most plausible picture now is that the functioning of the brain involves not only electrical pulses in a

[103] D. G. Fink, *Computers and the Human Mind* (New York, 1966), p. 12. Since the computers are estimated to scan their memory cores, to sort, and to compute a million times (at most) faster than man, the above figure must have allowed for the immense time needed by man to write down all intermediary computation.

[104] As Ashby does in "Design for an Intelligence-Amplifier," pp. 216, 220.

[105] It is beyond question that, as with all human inventions or discoveries, the designer may find that a computer intended for certain tasks can also be used for some unintended ones. So is the fact that a computer may cause, say, a state-wide blackout either by malfunction or if man experiments blindly with it. Cf. note 127, below.

[106] Neumann, "The General and Logical Theory of Automata" (cited in note 99, above), Discussion, p. 38, and Neumann, *The Computer and the Brain* (New Haven, 1958), p. 44.

still undecided pattern but also some chemical computing.[107] Should we be surprised to learn one day that it also involves some computing at the yet unexplored subquantum level?

A famous neuropsychologist, K. S. Lashley, spent his entire life looking for "memory cores" in the brain, and failed to find them. It is now admitted that memory is not in a particular place of the brain: it is a never-stopping process involving also some protein synthesis.[108] Even less is known about how the brain learns. Brain specialists are still asking why (or how) the brain does certain things no intelligence-machine can. For instance, the brain can handle entirely unforeseen errors and situations or repair itself to an appreciable extent.[109] As one authority on the brain, W. S. McCulloch, said tongue in cheek, students of the brain envy those scientists who study machines for which they possess complete anatomical and physiological blueprints. "The brain is like a computing machine, but there is no computing machine like the brain."[110] This is where things stand.

Turning to the limitations of what the computer can do in comparison to the intellectual performances of the brain, we must bear in mind that a digital computer has a *finite and discrete* structure. At any one time, through any relay there either passes an electrical pulse or none: the operation is based on an "all-or-none" configuration.[111] The famous theorem of McCulloch and Pitts—that "anything that can be *completely and unambiguously put into words*, is ipso facto realizable by a suitable finite neural [relay] network"[112]—is a technically interesting yet not unexpected result. Neumann notes that the digital computer, because of its all-or-none mode of operation, is subject to the same "unattractive" limitations as Logic is; he argues, however, that the only drawback of the digital computer is that it cannot handle problems of mathematical analysis in which the infinite intervenes.[113] Now, an analog computer

[107] Halstead, "Brain and Intelligence," pp. 269 f.

[108] See the recent work on the subject by E. Roy John, *Mechanisms of Memory* (New York, 1967). And let us not fail to score also the miraculous quality of the brain to rebel against memorizing irrelevant things and to forget them quickly if memorized. See Chapter I, note 15, above.

[109] Various discussions in *Cerebral Mechanisms in Behavior*, ed. Jeffress, pp. 32 ff, 193 f. The objection of W. S. McCulloch, "Why the Mind Is in the Head," *ibid.*, pp. 54 f, that for a computer with as many "neurons" as the brain (about 10^{10}) Niagara Falls would not suffice to supply the necessary current and Niagara River to cool off the plant, does not seem to me essential. This indeed may be only a temporary technical impediment.

[110] W. S. McCulloch, quoted in Fink, *Computers and the Human Mind*, p. 178.

[111] Turing, "Computing Machinery," pp. 439 ff; Neumann, "The General and Logical Theory," pp. 15 f.

[112] Neumann, *ibid.*, p. 23 (my italics). For the theorem see the article by McCulloch and Pitts cited in note 99, above.

[113] Neumann, "The General and Logical Theory," p. 16.

should be free from this drawback. Yet its structure is still arithmomorphic. From what I have said in this and the previous chapter it follows that, regardless of its type, no computer can perform any operation that is directly or indirectly connected with dialectical reasoning. Such an operation is the exclusive prerogative of the human brain.

Between the plasticity of the brain and the machinistic structure of a computer there is an unbridgeable gap that is even wider than that between syllogizing and reasoning. From whatever angle we look at the living thought, we reach the same inescapable conclusion: thinking, even mathematical thinking, would come to a standstill if confined to self-identical notions. Whitehead warned us, "As soon as you leave the beaten track of vague clarity, and trust to exactness, you will meet difficulties."[114] Infinitely continuous qualities, dialectical penumbras over relations and ideas, a halo of varying brightness and contour, this is thought: a gaseous medium as Wittgenstein pictured it after his vain efforts (among the most brilliant ones) to reduce reason to an arithmomorphic basis.[115] The reason why no computer can imitate the working of the human brain is that thought is a never-ending process of Change which, as I endeavored to show in this chapter, is in essence dialectical. The arithmomorphic structure of any computer, on the other hand, is as inert in relation to novelty and Change as number itself. Without the dialectical nature of thought, no association of ideas, let alone the emergence of novel ones, would be possible.

A computer can be programmed, for instance, to play an excellent game of NIM[116] by the very same technique used in "teaching" machines to play checkers or chess.[117] Actually, in case the dimensions of the game are not too great, the computer can scan the *entire* tree of the game and retain only the winning ramifications—which is a complete solution of that particular game. No man could do this, except for irrelevantly small dimensions. Even for the simple NIM pattern (1, 2, 3) the tree has as many as 182 ramifications. Try to draw it on paper! But man can do what the machine cannot: he has discovered the formula for the winning moves for *any pattern*. This should surprise anyone able to appreciate a wonder. And if there is a winning formula for checkers or chess, we may rest

[114] Alfred North Whitehead, *Science and Philosophy* (New York, 1948), p. 136.

[115] Wittgenstein, *Philosophical Investigations*, I. 109.

[116] For a description of the game and the winning formula, see W. W. Rouse Ball, *Mathematical Recreations and Essays* (New York, 1962), pp. 36–38.

[117] Basically, the technique, now called "heuristic," consists of using some point-count function (like that of Charles H. Goren for bridge) as a guide and arranging for the machine to memorize all losing boards as they occur during "training." In actual play the machine scans a few moves ahead on the tree of the game. See the instructive article by A. L. Samuel, "Some Studies in Machine Learning, Using the Game of Checkers," *IBM Journal of Research and Development*, III (1959), 210–229.

assured that only man will discover it, because only his mind can open for itself new avenues of knowledge. Even if the computer will per chance reveal a running regularity up to a certain number, a human brain will still be needed to prove the validity of the regularity for *any* number. The computer calculated the first hundred thousand decimals of π with a speed 30,000,000 times greater than Leibnitz could have done it. But it was Leibnitz, not a computer, who thought up the infinite series for $\pi/4$ on which the computer was programmed!

Let us also ask what geometry-theorem proving machine would be likely to "think" of another wonder—the simple proof thought up by H. A. Schwartz for an elementary theorem, in which proof a triangle is turned over six times for no obvious reason at all.[118] Also, which "heuristic" machine would hit upon Euler's seemingly inept yet brilliant idea of decomposing sin x into simple factors as if it were a polynomial?[119] Intuitions, such as Schwartz's or Euler's, distinguishes thought from "thought." Some of us may scorn intuition as a nebulous quality or a Kantian prejudice; yet intuition alone advances knowledge creatively. Very recently, in an editorial of *Science* praising some biochemical discovery, P. H. Abelson noted that improved research methods would not do alone for further progress; this "will depend on the quality of the intuitive judgment with which scientists select materials for study."[120]

Let us also observe that the mathematical problems mentioned above are formulated *completely in unambiguous words*. Therefore, the condition that constitutes the leitmotiv of the arguments for "computers think" cannot be invoked against the conclusions derived from these illustrations. That is not all. As I hope to have proved in the preceding sections, most of our important thinking involves dialectical notions and relations. By the very mode of their operation, computers are incapable of dialectical reasoning. This, perhaps, is what John von Neumann wished to admit in a posthumous monograph by saying that "*the language of the brain* [*is*] *not the language of mathematics*"—hence, not of the computer either.[121] And Norbert Wiener tells us that he had anticipated the difficulties of designing a computer that could recognize a square regardless of its relative position.[122] Very recently, in a sober account Oliver Selfridge stresses again that machines cannot recognize invariances of symmetry and even collinearity in a tick-tack-toe game "without being told," whereas man

[118] For which see H. Rademacher and O. Toeplitz, *The Enjoyment of Mathematics* (Princeton, 1957), pp. 29 f.

[119] See the fascinating story in G. Polya, *Mathematics and Plausible Reasoning* (2 vols., Princeton, 1954), I, 19–21.

[120] *Science*, May 31, 1968, p. 951.

[121] Neumann, *The Computer and the Brain*, p. 80.

[122] Wiener, *Cybernetics*, p. 18.

started to do this all by himself.[123] Let us note further that Gestalt does not mean only to recognize geometrical forms. It means also to recognize "notched, indented, lens-shaped, umbelliform, and the like" of which Husserl spoke. Above all, it means to recognize "democracy," "species," "want," etc., regardless of individual irregularities. And let us not forget, it means even to recognize Gestalt, which is itself a dialectical notion.

If an *ad hoc* definition of thought could help us disentangle the issues, I would endorse J. P. Eckert (a co-designer of ENIAC, the first digital electronic computer), who declared that after an experience of seventeen years he has been "forced to adopt the definition that thinking is what computers cannot do."[124] For the reasons developed in the foregoing paragraphs, this definition of Thought is far from being vacuous. Wiener warned us that "the ultra-rapid computing machine will certainly not decrease the need for [high class] mathematicians."[125] We can go one step further and say that heuristic, theorem-proving, game-playing, or what-not computers will not decrease the need for Thought. The most apropos proof is offered by the very papers in which Turing, Neumann, Simon *et al.* blend novel ideas with dialectical reasoning into forceful arguments which no computer could reproduce. Turing, I am confident, would not, even for the sake of the cause, have gone so far as to contend that his fascinating article could be written by a computer in answer to the question "Can a machine think?"

The point of Lady Lovelace in connection with Babbage's Analytical Engine—that a machine can do only what we order it by design to do—still stands. It is vouched for by a most distinguished veteran in programming "learning" machines, A. L. Samuel, who adds that "computers . . . are giant morons, not giant brains."[126] They are morons because they cannot Think. For this reason, we need not be terrified—as Samuel Butler and Norbert Wiener say we ought to be—by the idea that computers could by themselves spell the end of the human species.[127] As with atomic

[123] Oliver Selfridge, "Reasoning in Game Playing by Machine," *Symposium on Computer Augmentation of Human Reasoning* (note 102, above), p. 5.

[124] Quoted in Fink, *Computers and the Human Mind*, p. 208.

[125] Wiener, *Cybernetics*, p. 131.

[126] A. L. Samuel, "Artificial Intelligence: A Frontier of Automation," *Annals of The American Academy of Political and Social Science*, CCCXL (March 1962), 13.

[127] The basic position of N. Wiener, summarized in his "Some Moral and Technical Consequences of Automation," *Science*, May 6, 1960, pp. 1355–1358, is that because machines work incredibly faster than man, man may not be able to stop a machine in time if he realizes that what the machine does is cataclysmal—a predicament which he likens to that of "The Sorcerer's Apprentice" of Goethe. He supports it, not by the possibility of man's making a wrong use of machines, but by the familiar assertion that machines transcend "the limitations of their designers." See "A Refutation" by A. L. Samuel in *Science*, September 16, 1960, pp. 741 f.

power, the danger may come only from the uses to which Thought might put the moron brains (but most probably from some brainless creatures—a fungus, a bacteria, or a virus).

Like all arguments concerning factual issues, both those favoring "machines can think" and those (including mine) opposing it necessarily make use of incomplete induction. Some argue that since "machines cannot compute" has been proved false, we have no longer any inductive basis for disbelieving that "machines can think."[128] Others use the inductive argument in a direct and explicit way. "The making of a synthetic brain requires now little more than time and labor."[129] Or, "step by step, the remaining limitations are being attacked."[130] Arguments such as these remind us of the glorification of mechanics more than a century ago. Actually, they sprout from the same root, the dogma that in nature there is no element irreducible to precise, finite, and reproducible laws.

The mechanistic bedrock of the general "theory" of automata is unmistakably revealed by a famous proposition proved by John von Neumann: a Turing universal *machine* can be so designed that if left in a floating medium together with a great number of its elementary parts it will reproduce itself.[131] What else do we need for a machinistic replica of life, hence, of thought itself? But let us observe that in view of the universality of the machine and of the implicit assumptions of the proof, Neumann's proposition can be made much stronger. One could extend it to a machine that includes also all the necessary mining, manufacturing, transportation operations and that will reproduce itself if left alone in the bare material environment of this planet. Moreover, man need not be completely eliminated from the intermediate outputs of the machine. Turing, himself, implies that this extension is not an aberration, for otherwise his explicit instruction that "men born in the usual manner" should not be an elementary part of the machine would have had no purpose.[132] This statement clearly suggests a truly grandiose view—that a machine may even reproduce the entire evolution of man from the hot mud that existed before the first living cell came into being.

128 Cf. Turing, "Computing Machinery," p. 448. Richard Laing concludes his review of M. Taube, *Computers and Common Sense* (New York, 1961), thus: "there do not appear to be any rigorous arguments against the possibility of computers doing things typical of human intelligence." *Behavioral Science*, VII (1962), 240.

129 Ashby, "Design for a Brain" (note 93, above), p. 382.

130 Simon and Newell, "Heuristic Problem Solving by Computer" (note 102, above), p. 32. See also the remarks concerning Polya's idea of Plausible Reasoning by H. Gelernter, "Realization of a Geometry-Theorem Proving Machine," *Computers and Thought*, eds. E. A. Feigenbaum and J. Feldman (New York, 1963), p. 135.

131 Neumann, "The General and Logical Theory of Automata," pp. 28–30.

132 Turing, "Computing Machinery," p. 435.

However, the more we have learned about nature since Laplace's apotheosis of mechanics, the clearer the qualitative infinity of nature has become. And, as I have argued earlier (Chapter II, Section 7), what prevents us from reducing the physical world to arithmomorphic concepts is the dialectical factor denoted by "random." The two ends now meet: the nature of Thought being dialectical, Thought cannot be reproduced by machines constructed on arithmomorphic blueprints.

To minimize the risk that the above critique of the computer's apotheosis should be misinterpreted as to its scope, I wish to emphasize in conclusion that nothing is further from my thought than to belittle the usefulness of this unique invention—the computer—or to deny the value of the various "heuristic" experiments as a source of inspiration for new and more interesting uses of it, but not as a march toward the synthetical brain. My only reason for writing this critique is that the computer constitutes a laboratory experiment, as it were, by which arithmomorphic thinking is isolated from dialectical reasoning and, through its limitations, provides experimental proof of what I have tried to preach to my fellow economists ever since some professional experiences awoke me from arithmomorphic slumber: "there is a limit to what we can do with numbers, as there is to what we can do without them."[133] For if we ignore or deny this truth, we are apt to think—as, by and large, we now do—that locomotion, machines to make machines, is all that there is in economic life. By thus steering away from the very core of the economic process where the dialectical propensities of man are mainly at work, we fail in our avowed aim as economists—to study man in the hope of being able to promote his happiness in life.

[133] See my article "The Nature of Expectation and Uncertainty" (1958), reprinted in AE, p. 275.

CHAPTER IV *Measure, Size, and Sameness: Some Object Lessons from Physics*

1. Physics and Philosophy of Science. A social scientist seeking counsel and inspiration for his own activity from the modern philosophy of science is apt to be greatly disappointed, perhaps also confused. For some reason or other, most of this philosophy has come to be essentially a praise of theoretical science and nothing more. And since of all sciences professed today only some chapters of physics fit the concept of theoretical science, it is natural that almost every modern treatise of critical philosophy should avoid any reference to fields other than theoretical physics. To the extent to which these other fields are mentioned (rarely), it is solely for the purpose of proving how unscientific they are.

Modern philosophy of science fights no battle at all. For no one, I think, would deny that the spectacular advance in some branches of physics is due entirely to the possibility of organizing the description of the corresponding phenomenal domain into a theory. But one would rightly expect more from critical philosophy, namely, a nonprejudiced and constructive analysis of scientific methodology in all fields of knowledge. And the brutal fact is that modern works on philosophy of science do not even cover fully the whole domain of physics.

The result of this uncritical attitude is that those who have worked inside the edifice of physics do not always agree with those who admire it only from the outside. The insiders admit, to their regret, that the crown of physics has lost some of the sparkling jewels it had at the time of Laplace. I have already mentioned one such missing jewel: the impossibility, which becomes more convincing with every new discovery, of a noncontradictory logical foundation for all properties of matter. For the biologist or social scientist this constitutes a very valuable object lesson, but there are other lessons at least equally significant. In what follows I shall attempt to point them out.

I begin by recalling an unquestionable fact: the progress of physics has been dictated by the rhythm with which attributes of physical phenomena have been brought under the rule of measure, especially of instrumental measure. More interesting still for our purpose is the correlation between the development of various compartments of physics and the *nature* of the attributes conquered by measure.

As we may find it natural *ex post*, the beginning was made on those variables whose measure, having been practiced since time immemorial, raised no problem. Geometry, understood as a science of the *timeless* properties of bodily objects, has only one basic attribute: length, the prototype of a quality-free attribute. Mechanics was the next chapter of physics to become a complete theoretical system. Again, measures for the variables involved had been in practical use for millennia. It is very important for us to observe that what mechanics understands by "space" and "time" is not *location* and *chronological time*, but *indifferent distance* and *indifferent time interval*. Or, as the same idea is often expressed, mechanical phenomena are independent of Place and Time. The salient fact is that even the spectacular progress achieved through theoretical mechanics is confined to a phenomenal domain where the most transparent types of measure suffice. The space, the time, and the mass of mechanics all have, in modern terminology, a *cardinal* measure.

The situation changed fundamentally with the advent of thermodynamics, the next branch of physics after mechanics to acquire a theoretical edifice. For the first time *noncardinal* variables—temperature and chronological time, to mention only the most familiar ones—were included in a theoretical texture. This novelty was not a neutral, insignificant event. I need only mention the various scales proposed for measuring temperature, i.e., the level of heat, and, especially, the fact that not all problems raised by such a measure have been yet solved to the satisfaction of all.[1]

The extension of theoretical structure to other fields met with still greater difficulties. This is especially clear in the case of electricity, where all basic variables are *instrumentally* measured and none is connected directly with a sense organ—as are most variables in other branches of physics. It is perfectly natural that the invention of the special instruments

[1] For example, P. W. Bridgman, in *The Logic of Modern Physics* (New York, 1928), p. 130, observes that "no physical significance can be directly given to flow of heat, and there are no operations for measuring it."

for measuring electrical variables should have taken longer. Electricity, more than other branches, advanced each time only to the extent to which each measuring instrument could clear additional ground. The opposite is true for mechanics; its progress was not held up much by the problem of measure. We all know the fascinating story of how Galileo discovered the isochronism of the pendulum by comparing the swinging of a candelabrum in the cathedral of Pisa against his own pulse.

We usually stop the survey of physics at this point and thus miss a very important object lesson from such fields as structural mechanics or metallurgy. The complete story reveals that these fields—which are as integral a part of the science of matter as is atomic theory—are still struggling with patchy knowledge not unified into a single theoretical body. The only possible explanation for this backwardness in development is the fact that most variables in material structure—hardness, deformation, flexure, etc.—are in essence *quantified qualities*. Quantification in this case—as I shall argue presently—cannot do away completely with the peculiar nature of quality: it always leaves a qualitative residual which is hidden somehow inside the metric structure. Physics, therefore, is not as free from metaphysics as current critical philosophy proclaims, that is, if the issues raised by the opposition between number and quality are considered—as they generally are—metaphysical.

2. Measure, Quantity, and Quality. As one would expect, man used first the most direct and transparent type of measure, i.e., he first measured *quantity*. But we should resist the temptation to regard this step as a simple affair. Quantity presupposes the abstraction of any qualitative variation: consequently, only after this abstraction is reached does the measure of quantity become a simple matter, in most instances. Undoubtedly it did not take man very long to realize that often no qualitative difference can be seen between two instances of "wheat," or "water," or "cloth." But an immense time elapsed until weight, for instance, emerged as a general measurable attribute of any palpable substance. It is this type of measure that is generally referred to as *cardinal*.

In view of the rather common tendency in recent times to deny the necessity for distinguishing cardinal from other types of measure, one point needs emphasis: cardinal measurability is the result of a series of specific *physical* operations without which the paper-and-pencil operations with the measure-numbers would have no relevance.[2] Cardinal measurability, therefore, is not a measure just like any other, but it reflects a

[2] For an axiomatic analysis of how cardinal measure is derived from these physical operations, see the author's "Measure, Quality, and Optimum Scale," in *Essays on Econometrics and Planning Presented to Professor P. C. Mahalanobis on the Occasion of His 70th Birthday*, ed. C. R. Rao (Oxford, 1964), pp. 232–246.

particular physical property of a category of things. Any variable of this category always exists as a *quantum* in the strict sense of the word (which should not be confused with that in "quantum mechanics"). Quantum, in turn, possesses simple but specific properties.

Whether we count the number of medicine pills by transferring them one by one from the palm into a jar, or whether we measure the amount of water in a reservoir by emptying it pail by pail, or whether we use a Roman balance to weigh a heap of flour, cardinal measure always implies *indifferent subsumption* and *subtraction* in a definite physical sense. To take a most elementary example: by a *physical* operation independent of any measure we can subsume a glass of water and a cup of water or take out a cup of water from a pitcher of water. In both cases the result is an instance of the same entity, "water."

Of these two conditions (which are necessary but not sufficient for cardinality), subtraction is the more severe. We can subsume baskets of apples and pears, for instance, and by some definite device even colors. But the condition of subsumption suffices to disprove the cardinality of a great number of variables that are treated by economists as cardinal—if not continuously, at least significantly often. Even Bentham, in a moment of soul searching, invoked the absence of subsumption against his own notion of a cardinal utility for the entire community: "'Tis in vain to talk of adding quantities which after the addition will continue distinct as they were before, . . . you might as well pretend to add twenty apples to twenty pears."[3] Actually, the same argument provides the simplest way of exploding the thesis of cardinal utility even for the individual. For where, may I ask, is that reservoir where the utilities and disutilities of a person accumulate? Utility and disutility viewed as a relation between an external object and the individual's state of mind, not as a property intrinsic to the object, are psychic fluxes. By the time we feel exhausted at the end of one day's work, no one can tell where the pleasure felt during one phase of that work is. Like the past itself, it is gone forever. But the example that should suffice to settle the issue of the necessity of distinguishing cardinality from pure ordinality, because it is so crystal-clear and also familiar to everybody, is chronological time, or "historical date," if you wish. Clearly, there is absolutely no sense in which we can subsume two historical dates meaningfully, not even by paper-and-pencil operations after some number has been attributed to each one of them.

[3] Quotation, from an unpublished manuscript, in Elie Halévy, *The Growth of Philosophic Radicalism* (Boston, 1955), p. 495. But, like countless others, Bentham went on to argue that this is the voice of "indifference or incapacity," explaining that, even though the addibility of the happiness of different individuals is fictitious, without it "all political reasoning is at a stand." See also *The Works of Jeremy Bentham*, ed. J. Bowring (11 vols., Edinburgh, 1838–1843), I, 304.

"Historical date" is not a cardinal variable. And no rational convention can make it so.[4]

To complete all sides of my argument against the relativist position, let me observe that if you interrupt a natural scientist in the course of one of his arguments and ask him what corresponds in actuality to any equation he may have written on the blackboard, he will give you a definite, perfectly intelligible answer. He might even invite you into his laboratory to show you the operational meaning of that equation. In contrast to this situation, social scientists, generally, go on a spree of arithmomania and apply arithmetical operations on paper to any numbers they can get hold or think of, without stopping for one moment to consider whether these operations have any meaning at all. Do we not frequently see economists adding discounted utilities of future dates—i.e., discounted future fluxes—as if they were annuities paid in money (a cardinal variable)? How often also do we see the formula of the arithmetic mean—which implies addition—applied to variables for which subsumption makes absolutely no sense? Even if we grant that performances in an examination, for instance, are ordered in the same way by all members of the committee, the grades of different members will hardly yield the same arithmetic mean. The consequence is that student A will be the typical candidate according to member X, student B according to member Y, and so forth.[5] Psychologists and specialists on education have gradually become aware of the fallacy and now employ only the median, an order statistic, to describe attributes that are purely ordinal variables. Economists seem to be still very far from coming of age in this respect.

As I have intimated, quantity cannot be regarded as a notion prior to quality, either in the logical or evolutionary order. Undoubtedly, before the thought of measuring quantities of, say, wheat, man must have first come to recognize that one pile of wheat is greater than another *without weighing them*. For a long time "colder" and "hotter" had no measure. Distinctions, such as between "more friendly" and "less friendly" and, especially, between "earlier" and "later," which reflect qualitative differences, must have long preceded the practice of quantity measure. The things to which terms such as these apply came to be arranged in a definite mental order. Only later was a ranking number assigned to each of them, as must have happened first with events in time and, probably, with kinship. This "ranking" step represents the basis of the modern

[4] See further remarks on this point in Section 6 of this chapter.

[5] For a general discussion of the concept of "average" from this particular angle, see my essay "An Epistemological Analysis of Statistics as the Science of Rational Guessing," *Acta Logica*, X (1967), 61–91.

concept of *ordinal* measure. But the precedence of the ranking concept over that of quantity had a lasting influence upon the development of our ideas in this domain. Bertrand Russell rightly observed that philosophers are wholly mistaken in thinking that quantity is essential to mathematics; wherever it might occur quantity is not "*at present* amenable to mathematical treatment."[6] But even nowadays, order, not quantity, occupies the central place in pure mathematics.

Old as the basic principles of measure are and frequently as they have been discussed in recent years, we have been rather slow in perceiving the essential difference between cardinal and purely ordinal measure. Specifically, from the fact that cardinal measure presupposes ordinality we have often concluded that distinguishing between cardinal and purely ordinal measure is irrelevant hairsplitting. This position completely ignores the shadow that quality casts over purely ordinal measure. The things encompassed by a purely ordinal measure must necessarily vary qualitatively, for otherwise there would be absolutely nothing to prevent us from subsuming and subtracting them physically and, hence, from constructing a cardinal measure for them.

On the other hand, we must recognize that cardinal and purely ordinal measurability represent two extreme poles and that between these there is room for some types of measure in which quality and quantity are interwoven in, perhaps, limitless variations. Some variables, ordinally but not cardinally measurable, are such that what appears to us as their "difference" has an indirect cardinal measure. Chronological time and temperature are instances of this. There is only one rule for constructing a measuring scale for such variables that would reflect their special property. Because of its frequency among physical variables, I proposed to distinguish this property by the term *weak cardinality*.[7] For self-evident reasons, a weak cardinal measure, like a cardinal one, is readily transformed into an instrumental one.

At this juncture a thorny question inevitably comes up: are there ordinally measurable attributes that could not possibly be measured by a pointer-reading instrument? Any definitive answer to this question implies at least a definite epistemological, if not also a metaphysical, position. The prevailing view is that all attributes are capable of instrumental measure: with time we will be able to devise a pointer-reading instrument for every attribute. F. P. Ramsey's faith in the eventual invention of some sort of psychogalvanometer for measuring utility, for

[6] Bertrand Russell, *The Principles of Mathematics* (Cambridge, Eng., 1903), p. 419. Italics mine, to emphasize that the mathematical theory of measure was yet rather an esoteric topic at the time of Russell's statement.

[7] Cf. the author's "Measure, Quality, and Optimum Scale," p. 241.

example, clearly reflects this position.[8] In Ramsey's favor, one may observe that nowadays a meter of an electronic computer could show the I.Q. of an individual within a fraction of a second after he has pushed a system of buttons related to the truth-falsehood of a series of questions. And if one is satisfied with the idea that the I.Q. measures intelligence, then intelligence *is* measured by a pointer-reading instrument. On the other hand, there is the fact that hardness has so far defied the consummate ingenuity of physicists, and its scale is still exclusively qualitative. But probably the most salient illustration in this respect is supplied by entropy: basic though this variable is in theoretical physics, there is no *entropometer* and physicists cannot even suggest how it might be designed.[9] Thus, although the evidence before us shows that physics has been able to devise measuring instruments for an increasing number of measurable attributes, it does not support the view that potentially all measures are reducible to pointer-readings.

3. The Qualitative Residual. Variables in all equations of physics, whether in mechanics or in material structure, represent numbers. The only way in which quality appears explicitly in these equations is through a differentiation of symbols, as in $E = mc^2$ where E, m, and c stand for discretely distinct categories or constants. Ordinarily a physicist is not at all preoccupied by the fact that some variables are quantity measures while others measure quantified qualities. However, the quantification of a qualitative attribute—as I argued in the preceding section—does not change the nature of the attribute itself. Nor can quantification therefore destroy the quality ingredient of a phenomenon involving such an attribute. It stands immediately to reason that, since quantification does not cause quality to vanish, it leaves a qualitative residual which perforce must be carried over into the numerical formula by which the phenomenon is described. Otherwise this formula would not constitute an adequate description. The problem is to find out under what form the qualitative residual is hidden in a purely numerical pattern.

An examination of the basic laws of classical mechanics will show us the direction in which the answer lies. As already pointed out, this oldest branch of physics covers only cardinal variables. Newton's Second Law

[8] F. P. Ramsey, *The Foundations of Mathematics and Other Logical Essays* (New York, 1950), p. 161. As one may expect, Ramsey had some famous predecessors among Hedonists. Bentham seems to have been the first to dream of a "moral thermometer" (*Works*, I, 304). Later, F. Y. Edgeworth, *Mathematical Psychics* (Reprint, London, 1932), p. 101, even coined a word, "hedonimeter," for the device of his hopes.

[9] Another important variable in modern physics that is not instrumentally measurable is the wave function, Ψ. Louis de Broglie, *Physics and Microphysics* (London, 1955), p. 80.

states, first, that the effect of a force upon a given body, the acceleration of that body's motion, is *proportional* to the quantum of force, and second, that the effect of a given force upon any particular body is *proportional* to the latter's mass. Furthermore, the essence of Newton's Law of Gravitation can be formulated in a similar manner: the attraction exerted by one body upon a unit of mass is *proportional* to the mass of the body and uniformly diffused in all directions.

One could cite other basic laws in physics that also affirm the proportional variation of the variables involved: the various transformation laws of energy, or such famous laws as Planck's ($E = h\nu$) and Einstein's ($E = mc^2$). The point I wish to make is that this simple pattern is not a mere accident: on the contrary, in all these cases the proportional variation of the variables is the inevitable consequence of the fact that every one of these variables is free from any qualitative variation. In other words, they all are cardinal variables. The reason is simple: if two such variables are connected by a law, *the connection being immediate in the sense that the law is not a relation obtained by telescoping a chain of other laws*, then what is true for one pair of values must be true for all succeeding pairs. Otherwise, there would be some difference between the first and, say, the hundredth pair, which could mean only a qualitative difference. This characteristic property of the *cardinal* laws (as the laws under discussion may be properly called) constitutes the very basis on which Cantor established his famous distinction between ordinal and cardinal number. We arrive, Cantor says, at the notion of cardinal number by abstracting from the varying quality of the elements involved and from the order in which we have "counted" them.[10] In fact, the first condition is the essential one, for without a qualitative difference of some sort, the order in which we count the elements remains arbitrary and, hence, becomes immaterial.

There is therefore an intimate connection between cardinality and the homogeneous linearity of a formula by which a direct law is expressed. On the basis of this principle, nonhomogeneous linearity would generally indicate that some of the variables have only a weak cardinality. Indeed, a nonhomogeneous linear relation is equivalent to a linear homogeneous relation between the finite differences of all variables.

A counter-proof of the principle just submitted is even more enlightening. For this we have to turn to the least advertised branch of physics, that of material structure. This field abounds in quantified qualities: tensile strength, elastic limit, flexure, etc. We need only open at random any treatise on material structure to convince ourselves that no law

[10] G. Cantor, *Contributions to the Foundations of the Theory of Transfinite Numbers* (New York, n.d.), p. 86.

involving such variables is expressed by a linear formula. (In fact, in some cases there is no formula at all but only an empirically determined graph.) The reason is, again, simple. Successive pounds of load may be regarded as equal causes, but their individual effect upon the shape of a beam is not the same. Deformation being a measurable quality, the nth degree of deformation is not qualitatively identical to any of the preceding degrees. Nor does "n degrees of deformation" represent the subsumption of n times "one degree of deformation." We thus reach the correlative principle to the one stated in the preceding paragraph: nonlinearity is the aspect under which the qualitative residual appears in a numerical formula of a quality-related phenomenon.

One may think of refuting this conclusion by *implicit measuring*, i.e., by choosing an ordinal scale for the quantified quality so as to transform the nonlinear into a linear relation. Joan Robinson once tried this idea for labor efficiency.[11] The reason why her attempt failed is general: we have to establish an implicit measure for *every* situation to which the relation applies. That would be no measure at all. Moreover, most quality-related phenomena have a sort of climax, followed by a rapid breakdown; such a nonmonotonic variation cannot possibly be represented by a linear function.

The situation is not as limpid in the case of homogeneous linearity. Some laws covering quantified quality are nevertheless expressed as proportional variations. An example is Robert Hooke's law: elastic stress is proportional to the load strain. But the contradiction is purely superficial, for in all such cases the linear formula is valid only for a limited range and even for this range it represents only a convenient approximation, a rule of thumb.[12] Such cases suggest that some of the other laws now expressed by linear formulae may be only a rule of thumb. One day we may discover that linearity breaks down outside the range covered by past experiments. The modern history of physics offers several examples of such discoveries. Perhaps the most instructive is the famous formula proposed by H. A. Lorentz for velocity addition. In the classical formula, which proceeds from the principle that equal causes produce equal effects on velocity, we have $V = v + v + \cdots + v = nv$, which is a homogeneous linear function of n, that is, of scale. But, for the same situation, as is easily proved, the Lorentz law yields $V = c[(c + v)^n - (c - v)^n]/[(c + v)^n + (c - v)^n]$. In this case, the effect of each additional v

[11] Joan Robinson, *The Economics of Imperfect Competition* (London, 1938), p. 109 and *passim*.

[12] Similar examples are far more frequent in the organic domains. In psychology, the Weber law says that the threshold of perception is proportional to the intensity of the applied stimulus; in economics, we have the Acceleration Principle.

decreases with the scale n. We can then understand why physicists lose no opportunity to decry the extrapolation of any known law outside the range of actual experiments.[13] Even though the protests should not be taken literally, their ground is unquestionable. It would seem, therefore, that if we take cardinality to be a physical property we should also admit that this property too might be limited to a certain range of the quantum. This would vindicate Hegel's dictum, that quantitative changes in the end bring about qualitative changes,[14] over the entire domain of physics—and perhaps to an extent not intended even by Hegel. Indeed, if the dictum applies to quantity itself then it loses all meaning.[15]

On the other hand, no physicist—to my knowledge—has denounced the extrapolation of cardinality, much less the existence of cardinal variables. For a physicist, the typical instrumental measure means the comparison of two instances of the *same* variable. Only in a few cases, however, can two such instances be subsumed or compared *directly*. Length and mass are the examples par excellence, a reason why they are included in the fundamental system of units upon which the instrumental operationality of physics rests. Even though this system includes time as well, time is not a primary variable in the same sense in which length and mass are. To subsume or compare two time intervals we must first measure them by some sort of clock (which provides an indirect measure of time by length). The measure of time, like a host of other measures in physics, rests on a convention which, to a large extent, is arbitrary.[16] For these very reasons, when the cardinality of length appeared menaced by the Lorentz contraction formula it was the cardinality of the Newtonian time that was sacrificed instead: Einstein's formula for the "contraction" of time saved the cardinality of length. The upshot is that if a variable is such that its present cardinal measure is established within a closed system—i.e., without reference to *other* variables through some measuring instrument(s)—it is hard to conceive of reasons for abandoning its cardinality in the future. Perhaps in Hegel's intention "quantity" should apply only to such primary variables.

Be this as it may, we may discover that some variables presently

[13] Bridgman, *Logic of Modern Physics*, p. 203; P. W. Bridgman, *The Intelligent Individual and Society* (New York, 1938), p. 13; Werner Heisenberg, *Physics and Philosophy: The Revolution in Modern Science* (New York, 1958), pp. 85 f.

[14] *The Logic of Hegel*, tr. W. Wallace (2nd edn., London, 1904), pp. 203 and *passim*.

[15] "In quantity we have an alterable, which in spite of alterations still remains the same." *Ibid.*, p. 200.

[16] Precisely because time is not a primary variable, E. A. Milne, *Kinematic Relativity* (Oxford, 1948), p. 37, was free to suggest that time should be measured not on the scale t of ordinary clock-time but on the scale e^t.

considered cardinal are not really so—as happened with time and velocity—but it does not seem possible for any science to get rid of quantity altogether any more than to ignore quality completely. For then all laws of physics would be reduced to nonmetric, topological propositions and its success in practice would almost come to an end. The point is important, and I shall presently illustrate it with examples from economics.

4. The Problem of Size. Without forgetting the caveats I have inserted into the preceding analysis, we can generally expect that if the variables *immediately connected* by a phenomenon are cardinally measurable, then they can all be increased in the same proportion and still represent the same phenomenon. The formula describing the phenomenon then must be homogeneous and linear or, more generally, a homogeneous function of the first degree. On the other hand, if some variable is a quantified quality, nothing seems to cast doubt over our expectation that the formula will be nonlinear.

Since the first situation characterizes a phenomenon (or a process) *indifferent to size,* it is clear that the problem of size arises only for processes involving quantified qualities, and conversely. Needless to add, the same would apply with even greater force to processes involving nonquantifiable qualities. The conclusion is that the problem of size is strictly confined to quality-related processes.

The point I wish to emphasize is that in support of this conclusion I have not invoked one single piece of evidence outside the domain of inert matter. The fact that it is physics which teaches us that size is indissolubly connected with quality, therefore, deserves special notice on the part of students of life phenomena. Indeed, the prevailing opinion regarding size, which constitutes one of the most important chapters in biology and social sciences, has been that the problem arises only in these sciences because they alone have to study organisms.

The idea that the optimum size of an elephant or of a mosquito is determined not by the whim of the creature or by some accident but by physical laws having to do with quantified qualities, curiously, is relatively old—older than the origin of systematic biology. It was first expounded by Galileo in such a piercing form that his summarizing paragraph deserves to be quoted in full:

"From what has already been demonstrated, you can plainly see the impossibility of increasing the size of structures to vast dimensions either in art or in nature; *likewise the impossibility of building ships, palaces, or temples of enormous size* in such a way that their oars, yards, beams, iron-bolts, and, in short, all their other parts will hold together; nor can nature produce trees of extraordinary size because the branches would break down under their own weight; so also it would be impossible to build up the bony structures of men, horses, or other animals so as to hold together and perform their normal functions if these animals were to be increased enormously in height; for this increase in height can be accomplished only by employing a material which is harder and stronger than usual, or by enlarging the size of the bones, thus changing their shape until the form and appearance of the animals suggest a monstrosity."[17]

If biologists—ever since Herbert Spencer rediscovered Galileo's argument—have been able to accept the idea of the intimate connection between biological size and the laws of material structure and explore attentively its implications,[18] it is because biologists alone have been interested in what happens inside the individual organism. The common flow-complex of the economist leads to the position that what happens inside a production unit concerns exclusively the engineer, that economics is concerned only with the flows observed at the plant gate, i.e., with *inter-unit flows*. And this flow-complex is responsible for many myopic views of the economic process.

Actually, outside a few exceptions, economists (as well as other social scientists) have opposed any suggestion that the general concept of organism may be a useful tool in their own domain. Their inbred mechanistic outlook of scientific explanation has prompted many even to denounce that concept as unscientific and, naturally, deny its legitimate use in any special science. As a result, the problem of whether an optimum size exists for the unit of production is still unsettled in economics. Apparently tired of the endless controversy, numerous economists have now ceased to pay any attention to the problem. But the literature of yesteryears and yesterdays on this issue contains some highly ingenious, albeit off the mark, arguments against the existence of optimum size. Strangely, most of these arguments, on both sides of the fence, do involve biological analogies. Some run in terms of "ant-men" and "ant-machines."[19]

One of the arguments, which attacks the problem from an independent position and which many economists are greatly fond of, may seem, at

[17] Galileo Galilei, *Dialogues Concerning Two New Sciences*, tr. H. Crew and A. de Salvio (Chicago, 1939), p. 130. My italics.

[18] Herbert Spencer, *The Principles of Biology* (2 vols., New York, 1886), I, 121 ff. Practically every noted biologist has written on this matter: e.g., J. B. S. Haldane, *Possible Worlds and Other Papers* (New York, 1928), pp. 20–28. But none has covered it more masterly and thoroughly than D'Arcy W. Thompson, *On Growth and Form* (2 vols., 2nd edn., Cambridge, Eng., 1952), I, 22–77.

[19] For a critique of the most popular of these arguments, see my paper "Chamberlin's New Economics and the Unit of Production," in *Monopolistic Competition Theory: Studies in Impact*, ed. R. E. Kuenne (New York, 1967), pp. 31–62.

first sight, to have some affinity with the principle advocated at the beginning of this section. The argument claims that if *absolutely all* pertinent elements of phenomena are taken into account, natural laws are always expressed by homogeneous functions of the first degree; if our observations lead us—as is often the case—to a different type of formula, this is no proof against the claim but a positive sign that we have ignored some factors. The curious fact about this thesis is that, even if it were valid, it has no bearing on the economic issue of the optimum size of the unit of production. Certainly, the economic issue does not depend on all pertinent factors—there are some that are free goods. On the other hand, if viewed as a general principle of nature, the thesis is not only operationally idle—as Samuelson pointed out[20]—but also downright incongruous. Indeed, let $y = f(x_1, x_2, \ldots, x_n)$ be the linearly homogeneous expression of some complete law, i.e., when all the pertinent factors $X_1, X_2, \ldots, X_n$ are taken into account. Let us now assume that X_n is the ignored factor and that there are no errors of observation.[21] In the general case, our observations will be scattered over an n-dimensional domain of the space $(y, x_1, x_2, \ldots, x_{n-1})$ and, consequently, we will not be able even to suspect the existence of a law. If, however, all observations happen to lie on a hyper-surface, $y = g(x_1, x_2, \ldots, x_{n-1})$, X_n is a spurious factor and the last equation is the complete law. That is, all observed laws must be complete laws. And since not all observed laws are expressed by linearly homogeneous functions, the absurdity of the thesis is proved.

5. Sameness and Process. In continuation of the preceding remarks, one point can hardly be overemphasized at this stage: the problem of size is indissolubly connected with the notion of sameness, specifically with the notion of "same phenomenon" or "same process." In economics we prefer the term "unit of production" to "same process," in order to emphasize the abstract criterion by which sameness is established. Whatever term we use, sameness remains basically a primary notion which is not susceptible to complete formalization. "The same process" is a class of analogous events, and it raises even greater difficulties than "the same object." But we must not let our analysis—in this case any more than in others—run aground on this sort of difficulty. There are many points that can be clarified to great advantage once we admit that "sameness," though unanalyzable, is in most cases an operational concept.

Let then P_1 and P_2 be any two distinct instances of a process. The

[20] Paul A. Samuelson, *Foundations of Economic Analysis* (Cambridge, Mass., 1948), p. 84.

[21] The last assumption is absolutely necessary for the probing of the thesis which, it should be emphasized, has nothing to do with observational errors—nor, in fact, with the question of successive approximations.

problem of size arises only in those cases where it is possible to subsume P_1 and P_2 *in vivo* into another instance P_3 of the *same* process. If this is possible we shall say that P_1 and P_2 are added internally and write

$$P_1 \oplus P_2 = P_3. \tag{1}$$

We may also say that P_3 is *divided* into P_1 and P_2 or that the corresponding process (P) is *divisible*. For an illustration, if the masses m_1 and m_2 are transformed into the energies E_1 and E_2 respectively, by two distinct instances P_1 and P_2, these individual instances can be added internally because there exists an instance of the same process which transforms $m_1 + m_2$ into $E_1 + E_2$. We can also divide P_3 into P_1 and P_2 or even into two half P_3's—provided that P does not possess a natural, indivisible unit. Needless to say, we cannot divide (in the same sense of the word) processes such as "elephant" or even "Harvard University."

It is obvious that it is the internal addition *in vivo* that accounts for the linearity of the corresponding paper-and-pencil operation. For even if the subsumption of P_1 and P_2 is possible but represents an instance of a *different* process, our paper-and-pencil operations will reveal a nonlinear term.[22]

Another point that deserves emphasis is that processes can also be *added externally*. In this case, P' and P'' need not even be instances of the same process. In the external addition,

$$P' + P'' = P''', \tag{2}$$

P' and P'' preserve their individuality (separation) *in vivo* and are lumped together only in thought or on paper. External and internal addition, therefore, are two entirely distinct notions.

When an accountant consolidates several balance sheets into one balance sheet, or when we compute the net national product of an economy, we merely add all production processes externally. These paper-and-pencil operations do not necessarily imply any real amalgamation of the processes involved. In bookkeeping all processes are additive. This is why we should clearly distinguish the process of a unit of production (plant or firm) from that of *industry*. The point is that an industry may expand by the accretion of *unconnected* production processes, but the growth of a unit of production is the result of an internal morphological change.

It follows that if the units which are externally added in the bookkeeping process of industry are *identical*, then proportionality will govern

[22] This term reflects what we may call the interaction generated by the merging of two distinct individual phenomena. For an instructive illustration the reader may refer to Schrödinger's interpretation of the nonlinear term in the equation of wave mechanics. E. Schrödinger, "Are There Quantum Jumps?" *British Journal for the Philosophy of Science*, III (1952), 234.

the variations of the variables involved—inputs and outputs. The constancy of returns to scale therefore is a tautological property of a granular industry.[23] To the extent that an actual industry represents an accretion of practically identical firms, no valid objection can be raised against the assumption of constant coefficients of production in Wassily Leontief's system.

One point in connection with the preceding argument is apt to cause misunderstanding. Since I have argued that phenomena involving only cardinally measurable variables necessarily are indifferent to scale, one may maintain that I thereby offered the best argument against the existence of the optimum scale of the plant, at least. Indeed, a critic may ask: by and large, are not plant inputs and outputs cardinally measurable?

Such an interpretation would ignore the very core of my argument, which is that only if the cardinally measurable variables are immediately connected—as cause and effect in the strictest sense of the terms are—can we expect the law to be expressed by a homogeneous linear formula. To return to one of the examples used earlier, we can expect acceleration to be proportional to force because force affects acceleration directly: to our knowledge there is no intermediary link between the two. I have not even hinted that cardinality by itself suffices to justify homogeneous and linear law formulae. I visualize cardinality as a physical property allowing certain definite operations connected with measuring, and, hence, as a property established prior to the description of a phenomenon involving cardinal variables. Precisely for this reason, I would not concur with Schrödinger's view that energy may be in some cases "a 'quantity-concept' (Quantitätsgrösse)," and in others "a 'quality-concept' or 'intensity-concept' (Intensitätsgrösse)."[24] As may be obvious by now, in my opinion the distinction should be made between internally additive and nonadditive processes instead of saying that the cardinality of a variable changes with the process into which it enters.

As to the case of a unit of production, it should be plain to any economist willing to abandon the flow-complex that inputs and outputs are not *directly connected* and, hence, there is no *a priori* reason for expecting

[23] I am using the term "granular industry" instead of "atomistic industry," not only because nowadays the latter may cause confusion, but also because the property of constant returns to scale does not necessarily require the number of firms to be extremely large. This number should be only so large as to make it impracticable for any firm to predict the *ultimate* effect on the market of any strategy the firm may adopt, that is, so large that intraspecies competition shall be fruitless. It is elementary that there is a penumbra of numbers that just satisfy this condition and that its order of magnitude depends on the state of strategy analysis, the entrepreneurs' awareness of this analysis, and of the information currently available to each firm.

[24] Schrödinger, "Are There Quantum Jumps?" 115.

the production function to be homogeneous of the first degree. The familiar plant-production function is the expression of an external addition of a series of physical processes, each pertaining to one of the partial operations necessary to transform the input materials into product(s). It is because most of these intermediary processes are quality-related that no plant process can be indifferent to scale. We know that the productive value of many inputs that are unquestionably cardinally measurable does not reside in their material quantum. Although materials are bought by weight or volume, what we really purchase is often resistance to strain, to heat, etc., that is, quality, not quantity. This is true whether such materials are perfectly divisible or not. Consequently, the so-called tautological thesis—that perfect divisibility of factors entails constant returns to scale—is completely unavailing. If, nevertheless, it may have some appeal it is only because in the course of the argument "divisibility of factors" is confused with "divisibility of processes." Whenever this is the case the argument no longer applies to the unit of production; with unnecessary complications it only proves a tautological feature of a molecular industry.

6. Cardinality and the Qualitative Residual. Perhaps the greatest revolution in modern mathematics was caused by Evariste Galois' notion of group. Thanks to Galois' contribution, mathematics came to realize that a series of properties, which until that time were considered as completely distinct, fit the *same abstract* pattern. The economy of thought achieved by discovering and studying other abstract patterns in which a host of situations could be reflected is so obvious that mathematicians have turned their attention more and more in this direction, i.e., towards formalism. An instructive example of the power of formalism is the abstract pattern that fits the point-line relations in Euclidean geometry and at the same time the organization of four persons into two-member clubs.[25] Time and again, the success of formalism in mathematics led to the epistemological position that the basis of knowledge consists of formal patterns alone: the fact that in the case just mentioned the pattern applies to points and lines in one instance, and to persons and clubs in the other, is an entirely secondary matter. By a similar token—that any measuring scale can be changed into another by a strictly monotonic transformation, and hence the strictly monotonic function is the formal pattern of measure—cardinality has come to be denied any epistemological significance. According to this view, there is no reason whatsoever why a carpenter should not count one, two, . . . , 2^n, as he lays down his yardstick once,

[25] Cf. R. L. Wilder, *Introduction to the Foundations of Mathematics* (New York, 1956), pp. 10–13.

twice, . . . , n-times or, as Broglie suggests, use a ruler graduated logarithmically.

Such being the typical objections against the distinction between various kinds of measurabilities, they are tantamount to arguing that there are no objective reasons for distinguishing, say, a fish from an insect. Why, both are animals! Louis de Broglie goes on to say that cardinality is an arbitrary idea (at most, a pure convention) reflecting unconscious habits and shallow intuitions[26]—leaving one thus wondering whether a physicist deprived of such impurities could still perform any routine operation in his laboratory. To prove then that cardinality is a matter of convention, Broglie takes the case of two gases having the *same* number of molecules. E_1 and E_2 being the heat-energies of these gases, the relation $E_1 = kE_2$, as is well known, entails that the absolute temperatures, T_1 and T_2, satisfy the analogous relation $T_1 = kT_2$. Ergo, we have here a convention that transfers the cardinality of heat-energy to the absolute temperature. The argument, however, succeeds only in making one more fully aware of why absolute temperature is not a cardinal variable. For the contrast is now sharper: length is a cardinal variable because we can perform certain operations with it directly (we can, for instance, subsume lengths) and not because we can make a completely analogous convention to Broglie's by using the fact that the lengths of two rectangles of the *same* width are proportional to the two areas.

There are also economists who have propounded the relativity of measure. Apparently, they failed to see that this view saps the entire foundation upon which the economic science rests. Indeed, this foundation consists of a handful of principles, all stating that some particular phenomenon is subject to increasing or decreasing variations. There is no exception, whether the principle pertains to consumption or production phenomena: decreasing marginal utility, decreasing marginal rate of substitution, increasing internal economies, and so on.

It is a relatively simple matter to see that these principles lose all meaning if cardinality is bogus. Clearly, if there is no epistemological basis for measuring corn one way or the other, then marginal utility may be freely increasing or decreasing over any given interval. Surprising though it may seem, the relativity of measure would cause a greater disaster in the study of production than in that of consumption. Isoquants, cost curves, scale curves could then be made to have almost any shape we choose.[27] The question whether theoretical physics needs a cardinal basis is beyond the scope of this essay, but there can hardly be

[26] Louis de Broglie, *Physics and Microphysics*, pp. 81 f.

[27] For details, see my article cited above, "Measure, Quality, and Optimum Scale," pp. 234, 246.

any doubt that economic activity, because of its pedestrian nature, cannot exist without such a basis.

We buy and sell land by acres, because land is often homogeneous over large areas; and because this homogeneity is not general, we have differential rent. How unimaginably complicated economic life would be if we adopted an ordinal measure of land chosen so as to eliminate differential rent, let alone applying the same idea to all economic variables involving qualitative variations!

Since cardinality is associated with the complete absence of qualitative variation, it represents a sort of natural origin for quality. To remove it from the box of our descriptive tools is tantamount to destroying also any point of reference for quality. Everything would become either "this" or "that." Such an idea would be especially pernicious if followed by economics. Any of the basic principles, upon which a good deal of economic analysis rests, is at bottom the expression of some qualitative residual resulting from the projection of quality-related phenomena upon a cardinal grid. The principle of decreasing elasticity of factor substitution, to take one example, is nothing but such a residual. A critical examination of its justification would easily disclose that substitutable factors belong to a special category mentioned earlier: they participate in the production process through their qualitative properties. The other category of factors, which are only carried through the process as mere substances of some sort, cannot, strictly speaking, cause any qualitative residual and, hence, give rise to substitution. For an illustration one can cite the inputs of copper and zinc in the production of a particular brass. We thus come to the conclusion that every relation between two inputs, or an input and the output, may or may not show a qualitative residual depending on the kind of role the corresponding factors play in the production process. This difference is responsible for the great variety of patterns which a production function may display and which is covered by the general notion of limitationality.[28]

Many economists maintain that economics is a deductive science. The preceding analysis of the nature of the basic principles pertaining to the quantitative variations of cardinally measurable economic goods justifies their position, but only in part. Certainly, to affirm the existence of a qualitative residual is an *a priori* synthetic judgment rather than an empirical proposition. But only by factual evidence can we ascertain whether the qualitative residual is represented by increasing or decreasing

[28] The above remarks may be regarded as some afterthoughts to my 1935 paper, "Fixed Coefficients of Production and the Marginal Productivity Theory," reprinted in AE, which in all probability represents the first attempt at a general analysis of limitationality in relation to the pricing mechanism.

variations. The point seems obvious enough. Nevertheless, I wish to illustrate it by a particularly instructive example.

Still groping towards the idea that the basic feature of the preference map in *a field of cardinally measurable commodities* reflects a qualitative residual, in a 1954 article I replaced the principle of decreasing marginal rate of substitution by a new proposition which brings quality to the forefront. To put it elementarily, my point of departure was that if *ten pounds* of potatoes and *ten pounds* of corn flour happen to be equivalent incomes to a consumer, then an income of *ten pounds* of any mixture of potatoes *and* corn flour could not be equivalent to either of the initial alternatives. This negative statement simply acknowledges the existence of a qualitative residual in the preference map and, hence, needs no empirical support: the "axiom" that choice is quality-related suffices. But under the influence of the tradition-rooted notion that indifference curves are *obviously* convex, I went one step further and asserted that the ten-pound mixture is (generally) preferred to either of the other two. For obvious reasons, I called the postulate thus stated the *Principle of Complementarity.*[29] Carl Kaysen questioned the postulate on the ground that some ingredients may very well produce a nauseating concoction. At the time, I was hardly disturbed by the objection, for I was satisfied by the observation that my postulate compels the individual neither to actually mix the ingredients nor to consume them in a certain order. It was only later that I came to see the relevance of Kaysen's question, as I struck upon a simple counter-example of the postulate: a pet lover may be indifferent between having two dogs or two cats but he might find life unpleasant if he had one dog and one cat. The example shows that since some commodities may have an antagonistic effect the Principle of Complementarity is not generally valid. As I have said, only factual evidence can determine in which direction the qualitative residual disturbs proportionality. And since without specifying this direction the basic principles of economics are practically worthless, the position that they are *a priori* synthetic truths is only half justified. Like all half-truths, this position has had some unfortunate effects upon our thoughts.

[29] See Section II of "Choice, Expectations, and Measurability" (1954), reprinted in AE.

CHAPTER V *Novelty, Evolution, and Entropy: More Object Lessons from Physics*

1. Theory and Novelty. Modern philosophy of science usually fails also to pay sufficient attention to the fact that the study of inert matter is divided between physics and chemistry. Probably it is thought that the separation of chemistry from physics is a matter of tradition or division of labor. But if these were the only reasons, chemistry would have long since become an ordinary branch of physics, like optics or mechanics, for instance. With the creed of unified science sweeping the intellectual world, why are the frontier posts still in place? The recent establishment of physical chemistry as an intermediary link between physics and chemistry clearly indicates that the complete merger is prevented by some deep-lying reason. This reason is that chemistry does not possess a theoretical code of orderliness. Hence, only harm could result from letting that Trojan horse inside the citadel of physics.[1]

One may be greatly puzzled by the observation that there is no chemical theory. After all, chemistry, like physics, deals with quantities and quantified qualities. That two atoms of hydrogen and one atom of oxygen combine into a molecule of water is an example of a quantitative chemical proposition. True, chemistry does study some quantified qualities of substance: color, hardness, acidity, water repellence, etc. But in the end, even these qualitative properties are expressed by arithmomorphic

[1] The reader who might come across the statement in W. Heisenberg, *Physics and Philosophy: The Revolution in Modern Science* (New York, 1958), p. 101, that physics and chemistry "have come to a complete union" should note that Heisenberg meant only that chemistry, too, now treats matter as consisting of Bohr atoms.

propositions. *Prima facie*, therefore, nothing could prevent us from passing all chemical propositions through a logical sieve so as to separate them into an α-class and a β-class (as suggested in Chapter I, Section 5). Why then is there no chemical *theory*?

To answer this question, let us observe that physics, in spite of the stochastic form of its laws and the indeterminacy of the instrumental observations, is still a mechanistic science if this term is given a broader meaning that retains the crucial article of the classical faith. In this sense, a science is mechanistic if, first, it assumes only a finite number of qualitatively different elements, and if, second, it assumes a finite number of fundamental laws relating these elements to everything else in the same phenomenal domain. The fact that the fundamental laws may be complementary in Bohr's sense is not an impediment in this connection. Physics may cease, however, to be mechanistic if the present trend of discoveries of one elementary particle after another leads to the conclusion that the number of such particles is limitless. With a *qualitative* infinity at the elementary level, the system of fundamental laws too will have to be infinite. In that case, it would be no longer possible to achieve any economy of thought through a logical sifting of all propositions. The most we could do would be to classify all propositions into a finite number of classes by some affinity criterion or, more practical, into groups according to the frequency each proposition is needed in everyday activity.

At present, in chemistry we have an analogous situation, but created by factors coming from another direction. Chemistry is not interested only in how the finite number of chemical elements combine themselves into numberless other chemical substances. As noted above, chemistry is also interested (and even more so) in the various qualities of substances *in the bulk*. And the brute fact is that most of these qualities cannot be deduced from the simple properties of the elements involved in the chemical formula. The rules that are now used for predicting the qualities of a substance from its chemical formula are spotty. Moreover, most of them have been established by purely empirical procedures and, hence, are less likely to carry much weight beyond the cases actually observed. From the viewpoint of extant knowledge, therefore, almost every new compound is a *novelty* in some respect or other. That is why the more chemical compounds chemistry has synthesized, the more baffling has become the irregularity of the relation between chemical structure and qualitative properties. If this historical trend teaches us anything at all, it is that nothing entitles us to expect that this increasing irregularity will be replaced by some simple principles in the future.

Let us suppose that we have taken the trouble to sift all *known* propositions of chemistry into an α-class and a β-class. But new chemical compounds are discovered by the hundreds almost every day.[2] From the preceding paragraph it follows that with every such discovery, even a minor one, the α-class has to be increased by new propositions, at times more numerous than those to be added to the β-class. Known compounds being as numerous as they are, we do not actually have to determine today's α-class of chemistry in order to ascertain that it contains an immense number of propositions, perhaps greater than that of the β-class. It is thus obvious why no one has attempted to construct a logical foundation for chemistry. As I argued in the first chapter of this book, the *raison d'être* of a theoretical edifice is the economy of thought it yields. If novelty is an immanent feature of a phenomenal domain—as is the case in chemistry—a theoretical edifice, even if feasible at all, is uneconomical: to build one would be absurd.

It is not necessary to see in novelty more than a relative aspect of knowledge. In this sense, the concept is free from any metaphysical overtones. However, its epistemological import extends from chemical compounds to all forms of Matter: colloids, crystals, cells, and ultimately biological and social organisms. Novelty becomes even more striking as we proceed along this scale. Certainly, all the qualitative attributes which together form the entity called elephant, for example, are novel with respect to the properties of the chemical elements of which an elephant's body is composed. Also, what a particular society does is not entirely deducible from the biological properties of each one of its members. We can explain, for instance, why every primitive settlement has been founded near a body of fresh water: the biological man needs water. But we cannot explain in the same manner why human societies have felt the need for belief in some God, for pomp, for justice, and so on. Combination *per se*—as an increasing number of natural scientists admit—contributes something that is not deducible from the properties of the individual components.[3] The obvious conclusion is that the properties of simple elements, whether atomic or intra-atomic, do not describe Matter exhaustively. The complete description of Matter includes not only the property of the atom of, say, carbon, but also those of all organizations of which carbon is a constituent part.

From what I have said so far about novelty it follows that not all phenomena can be discovered by the tip of a pencil doing some algebra or logistic calculus on paper as is the case in mechanics. Those that can

[2] Linus Pauling, in "The Significance of Chemistry," *Frontiers in Science: A Survey*, ed. E. Hutchings, Jr. (New York, 1958), p. 280, estimated "that about 100,000 new chemical facts are being discovered each year, at present." And that was more than ten years ago!

[3] P. W. Bridgman, *The Nature of Physical Theory* (Princeton, 1936), p. 96; L. von Bertalanffy, *Problems of Life* (New York, 1952), chap. ii.

be so deduced constitute our β-class and may be referred to as *rational phenomena of the first order.* Others cannot be known unless they are actually observed first. These, of course, do not violate any established law any more than a certain square being yellow violates the laws of geometry. Their rationality, however, is of a different kind than that of deducible phenomena; we may refer to it as *rationality of the second order.* Obviously, they belong to the α-class. But as we move up from the physico-chemical to the organic and, especially, the superorganic domains, novelty acquires a new dimension which raises an unsuspected issue regarding the division of all propositions into an α- and a β-class.

For a chemist the behavior of a newly obtained compound may display many novelties. Yet, once this chemical compound has been synthesized, the next time the chemist prepares it he will no longer be confronted with another novelty by combination: matter, at the physico-chemical level, is uniform. More often than not, this permanence is absent from the organic and superorganic domain. For a glaring yet simple example: in some human societies the bride is bought, in others she brings a dowry into the new family, and in still others there is no matrimonial transaction of any sort. Yet the elements of the next lower level, the biological humans that form by combination each of these societies, are the same. From the same basis, therefore, a multiplicity of novel forms may spring up. And for a most striking example from biology, where the same phenomenon also abounds, one need only take a look at the wide spectrum of horn shapes encountered among antelopes.[4] The famous French paleontologist Georges Cuvier thought and taught that one can predict, say, the shape of an animal's teeth from that of its feet. Nowadays we know that the Cuvier correlation law is full of important exceptions. This second aspect of novelty is of far greater import than the first: it prevents us from predicting the outcome even after the same combination has been actually observed once, or twice, or even several times. I propose to refer to phenomena of this category as *rational of the third order.* Indeed, by the very fact that they are actual they cannot violate any law of elementary matter (or of organic matter, if they are superorganic); hence, there is no reason whatsoever for taxing them as irrational.[5] But let us not fail to note that this peculiarity separates by a broad line the sciences of life-bearing structures from those of inert matter.

The upshot of the last remarks is that to say that Matter has infinitely many properties may not represent the whole truth and, hence, may be misleading. The whole truth is that Matter has infinitely many *potentiae*

[4] See the illustration in G. G. Simpson, *The Meaning of Evolution* (New Haven, 1949), p. 166.

[5] More on this in Chapters VII and XI, below.

which all are as real as the properties of elementary matter. How could we otherwise account for the fact that exactly the same batch of chemical elements constituting some inert compounds may also find themselves assembled as in a living cell which displays a property than no inert compound has—life? In my opinion, Bohr's observation[6] that the Principle of Complementarity is most clearly illustrated by how a cell behaves *in vivo* and its matter behaves *in vitro* does not quite hit the mark. A photon behaves at times as if it were a wave, at others as if it were a particle; yet no physicist—to my knowledge—has claimed that the particle (or the wave) is no longer in existence when the electron manifests itself as a wave (or a particle). Now, if we cannot complete the chemical analysis of a living cell—i.e., discover the complete behavior of its body as elementary matter—without ultimately bringing about the death of the cell, one question cries for an answer: where did life go? Myself, I cannot think of any scientific way of answering it other than to say that life is a permanent *potentia* of Matter and that, as a *potentia*, it is present also in the precipitate seen in the test tube. Yes, Mind, consciousness, the feeling of being alive, are all such *potentiae*.

Physicists and chemists may not like to hear about *potentiae*. Yet most of them will go along with Eddington in recognizing that Mind "is the first and most direct thing in our experience; all else is remote inference."[7] Some philosophers, however, condemn this position as a "hoary fallacy." As we have already seen, the Oxford philosopher G. Ryle[8] argues that Mind and like terms should be abolished because no one has seen a mind not associated with matter whereas everyone can see matter without mind. And he is not the only one in this exaggeration of logical positivism, that everything is just matter. By this token we should cease in the first place to speak of radiation energy which also is never seen alone, by itself. It is hard to imagine what philosophers want to achieve by this untenable position. All we can be certain of is that it succeeds in diverting the attention of many students of the organic and the superorganic from the most important problems of and the most adequate methods to these domains. When the chips are down, however, even those who share Ryle's position go along, I am sure, with M. H. Pirenne's repartee[9]—that they, too, have a mind and a consciousness, and feel alive.

Scholars working in domains where the second aspect of novelty by combination is totally absent are apt to extrapolate this attractive

[6] Niels Bohr, *Atomic Physics and Human Knowledge* (New York, 1958), pp. 7–9; also Louis de Broglie, *Physics and Microphysics* (London, 1955), p. 139.

[7] A. S. Eddington, *New Pathways in Science* (Ann Arbor, 1959), pp. 5, 322.

[8] Cited in Chapter III, note 90, above.

[9] M. H. Pirenne, "Descartes and the Body-Mind Problem in Physiology," *British Journal for the Philosophy of Science*, I (1950), 45.

condition beyond their own spheres of activity and, on this basis, dispense—as some have done—high cathedra advice to their colleagues across the border. With a smile in his pen, Henri Poincaré wrote that social sciences have "the most methods and the fewest results."[10] That social sciences should have more methods than the others is a logical necessity: they need also methods suited to the study of rational phenomena of the third order. From this viewpoint, we should deplore rather than applaud the current tendency of social sciences to rely less on these *sui generis* methods and more on those imported from physico-chemistry or, worse, from mechanics. For Poincaré, as well as for any student of physico-chemical sciences, it may be perfectly natural to count as "results" only the propositions that express a logical or factual necessity. Because, again, of the overwhelming frequency of rational phenomena of the third order, such propositions are rather the exception in social sciences—and everything we know suggests that nobody can do anything about it. Actually, from what has been going on in these sciences for quite some time now, it would be more appropriate to say that they have too many, not too few, results of the kind appreciated by the student of inert matter. Indeed, an increasing number of social scientists, whom Jonathan Swift would have described as "dextrous enough upon a Piece of Paper," produce one "positive result" after another, day after day. But only a few of these results have any connection with actual phenomena.

The simple form of novelty by combination suffices to explain why even chemistry or the engineering disciplines concerned with the qualitative properties of matter in bulk cannot be theoretical extensions of physics. A more telling way to put it is to say that there is no chemico-physical parallelism. Much less, therefore, can there be a bio-, a psycho-, or a socio-parallelism, for in all these domains novelty by combination appears also with its second, more baffling, dimension. Those physicists, especially physical chemists, who are fully aware of the limits to what can be derived from the properties of inert matter, are not likely to frown at this conclusion. Actually, some great names in physics and chemistry have denounced the bio- and the psycho-physical parallelism in most categorical terms.[11] By contrast, many, perhaps most, students of life phenomena would nowadays meet the conclusion with strong protests. Only now and then do we find a biologist such as Max Delbrück—who may be credited with starting the ball of molecular biology rolling—continuously defending

[10] Henri Poincaré, *The Foundations of Science* (Lancaster, Pa., 1946), p. 365.

[11] E.g., C. N. Hinshelwood, *The Structure of Physical Chemistry* (Oxford, 1951), pp. 456, 471; Max Born, *Physics in My Generation* (London, 1956), p. 52; Niels Bohr, *Atomic Physics*, p. 78 and *passim*.

the point that just as some features of the atom "are not reducible to mechanics, we may find features of the living cell . . . not reducible to atomic physics."[12] And very recently, commenting upon a reduction of a paradox in molecular biology, Delbrück pinpointedly observed that the reduction was based on a novel law "that nobody could have pulled out of quantum mechanics without first having seen it in operation."[13] According to the present "academic" orientation of molecular biology, Delbrück's position (once held also by others who now deny having done so) represents the "romantic" view. Yet the same academic school of thought lives not by quantum mechanics alone but also by a series of new postulates that cannot be classified as β-propositions of that science. And in the end this school, too, has to admit that "there exist processes which, though they clearly obey [do not violate] the laws of physics, can *never* be explained [by physics]."[14] But if the sentence is completed as it should be and as I have done, the position does not differ by an iota from what I have contended in this section.

Almost every epistemological position—history attests it—has produced its grain of worth to knowledge. The belief in the existence of a theoretical passage between biology and quantum mechanics is no exception—as the momentous discoveries pertaining to the so-called genetic code amply prove. The price (there must always be a price) is the neglect of other numerous phenomena displaying those peculiar properties of life that have caused a consummate physiologist (and a Nobel laureate) such as Charles Sherrington to marvel at and to write about with inimitable insight.[15]

The veil over a small yet important corner of this last domain was raised when H. Driesch proved experimentally that embryos in early stages after having been on purpose drastically mutilated develop nevertheless into normal individuals. Now, the chips from a broken cup will not try to reassemble themselves into the unbroken cup, nor would a single chip grow by itself into a new cup. True, Driesch overstated his case and as a result he left himself open to the repeated accusation of having seen in his discovery proof of the existence of a vital force, an actual entelechy.

[12] Max Delbrück, "A Physicist Looks at Biology," *Transactions of the Connecticut Academy of Arts and Sciences*, XXXVIII (1949), 188. [At the time when I wrote these admiring words about Delbrück's standpoint, he was not yet a Nobel laureate.]

[13] Quoted by G. S. Stent, "That Was the Molecular Biology That Was," *Science*, April 26, 1968, p. 395, note 11.

[14] Stent, p. 395. Other examples can be found at will. Simpson (*Meaning of Evolution*, pp. 124 ff), after explicitly admitting that "it is merely silly to maintain that there is no essential difference between life and nonlife," strikes at vitalism on the ground that all differences are inherent in life's "organization only." If by this he means that life cannot be explained *only* by the properties of inert matter, he merely turns around verbally the neovitalist thesis.

[15] Charles Sherrington, *Man on His Nature* (New York, 1941).

But this does not make his discovery less relevant. In fact, he translated his results into the principle of *equifinality*.[16] This principle, rechristened as the principle of restitution and regulation of living organisms,[17] has later been completed with various other concepts connected more directly with the phenomena observed at the submicroscopic level. Recently, Paul A. Weiss, who has obtained along the same line even more startling results than Driesch, renewed the warning against the hope that the synthesizing of a cell would "turn out to be not just a logical construct but a demonstrable physical reality."[18] But the scientific temper of our age is no exception to the past: it is an intellectual terror without regard for the due process of law. So, most biologists seek to avoid the shameful stamp of "vitalist" by some verbal swindle of the sort we have seen above.

The earlier considerations of this section entail the expectation that the effect of the "academic" position should be far more obvious in social science than in biology. And so it is. The problems in which the romantic economists—Marx at his best, the historical school, or the institutionalists—were interested as well as their methods of research are practically forgotten, often treated with high scorn. Novelty by combination under its twofold aspect is no longer bothering the academic economist. For him, only what can be erected into theory counts in economic life. As he sees it, everything is reducible to locomotion systems and man, as an agent in the economic process, is guided by no other principles of self-assertion.

2. Novelty and Uncertainty. There are several object lessons that students of life phenomena could derive from the emergence of novelty by combination. The most important one for the social scientist bears upon those doctrines of human society that may be termed "chemical" because they overtly recognize chemistry as their source of inspiration and model. Since the problem raised by these doctrines is of crucial importance for the orientation of all social sciences, especially economics, it deserves to be discussed in detail. This will be done in a special section later on. But at this juncture, I propose to point out one object lesson which pertains to the difference between risk and uncertainty.

Since an exhaustive description of Matter implies the experimenting

[16] H. Driesch, *The Science and Philosophy of the Organism* (2 vols., London, 1908), I, 59–65, 159–163.

[17] Bertalanffy, *Problems of Life*, pp. 59, 142 f, 188 f.

[18] Paul A. Weiss, "The Compounding of Complex Macromolecular and Cellular Units into Tissue Fabrics," *Proceedings of the National Academy of Sciences USA*, XLII (1956), 819. Among other experiments confirming the principle of equifinality, Weiss reports that skin cells from chick embryos "thrown together at random had managed, even outside the organism, to synthesize a feather—a higher-order unit—by harmonious collective action, as if in concert" (p. 827). See also Appendix G in this volume.

with, and study of, an essentially limitless set of material combinations (or organizations), it goes without saying that the fate of human knowledge is to be always incomplete. From the analysis in the preceding section, the meaning of "incomplete" should be perfectly clear. However, in the controversies over the difference between risk and uncertainty, *incomplete* knowledge has often been confused with what may be termed *imperfect* knowledge. The point is that—in the terminology here adopted—*incomplete* refers to knowledge as a whole, but *imperfect* refers to a particular piece of the extant knowledge. Some illustrations may help clarify the difference. Our knowledge is incomplete because, for instance, we have absolutely no idea what sort of biological species will evolve from *homo sapiens*, or even whether one will evolve at all. On the other hand, we know that the next birth (if normal) will be either a boy or a girl. Only we cannot know far in advance which it will be, because our knowledge concerning the sex of future individuals is imperfect, the main cause of this imperfection being the intrinsic randomness involved in the determination of the sex. Knowledge of pertinent laws—say, the correlation of an infant's sex with the mother's age, with the sex of his elder siblings, etc.—would enable us only to guess correctly more often, not to reach perfect knowledge.[19]

Risk describes the situations where the exact outcome is not known but the outcome does not represent a novelty. Uncertainty applies to cases where the reason why we cannot predict the outcome is that the same event has never been observed in the past and, hence, it may involve a novelty.

Since I have insisted elsewhere[20] upon the necessity of this distinction probably more strongly than the authors who first broached the issue, further comments at this place may seem unnecessary. However, it may be instructive to illustrate by a topical problem the connection between novelty arising from new combinations and the nature of uncertainty.

Notation being the same as in Chapter II, Section 7 above, let E_1, E_2, . . . , E_n be the evidences of the n members of a committee *before* they meet on a given occasion. Let us also assume that the committee is not a pseudo committee. This rules out, among other things, the existence of a "best mind" (in all relevant respects) among members as well as their complete identity. In these circumstances, during the discussion preceding the vote, part of the evidence initially possessed by one member but not by another will combine with the initial evidence of the latter. In the end, everybody's evidence is increased and, hence, everybody will have a new

[19] Cf. Chapter II, Section 7, above.
[20] "The Nature of Expectation and Uncertainty" (1958), reprinted in AE.

expectation, $\mathscr{E}_k(E'_k)$. The new combination should normally produce some novelty: the decision adopted may be such that no one, whether a member or a poll-taker, could think of it prior to the meeting.[21]

The point bears upon Shackle's original idea of analyzing expectations in terms of the degree of surprise caused by their realization instead of the degree of belief in their outcome.[22] In one respect the idea has a definite merit. While the occurrence of any event for which there is an *ex ante* degree of belief will cause a degree of surprise (the greater, the smaller is the degree of belief), for a truly novel event there is an *ex post* surprise but no *ex ante* belief in it. Thus, by saying that everybody was surprised at the announcement by President Johnson not to seek or accept the 1968 presidential nomination we do not mean that the *ex ante* belief in his move had been extremely small: we simply mean that nobody else had thought of it.

The novelty by combination, of which I have spoken in the preceding section, is not necessarily a novel event in this sense. The chemist, for instance, expects a new chemical compound to have a definite hardness, solubility, elasticity, and so on; he only is unable, most of the time, to predict every one of these numerous qualities exactly. Yet significant discoveries are usually novel events as well. We can be pretty sure that there was a series of surprises following the synthesis of nylon and absolutely sure that every "first" discoverer of gunpowder was shockingly surprised. Similarly, all of us would be surprised at the forms of government in existence around A.D. 5000, if someone could reveal them to us now.

3. Hysteresis and History. The unparalleled success of physics is generally attributed to the sole fact that physics studies only matter and matter is uniform. It would be more appropriate to say that physics studies only those properties of Matter that are uniform, that is, independent of novelty by combination and of Time as well—conditions that are closely related. Were physics to study all possible novelties by combination of elementary matter, it would include not only chemistry but also biology, psychology, sociology—in a word, everything. It would then study properties that are not independent of Time. True, physics

[21] The fact that most, perhaps all, behavioristic models completely ignore this particular group effect is self-explanatory: a predicting model must keep novelty off the field. But it is highly surprising to find the point ignored by analyses of another sort. A salient example: N. Kaldor, in "The Equilibrium of the Firm," *Economic Journal*, XLIV (1934), 69n1, states that "the supply of coordinating ability could probably be enlarged by dismissing the Board and leaving the single most efficient individual in control."

[22] G. L. S. Shackle, *Expectation in Economics* (Cambridge, Eng., 1949); *Uncertainty in Economics and Other Reflections* (Cambridge, Eng., 1955). For a discussion of Shackle's ideas see my paper cited in note 20, above.

has not been able to keep Time entirely off premises: thermodynamics and astrophysics are concerned with changes of matter in Time. Astrophysicists speculate that the present form of matter sprang up by combination from a previous form—ylem—so entirely different that no one has any idea of what it looked like.

A frequent definition of the uniformity of matter is that the behavior of matter is determined only by its *present conditions*. The definition, however, does not quite suffice. We must add, first, that this behavior does not depend upon when "present" is located in Time, and second, that the "present conditions" are to be interpreted in the strict sense: those that can be established "now" without any reference to *past history*.[23] Only these explicit conditions mirror the fact that at the elementary level an atom of hydrogen behaves always in the same manner regardless of whether an instant before it was reacting with some other chemical element. To put it differently, a combination of temporal states yields no novelty. Undoubtedly, matter often behaves in this manner. For what would the world be like if drops of water or grains of salt behaved differently according to their individual histories? And if in addition matter remained indestructible—as it is believed to be—then a physical science would be quite impossible.

Yet, in some cases even physical behavior does depend upon past history as well. The most familiar case is the behavior of a magnet, or to use the technical term, the magnetic hysteresis. But hysteresis is not confined to magnetism: structural deformation and the behavior of many colloids too depend upon past history. According to a recent idea of David Bohm, shared also by Louis de Broglie, the Heisenberg indeterminacy may be the result of the fact that the past history of the elementary particle is not taken into account in predicting its behavior.[24] The case where all past causes work cumulatively in the present, therefore, is not confined to life phenomena.

There is, however, one important difference between physical hysteresis and the historical factor in biology or social sciences. A physicist can always find as many bits of nonmagnetized iron—i.e., *magnets without history*—as he needs for proving experimentally that magnets with an identical history behave identically. It is vitally important to observe

[23] In technical jargon this idea is expressed by saying that the behavior of matter is described by a system of differential equations in which time does not enter explicitly. Hence, if $x = f(t)$ is a solution of that system, so is $x = f(t - t_0)$ for any t_0. Cf. R. B. Lindsay and H. Margenau, *Foundations of Physics* (New York, 1936), p. 522.

[24] Cf. Heisenberg, *Physics and Philosophy*, pp. 130–131. See also the Foreword by Broglie and chapter iv in David Bohm, *Causality and Chance in Modern Physics* (London, 1957).

that if we were unable to experiment with cases where *the level of history is zero*, we could not arrive at a complete law of magnetic hysteresis. But in the macro-biological and social world getting at the zero level of history seems utterly impossible. That is why in these two domains the historical factor invites endless controversy. We may recall in this connection the elementary remark of C. S. Peirce that universes are not as common as peanuts. Because there is only one Western civilization, the question of whether its historical development merely follows a trajectory determined entirely by the initial condition or whether it represents a hysteretic process can be settled neither by an effective experiment nor by the analysis of observed data. Unfortunately, the answer to this sort of question has an incalculable bearing upon our policy recommendations, especially upon those with a long-run target—such as the policies of economic development.

Physicists not only can determine the law that relates present behavior of a magnet to its history, but also can make history vanish by demagnetization. In other words, for any given history, $\mathscr{H}$, there is an $\mathscr{H}'$ such that $\mathscr{H} + \mathscr{H}' = 0$; moreover, $\mathscr{H}'$ is a very short history. Forth and back, back and forth, it is as easy as that in physics and, often, in chemistry, too. Across the border, hysteresis raises problems of ungraspable dimensions into whose answers we have been unable even to peek yet. As Delbrück observed, any living cell is "more an historical than a physical event [for it] carries with it the experiences of a billion years of experimentation by its ancestors. You cannot expect to explain so wise an old bird in a few simple words."[25] Is it only this astronomically long time that bars us from reproducing the hysteresis process by which the present behavior of a cell has been attained? No, far more important is our ignorance of how to make the first step.

According to the currently accepted view, the first living cells arose *by chance* from a warm sea of inert matter having the same properties as matter has today.[26] Nature may have had no other means for opening the combination safe than by trying and trying and trying at random. What prevents us then, now that we know the successful combination, from opening the safe by a few twists of the hand? For, to quote Francis Bacon, "if nature be once seized in her variations, and the cause be manifest, it will be easy to lead her by art to such deviation as she was at

[25] Delbrück, "A Physicist Looks at Biology," p. 177.

[26] This idea was first propounded systematically by A. I. Oparin, *The Origin of Life* (New York, 1938). For a brief survey of subsequent elaborations, see J. D. Bernal, *The Physical Basis of Life* (London, 1951). For the arguments advanced, by Charles E. Guye in particular, against the idea of life emerging by chance, see P. Lecomte du Noüy, *The Road to Reason* (New York, 1948), pp. 123–128. Also my Appendix F in this volume.

first led to by chance."[27] Is it because some *potentiae* of Matter unknown to us were actualized as some pre-life forms only in that primeval sea of warm mud? If the answer is yes, the accepted explanation is no explanation. Or is it because when we say "by chance" we mean that there was a factor at work that cannot be reduced to a list of arithmomorphic instructions to be followed by him who would wish to assemble the quantum parts into the living set?

We can only feel but not grasp entirely the issues raised by the reproducibility of the hysteresis that shaped the behavior of more complex organisms, of man himself or of his various societies. Is it possible, for instance, to erase even a very small part of man's recent hysteresis, as the physicist can do for a magnet, so as to train him to behave in some direction chosen by us? In point of fact, can the socialist man be created so as not to show any hysteresis trace of his bourgeois or peasant past? Perplexing though this and other similar issues raised by human hysteresis seem to be, no search for a complete description of social phenomena can avoid them. Actually, the stronger our intention of applying knowledge to concrete practical problems—like those found in economic development, to take a topical example—the more urgent it is for us to come to grips with these issues.[28]

The difficulties of all sorts which arise can be illustrated, though only in part, by the simple, perhaps the simplest, instance of the hysteresis of the individual consumer. The fact that the individual's continuous adjustment to changing price and income conditions changes his tastes seems so obvious that in the past economists mentioned it only in passing, if at all. Actually, there is absolutely no stand upon which this phenomenon could be questioned. In 1950 I attempted a sketchy formalization of the problem mainly for bringing to light the nasty type of questions that besiege the Pareto-Fisher approach to consumer's behavior as soon as we think of the hysteresis effect.[29] By means of a simple analytical example I showed that in order to determine the equilibrium of the consumer (for a fixed budget and constant prices) we need to know much more than his particular hysteresis law. Still worse, this law being expressed by a very complex *set function*, we can only write it on paper but not determine it in actual practice. Set functions cannot be extrapolated in any useful way. Consequently, however large the number of observations, the effect of the last experiment can be known only after we observe what we wish to predict. The dilemma is obvious. How nasty

[27] Francis Bacon, *Novum Organum*, Bk. II, Sec. 29.

[28] More on this in Chapter XI, Section 4, below.

[29] "The Theory of Choice and the Constancy of Economic Laws" (1950), reprinted in AE.

this dilemma may be is shown by the case where the order of observations too matters. In this case, even if it were possible to make the consumer experiment with all possible situations we would not be able to know the general law of the hysteresis effect. All the more salient, therefore, are the contributions of James Duesenberry and Franco Modigliani on the hysteresis effect upon the saving ratio.

But the most unpleasant aspect of the problem is revealed in the ordinary fact that behavior suffers a qualitative shock, as it were, every time the individual is confronted with a *novel commodity*.[30] This is why we would be utterly mistaken to believe that technological innovations modify supply alone. The impact of a technological innovation upon the economic process consists of both an industrial rearrangement and a consumers' reorientation, often also of a structural change in society.

4. Physics and Evolution. The analysis of the two preceding sections leads to a few additional thoughts. The first is that history, of an individual or of a society, seems to be the result of two factors: a hysteresis process and the emergence of novelty. Whether novelty is an entirely independent element or only a consequence of the hysteresis process is perhaps the greatest of all moot questions, even though at the level of the individual it is partly tractable. Unquestionably, the invention of the carbon telephone transmitter was a novelty for all contemporaries of Edison. But what about Edison himself? Was his idea a novelty for him too or was it a result, partly or totally, of his own hysteresis process?

Be this as it may, we cannot avoid the admission that novel events, beginning with the novelty of chemical transformations, punctuate the history of the world. The several philosophical views which speak of "the creative advance of nature"[31] are not therefore as utterly metaphysical or, worse, as mystical, as many want us to believe. However, we need more than the existence of novelty to support the notion of a nature advancing from one creative act to another. Novelty, as I have tried to stress, need not represent more than a relative aspect of our knowledge; it may emerge for us without nature's advancing on a path marked by novel milestones. The masterpieces in a picture gallery are not being painted just as we walk from one to the next. On the other hand, geology, biology, and anthropology all display a wealth of evidence indicating that at least on this planet there has always been evolution: at one time the earth was a ball of fire which gradually cooled down; dinosaurs emerged,

[30] *Ibid.*

[31] The most outstanding representatives of this philosophy are Henri Bergson (*Creative Evolution*, New York, 1913, pp. 104 f and *passim*) and Alfred North Whitehead (*An Enquiry Concerning the Principles of Natural Knowledge*, 2nd edn., Cambridge, Eng., 1925, pp. 63, 98; *The Concept of Nature*, Cambridge, Eng., 1930, p. 178; *Process and Reality: An Essay in Cosmology*, New York, 1929, p. 31).

vanished and, undoubtedly, will never appear again; man moved from cave dwellings into penthouses. Impressive though all this evidence is, all efforts of biologists and social scientists to discover an evolutionary law for their phenomenal domains have remained utterly fruitless. But, perhaps, we should clarify this statement by defining an evolutionary law.

An evolutionary law is a proposition that describes an ordinal attribute E of a given system (or entity) and also states that if $E_1 < E_2$ then the observation of E_2 is later in Time than E_1, and conversely.[32] That is, the attribute E is an *evolutionary index* of the system in point. Still more important is the fact that the ordinal measure of any such E can tell even an "objective" mind—i.e., one deprived of the anthropomorphic faculty of sensing Time—the direction in which Time flows. Or, to use the eloquent term introduced by Eddington, we can say that E constitutes a "time's arrow."[33] Clearly, E is not what we would normally call a cause, or the unique cause, of the evolutionary change. Therefore, contrary to the opinion of some biologists, we do not need to discover a single cause for evolution in order to arrive at an evolutionary law.[34] And in fact, almost every proposal of an evolutionary law for the biological or the social world has been concerned with a time's arrow, not with a single cause.

Of all the time's arrows suggested thus far for the biological world, "complexity of organization" and "degree of control over the environment" seem to enjoy the greatest popularity.[35] One does not have to be a biologist, however, to see that neither proposal is satisfactory: the suggested attributes are not ordinally measurable. We may also mention the interesting but, again, highly questionable idea of R. R. Marett that increasing charity in the broad sense of the word would constitute the time's arrow for human society.[36]

It is physics again that supplies the only clear example of an evolutionary law: the Second Law of Thermodynamics, called also the Entropy Law. But the law has been, and still is, surrounded by numerous con-

[32] I should explain that $E_1 < E_2$ means that E_2 follows E_1 in the ordinal pattern of E.

[33] A. S. Eddington, *The Nature of the Physical World* (New York, 1943), pp. 68 f.

[34] Julian Huxley, *Evolution: The Modern Synthesis* (New York, 1942), p. 45.

[35] For a comprehensive (but not wholly unbiased) discussion of these criteria see Huxley, *ibid.*, chap. x; also Theodosius Dobzhansky, in *Evolution, Genetics, and Man* (New York, 1955), pp. 370 ff, who argues that all sensible criteria of evolution must bear out the superiority of man. One should, however, appreciate the objection of J. B. S. Haldane, in *The Causes of Evolution* (New York, 1932), p. 153, that man wants thus "to pat himself on the back."

[36] R. R. Marett, *Head, Heart, and Hands in Human Evolution* (New York, 1935), p. 40 and *passim*, and the same author's "Charity and the Struggle for Existence," *Journal of the Royal Anthropological Institute*, LXIX (1939), 137–149. Also Haldane, in *Causes of Evolution*, p. 131, argued that altruistic behavior may represent a Darwinian advantage.

troversies—which is not at all surprising. A brief analysis of entropy and a review of only its most important issues cannot avoid some technicalities. It is nevertheless worth doing, for it uncovers the unusual sort of epistemological difficulty that confronts an evolutionary law even in the most favorable circumstances, those of the qualityless world of elementary matter. Although these difficulties were felt only in the later period and only serially, they are responsible for the agitated history of thermodynamics.

Thermodynamics sprang from a memoir of Sadi Carnot in 1824 on the efficiency of steam engines.[37] One result of this memoir was that physics was compelled to recognize as scientific an elementary fact known for ages: heat always moves by itself from hotter to colder bodies. And since the laws of mechanics cannot account for a unidirectional movement, a new branch of physics using nonmechanical explanations had to be created. Subsequent discoveries showed that all known forms of energy too move in a unique direction, from a higher to a lower level. By 1865, R. Clausius was able to give to the first two laws of thermodynamics their classic formulation:

The energy of the universe remains constant;
The entropy of the universe at all times moves toward a maximum.

The story is rather simple if we ignore the small print. According to Classical thermodynamics, energy consists of two qualities: (1) *free* or *available* and (2) *bound* or *latent*. Free energy is that energy which can be transformed into mechanical work. (Initially, free heat was defined roughly as that heat by which a hotter body exceeds the colder one, and which alone could move, say, a steam engine.) Like heat, free energy always dissipates by itself (and without any loss) into latent energy. The material universe, therefore, continuously undergoes a qualitative change, actually a qualitative degradation of energy. The final outcome is a state where all energy is latent, the Heat Death as it was called in the earliest thermodynamic theory.

For some technical reasons, which need not interest us, entropy was defined by the formula[38]

(1) Entropy = (Bound Energy)/(Absolute Temperature).

The theoretically consecrated formula, however, is

$$\Delta S = \Delta Q/T, \tag{1a}$$

[37] A full translation appears in *The Second Law of Thermodynamics*, ed. and tr. W. F. Magie (New York, 1899).

[38] For which see J. Clerk Maxwell, *Theory of Heat* (10th edn., London, 1921), pp. 189 ff, and Max Planck, *Theory of Heat* (London, 1932), pp. 81 f.

where ΔS is the entropy increment, ΔQ the increment of the heat transferred from the hotter to the colder body, and T the absolute temperature at which the transfer is made. An important point, which practically never accompanies this formula, is that the increments are determined in the direction of Time, i.e., from the *earlier* to the *later* moment in Time. With this addition the Entropy Law needs no further explanation. We should notice, however, that it is strictly an evolutionary law with a clearly defined time's arrow: entropy. Clausius seems to have thought of it in the same way, for he coined "entropy" from a Greek word equivalent in meaning to "evolution."

5. *Time: The Great Mystery.* A short word though it is, Time denotes a notion of extreme complexity. This was already obvious from our earlier discussion of the texture of Time. As we have seen in Section 4 of Chapter III, Time cannot be reconstructed from the arithmetical continuum of its instants. But this feature alone does not differentiate Time from Space. The texture of Time consists not of abutting but of overlapping durations (or specious presents, as they are often called). Moreover, they overlap in a dialectical, not arithmomorphic, structure. The peculiarly unique feature of Time is its fleeting nature combined with its ever-presentness. Time flows; yet it is always present. That is why the problem of Time has tormented the minds of all great philosophers far more than its correlative, Space. To most of us Time does seem "so much more mysterious than space,"[39] and no one has yet proved that we are mistaken. To be sure, there are some who maintain that Einstein's relativity theory has proved that they are in fact one.[40] This sort of argument ignores, however, that Einstein's four-dimensional manifold is "a purely formal matter." A paper-and-pencil operation cannot possibly abolish the qualitative difference between the elements involved in it.[41] It is elementary that no observer can make proper records if he does not distinguish between time and space coordinates. The four-dimensional manifold is a fallacious, albeit serviceable, schema which pushes to the extreme the geometrization of Time that, in the words of Broglie, "conceals from us one part of the real flux of things."[42]

The fleeting nature of Time has induced many a great philosopher to

[39] Eddington, *Nature of Physical World*, p. 51.

[40] E.g., H. Margenau, *The Nature of Physical Reality* (New York, 1950), pp. 149 ff.

[41] Cf. P. W. Bridgman, *The Logic of Modern Physics* (New York, 1928), p. 74; *Reflections of a Physicist* (2nd edn., New York, 1955), p. 254. In another place, *The Intelligent Individual and Society* (New York, 1938), p. 28, Bridgman even denounces the thesis of the fusion as "bunkum."

[42] Louis de Broglie, *Physique et microphysique* (Paris, 1947), p. 196 (my translation). Also E. A. Milne, *Relativity, Gravitation and World-Structure* (Oxford, 1935), pp. 18 f.

think that Time is pure illusion. Undoubtedly because of its arithmomorphic framework, the most famous argument against the reality of Time is that of John McTaggart.[43] He begins by pointing out that "there could be no Time if nothing changed"—a proposition hard to deny—and sets out to prove that Change involves a logical contradiction. "Positions in Time," McTaggart notes, belong to a *B*-series in which they are ordered by the asymmetrical and transitive dyadic relation "earlier than." Clearly, this series cannot reflect change. If X is earlier than Y, it remains so eternally.[44] Or, to put it in logistic terms, the proposition "X is earlier than Y" has an atemporal *truth value*, i.e., it is either true or false in the same sense in which "3 is a prime number" or "5 is an even number" are.

In fact, the *B*-series is the geometrical image of eternity, entirely analogous to the trajectory drawn in space by a moving object. On this trajectory, once drawn, there are, as Bergson put it, only "immobilities."[45] Like Bergson's, McTaggart's point that the events represented by positions in the *B*-series are immobilities and, hence, cannot provide a basis for Change is beyond question. The content of an event—say, the death of Napoleon—is absolutely immutable. This is true, we should note, also for future events after they are revealed; so the objection that at the time when this is written no one knows whether "the unification of Europe into one state" represents an event is unavailing.

One should also go along with McTaggart's point that the only basis for Change we can find *in events as such* is the fact that any event that is now Present, was Future, and will be Past. With respect to these attributes, events form another series, the *A*-series, which is formed by events "as observed by us." I see no reason for not accepting also his next contention that the *A*-series is fundamental whereas the *B*-series is a secondary derivation of it. Our minds order events in eternity on the basis of the *A*-series. We have no other *direct* way for knowing which of two events is

[43] John M. E. McTaggart, *The Nature of Existence* (2 vols., Cambridge, Eng., 1927), II, chaps. xxxiii and li. McTaggart's philosophical system is, perhaps, even more complex and intricate than Hegel's. The reader interested in pursuing McTaggart's argument on Time may find more tractable the critical elaboration of C. D. Broad in *Examination of McTaggart's Philosophy* (2 vols., Cambridge, Eng., 1933–1938), II, part I.

[44] A peculiar, and certainly wrong, argument of McTaggart, in *Nature of Existence*, II, 241–246, implying that any ordinal variable has a weak cardinal measure (in the sense of Chapter IV, Section 2, above), justifies the stronger form used by Broad, *Examination*, II, 290, 298: "If X *ever* precedes Y by a certain [algebraic] amount, then it *always* precedes Y by precisely that amount." There is, however, no need to assume a quantum between X and Y; an ordinal set instead would suffice for the stronger form.

[45] Henri Bergson, *Matter and Memory* (London, 1913), pp. 246 ff.

earlier. Even a geologist, for example, in explaining that a certain formation is earlier than another, implicitly says that when the first was Present, the second was Future (or, alternatively, when the second was Present, the first was Past).

From this basis, McTaggart argues that the A-series is spurious. On the one hand, Past, Present, and Future must "belong to each event" because nothing about events can change; on the other hand, they are "incompatible determinations."[46] To be sure, we may counter that, in contrast with "3 is a prime number," "X is present" is true at a certain *moment* and false at all others.[47] Again, I can see nothing wrong in McTaggart's logic that this answer only hides the contradiction: we now have a second A-series—that of *moments*—as contradictory as that of events. And if we try to circumvent this new contradiction by the same procedure, we are drawn into an infinite regress. On this basis—that the A-series is delusive and that without it we cannot account for Change—McTaggart concludes that Time itself is not real. According to him, the source of our illusion of Time is an atemporal C-series related to our prehension and ordered by the relation "inclusion," a relation that we mistake for "earlier than."[48]

McTaggart's thesis has been variously criticized. Like all controversies about the nature of Time, the one around this thesis is only instructive, not decisive. Thus, it has brought to light the essential difference between the ordinary copula "is" and the temporal copula "is now," or between Absolute Becoming as in "this event became present" and Qualitative Becoming as in "this water became hot."[49] It has also added evidence in support of St. Augustine's thesis that you cannot discuss Time without referring to Time.

Above all, it has strengthened the view, to which even one eminent physicist after another was ultimately led, that there is no other basis for Time than "the primitive form of the stream of consciousness."[50] In fact, temporal laws in any science require a distinction between earlier and later,

[46] McTaggart, *Nature of Existence*, II, 20.

[47] As we have already seen (Chapter III, Section 5), Bertrand Russell uses this temporal truth to reduce Change to an arithmomorphic structure.

[48] McTaggart, II, 271.

[49] Broad, *Examination*, II, 272, 280 f. Broad is right in saying that Qualitative Becoming implies Absolute Becoming. But he does not seem right in saying that Absolute Becoming (which is reflected by the A-series) suffices to account for Time. What could be absolutely becoming in a self-identical universe? In my own judgment, the basic error in McTaggart's argument is that it considers Change solely in relation to events, whereas only Objects are susceptible of Change. A becoming Being makes sense; a becoming Becoming does not.

[50] H. Weyl, *Space, Time, Matter* (New York, 1950), p. 5; Milne (cited in note 42, above), p. 14; Arthur H. Compton, "Time and the Growth of Physics," in *Time and Its Mysteries*, ed. H. Shapley (New York, 1962), p. 166.

which only consciousness can make.[51] The Entropy Law is an excellent example in point. In formal terms, the law reads: let $E(T_1)$ and $E(T_2)$ be the entropies of the universe at two different moments in Time, T_1 and T_2, respectively; if $E(T_1) < E(T_2)$, then T_2 is later in Time than T_1—and conversely. But, clearly, if we did not know already what "later" means, the statement would be as vacuous as "the gander is the male of the goose and the goose the female of the gander." The full meaning of the law is that the entropy of the universe increases *as Time flows through the observer's consciousness*. Time derives from the stream of consciousness, not from the change in entropy; nor, for that matter, from the movement of a clock. In other words, the relationship between Time and any "hour-glass" is exactly the reverse of what we generally tend to think. If we know that Napoleon's death occurred later than Caesar's assassination it is only because the two events have been encompassed by the historical consciousness of humanity formed by the splicing of the consciousness of successive generations.[52] By going one step further and extrapolating in thought such a communal consciousness, we arrive at the notion of Eternity, without beginning and without end. This is the basis of Time.

All this does not mean that all mysteries of Time are thus solved. One may go along with McTaggart in arguing that if this morning's breakfast is "earlier than" today's lunch it is because one's prehensions at breakfast are included in those at lunch. On this admission, if all the prehensions of one self at each moment would be written on a separate card and all these cards shuffled, another self would readily rearrange them in the "right" order. Also, how the Time scales of two selves communicating with each other are spliced consistently in both directions presents no great difficulty. But McTaggart's thesis does not explain why in the prehensions of each self, separately, seeing the lightning is "earlier than" hearing the thunder, or low entropy is "earlier than" high entropy. In this connection, it is highly interesting to mention the interpretation of entropy irreversibility suggested by an authority on thermodynamics. According to D. ter Haar, the basis of the irreversibility is "psychological," for it derives from the fact that "we have a memory of the past and we can thus possess knowledge of what happened at an earlier time, but not of what will happen at a later moment."[53] The Entropy Law, too, would then be one facet of the inclusion property of our prehension, and, hence, could not provide an objective root in nature for our sense of Time.

[51] On this point too we have not been able to go beyond Aristotle's teachings: *Physics*, 219ª 22 ff, 223ª 25 ff.

[52] Cf. the remarks of Bridgman, *Reflections*, pp. 320 f, concerning the necessary continuity in observing a physical phenomenon.

[53] D. ter Haar, "Foundations of Statistical Mechanics," *Reviews of Modern Physics*, XXVII (1955), 292.

Such a thought is, however, repelling to many who believe in an external world independent of whether there be an observer. They feel that it is going too far to make the most important coordinate of knowledge and existence depend on human consciousness, nay, on the uniformity of consciousness for all selves. There must be, they say, at least one feature of nature that parallels, if it does not also guide, the consciousness of every self in sensing the direction of Time. Eddington, as we have already seen, argues that the objective direction of Time, the time's arrow, is indicated by the unavoidable increase of entropy—a position opposite to that of McTaggart, Haar, and the modern theory of thermodynamics. Others, who represent the realist school, maintain that neither Time nor the world are subjective. The temporal direction as felt by any individual consciousness corresponds to the fact that "in the [objective] world [there] is, on the one side, causal concatenation, on the other, unidirectional, temporal succession."[54] But this idea has even less chances of winning the admission of those large circles of philosophers for whom causality itself is a construction of our minds without any root in reality. A main argument in support of this thesis is that the reversibility of the *dynamic* time (which shall occupy us next) disposes immediately of the notion of temporal causality.

6. Time and "Time." The word "time" is frequently used with many meanings, some of which seem quite surprising. For example, the statement that "the time and the means for achieving [human] ends are limited," suggests that the term is used to represent not an endless flow but a scarce stock.[55] Economics abounds in similar loose uses of "time." A more stringent illustration is the use of "summation over time" to describe the operation by which the average age of a given population is computed. Surprising though it may seem, the terminological license originated in physics, where both a moment in Time and the interval between two such moments continued to be denoted, loosely, by the same term even after the distinction between the two meanings became imperative. The story of how this necessity was revealed is highly instructive.

The apparently innocuous admission that the statement "heat always moves by itself from hotter to colder bodies" is a physical law triggered one of the greatest crises in physics—which moreover is not completely

[54] L. Susan Stebbing, "Some Ambiguities in Discussions Concerning Time," in *Philosophy and History*, ed. R. Klibansky and H. J. Paton (New York, 1963), p. 122.

[55] Lionel Robbins, *An Essay on the Nature and Significance of Economic Science* (2nd edn., London, 1948), p. 12. The argument that "there are only twenty-four hours in the day" (*ibid.*, p. 15) increases the reader's difficulty in understanding Robbins' position. Would the fact that there are one million microns in one meter make space (land) plentiful?

resolved. The crisis grew out from the fact that mechanics cannot account for the unidirectional movement of heat, for according to mechanics all movements must be reversible. The earth, for instance, could have very well moved in the opposite direction on its orbit without contradicting any mechanical laws.[56] It is obvious that this peculiarity of mechanical phenomena corresponds to the fact that the equations of mechanics are invariant with respect to the sign of the variable t, standing for "time." That is, the same system of equations "predicts" indifferently the position the earth will occupy one hundred years from now or the one it had one hundred years ago. It is on this basis that positivist philosophy proclaims that temporal causality is a bogus. Some have carried the point so far as to argue that it is equally right (but equally meaningless) to say that the fall of Rome caused the discovery of America as to say that the latter was the cause of the former.[57] Naturally, the crucial point, namely, whether *historical* events are related to each other by a system of equations invariant with respect to the sign of time, is completely ignored. The position has led to the idea that in reality there are two Times: a reversible Time in which mechanical phenomena take place, and an irreversible Time related to thermodynamic phenomena. Obviously, the duality of Time is nonsense. Time moves only forward, and all phenomena take place in the same unique Time.[58]

Behind the idea of the duality of Time there is the confusion between the concepts I have denoted by T and t, a confusion induced by the practice of using the same term, "time," for both. In fact, T represents Time, conceived as the stream of consciousness or, if you wish, as a continuous succession of "moments," but t represents the measure of an interval (T', T'') by a *mechanical clock*. Or to relate this description to our discussion of measurability (Section 2 of Chapter IV), T is an ordinal variable, but t is a cardinal one. The fact that a weak cardinal scale can be constructed for T on the basis of $t = \text{Meas}\ (T', T'')$, does not mean that it is not necessary to distinguish between t and T, even though we must reject the duality of Time.

It is the essential difference between the temporal laws which are functions of T and those which are functions of t that calls for a distinction

[56] It is instructive to point out that, long before the crisis emerged in physics, G. W. F. Hegel, in *The Phenomenology of Mind* (2nd edn., New York, 1931), pp. 204 f, observed that the same scientific explanation would work for the inverted world.

[57] See, for instance, W. F. G. Swann, "What Is Time?" in *Time and Its Mysteries*, pp. 135 f.

[58] Cf. Bridgman, *Logic of Modern Physics*, p. 79. Perhaps I ought to explain also that the impossibility of two observers to synchronize their clocks does not prove the multiplicity of Time. As anyone can verify it, this impossibility cannot be explained without referring events in both systems to a common Time-basis.

between the two concepts. If we happen to watch a movie showing marshy jungles full of dinosaurs, we know that the event the movie intends to depict took place earlier than the founding of Rome, for instance. The reason invoked in this case is that the law governing such events—assuming that there is one—is, like the Entropy Law, a function of T. On the other hand, a movie of a purely mechanical phenomenon is of no help in placing the event in Time. For a pendulum moves and a stone falls in the same way irrespective of when the event occurs in Time. Mechanical laws are functions of t alone and, hence, are invariable with respect to Time. In other words, *mechanical phenomena are Timeless, but not timeless.*

Because only in thermodynamics, of all branches of physics, are laws functions of T, there was no strong compulsion for physics to eliminate the ambiguous use of "time." But it is hard to understand why other sciences, where the situation is not the same as in physics, have on the whole ignored the problem. All the greater is Schumpeter's merit for stressing, in his later writings, the difference between *historical* and *dynamic* time, by which he understood T and t respectively.[59] However, the root of the distinction does not lie in historical (evolutionary) sciences but—as we have seen—in the heart of physics, between mechanics and thermodynamics.

7. *Temporal Prediction and Clock-Time.* Ever since ancient astronomers succeeded in forecasting eclipses our admiration for the precision with which physics—the term being understood in the narrow sense, excluding thermodynamics—can predict future events, has steadily increased. Yet the reasons why only physics possesses this power are still obscure. The usual explanation that the future is determined exclusively by the initial (present) conditions, and that of all sciences physics alone has succeeded in ascertaining these conditions through measurements, raises more questions than it answers. In any case, it draws us into the muddled controversy over strict determinism, for which we are not ready yet.[60]

The immediate reason why the temporal laws of physics are predictive is the fact that they are all functions of t, i.e., of the measure of Time-interval by a *mechanical clock.* What such a law tells us in essence is this: You set your mechanical clock to "zero" at the exact moment when you drop a coin from the top of the tower of Pisa; the tip of the clock hand will reach the mark t_0 at exactly the same moment as the coin

[59] Joseph A. Schumpeter, *Essays*, ed. R. V. Clemence (Cambridge, Mass., 1951), p. 308, and, especially, Schumpeter's *History of Economic Analysis* (New York, 1954), p. 965n5.

[60] See Chapter VII, below.

reaches the ground. As this example shows, any temporal law of pure physics is nothing but the enunciation of a temporal parallelism between two mechanical phenomena, one of which is a mechanical clock. From this it follows that all mechanical phenomena, including that of a clock, are parallel in their ensemble. In principle therefore we could choose any such phenomenon to serve as the common basis for the enunciation of the parallelism. In part we have done so.

The point I wish to emphasize is that physical prediction is a symmetrical relation: we can very well say that the "falling body" predicts the "clock," or for that matter any other mechanical phenomenon. Why then do we prefer a clock mechanism over all other mechanical phenomena as the standard reference?

From the few physicists who cared to analyze the problem of "clock," we learn that the choice is determined by the condition that the concrete mechanism must be free, as much as possible, from the influence of factors that are not purely physical. This means that the clock must be almost Timeless, or in other words almost impervious to the march of entropy. As Eddington pointedly observed, the better the "clock" is, the less it shows the passage of Time.[61] That is why Einstein regarded the vibrating atom as the most adequate clock mechanism for physics.[62]

We can perfectly understand that if pure physics is to be a closed system, it needs a purely mechanical clock. But this internal necessity does not explain also why we associate the flow of Time with the movement of stars, of the sand in an hour-glass, or of a pendulum—all mechanical clocks. This association precedes by millennia the modern thoughts on clock. On the other hand, physics offers no proof that the clock hour just elapsed is equal to the one just beginning.[63] Time intervals cannot be superimposed so as to find out *directly* whether they are equal. Nevertheless, we have a strong feeling that they are, that Time flows at a constant rate hour by hour—as Newton taught. Perhaps the reason why we feel that the clock shows how fast Time flows is that suggested by Karl Pearson: in every clock hour there is packed "the same amount of consciousness."[64] The suggestion, however, could be accepted, if at all, only for two consecutive infinitesimal intervals. There is some evidence that the hours seem shorter as we grow older because—as has been

[61] Eddington, *Nature of Physical World*, p. 99.

[62] For various remarks on the problem of "clock," see Bridgman, *Nature of Physical Theory*, p. 73; Erwin Schrödinger, *What Is Life?* (Cambridge, Eng., 1944), pp. 84 ff; Weyl, *Space, Time, Matter*, pp. 7 f.

[63] Karl Pearson, *The Grammar of Science* (Everyman's Library edn., London, 1937), pp. 161 f; Henri Poincaré, *The Foundations of Science*, pp. 224 f.

[64] Pearson, *Grammar of Science*, p. 159. The point recalls McTaggart's position that differences of prehensions are cardinally measurable (note 44, above).

suggested—the content of our consciousness increases at a decreasing rate. On the basis of the evidence available at present this is perhaps all we can say concerning the admiration scientists and laymen alike have for the prediction of future events by clock-time.

But, time and again, a legitimate admiration has turned into biased evaluation. Thus, at times, we can detect a deprecating intent in the statement that thermodynamics has no predictive power. The bare fact is that the Entropy Law tells us only that in, say, one clock-hour from now the entropy of the universe will be greater, but not by how much.[65] This imperfection may very well be the consequence of the fact that we insist on referring to the clock-time. In phenomenal domains where (as in thermodynamics) all temporal laws are functions of T alone, the simple regularity without which no law can be strictly operational in prediction may not exist if the corresponding phenomena are correlated with the clock-time. But I see no reason why we should believe that in such domains there can be no prediction of exactly the same nature as that by the clock-time of physics.

Indeed, let us suppose that we knew a Fourth Law of thermodynamics—which conceivably may be discovered any day. Let this law express the fact that some new variable of state, say, I, is a function of T. In this case, we could take either this new law or the Entropy Law as a "thermodynamic clock," and formulate the remaining law in exactly the same predictive form as we have cast earlier the law of falling bodies: When the thermodynamic clock will show I_0, the entropy of the system will simultaneously reach the level E_0. This example shows that, unless Pearson's suggested explanation of the constant rate of Time flow is substantiated, there can be no difference between prediction by clock-time and prediction by any time's arrow. (And even if it could be proved that Pearson's idea has a real basis, the superiority of prediction by clock-time would have only a purely anthropomorphic justification.) If some have nevertheless

[65] Cf. W. J. Moore, *Physical Chemistry* (2nd edn., Englewood Cliffs, N.J., 1955), p. 23; Margenau, *Nature of Physical Reality*, pp. 210 f; Philipp Frank, "Foundations of Physics," *International Encyclopedia of Unified Science* (Chicago, 1955), I, part 2, 449. It may be contended that the First Law is nevertheless predictive by clock-time; however, the constancy of total energy represents a rather vacuous case of clock-time law. Perhaps I ought to explain also that the Third Law, ordinarily called Nernst's Law, in essence states that the zero of absolute temperature can never be attained. But "never," too, is a temporal notion that does not need a clock. Incidentally, the obvious implication of Nernst's Law is that energy, like Time, is a weak cardinal variable and, hence, what we call energy in the universe may be a bottomless ocean of which we can observe effectively only the waves on its surface. I think that the same idea is implied by Bridgman's observation that energy and entropy can be measured only if the same situation can be attained again (*Reflections of a Physicist*, p. 331). See, however, Chapter VI, note 8, below.

arrived at the contrary opinion, that thermodynamics has no predictive value, it is no doubt because there the issue is obscured by another factor: in thermodynamics there is *only one* truly temporal law, the Entropy Law. But a single law, clearly, is useless for prediction: no law can be its own "clock." The difficulty is of the same nature as that inherent to any implicit definition.

Furthermore, there is absolutely no reason why in every domain of inquiry phenomena should be parallel to that of a mechanical clock. Only the dogma that all phenomena are at bottom mechanical could provide such a reason. But, as I have repeatedly emphasized, the mechanistic dogma has been abandoned even by physical sciences. We should therefore regard as a sign of maturity the reorientation of any science away from the belief that all temporal laws must be functions of clock-time. Wherever it has taken place, the reorientation paid unexpected dividends. For instance, many biological phenomena which appeared highly irregular as long as they were projected against a clock-time scale have been found to obey very simple rules when compared with some biological phenomenon serving as a "clock."[66]

Hoping that this book will achieve one of its main objectives, namely, that of proving that the economic process as a whole is not a mechanical phenomenon,[67] I may observe at this juncture that the abandonment of Clément Juglar's formula for business cycles was a step in the right direction. Indeed, that formula implies the existence of a strict parallelism between business activity and a mechanical clock—the movement of sun spots. On the other hand the Harvard Economic Barometer, unhappy though its final fate was, reflects a more sound approach to the same problem. For in the ultimate analysis any similar type of barometer affirms a parallel relationship between some economic phenomena, one of which serves as a "clock"—an economic clock, that is. Most subsequent studies of business cycles have in fact adopted the same viewpoint. The palpable results may not be sufficiently impressive; hence doubts concerning the existence of an *invariant parallelism* between the various aspects of economic activity are not out of place. However, the alternative idea that the march of the entire economic process can be described by a system of differential equations with clock-time as the independent variable—an idea underlying many macro-dynamic models—is in all probability vitiated *ab ovo*.

It is perfectly understandable that we should feel inclined to think of prediction only by the clock-time: we adjust our everyday business by

[66] Cf. P. Lecomte du Noüy, *Biological Time* (New York, 1937), pp. 156 ff.

[67] And if on this point I am fighting a straw man, it is all the better for my other theses that depend upon the validity of the point.

the clock because all clocks, being mechanical systems, *predict each other*. However, the evidence of phenomena that are not slave to the mechanical clock is so crushing that we must conclude that the laws of mechanics do not determine every mode of being of nature. Within what is thus left undetermined in nature, laws of a different essence may be at work without contradicting each other and, hence, without each one being able by itself to remove the whole indeterminacy. That this thought is not a mere flight of fancy is plainly shown by the Entropy Law, which so far is the only law we definitely know not to be tied to the mechanical time. We should not therefore be surprised to see that the connection between the Entropy Law and the sciences of life phenomena (where attempts at a mechanistic explanation have constantly met with lack of success) is growing stronger every day. But there are also other more substantial reasons for this connection.

CHAPTER VI *Entropy, Order, and Probability*

1. Entropy: Order and Disorder. In order to proceed systematically I have considered thus far only those lessons that a social scientist may learn from Classical thermodynamics. But the story has a very important epilogue.

It was quite difficult not only for physicists but also for other men of science to reconcile themselves to the blow inflicted on the supremacy of mechanics by the science of heat. Because the only way man can act upon matter directly is by pushing or pulling, we cannot easily conceive any agent in the physical universe that would have a different power. As Lord Kelvin, especially, emphasized, the human mind can comprehend a phenomenon clearly only if it can represent that phenomenon by a *mechanical model.* No wonder then that ever since thermodynamics appeared on the scene, physicists bent their efforts to reduce heat phenomena to locomotion. The result is a new thermodynamics, better known by the name of statistical mechanics.

First of all, we should understand that in this new discipline the thermodynamic laws have been preserved in exactly the same form in which Clausius had cast them. Only the meaning of the basic concepts and the explanation of thermodynamic equilibrium have been radically changed. If technical refinements are ignored, the new rationale is relatively simple: heat consists of the *irregular* motion of particles, and thermodynamic equilibrium is the result of a *shuffling* process (of particles and their velocities) which goes on by itself. But I must emphasize one initial difficulty which still constitutes the stumbling block of statistical mechanics. The spontaneous shuffling has never been appropriately defined. Analogies such as the shuffling of playing cards or the beating of an egg have been used in an attempt at explaining the meaning of the term. In a more striking analogy, the process has been likened to the utter devastation of a library by an *unruly* mob.[1] Nothing is *destroyed* (The First Law of Thermodynamics), but everything is scattered to the four winds.

According to the new interpretation, therefore, the degradation of the universe is even more extensive than that envisaged by Classical thermodynamics: it covers not only energy but also material structures. As physicists put it in nontechnical terms,

In nature there is a constant tendency for order to turn into disorder.

Disorder, then, continuously increases: the universe thus tends toward Chaos, a far more forbidding picture than the Heat Death.

Within this theoretical framework, it is natural that entropy should have been redefined as a measure of the degree of disorder.[2] But as some philosophers and physicists alike have pointed out, disorder is a highly relative, if not wholly improper, concept: something is in disorder only in respect to some objective, nay, purpose.[3] A heap of books, for instance, may be in perfect order for the billing clerks but not for the cataloguing department of a library. The idea of disorder arises in our minds every time we find an order that does not fit the particular purpose we have at that moment. From the viewpoint advocated in this book, we associate the random order with disorder because it does not correspond to the analytical order we expect to find in nature. Nature is ordered only to the extent to which its modes of being can be grasped analytically, by our Understanding. All the less can we see how disorder can be ordinally measurable. Statistical mechanics circumvents the difficulty by means of two basic principles:

A. *The disorder of a microstate is ordinally measured by that of the corresponding macrostate.*

B. *The disorder of a macrostate is proportional to the number of the corresponding microstates.*[4]

[1] Erwin Schrödinger, *Science, Theory, and Man* (New York, 1957), pp. 43 f.

[2] For an authoritative discussion of this point, see P. W. Bridgman, *The Nature of Thermodynamics* (Cambridge, Mass., 1941), pp. 166 ff.

[3] Henri Bergson, *Creative Evolution* (New York, 1913), pp. 220 ff and *passim*; Bridgman, *Nature of Thermodynamics*, p. 173; Jacques Hadamard, review of J. Willard Gibbs, *Elementary Principles in Statistical Mechanics*, in the *Bulletin of the American Mathematical Society*, XII (1906), 207 f.

[4] Cf. H. Margenau, *The Nature of Physical Reality* (New York, 1950), pp. 279 ff.

A microstate is a state the description of which requires that each individual involved be named. "Mr. X in the parlor, Mr. and Mrs. Y in the living room," is an illustration of a microstate. The macrostate corresponds to a nameless description. Thus, the preceding illustration corresponds to the macrostate "One man in the parlor, one man and one woman in the living room." But it may equally well belong to the macrostate "One person in the parlor, two persons in the living room." This observation shows that the degree of disorder computed according to rule B—which is nothing but the old Laplacean rule—depends upon the manner in which microstates are grouped in macrostates. A second factor which affects the same measure is the criterion which determines whether or not a given microstate is to be counted. For example, in connection with the illustration used above it matters whether Emily Post rules that "Mrs. Y in the parlor, Mr. X and Mr. Y in the living room" is an unavailable microstate in a well-bred society.

Since statistical thermodynamics is concerned only with mechanical coordinates (position and momentum) of particles, all particles are treated as qualityless individuals distinguishable only by their names. The concept of macrostate, in which no particle names are used, corresponds to the obvious fact that the physical properties of an assembly of particles do not depend on which particular particle occupies a certain *state*, i.e., has a certain position and a certain momentum. Each "personal" arrangement of particles in a given macrostate constitutes a microstate. However, the criterion according to which two such arrangements constitute two *different* microstates is an additional convention which varies from one approach to another. And as hinted above, so does the criterion for what constitutes an acceptable macrostate.

In the earliest but still the basic approach, that of Ludwig Boltzmann, two arrangements constitute two different microstates if and only if the names of the particles in some state(s) are not the same. Furthermore, no restriction is imposed upon the number of particles having the same state. For example, let U, X, Y, Z be four particles and A, B, C, D, E the possible states. In Boltzmann's statistics, to the macrostate "two particles in A, two particles in B" there corresponds six microstates as shown in Table 1. According to the rules stated above, the ordinal measure of the disorder of any of these microstates as well as of the macrostate ($N_A = N_B = 2$, $N_C = N_D = N_E = 0$) is 6. In general, if there are m states and N particles, the measure of the disorder of the macrostate ($N_1, N_2, \ldots, N_m$), $\sum N_i = N$, is given by the familiar formula of combinatorial calculus

$$W = \frac{N!}{N_1!\,N_2!\cdots N_m!}. \tag{1}$$

TABLE 1

Microstates of $N_A = 2$, $N_B = 2$

Microstate number	Particles			
	U	X	Y	Z
1	A	A	B	B
2	A	B	A	B
3	A	B	B	A
4	B	A	A	B
5	B	A	B	A
6	B	B	A	A

For five states and four particles, the greatest disorder, $W = 4! = 24$, corresponds to the macrostate ($N_1 = N_2 = N_3 = N_4 = 1$, $N_5 = 0$) or its correlatives. The smallest disorder, $W = 1$, corresponds to the type ($N_1 = 4$, $N_2 = N_3 = N_4 = N_5 = 0$).

Boltzmann's epoch-making formula for entropy viewed as a measure of disorder is

$$\text{Entropy} = S = k \ln W, \tag{2}$$

where $\ln W$ is the natural logarithm of W and $k = 1.38 \times 10^{-16}$ ergs per degree of temperature is a *physical* constant known as Boltzmann's constant.[5]

A more general formula covers the case in which particles may occupy the various states with different "probabilities." To avoid bringing in the probability concept at this stage, I shall adopt a different but formally equivalent approach by assuming that each state is represented by several identical "rooms," so to speak. In the earlier illustration, we may assume, for instance, that the state A is represented by two "rooms," A_1, A_2. In this case, each microstate of Table 1 is replaced by four microstates as illustrated in Table 2.[6] It is easy to see that, in general, if the i-th state consists of s_i "rooms," formula (1) becomes

$$W = \frac{N!}{N_1!\, N_2! \cdots N_m!}\, s_1^{N_1} s_2^{N_2} \cdots s_m^{N_m}. \tag{3}$$

[5] In the ultimate count, W covers only those microstates in which the particles share exactly the total energy of the system. (See, for instance, Max Planck, *Theory of Heat*, London, 1932, pp. 239–242.) But this technical detail does not bear on the issue discussed in this section. We should retain it, however, for the discussion of the ergodic principle, a few pages below.

[6] It should be noted that treating the arrangements of Table 2 as distinct microstates is in line with the mentioned rationale of Boltzmann's statistics. For other statistics see note 11, below.

TABLE 2
Microstates Corresponding to Microstate 1 of Table 1

	Particles			
Microstate	U	X	Y	Z
1(1)	A_1	A_1	B	B
1(2)	A_1	A_2	B	B
1(3)	A_2	A_1	B	B
1(4)	A_2	A_2	B	B

For *large values* of N_i, with the aid of Stirling's asymptotic formula, (1) becomes

(4) $$\ln W = N \ln N - N - \sum N_i \ln N_i + \sum N_i = -\sum N_i \ln (N_i/N).$$

Putting $f_i = N_i/N$, we can write (2) as follows:

(5) $$S = -kNH,$$

where

(6) $$H = \sum f_i \ln f_i$$

is the famous H-function used by Boltzmann in his statistical approach to thermodynamics. Clearly, $-kH$ represents *the average entropy per particle*. For later reference, let us note that H and S vary in opposite directions.

In connection with the foregoing algebraic formalism several points deserve special emphasis. To begin with, granted that disorder is ordinally measurable and that rules A and B provide such a measure, any monotonically increasing function of W should do for defining disorder. However, the observable entropy is a *physical* coordinate, a variable of state, related to other *physical* coordinates according to the Classical formula mentioned in the previous chapter:

(7) $$\Delta S = \Delta Q/T.$$

Therefore, any acceptable definition of entropy based on order must lead to the same values as formula (7) in all cases. The question then is this: Does Boltzmann's formula (2) satisfy this particular condition? Curiously, crucial though this question is for the operational value of the new approach, it has received hardly any attention. Whenever a justification of Boltzmann's formula has been offered, it has been based on purely formal grounds, such as, for instance, the condition that the total entropy of two independent systems should be the sum of the individual entropies—in other words, that entropy should be a subsumptive variable.[7] The issue hidden behind this argument is related to the distinction between a weak cardinal and a cardinal entity introduced in Chapter IV, Section 2. Indeed, the only formula by which entropy is defined in relation to some directly observable variables of state is difference relation (7). All we can say according to this fundamental (and operational) formula is that entropy is a weak cardinal variable. We can, of course, choose any instance as the arbitrary origin for a weak cardinal scale; alternatively, we can attribute to any instance some particular value. In this light, Boltzmann was perfectly justified in proposing that the value of the entropy for a chaotic state (a state in thermodynamic equilibrium) be given by (2). But to go on to say that (2) measures also the entropy as defined by (7) for any other state is, from all we know, a gratuitous step.

As I have pointed out in the previous chapter (note 65), the Nernst Law further strengthens the view that entropy—regarded as a measurable physical coordinate—is a weak cardinal variable. For this law says that we cannot actually reach the "zero" level of entropy any more than we can attribute an origin to Time. The similarity does not stop here. Also, the sum of two entropies of the universe at two different moments makes no more sense than the sum of two chronological dates. This may be the purist's viewpoint—as Planck scorns it—but without it we would not notice the legerdemain by which he justifies the general validity of (2) and, by the same stroke, transforms entropy into a cardinal entity.[8] Indeed, we owe to Planck the idea of imposing, without any ado, the subsumption condition on entropy. We must therefore ask whether this paper-and-pencil operation is correlated with the bedrock of facts. No one has shown yet that it is. In fact, we can rest assured of the contrary. As Khinchin pointed out, all attempts to establish the equivalence of (2) and (7) for all cases rest on "an aggregate of logical and mathematical errors [and] a general confusion in the definition of the basic quantities."[9]

But even if we grant the equivalence for all cases, there remains an even more formidable problem: how can the new formulation of the Entropy Law be deduced from the laws of mechanics which, according to Boltzmann's point of departure, govern the motions of the particles?

[7] E.g., Planck, *Theory of Heat*, p. 227.

[8] For his criticism of the purist viewpoint and his claim concerning the "absolute" (cardinal) measure of entropy, see Max Planck, *The New Science* (New York, 1959), pp. 132–136, 142 f.

[9] A. I. Khinchin, *Mathematical Foundations of Statistical Mechanics* (New York, 1949), pp. 137–142.

New arguments piled up from many authoritative sources. Yet the logical issue, as we shall see presently, is as wide open as when it was first exploded.[10]

That a logical justification is more badly needed for Boltzmann's approach than for other open questions in physics is proved by the fact that, with time, it was discovered that his formula for W does not fit all conditions. Two new statistics, the Bose-Einstein and the Fermi-Dirac, had to be naturalized in order to fit new facts into (5).[11] This proves most eloquently that the double arbitrariness involved in rule B must in the end play havoc with any endeavor to establish microstates and macrostates by purely formal considerations.[12]

Though each problem discussed thus far uncovers some flaw in the foundation on which the measure of disorder rests, all are of an elementary simplicity. They can hardly justify, therefore, the occasional but surprising admission that the concept of statistical entropy "is not easily understood even by physicists."[13] As far as mere facts are concerned, we know that ever since its conception statistical entropy has been the object of serious criticism; it still is. Although the risks of a layman's expressing opinions are all the greater in a situation such as this, I wish to submit that the root of the difficulty lies in the step by which statistical

[10] The issue began its controversial history in the pages of *Philosophical Magazine* of the mid-1880's. The volumes of *Nature* for 1894 and 1895 also contain a long series of contributions to the controversy. A clear symptom of the lack of any progress is the fact that a very able survey of the controversy, published in German in 1912, is still so actual that it has recently been translated into English: Paul and Tatiana Ehrenfest, *The Conceptual Foundations of the Statistical Approach in Mechanics* (Ithaca, N.Y., 1959).

[11] Schrödinger, *Science, Theory, and Man*, pp. 212 ff. Also R. W. Gurney, *Introduction to Statistical Mechanics* (New York, 1949), pp. 1–6, 47–49. To use our elementary illustration, in the Bose-Einstein statistics the arrangements 1(2) and 1(3) in our Table 2 are not counted as distinct microstates. In the Fermi-Dirac statistics, no "room" can be occupied by more than one particle. Thus, if the state B is ignored for the occasion, only 1(2) and 1(3) are valid arrangements. For the same macrostate, W is greatest for Boltzmann's and smallest for Fermi-Dirac statistics. Let us also note that J. Willard Gibbs, *Elementary Principles in Statistical Mechanics* (New York, 1960), p. 183, proposes three different statistical analogues for entropy. The fact that all these definitions are asymptotically equivalent does not disprove the partial arbitrariness of the rules by which order-entropy can be defined.

[12] Cf. my criticism of Carnap's probability doctrine, in a paper reprinted in AE, "The Nature of Expectation and Uncertainty" (1958), Section IV. It is beyond doubt that formal considerations often are a source of fruitful inspiration. Their danger lies in our inclination to forget thereafter their insubstantiality. A topical example of this danger is the alleged identity between physical entropy and "the amount of information" as defined in computer science. See Appendix B in this volume.

[13] D. ter Haar, "The Quantum Nature of Matter and Radiation," in *Turning Points in Physics*, R. J. Blin-Stoyle, *et al.* (Amsterdam, 1959), p. 37. Also K. Mendelssohn, "Probability Enters Physics," in the same volume, pp. 49 f.

entropy is endowed with an additional meaning—other than a disorder index.

2. Entropy and Probability. In order to isolate the issues let us beg the question of whether disorder is ordinally measurable. It is then obvious that nothing could be wrong with choosing that index of disorder which is computed according to the principles A and B, *provided that the index thus obtained fits the facts described by the Entropy Law in its new formulation*, that is, provided the index of any isolated system increases with T. (For the discussion of this issue, we may also beg the question of whether the values of (2) and (7) necessarily coincide always.) The point I wish to emphasize now is elementary: from the fact that A and B serve as rules also for computing Laplacean probability *it does not follow that the index of disorder is a probability.*[14] For, as it should be clear by now, the degree of disorder may be defined by other appropriate rules besides A and B. For example, it may be defined by the formula[15]

$$S^* = -k \sum N_i^2/N^2. \tag{8}$$

Yet every version of statistical mechanics takes the position that the disorder index computed according to A and B represents at the same time *the physical probability of the corresponding macrostate to occur.* It is this step, by which entropy acquires a double meaning, that constitutes the most critical link in the logical framework of new thermodynamics.

It is ultra-elementary that if a system is governed by rigid laws—such as those of mechanics—it passes from one microstate to another in a succession that is completely determined by those laws. It may seem paradoxical therefore that the probabilistic interpretation has found its fiercest defenders among those who professed a boundless enthusiasm for the supremacy of mechanics as a law of nature. Actually, this interpretation originated with Boltzmann, who is also known for preaching that "the most superficial observation shows that the laws of mechanics are not limited to the inanimate nature only."[16] But the paradox vanishes if we realize that probability was introduced into thermodynamics precisely for saving the mechanistic representation of nature.

What has aided the strange marriage between mechanics, the paradigm of determinism, and probability, the expression of an uncontrollable factor, is the forbidding complexity of a system of numerous particles

[14] An earlier remark (Chapter IV, Section 6) illustrates the point just made. From the fact that the same propositions describe a geometrical as well as a social structure it does not follow that individuals and their associations *are* points and lines.

[15] Like S of formula (5), S^* is maximum for $N_i = N/m$, $i = 1, 2, \ldots, m$, and minimum for $N_1 = N$. Cf. Appendix B in this volume.

[16] L. Boltzmann, *Populäre Schriften* (Leipzig, 1905), p. 312. My translation.

moving according to mechanical laws. In such a complex structure one may easily find one straw after another on which he may hope, each time, to support a new theoretical edifice. The history of statistical mechanics is simple: as soon as one version was challenged or refuted, another on a still more complex level was offered instead. The successive contributions of Boltzmann illustrate this point amply as well as instructively.

In his first contribution to statistical mechanics—or the kinetic theory of gases, as he preferred to call it—Boltzmann polished an earlier result of Maxwell. But in contrast to his predecessor, Boltzmann also claimed for that result a validity beyond dispute. The proposition in point is as follows:

If a macrostate has a chaotic structure, it will preserve this structure forever; if it does not have a chaotic structure, it will necessarily tend to it.

Since the entropy is maximum for the chaotic state, the proposition is tantamount to the Entropy Law in its *strong* form, i.e., not as a probabilistic statement. Given that the H-function defined by (6) may be taken as a measure of the departure of a macrostate from the chaotic structure, Boltzmann formulated his result as the inequality $dH/dt \leq 0$ and named it "the H-theorem"—a term that was to become famous.[17] Boltzmann's claim—certainly, impressive—was that the theorem followed only from the Hamiltonian equations of motion supplemented by a *statistical* postulate concerning the occurrence of collisions between the particles of the system. The postulate says:

The proportion of the particles in any state that pass into any other state during any small interval, Δt, is the same for all initial and final states.

Clearly, this assumption can be made about any initial state without violating the laws of mechanics: these allow us to choose any initial conditions we may please. But if the algebra used in proving the H-theorem is to have any physical relevance, the postulate must be fulfilled also by the state reached at the end of Δt. On the other hand, the motions of the particles being rigidly determined by the laws of mechanics, we cannot take this condition for granted. And the rub is that unless the system is specially chosen the validity of the statistical postulate will not be passed on from one state to the next.[18] Should we accept the view that the postulate is fulfilled by all systems in actuality, the issue of why all

[17] L. Boltzmann, "Weitere Studien über Wärmegleichgewicht unter Gasmolekülen (H-Theorem)," *Sitzungberichte der K. Wiener Akademie*, LXVI (1872), 275–370, and his *Lectures on Gas Theory* (Berkeley, 1964), pp. 50–55.

[18] All the above points about the H-theorem may be illustrated by a model which, though relatively simple, must be relegated to the technical Appendix C in this volume.

initial states, going back to $t = -\infty$, should be special in this sense would still cry for an answer.

The first authoritative voice raised against the H-theorem was that of Loschmidt. He simply observed that if a given system evolves so that the H-function constantly decreases—as Boltzmann claimed—then by reversing the velocities of all particles in any of its subsequent states we obtain a system for which, according to the laws of mechanics, H increases. "It is obvious that, in a perfectly general way, in any system whatsoever, the entire development of events must follow the reversed order if the velocities of all elements are suddenly reversed."[19] The elementary impossibility of deriving a unidirectional law from the reversible laws of mechanics was thus called again in the debate. But when untenable positions are defended at any price, we must expect logic to be manhandled. Thus, we need not be surprised to see the basic point disregarded in the proof of the H-theorem immediately invoked against Loschmidt's objection by the defenders of that very theorem, including Boltzmann himself.[20] They all countered that since nothing guarantees that the reversed system will satisfy the collision postulate, Loschmidt's criticism is unavailing.

Yet this criticism in the end compelled Boltzmann to seek a new interpretation of his H-theorem. It was on this occasion that he laid down the three principles that have ever since passed as the basic articles of faith in statistical mechanics.[21] The first principle identifies "the magnitude customarily called entropy with the probability of the corresponding [macro]state." The second proclaims that all microstates are equally probable. The third goes on to say that the increase of entropy "can only mean that the probability of the state of the system of particles must constantly increase: the system can pass only from one state to a more probable one." The shift is crucial: the behavior of the H-function is no longer subject to the *strict* law $dH/dt \leq 0$ but to the general laws of probability interpreted in a special way by this third principle. Hardly any trace of Boltzmann's fervent enthusiasm for mechanics is present in his explicit admission that the H-theorem *and* the Entropy Law are "only theorems of probability [which] can never be proved mathematically by

[19] J. Loschmidt, "Über den Zustand des Wärmegleichgewichtes eines Systems von Körpern mit Rücksicht auf die Schwerkraft," *Sitzungberichte der K. Wiener Akademie*, LXXIII (1876), 139 (my translation); also, Ehrenfest, *Conceptual Foundations*, pp. 14 f.

[20] See, for instance, S. H. Burbury, "Boltzmann's Minimum Function," *Nature*, LI (1894), 78 and (1895), 320, and Boltzmann, *Lectures on Gas Theory*, pp. 58 f.

[21] L. Boltzmann, "Über die Beziehung zwischen dem zweiten Hauptzatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung respektive den Sätzen über das Wärmegleichgewicht," *Sitzungberichte der K. Wiener Akademie*, LXXVI (1877), 373–435. The quotations that follow are my translations.

means of the equations of dynamics alone."[22] And he thought that on this basis he could also justify the statistical postulate of his early proof of the H-theorem by claiming that this postulate means only "that the laws of probability are applicable for finding the number of collisions."[23] But as this new position, too, came under heavy fire, Boltzmann tried to prove with the aid of some analogies that the Entropy Law follows quite simply from the fact that the chaotic state is the most probable one and, implicitly, the H-curve consists mainly of "peaks."[24] These analogies, however, were neither quite to the point nor handled with sufficient accuracy.[25]

On a basis such as this Boltzmann claimed to have disposed of Loschmidt's objection.[26] He does admit that if for some system the H-function constantly decreases during the time interval (t_0, t_1), then H will increase exactly in the reversed order if all the velocities at t_1 are reversed. But he counters Loschmidt's objection by arguing that if "one reverses all the velocities at time t_0, he would by no means arrive at a motion for which H must increase; on the contrary, H would probably still decrease."[27] It is obvious, however, that for this to be true the H-curve must have a "peak" at t_0. But even if this is the case, the argument has no connection with Loschmidt's objection. Regardless of how frequent or infrequent are the "peaks" of the H-curve, taking the very case considered by Boltzmann, one may reverse all the velocities at any t' such that $t_0 < t' < t_1$ and obtain a motion for which H increases. This case, and no other, is pertinent to Loschmidt's objection.

On the very basis of the probabilistic approach we can see that, t being a given time interval, there are as many cases in which H decreases in an interval $(t^0, t^0 + t)$ as those in which H increases in a congruent interval $(t^1, t^1 + t)$. The H-curve is necessarily symmetrical in this special sense simply because the notion of physical probability based on mechanical considerations alone is entirely independent of the temporal direction.[28]

[22] L. Boltzmann, "On Certain Questions of the Theory of Gases," *Nature*, LI (1895), 414.

[23] L. Boltzmann, "On the Minimum Theorem in the Theory of Gases," *Nature*, LII (1895), 221. Of the same nature, and equally incomprehensible, is Borel's answer to Loschmidt's objection, namely, that the reversal of all velocities is "physically impossible." Émile Borel, *Mécanique statistique classique* (Paris, 1925), pp. 59 f.

[24] Boltzmann, "On Certain Questions," p. 415; also his "Über die sogenannte H-curve," *Mathematische Annalen*, L (1898), 325–332.

[25] For more details, see Appendix D in this volume.

[26] Many fully endorse this claim, e.g., Ehrenfest, *Conceptual Foundations*, pp. 32–38.

[27] Boltzmann, *Lectures on Gas Theory*, pp. 58 f. On this point see Appendix C in this volume.

[28] See my article, "The Nature of Expectation and Uncertainty" (1958), reprinted in AE, p. 251.

In other words, if "peaks" and "troughs" are excepted, there are as many moments at which the entropy of a system increases as those at which it decreases. This point, clearly, deprives Boltzmann's Entropy Law of almost any relevance. If the point has escaped notice, as it seems, it is probably because the discussion of the H-curve led to the impression that the curve must necessarily consist of immensely long waves. The same idea appears again in Boltzmann's answer to another objection raised later by E. Zermelo.[29]

Zermelo invoked a famous theorem of Poincaré which says that any isolated mechanical system in which the positions and the velocities of the particles remain within bounds will return to any previous state either exactly (hence, *periodically*) or approximately (hence, *quasi periodically*).[30] Consequently, argued Zermelo, the H-function of any system described by the canonical equations of dynamics must, sooner or later, return to any previous value, if not exactly, at least approximately.

In his reply, Boltzmann maintained that, far from refuting his H-theorem, Zermelo's point confirms it. As he put it, according to his theory "a closed system of a finite number of molecules, when it is initially in an ordered state and then goes over to a disordered state, finally after an *inconceivably long time* must again return to the ordered state."[31] It should be obvious, however, that the argument is specious: according to the probabilistic standpoint on which the H-theorem is proved, nothing prevents the disordered state from returning to the ordered state after a *surprisingly short time.* Time and again, Boltzmann shifted from one axiomatic basis to another according to the needs of the moment. Indeed, to show how long the return time from order back to order is, Boltzmann assumed that a mechanical system must pass through *all possible states consistent with its total energy* before returning to the same state.[32]

The assumption had already been used by Maxwell, who justified its plausibility on statistical grounds—as a consequence of the great number of the collisions of particles with the wall of the container.[33] The general opinion is that, similarly, Boltzmann regarded it as one facet of the main article of statistical faith, namely, that all microscopic states are equally

[29] E. Zermelo, "Über einen Satz der Dynamik und die mechanische Wärmetheorie," *Annalen der Physik und der Chemie,* LVII (1896), 485–494; also Ehrenfest, *Conceptual Foundations*, pp. 15 f. Apparently, Zermelo accepted the counter-arguments to Loschmidt's objection.

[30] Henri Poincaré, "Sur le problème des trois corps et les équations de la dynamique," *Acta Mathematica,* XIII (1890), 67–73 (especially).

[31] Boltzmann, *Lectures on Gas Theory,* p. 443. My italics.

[32] L. Boltzmann, "Entgegnung auf die wärmetheoretischen Betrachtungen des Hrn. E. Zermelo," *Annalen der Physik und der Chemie,* LVII (1896), p. 783 f.

[33] J. Clerk Maxwell, *Scientific Papers* (2 vols., Cambridge, Eng., 1890), II, 714 f.

probable. Most likely, he believed the property to be a consequence of the laws of mechanics: in his work, just cited, Boltzmann correlates it with Poincaré's theorem, a proposition of pure mechanics. These varying thoughts clearly reflect the imbroglio that, from the outset down to the present, has surrounded this vital assumption for the probabilistic interpretation of entropy.

3. The Ergodic Hypothesis and the Ergodic Theorem. The above-mentioned property, which Maxwell and Boltzmann attributed to any mechanical system and according to which any such system passes systematically through every state compatible with its total energy, has come to be known as the *ergodic* principle—a term coined by Boltzmann himself. "The ergodic hypothesis," however, seems a much safer label. For years, the hypothesis was defended and attacked with all sorts of arguments. Ultimately, it was discovered that, excepting some uninteresting cases in one-dimensional space, no mechanical system can satisfy it. With the modern theory of measure, this impossibility is now a commonplace. The basic idea can be explained intuitively by an elementary illustration that will prove instructive from other viewpoints.

Let us imagine a single particle moving like a perfectly elastic ball without friction on a horizontal, circular, and perfectly elastic billiard table. Let the initial position of the ball be at L_0 and the initial velocity vector be in the direction of L_0A_1 (Figs. 1 and 2). Because of the law of elastic reflection, the ball will move with uniform speed on a path $A_1A_2A_3 \cdots$ such that any arc A_nA_{n+1} is equal to that arc AA_1 which is not greater than π. Denoting this last arc by $r\pi$, $0 < r \leq 1$, we have two cases to distinguish. If r is a *rational* number, the ball will describe a closed regular polygon (Fig. 1). Hence, the system will return to any previous state (which includes position *and* direction of the velocity vector). The movement of the system is *periodical*. If r is an *irrational* number, the path is an open polygonal line; no A_n coincides with A (Fig. 2). The ball will never return to the same position on the circle (A). Inside the circle, it may pass again through the same point but not more than once for the simple reason that only two chords of equal length can pass through an interior point. But even if the ball returns to a previous position, its velocity vector does not have the same direction as at first. However, after a sufficiently long time, the system will return as close as we may wish to any previous state.[34] We say that such a system is *quasi periodical*. The two cases illustrate the gist of Poincaré's theorem mentioned in the preceding section.

[34] The propositions regarding the case of Fig. 2 are so intuitive that I felt no need for including their proofs here. The interested reader, I am sure, will have no difficulty in providing the simple demonstrations himself.

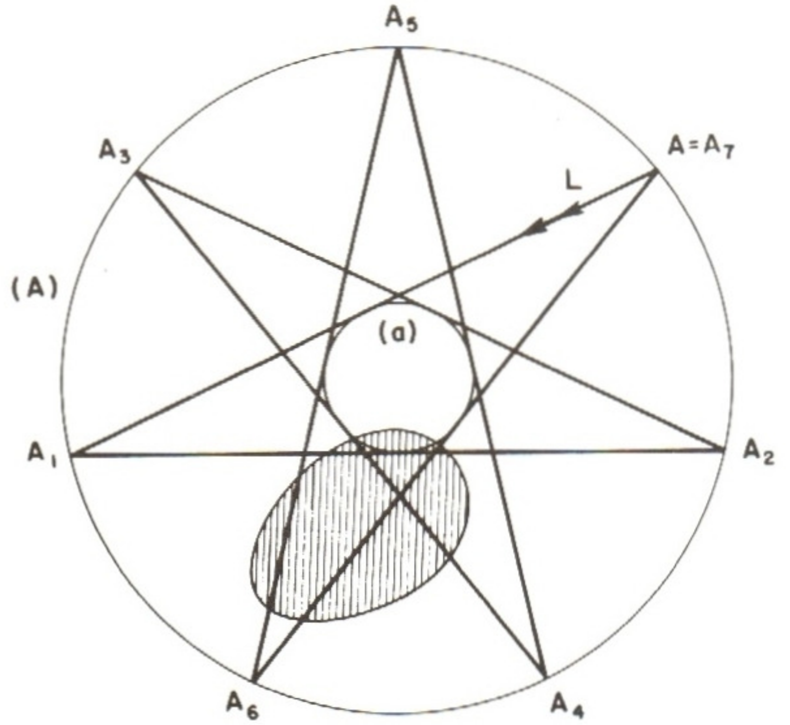

Fig. 1

Let us now observe that the mere knowledge of the *magnitude* of the initial velocity of the ball provides no information whatsoever about the movement of the system, i.e., about the path $A_1A_2A_3 \cdots$. *All such paths are, therefore, consistent with any magnitude of the ball's velocity, respectively with any amount of total energy of the system.* The upshot is that, for the ergodic hypothesis to be true, any *individual* movement must pass through every point of the domain (A), not only once but infinitely many times, each time from a different direction.

The case of Fig. 1 shows without any ado how absurd this hypothesis is. But perhaps one may reply—in the vein now current—that because the rational numbers r form a set of zero measure in the interval $(0, 1)$, we should pay no attention to this case. Fig. 2 shows, however, that even if r is irrational the system will not pass through any point B interior to the circle (a), although such a position is consistent with the total energy of the system. By fabricating arguments *ad hoc* for meeting any objection, one may retort that the ergodic hypothesis is after all fulfilled inside the ring between the circles (a) and (A). This thought, too, is unavailing. True, any individual path such as that of Fig. 2 passes as close as we may wish to any point C within the ring. But, as is easily seen from the geometry of the situation, the direction of the velocity at C cannot lie within the angles pCq or rCs.[35]

[35] Nothing need be added to see the fantastical exaggeration of the "magic" figure of $10^{10^{10}}$ years for the return time at which Boltzmann arrived by the ergodic hypothesis. See Section 3 of Appendix F in this volume.

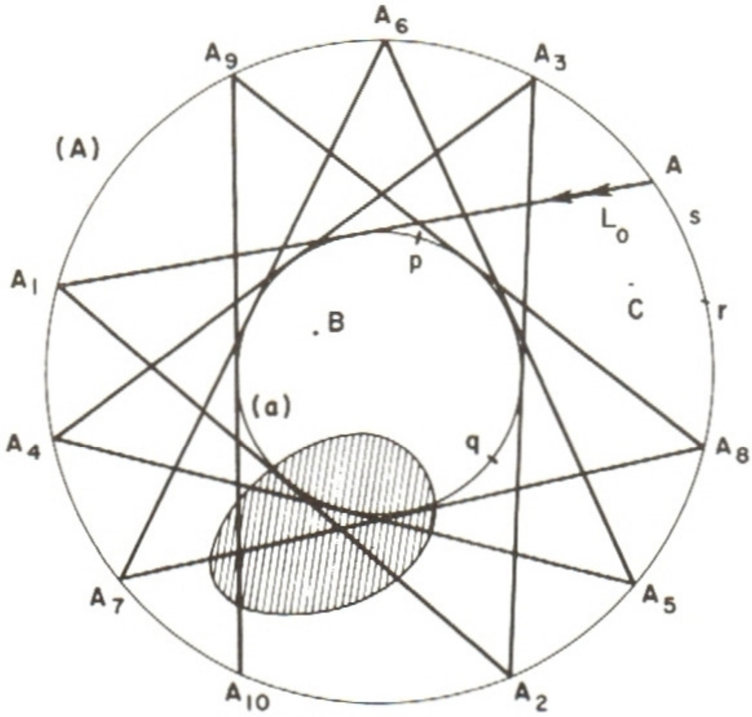

Fig. 2

The hopes shaken by the explosion of the ergodic hypothesis were revived by a new idea. This idea, to which it is proper to refer as the *quasi ergodic* hypothesis, is that a mechanical system left to itself will come as close as we may wish to any state consistent with its total energy. To be more specific, the phase-space in which all states are represented is divided into small and *equal* volumes to which we may refer as *gross* states. The hypothesis is that the system will pass through any such volume that contains a state consistent with the given total energy. The observations of the preceding paragraph show, however, that even this weaker hypothesis may not be always fulfilled. True, E. Fermi proved that, if a *general* dynamic system satisfies some relatively simple analytical conditions, the system is quasi ergodic. This time, too, the refrain that the systems that do not fulfill the conditions of the theorem should be ignored because they form a small category lifted the spirit of the trade to new heights where it has remained ever since. No wonder that an important element of Fermi's proof passed unnoticed. It is the fact that the proof implicitly excludes the *special* systems in which particles collide with each other or with the walls of the container (in which case the velocity vectors vary discontinuously). And, I am afraid, the same hidden assumption casts immense doubts on the alleged importance of many other theorems concerning statistical aspects of mechanical systems.[36]

[36] The conditions assumed by Fermi's theorem are spelled out in the résumé of the proof in D. ter Haar, "The Foundations of Statistical Mechanics," *Reviews of Modern Physics*, XXVII (1955), 328 f. For another theorem on the same issue see note 45, below.

The quasi ergodic hypothesis still did not provide an adequate base of operation for the probabilistic interpretation, for this hypothesis by itself says nothing about the relative frequencies with which the various states occur in the history of an individual system. The answer to this question came only in 1931 from a proposition proved by G. D. Birkhoff, now known as the quasi ergodic theorem or, simply, ergodic theorem.[37]

For an elementary yet trenchant illustration of this theorem let us assume that there are ten possible "states" in all, labeled 0, 1, 2, . . . , 9. Let the transformation be defined by

$$T = \begin{pmatrix} 0, 1, 2, 3, 4, 5, 6, 7, 8, 9 \\ 5, 8, 6, 1, 9, 3, 4, 0, 2, 7 \end{pmatrix}, \tag{9}$$

where a cipher in the second row shows the state that succeeds the state written above it. There are then only ten "systems," each one beginning with one of the ten "states." For instance, the history of the system beginning with state "8" is described by the infinite sequence

$$\textstyle\sum_1 \ (8, 2, 6, 4, 9, 7, 0, 5, 3, 1, 8, 2, 6, 4, \ldots) \tag{10}$$

in which the finite sequence of the first ten states repeats itself indefinitely. Let N_1 denote the integer written with this finite sequence, $N_1 = 826 \ldots 1$. Alternatively, we may define $\sum_1$ by the sequence of the decimals of the fraction

$$n_1 = N_1/(10^{10} - 1). \tag{11}$$

Similarly, the history of the system beginning with state "1" is given by the decimals of the fraction

$$n_2 = N_2/(10^{10} - 1) \tag{12}$$

where $N_2 = 10N_1 - 8(10^{10} - 1)$. In the same way we can describe the history of every other system by a number n_k.

It is important to note the fundamental properties of the transformation (9). First, every state is transformed into only one state and every state is the transformation of only one state. We say that (9) is a one-to-one correspondence that transforms the set of *all* states into itself. Second, by the same transformation, a subset of i different states is transformed into a subset of exactly i different states.[38] For example, the subset of four states (1, 3, 4, 7) is transformed into the subset of four states (8, 1, 9, 0).

[37] G. D. Birkhoff, "Proof of a Recurrence Theorem for Strongly Transitive Systems," *Proceedings of the National Academy of Science*, XVII (1931), 650–655.

[38] In the case of T, this property follows from the previous one. This is no longer true if the set of all possible states has the power of the continuum. See Appendix E in this volume.

Third, as is easily verified, no proper subset of the set of all states is transformed into itself. A transformation that fulfills all these conditions is known as having the property variously called *metrical transitivity, indecomposability, or ergodicity.*[39] Birkhoff's theorem says that, regardless of the initial state, the system will in this case pass through every possible state and that each state will appear with the same relative frequency (in the limit). For a system defined by T, this limit is therefore $1/10$.

Because the point is sometimes overlooked let us note that Birkhoff himself used the term "ergodic" for a transformation that satisfies only the first two conditions stated in the preceding paragraph.[40] The essential difference is easily illustrated by the one-to-one transformation

$$T^* \begin{pmatrix} 0, 1, 2, 3, 4, 5, 6, 7, 8, 9 \\ 9, 4, 8, 0, 2, 7, 3, 1, 5, 6 \end{pmatrix}, \tag{13}$$

which is ergodic in Birkhoff's but not in the strict sense. Indeed, T^* is *decomposable* into two transformations

$$T' \begin{pmatrix} 1, 2, 4, 5, 7, 8 \\ 4, 8, 2, 7, 1, 5 \end{pmatrix} \qquad T'' \begin{pmatrix} 0, 3, 6, 9 \\ 9, 0, 3, 6 \end{pmatrix}. \tag{14}$$

This means that T^* transforms each of the proper subsets (1, 2, 4, 5, 7, 8) and (0, 3, 6, 9) into itself. Because of this decomposition, *a system beginning with a state of one of these subsets will never assume a state of the other subset.* To wit, the history of a state beginning with state "1" is described by the decimals of

$$n_1' = \frac{1}{7}, \tag{15}$$

that of a state beginning with "0" by the decimals of

$$n_1'' = \frac{963}{9999} = \frac{107}{1111}. \tag{16}$$

We cannot, therefore, say that any state will appear in the history of any system (as was the case for T). However, all the states *actually* appearing in the history of a system will appear with the *same* relative frequency. Only, this relative frequency is not the same for *all* possible states. The six states (1, 2, 4, 5, 7, 8) appear with equal relative frequencies, $1/6$, in the history of any system beginning with one of these states. The other four states will appear with equal relative frequency, $1/4$,

[39] See P. R. Halmos, *Lectures on Ergodic Theory* (Tokyo, 1956), p. 25.

[40] G. D. Birkhoff, "Proof of the Ergodic Theorem," *Proceedings of the National Academy of Science*, XVII (1931), 656–660, and, especially, the same author's "What Is the Ergodic Theorem?" *American Mathematical Monthly*, XLIX (1942), 222–226.

in any of the other systems. We should also note that, whatever the system and whatever the transformation (T or T^*), Poincaré's theorem applies: the system returns periodically to any *previous* state, which does not mean to *any possible* state.

There are therefore two theorems which must be clearly distinguished from one another. Turning to their relevance for our topic, let us recall an important property of a very broad class of mechanical systems: the transformation of one state of such a general system into the state assumed after a constant interval of time is ergodic in Birkhoff's sense.[41] Consequently, in the entire history of a general system every gross microstate consistent with the total energy of that system appears with a definite relative frequency. But only if the system happens to be such that the same transformation is strictly ergodic, i.e., it is indecomposable, can we affirm that this frequency is the same for all microstates. A very apropos illustration of the difference between the two situations is supplied by our billiard ball. As is intuitively seen from Figures 1 and 2, the ball will spend a definite proportion of time in any given region (such as that shaded in the graphs). But this proportion is not the same for all motions consistent with the assumed total energy. To be sure, the system is not ergodic in the strict sense.[42]

The upshot, as Birkhoff and Koopman were to put it later, is that "the *Quasi Ergodic Hypothesis* has been replaced by its modern version: the *Hypothesis of Metrical Transitivity*."[43] And the rub is that no general theorem exists on the suitable conditions for a system to be metrically transitive (viz. ergodic).[44] True, Oxtoby and Ulam have pushed the problem up to the case in which the phase-space is, at the topological level, a polyhedron of "dimension three or more" and for "continuous groups of measure-preserving automorphisms [transformations] not necessarily differentiable or derivable from differential equations."[45] George Gamow's assertion that "their results imply that in a certain sense *almost every* continuous transformation is metrically transitive,"[46] reflects the familiar sanguine hope and contrasts with the sober evaluations of the authors themselves. But apart from this, our simple model of the billiard

[41] This property is known as the Liouville's theorem, for which see Khinchin, *Mathematical Foundations* (note 9, above), pp. 15 f.

[42] This point involves a few technicalities which may be illustrated by the simpler case considered in Appendix E in this volume.

[43] G. D. Birkhoff and B. O. Koopman, "Recent Contributions to the Ergodic Theory," *Proceedings of the National Academy of Science*, XVIII (1932), 282.

[44] Halmos, *Lectures on Ergodic Theory*, p. 96.

[45] J. C. Oxtoby and S. M. Ulam, "Measure-Preserving Homeomorphisms and Metrical Transitivity," *Annals of Mathematics*, XLII (1941), 875 f.

[46] Translator's note in Khinchin, *Mathematical Foundations*, p. 54.

ball warns us—as I have already said—that the existence of collisions (a fundamental feature of any system of numerous particles) is generally incompatible with a continuous transformation.

4. The Antinomy of Statistical Mechanics. Ever since Boltzmann set a lasting pattern in this respect, the arguments in defense of statistical mechanics have surreptitiously shifted between two distinct gospels as well as between three different definitions of entropy. The proofs of some theorems invoke the deterministic laws of mechanics, others the laws applicable to random phenomena. To borrow a metaphor of Max Born, the whole theoretical edifice of statistical mechanics has been constructed after the so-called Jesuitic rule: "one hand must not know what the other does." To recall, by entropy it was first understood only a *physical variable of state* and the Entropy Law had only the Clausius formulation as given in Chapter V, Section 4, above. With Boltzmann,[47] entropy became associated with *the degree of disorder* (however defined). This second definition led to a new formulation of the Entropy Law, mentioned in Section 1 of the present chapter. The step was crucial: a concept for which a definite measure can be obtained from other instrumentally measurable variables was replaced by another for which there is neither a direct nor an indirect instrumental measure.

Now, if one accepts as plausible the idea that the heat of a body *at each instant* is produced by the irregular motions of its particles, then the first definition of entropy may easily be transformed into a particular instance of the second. Also, if the Entropy Law (order-interpreted) is accepted as an opaque law of nature, the classical formulation follows as a particular case. (But not vice versa!) However, once the concept of motion is brought inside any special science, one can hardly resist the temptation of bringing in also the whole mechanics. It is only after one succumbs to this temptation that the crucial problem presents itself: how to derive one opaque fact of nature, the Entropy Law, from the opaque facts of nature expressed by the laws of mechanics.

The idea for which Boltzmann fought passionately his entire life is that this problem is solved if we simply adopt a third definition of entropy as "the thermodynamic probability" that the corresponding degree of thermodynamic disorder shall occur. But, as I have already pointed out, the equivalence of this third definition with the other two has not been established to the satisfaction of all. There are not one but two separate snags that go against this equivalence.

As should be sufficiently clear from the foregoing sections, the first snag is that, once mechanics is brought into the court, random is forever excluded from the proceedings. In an analytical description of nature,

[47] Boltzmann, *Lectures on Gas Theory*, pp. 442 f.

nothing can be completely determined by rigid laws and at the same time behave in a fashion that defies any analytical description. This is the antinomy of statistical mechanics, an antinomy for whose reduction an immense amount of intellectual energy has imaginatively been spent.

In the procedure by which this antinomy is allegedly resolved, there is a second snag which is usually lost in the maze of the probabilistic arguments. One may very well define "height" as "the biological probability." One may also define "biological probability" as the probability of a certain height to occur. But if one adopts—as Boltzmann did—the two definitions together, then one implicitly proclaims a new law of nature.[48] In the case of thermodynamics, this law is in fact a third Entropy Law, which in its strict form is as follows:

Macrostates occur in nature with frequencies such that the greater the degree of disorder the greater is the corresponding frequency.

In connection with this proposition we should recall a keen remark by Gibbs, namely, that its generality extends well beyond the border of thermodynamics proper.[49] The whole truth, as I see it, is that the proposition comes very close to being a metaphysical principle of the same essence as the symmetrical idea according to which the homogeneous is the hotbed of the inevitable nonhomogeneous. It is this metaphysical essence which accounts for the insuperable difficulties encountered in justifying the new Entropy Law either experimentally or analytically by derivation from other basic laws. Indeed, in contrast with the Classical formulation, the new law seems well beyond the reach of experiment. All that its defenders have been able to do is to claim, first, that the law follows from the contradictory basis with which we are by now familiar—the laws of mechanics *and* a statistical law which says that for a particle all positions, on the one hand, and all velocities, on the other, occur with equal frequencies. And as the verification of this new statistical law is subject to the same experimental interdiction, as a last resort the same defenders claimed that the logical foundation on which statistical mechanics rests is validated by the agreement between its propositions and the observed facts.[50]

[48] Aware of the dangerous spell that Boltzmann's "thermodynamic probability" may exercise on our minds, Max Planck suggested a less hypnotizing term, "thermodynamic weight." In addition, he warned the reader not to confuse these terms with "'mathematical' probability or mechanical weight." Yet in the very end Planck, too, fell prey to the spell and equated "thermodynamic probability" with true probability. See Max Planck, *Theory of Heat* (London, 1932), pp. 54 f, 222–225.

[49] J. Willard Gibbs, *Elementary Principles in Statistical Mechanics*, (New York, 1960), p. viii.

[50] E.g., Haar, "Foundations," (note 36, above), 298, and, especially, Khinchin, *Mathematical Foundations*, pp. 52 f.

On the surface, the point seems beyond question. As noted in Chapter I, Section 4, above, the logical foundation of any special science normally includes some ω-propositions that are not susceptible of direct experimental verification. The validity of such a logical foundation is indirectly verified if the β-propositions *derived logically* from (ω) agree with the observed facts. If, on the contrary, some of these facts can be described only by propositions incompatible with (ω), the validity of the logical foundation must be brought into question. The defense of statistical mechanics, however, ignores these explicit considerations of indirect verification. For the truth is that the actual chain between the basic principles of statistical mechanics and the propositions that allegedly are borne out by facts contains numerous links which are known to be dubious or, worse, false.[51] This is one of the reasons why the success claimed by the statistico-mechanical interpretation is in fact spurious. As Zermelo argued long ago, the "apparent success must be due to faulty reasoning."

Another reason concerns the facts themselves. Anyone who has perused the numerous manuals of statistical mechanics must have realized that they sparkle through the absence of any experimental reports.[52] Many a physicist even views statistical mechanics as a mathematical construction of no physical interest. This is also the verdict of a mathematical authority: "statistical mechanics is, in essence, pure mathematics."[53] How much greater should then the scholarly stature of Gibbs appear to us as we read his frank admission that in his own contribution "there can be no mistake in regard to the agreement of the hypotheses with facts of nature, *for nothing is assumed in that respect*."[54]

In these circumstances, it is only natural that all efforts should be bent on justifying the statistical approach by other, general, considerations having nothing to do with the specific facts of thermodynamics. The thesis advanced by A. Krönig long ago (1856), that we must resort to probabilities because of our inherent inability to determine all the coordinates of a complex system and follow its history, is by now a familiar refrain.[55] There are two distinct interpretations of what this idea means.

[51] Such gaps, other than those pointed out in the preceding sections, are clearly marked out in Ehrenfest, *Conceptual Foundations*, *passim*, and Haar, "Foundations," 292–304.

[52] Only now and then do we find a mention of the attempts at verifying experimentally the Boltzmann-Maxwell distribution which, however, are rather inconclusive. Cf. James H. Jeans, *An Introduction to the Kinetic Theory of Gases* (Cambridge, Eng., 1940), pp. 124–130.

[53] Hadamard (cited in note 3, above), pp. 194 f. My translation.

[54] Gibbs, *Elementary Principles*, p. x. My italics.

[55] Even though citations seem superfluous here, for an authoritative sample of contemporary works the reader is referred to Haar, "Foundations," 292, and Khinchin, *Mathematical Foundations*, p. 1.

There is, first, the Boltzmann school of thought to which we may refer now as the *probabilistic* approach. In this approach, probabilities are used to describe what an *individual* system will do next. For this purpose, the limit of the relative frequency of a state in the history of a system is simply equated with the probability that the system shall assume that state next. Now, since all gross microstates are represented by equal volumes of the phase-space, their measures are the same. Hence, according to the ergodic hypothesis all gross microstates are equally probable.[56] The conclusion is that—to refer to the historical sequence (10)—of the preceding section all "states" have the same probability of occurring, namely, 1/10; hence, at any moment there is, for instance, a probability of 9/10 for the next state not to be "0."

No objection could be raised against this mode of reasoning if "probability" were interpreted in the subjective sense—i.e., as an index of our mental incertitude resulting from the inability to measure the initial conditions exactly or to solve the equations of motion completely. A person who for some reason or other has lost track of the time may very well say that there is a probability of 1/7 that "today" should be Sunday. This measure of his subjective probability is justified by the fact that there are seven weekdays in all. But clearly *the same fact does not entitle him to say also that the weekdays succeed each other at random*, i.e., as if whether today is Sunday or Friday would be decided by a random mechanism. Similarly, for the sequences of states (10) there is no sense in saying that there is a probability (in the physical sense) of 1/10 for the next state to be "7"; this cipher always appears only after "9." Furthermore, if the transformation is T^*, even the equivalence of the subjective probability with the ergodic limit is off: the ergodic limit for "7" is 1/6 in one class of sequences and zero in the other, whereas the same limit for "9" is zero and 1/4, respectively. To bring about the desired equalization of the subjective probability with 1/10 it is necessary to assume arbitrarily that the relative frequencies of the systems for which T' or T'' apply are 6/10 and 4/10 respectively. All this may seem elementary. Yet the fact that a physical constant such as the ergodic limit in a perfectly determined sequence may serve, in some circumstances, as an ordinal measure of subjective probability understood as a mental state has recently led to an unsuspected heresy. The heresy is to equate entropy with the "degree" of our ignorance or, what comes to the same thing, the so-called negentropy (i.e., negative entropy, which is Boltzmann's H-function) with the "amount" of our information. How our ignorance—a subjective element—can be a coordinate of a physical phenomena, like that expressed by the Entropy

[56] See formula (3) in Appendix E in this volume.

Law, is beyond the sheerest fantasy—at least beyond my comprehension. One can only wonder whether there is a limit to the twistings of the ergodic theorem.

Ergodic properties of all sorts have been established for some special classes of integers, including the prime numbers.[57] Yet it would be utterly absurd to conclude from such results and from the fact that we know no law yet for the occurrence of prime numbers in the sequence of integers that they occur at random.[58] Random—as I have insisted in Chapter II Section 7—presupposes regularity; but this regularity is not of the same essence as the obvious regularity with which "2" occurs in the illustrative sequence (10) or with the "hidden" regularity with which a prime number occurs in the sequence of integers. The random regularity must be irregular in the sense that it cannot be described by an analytical formula or an analytical rule. Without this kind of regularity the concept of random would be completely superfluous. This is precisely the point that those who belong to the so-called axiomatic school of probability and who equate the theory of probability with the theory of measure fail or refuse to understand. Hardly anyone would deny that probability implies a measure of some sort. But, as Richard von Mises warned us, measure as such is not probability.[59] And I should like to add that measure plus ignorance still does not make physical probability. The defenders of pure thermodynamics are set—so it seems—to ignore this point.

The second, more recent, school of thought advertises a *strictly statistical* viewpoint: because the individual system is so complex that it eludes us, we should study what happens *on the average*. The appeal of this project, initiated by Boltzmann and consecrated by Gibbs, stems from the fact that all we need to compute an average is to know the relative frequencies in the corresponding ensemble. Thus, the concept of probability proper is avoided altogether (although the term "probability" is actually used for "relative frequency"). Now, if the ultimate objective is to remain the same—namely, to learn something about the behavior of the individual system—the natural thing to do is to study the ensemble consisting of all microstates that occur in the history of a single system. It would thus seem that we will have to consider one ensemble for every distinct system,

[57] E.g., C. Benedetti, "Ricerche statistiche sui numeri primi," *Metron*, XXVI (1967), 237–313, or A. G. Postnikov, *Ergodic Problems in the Theory of Congruences and of Diophantine Approximations* (Providence, R.I., 1967).

[58] The issue debated above recalls one of Henri Poincaré's views on probability, discussed in Chapter II, Section 7, above. The confusion between an ergodic limit and physical probability is most clearly seen in the defense of statistical mechanics by Khinchin, *Mathematical Foundations*, p. 52.

[59] Richard von Mises, *Probability, Statistics and Truth* (2nd edn., London, 1957), pp. 98–100.

a quite formidable task. However, it comes out that we need not do so if all mechanical systems are metrically transitive. For in this case, all gross microstates consistent with a given total energy occur with the same frequency in the history of any system having that total energy. The ensembles corresponding to a given total energy are, therefore, identical. Ergo, the average of any "historical" ensemble can be calculated directly (and, as it happens, more easily as well) from the "static" ensemble in which each gross macrostate enters once only.[60]

If we beg the issue of metrical transitivity, all seems in order. And as concerns paper-and-pencil operationality, it is so. The trouble comes from another direction—the operational value of the averages calculated from the static ensemble. The averages of a static ensemble are inert coordinates; in averages the temporal order of events is effaced. They cannot tell us anything about the behavior of a single system which alone is the main objective of thermodynamics (or of any special science for that matter). Thus, when Boltzmann replied that the number of collisions can be calculated with the aid of the laws of probabilities, he did not parry Loschmidt's objection:[61] these laws allow us to calculate only the number of collisions for an "average" system and Loschmidt was speaking of an individual system.

The obvious upshot is that the purely statistical approach is idle as concerns the fundamental problem, that of finding out how an individual system behaves. This is, no doubt, why all writers who take the statistical approach as their point of departure in the end slide into the probabilistic interpretation and equate the relative frequencies of the static ensemble with the probabilities that a system should assume *at any moment* the compatible gross microstates.[62] Allegedly different as their points of departure are, both approaches end on the same spot: the Entropy Law

[60] Boltzmann, in *Lectures on Gas Theory*, p. 297, calls such an ensemble an *Ergoden*; for Gibbs, *Elementary Principles*, p. 115, it is a microcanonical ensemble. The relation of this ensemble with all the historical ensembles is simply illustrated by one of our examples. Any average pertaining to any of the historical ensembles, such as Σ_1 of (10), can be computed from the static ensemble of all states (1, 2, 3, . . . , 9, 0). If metrical transitivity does not obtain, the static ensemble is no longer unique. Given the complexity of a mechanical system, we must expect to be confronted with an infinity of static ensembles. These could not be combined in a single ensemble without assuming that they have a particular *a priori* distribution—a highly arbitrary assumption hidden behind many arguments in statistical mechanics.

[61] See note 23, above.

[62] Gibbs is no exception in this respect as can be seen by comparing various arguments in his *Elementary Principles*, e.g., pp. vii, 16 and 142. In fact, it was he who as early as 1875 made the often-quoted remark that "the impossibility of an incompensated decrease of entropy seems to be reduced to an improbability." (Quoted in Boltzmann, *Lectures on Gas Theory*, p. 215.) A more recent and clear-cut case is John von Neumann for whom statistics and probability are interchangeable. See his *Mathematical Foundations of Quantum Mechanics* (Princeton, 1955), p. 207n.

no longer states what will actually happen—as it does in Classical thermodynamics—but only what is likely to happen. The possibility that disorder will turn into order is not, therefore, denied. The event only has a very low probability.

But no matter how small the probability of an event is, over Time the event must occur infinitely many times. Otherwise the coefficient of probability would lose all physical significance. Consequently, over the limitless eternity, the universe necessarily reaches Chaos and then rises again from ashes an infinite number of times. Boltzmann overtly recognized that this is an unavoidable conclusion of the probabilistic interpretation: "whether we go back to the states of the universe in the infinitely remote past or forward in the most distant future, we are equally right in admitting as very probable that we shall arrive at a stage in which all differences of temperature have vanished."[63] The thought that the dead will one day rise again from their scattered and pulverized remains to live a life in reverse and find a new death in what was their previous birth is likely to appear highly strange—Eddington says "wholly retrograde."[64] Scientists, however, are used to each discovery's being wholly surprising. If many physicists have questioned the explanatory value of statistical mechanics it is only because the idea of a universe periodically aging and rejuvenating rests on a tottering foundation.

The most common and, perhaps, the most effective objection is that no factual evidence supports the idea that the rejuvenation of the universe has a non-null probability: no similar phenomenon has ever been observed in *nature* even at some smaller scale. Only factual evidence can endow a probability computed by a paper-and-pencil operation with physical significance.[65] The standard reply, that we have not yet witnessed a rejuvenating system because we have not observed nature long enough, may seem, though not decisive, at least acceptable. In my opinion, the reply is nevertheless fallacious. Very likely, it is thought that the answer is justified by the proposition that if we wait long enough then

[63] L. Boltzmann, *Wissenschaftliche Abhandlungen* (3 vols., Leipzig, 1909), II, 121 (my translation). The writers who unreservedly endorse this view do not constitute a rare exception: e.g., P. Frank, "Foundations of Physics," *International Encyclopedia of Unified Science* (Chicago, 1955), I, 452 f, and G. N. Lewis, "The Symmetry of Time in Physics," *Science*, June 6, 1930, p. 571.

[64] A. S. Eddington, *New Pathways in Science* (Ann Arbor, 1959), p. 59.

[65] The validity of this statement is in fact evidenced by the necessity of introducing the new statistics of Bose-Einstein and Fermi-Dirac, by which some microstates valid in Boltzmann's statistics are declared impossible. Formally, it can be illustrated by my earlier arithmetical example: it would be absurd to attribute a non-null ergodic limit to the microstate "9" in the decimal sequence of 1/7 on reasoning *a priori* that all ciphers are equally probable. The point forms the basis of my criticism of the exclusively subjectivist doctrine of probability; see Section V of my "The Nature of Expectation and Uncertainty" (1958), reprinted in AE.

a rare event must occur with quasi certainty. In fact the justification requires the converse proposition, namely, that a rare event cannot occur unless we wait a long time. But, as we know, this last proposition is false.[66]

Perhaps even more important is another flaw in the probabilistic setup. The flaw is that one link over which the usual arguments slide swiftly is a counterfeit. Granted the equivalence of (2) and (7) in Section 1, above, both relations define entropy. From a definition, however, nothing can be inferred about the behavior of the object defined: the definition of speed certainly does not entitle us to say whether the speed of the earth, say, increases or decreases. Even if we grant the passage from the combinatorial number W to probability we still cannot infer from (7) anything about the *tendency* of a state to go over into another state. To wit: from the fact that there are more men, say, with dark than with light eyes we can conclude that a man, whether with dark or light eyes, has a greater chance of meeting or sitting next to one with dark eyes. But it would be a gross license to say further that the light eyes of a person have a tendency to turn dark.

And if we accept the other prevalent view that the Entropy Law means only that "the higher the entropy, the greater is its probability of occurring"—"entropy" meaning "thermodynamic probability"—then instead of a law of nature we hold only a tautological application of the definition of probability. The point is that the probabilistic approach cannot claim to have produced a framework equivalent to that of Classical thermodynamics (the only one testable directly) unless it has put something in place of the basic proposition that $\Delta(S) = S(T_1) - S(T_0) \geq 0$ for T_0 earlier than T_1. I believe that this is precisely what Boltzmann sought to do with his first form of the H-theorem, that $dH/dt \leq 0$.

But Boltzmann's error—we can see it now—was that he ignored the difference between Time and dynamic time upon which I have duly insisted in an earlier section. To provide an equivalent substitute for the Classical proposition he should have proved not that $dH/dt \leq 0$ but that $dH/dT \leq 0$. The source of the celebrated imbroglio he thus created is the logical impossibility of deriving a proposition concerning dH/dT from a set of premises (the equations of a dynamical system) that involve only t.

Apparently, Boltzmann was prepared to pay any price for saving his H-theorem. So, in its second form he replaced temporality by probability difference. Remember the H-curve? The price paid was the negation of Time, as Boltzmann himself admitted unequivocally. "We would rather

[66] At the time when I first wrote this in 1963 (published in AE in 1966), the argument seemed to me so simple logically that I saw no need for a lengthier explanation. Since I found out subsequently that some professional philosophers of science disagree with me, I have now presented my argument in greater detail (Appendix F in this volume) so as to enable the reader to see where I might go wrong.

consider the unique directionality of time given to us by experience as a mere illusion arising from our specially restricted viewpoint . . . the two directions of time are indistinguishable, just as in space there is no up or down."[67] Quite understandably, Boltzmann did not want to reject altogether dynamical time, too. For he adds that just as at any point on the earth we call "down" the direction toward the center of the earth, so will any living being in any place in the universe distinguish the direction of time from the less probable to the more probable state. But G. N. Lewis spoke the truth by saying that "time is not one of the variables of pure thermodynamics."[68] Only, instead of praising thereby the probabilistic construction—as was his intent—he laid bare its basic weakness.

True, even in Classical thermodynamics all equations involve only the variables of state. But what distinguishes Classical thermodynamics from a form of geometry is the addition that the sign of ΔS is positive in the direction of Time. Lewis' statement is a recognition that, in contrast with Classical thermodynamics, pure thermodynamics is a form of geometry. Clearly then, it is useless as a science of the happenings in nature and we had better accept the verdict. Some, however, would not admit it. Instead, they prefer to do away with any form of time, unaware of the far-reaching consequences such a position has for *all* special sciences. The layout for this position was adumbrated by Boltzmann. In fact, he argued, there are places in the universe where processes are "*going in the opposite direction*. But the beings who observe such processes will simply reckon time [still] from the less probable to the more probable states."[69] The emphasized expression should make us immediately aware of the familiar verbal swindle. Just as we cannot prove the relativity of the local clock-time without admitting that there is an observer for whom all events are in the absolute time,[70] so is it impossible to demonstrate that Time is an anthropomorphic illusion without implicitly admitting that there is a universal direction in which processes are actually going.

Some recent works have endeavored to place Boltzmann's idea on an allegedly clearer basis. After setting forth the principle that all microstates (such as those considered in Section 1 above) are equally probable, these arguments go on to say that our inherent limitations prompt us to distinguish two classes: the descript and the nondescript classes. A descript microstate is one we can describe briefly, say, an all spade hand at bridge. Hands that require a longer description (in the extreme, the naming of each card) are nondescript. Entropy increases, it is said,

[67] Boltzmann, *Lectures on Gas Theory*, pp. 446 f.
[68] G. N. Lewis, "Symmetry of Time," p. 573.
[69] Boltzmann, *Lectures on Gas Theory*, pp. 447 f. My italics.
[70] Cf. Chapter V, note 58, above.

"when a *known* [descript] distribution goes into an *unknown* [nondescript] distribution."[71] But one such distribution may occur indifferently either before or after the other. We can see again that the argument implies that distributions follow each other in some order. They are not given at once spread irregularly on a flat and perfectly homogeneous table, as it were. In a word, they are not out of Time as abstract objects are. No proof that Time is an illusion can be accepted as valid if it does not start with distributions given at once and having no other linear ordering than their static quantitative or qualitative attributes may imply. All the more perplexing therefore appears the opinion of some logical positivists who maintain that Boltzmann's conception according to which "only certain sections of time have directions, and these directions are not the same . . . represents one of the keenest insights into the problem of time."[72] We may have purged—as Lewis claims—the anthropomorphic idea of Time, but only on paper. Not from physics, and all the less from the world in which we live.

From whatever angle we look at statistical thermodynamics we discover what was plain from the outset: the impossibility of explaining unidirectional processes by laws that are indifferent to direction. In the new thermodynamics this impossibility is metamorphosed into a logical contradiction between the two basic hypotheses: (1) particles move according to rigid laws, and (2) states follow each other in some random fashion. No defender of statistical mechanics—to my knowledge—has ever denied this contradiction. Among the various mending proposals, one is highly instructive. The proposal is to adopt an additional "special hypothesis" by which to deny the contradiction between hypotheses (1) and (2).[73] However, if a special hypothesis is welcome, then it would be far less extravagant to assume—as some ancient atomists did—that particles can swerve freely from their normal course "at times quite uncertain and uncertain places,"[74] or—as modern physicists now say—"that an atom erupts whenever *it likes* to."[75]

Many a physicist would probably dismiss the question of the logical consistency of statistical thermodynamics with the remark, reasonable to

[71] Lewis, "Symmetry of Time," pp. 572 f. Jeans, *An Introduction to the Kinetic Theory of Gases*, p. 270, speaks of "more easily distinguishable" and "less easily distinguishable" states. The implications are the same.

[72] H. Reichenbach, *The Direction of Time* (Berkeley, 1956), pp. 127 f. But Reichenbach's additional remark that "as in so many other points, the superiority of a philosophy based on the results of [physical] science has here become manifest," although inapplicable in this case, is in line with what now passes as philosophy of science.

[73] P. Frank, "Foundations of Physics" (cited in note 63, above), p. 452.

[74] Lucretius, *De rerum natura*, II. 218–220.

[75] F. Waismann, "The Decline and the Fall of Causality," in *Turning Points in Physics*, R. J. Blin-Stoyle, *et al.* (Amsterdam, 1959), p. 142. My italics.

some extent, that all has not been in vain. We now know more about thermodynamic equilibrium than before the statistical approach was tried out; also, the results obtained have been a source of inspiration for quantum mechanics (where probability and irreversibility are in their own right). Yet, in a broader intellectual perspective, the issue appears extremely important, for it pertains to whether the phenomenal domain where our knowledge is both the richest and the most incontrovertible supports or denies the existence of evolutionary laws. This is the reason why I felt it necessary to analyze the rationale of statistical thermodynamics in some detail even though to many students of social sciences such an analysis might at first appear to be a technical digression.

And I trust that this analysis has made it clear why the irreversibility of the entropic process is not a manifestation of chance. The run of the mill position—that the irregular Brownian movement of particles suspended in the air or in some liquid confirms that entropy may also decrease—does not stand scrutiny. It implicitly commits the same type of error as that which brought the downfall of Classical mechanics. Classical mechanics extrapolated the palpable billiard ball to atoms, statistical thermodynamics extrapolates the visible Brownian motion to the entropic transformation of energy. No wonder then that many physicists have refused to endorse a logical edifice that has so many patent flaws. Einstein, although he made the first contribution to a statistics different from that of Boltzmann, opposed to the last the probabilistic approach, which he regarded as a fashion analogous to that for "women's dress."[76] But Einstein's reason may have been his attachment to full causality rather than logical consistency. Be this as it may, we have seen that others explicitly rejected statistical thermodynamics for this last reason alone. Poincaré even said in plain words that we must abandon the Lagrangean and Hamiltonian systems of mechanics and seek elsewhere the explanation of irreversible processes.[77] It is symptomatic that he did not suggest where we should move our search. In the present landscape we can detect only two opposing elements, reversible locomotion and irreversible entropy. As things stand now, there is no purpose in denying that the Entropy Law represents another irreducible law of nature alongside Newton's Law of Inertia, for example. The Entropy Law, I contend, is the simplest form by which the existence of true happening in nature is recognized. But its exceptional importance for our epistemological orientation derives from the fact that the recognition comes from the science whose point of departure is that matter is not subject to Change.

[76] See Louis de Broglie, *New Perspectives in Physics* (New York, 1962), pp. 154 f.
[77] Henri Poincaré, *Oeuvres* (11 vols., Paris, 1934–1956), X, 233.

CHAPTER VII *Chance, Cause, and Purpose*

1. Determinism and the Law of Large Numbers. Like every other pillar of Understanding, the idea that "naught happens for nothing, but everything from a ground and of necessity," came down to us from the founders of philosophy in Ancient Greece.[1] And, as we have seen a while ago, the challenging of this idea also goes back to those ancient times. For a very long time, the controversy as to which idea—determinism or indeterminism—applies to actuality went on almost exclusively among speculative philosophers. The achievements of science until about two hundred years ago were not imposing enough to matter in the controversy. But the successful predictions of mechanics led scientists and philosophers alike to proclaim with complete assurance that nature is governed by complete causality, specifically that every event is the effect of some definite cause that precedes it in Time. If this dogma did not go always so far as to say also that everything was predestined once and for all at the moment of Creation, it was only because it also held that Time has no beginning and no end. This is why Laplace was able to answer Napoleon I that God was not mentioned in his works because he did not need that hypothesis. What mattered for the Laplacean school of thought was the idea that at any moment the future is completely determined by and the past entirely deducible from the state of nature at that moment alone.[2]

[1] See Chapter I, note 23, above.

[2] I should add that in addition to this classical definition of strict determinism there is a host of others. But for the scope of my argument the classical definition alone would do. For the best-known variations and their intricacies—some genuine, some artificial—see, for instance, Ernest Nagel, *The Structure of Science* (New York, 1961), pp. 278–293, 316–335.

The probabilistic approach in thermodynamics sounded the first signal for philosophers and many scientists to reopen the case of determinism. But the issue did not become the center of general attention until after the discovery of the quantum phenomena which revealed in a palpable way that not all events have a *definite* cause. Practically every great physicist took active part in the ensuing debates. Needless to say, the arguments advanced from both sides bespeak the fertile imagination of these eminent participants.

The problem for the indeterminist position in physics is to account for the fact that in countless situations the relationship between "cause" and "effect" is so striking that we can rely on it over and over again. We would not reach for the light switch if we had another opinion about the effect of electrical current on a filament of tungsten. Nor would we run away from a gunpowder keg when someone puts a lighted match to it. But why should we count on getting light from a bulb if the electrons in the filament jump when they like to? The indeterminist's answer is that what appears to us as a causal relation between the switching on of the current and the emission of light by the bulb is the result of the Law of Large Numbers. John von Neumann even argued that this cause-effect link "has certainly no other *cause* than the 'law of large numbers,'" the justification being that "the apparent causal order of the world in the large (i.e., for objects visible to the naked eye) . . . is completely independent of whether the natural laws governing the elementary processes are causal or not."[3] This argument obviously implies that the issue between determinism and indeterminism is totally irrelevant. On closer examination, it is seen to conceal our old friend—the confusion between an ergodic and a random sequence—behind the fact that the Law of Large Numbers as an *abstract mathematical proposition* applies to both sequences.

The true indeterminist, however, takes the position that not only the issue matters but also there is nothing in nature besides an irreducible random which is established beyond the shadow of a doubt.[4] Logically, he is forced to argue that there is a difference of essence between the prediction of an individual event and that of several events together. The ultra familiar argument is that, although we cannot predict whether Mr. X will die during the year, we can predict (with the aid of mortality tables) that *about* 723 out of 10,000 men of the same age as Mr. X will die

[3] J. von Neumann, *Mathematical Foundations of Quantum Mechanics* (Princeton, 1955), pp. 326 f. My italics.

[4] Cf. Richard von Mises, *Probability, Statistics and Truth* (2nd edn., London, 1957), p. 223.

during the same year.[5] Let us observe, however, that if "about" is left undefined analytically, the statement is vacuous. And if it is defined to mean, say, "less than 750 and more than 700," the claimed distinction between the two predictions becomes idle. At the end of the year, each prediction will be either fulfilled or falsified—there is no third outcome. The point, as I have argued in opposition to Mises, is that *every prediction is about a single event*. Only the event itself may be either simple or multiple.[6] Therefore, the distinction between one type of prediction and the other is not flawless from the viewpoint of Logic. Logic, to recall, demands that a division line be drawn either sharply or not at all.[7]

There is another, even more important, reason why pure indeterminism fails Logic. This reason is that when applied to random events, the Law of Large Numbers is not a mathematical tautology (as it is in the theory of measure). Conceivably, the law may be falsified by facts—an idea often exploited by cartoonists. Sociologists, for instance, know that suicides occur in any society with as stable a frequency as that of decaying atoms in a radioactive substance. Conceivably, during the next year the number of suicides in the United States may be nil. And if it follows the usual suicide rate—as will very probably happen—should we say that people colluded so as to vindicate the Law of Large Numbers? We need another explanation for the regularity expressed by that law. And it would not do to follow Neumann's example and to say that the cause of the regularity is the law itself. The only solution, as I have argued in Chapter II, Section 7, is to admit that there is a factor at work in nature which in a dialectical way combines regularity and irregularity in the manner expressed by the Law of Large Numbers. Consequently, when we invoke that law for natural phenomena, we invoke a law of nature, not a mathematical tautology. Random is a *cause*, and the Law of Large Numbers is its effect. The concept of cause thrown out by the back door of indeterminism thus comes back forcefully by the front door.

2. Determinism and the Principle of Indeterminacy. To build a bridge between random and causality is the problem of determinism, too. Only, determinism has to proceed from the other bank. It is natural therefore that determinists should hail Boltzmann's work as "one of the finest

[5] Louis de Broglie, *Physics and Microphysics* (London, 1955), pp. 133 f. See also Mises, just cited, pp. 11, 16–18, 45, and G. L. S. Shackle, *Expectation in Economics* (Cambridge, Eng., 1949), pp. 110–115. Curiously, the position is shared by many determinists as well. E.g., Max Planck, *The New Science* (New York, 1959), pp. 266 ff.

[6] See my articles "The Nature of Expectation and Uncertainty" (1958), reprinted in AE, p. 272, and "An Epistemological Analysis of Statistics as the Science of Rational Guessing," *Acta Logica*, X (1967), 61–91.

[7] For the familiar refrain that in the case of mass phenomena the probability for the prediction to come true can "in practice" be taken as unity, see Appendix F.

triumphs of theoretical investigation."[8] In view of what I have said in the preceding chapter, nothing need be added here about the imaginary existence of the bridge by which Boltzmann claimed to have reached randomness from causality. But something must be said about a different argument which, at times, is advanced in defense of thoroughgoing determinism. The argument is that if we cannot see that complete causality prevails also at the microscopic level it is only because "our observations are not sufficiently delicate and accurate."[9] The argument manifestly refers to the Heisenberg Principle of Indeterminacy.[10] How little support this principle can lend to determinism is adumbrated by the fact that it has also been invoked in support of the opposite thesis—that the behavior of a particle is not subject to causal law.[11] What does then indeterminacy at the level of elementary matter teach us?

It is often said that the epistemological object lesson of Heisenberg's principle is that any act of observation disturbs what it purports to observe. True, during the heyday of the mechanistic interpretation of nature it was generally thought that observation did not affect the observed phenomenon and scientists hoped that with the increased accuracy of our instruments we would be able to get as near as we wish to phenomena *as they really are*. But by now we have come to see that the disturbing effect of observation is a truism. The hard core of Heisenberg's principle is that for the atomic phenomena it is impossible for the observer to determine in what way, or by how much, observation disturbs the phenomenon observed. For an analogy: someone wishing to determine the size of the audience in a lecture hall at a certain moment enters the room, occupies a seat, and counts the vacant seats around him. In spite of the fact that by occupying a seat our observer has altered the situation, he can arrive at the true number of vacant seats by adding unity to the number of seats he *saw* to be free, provided that there is no reason for *another* correction. Conceivably, however, the arrival of our observer may have prompted some listeners to leave the hall hurriedly and in great numbers, thus making it impossible for him to count them. In this case, the observer can no longer determine the necessary correction of his actual count of vacant seats and know the size of the audience before he stepped in the hall. And if this is always the case, the observer will never know whether the size of an audience is determined by, say, the age of the speaker, by a random factor, or by both. This is the impenetrable wrap

[8] Planck, *The New Science*, p. 270.

[9] *Ibid.*, p. 100. Also Louis de Broglie, *Matter and Light* (New York, 1939), p. 189.

[10] For which see Chapter III, Section 4, and, especially, note 52 in that section.

[11] E.g., John C. Slater, *Introduction to Chemical Physics* (New York, 1939), pp. 11, 39–46.

(from man's viewpoint) surrounding the world of physics or the physical world image, as physicists variously call the complex we can reach through our senses directly or indirectly. Planck—the discoverer of the famous constant which bears his name and which sets the limit in Heisenberg's inequality—admitted that his discovery destroyed all hope "that the inevitable errors of observation could be reduced beneath any given limit." Beyond the limit set by Planck's constant there is "only doubt and contingency."[12] Whether beyond that limit there is another world, the real world, which is independent of our observations, nay, of whether there be anyone to observe it, is a purely metaphysical question that has divided the greatest thinkers and that cannot be decided in the manner of the famous Dr. Johnson by just kicking a stone.

The conclusion is that Heisenberg's principle offers no *logical* basis for either determinism or indeterminism. On the contrary, its philosophical implication is that neither thesis is experimentally testable. To quote Planck again, the issue "cannot be decided by referring it to any epistemological theory or by putting it to the test of research measurements, [all the less] on grounds of abstract reasoning."[13]

3. A Physicist's Plea for Causality. It must be admitted, however, that the real predicament of determinism does not come as much from Heisenberg's indeterminacy as from the unadulterated random of quantum phenomena. As a result, even the physicists who still cling fast to determinism have generally been at a loss to find a palatable argument in its favor. Einstein, for example, was content with affirming his conviction to friends, as he did in a letter to Max Born: "You believe in God playing dice and I in perfect laws in the world of things existing as real objects, which I try to grasp in my wildly speculative way."[14] Planck—a salient exception—devoted three major essays to an impassioned defense of his belief in a fundamental causality "that is ultimately independent of our senses and of our intelligence [of our existence as well] and is deeply rooted in that world of reality where a direct scientific scrutiny becomes impossible."[15]

It is very strange, though, to see Planck defending this metaphysical thesis and at the same time making the admissions cited a few lines earlier.

[12] Planck, *The New Science*, p. 247.

[13] *Ibid.*, pp. 57, 71 and *passim*.

[14] *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp (Evanston, Ill., 1949), p. 176. Max Born, in turn, by an equally piquant retort defends the statistical postulate and at the same time gives it the clearest expression that I know. "If God has made the world a perfect mechanism, he has at least conceded so much to our imperfect intellect that, in order to predict little parts of it, we need not solve innumerable differential equations but can use dice with fair success." All things come from Thee, O Lord!—even the Monte Carlo method.

[15] Planck, *The New Science*, p. 261.

Yet there is a valid reason for his plea and for the fact that numerous other famous physicists dislike indeterminism. The reason is that they are far more fully aware than others of the fact that great discoveries are the product of the analytical habit of which I spoke in the first chapter of this essay. The point shines in Planck's statement that the principle of causality "is neither correct nor incorrect; it is a heuristic principle; . . . it is the most valuable pointer that we possess in order to find a path through the confusion of events, and *in order to know in what direction scientific investigation must proceed so that it shall reach useful results.*"[16] For the man of science, just as for the awakening mind of a child, knowledge comes as an answer to some "why?"—the "why" of the ancient Greek philosophy. We can then understand why Einstein confessed that "I should be very, very loath to abandon *complete* causality,"[17] and why Kant argued that the notion of causality is an *a priori* category, so important is it for building up knowledge. And physics, too, can do with, perhaps also needs, some metaphysics of the right kind; in this case the Kantian is of the right kind. To recall a point made in the early part of this book, science would not be where it is today had it embraced a philosophy of positivistic thusness. And as we have seen in Section 1, above, even indeterminism cannot altogether avoid invoking a cause.

But what a difference between indeterminism and determinism for the living science! If a believer in determinism finds out that one of his *individual* predictions does not follow, he never ceases to think up a possible cause and put the product of his imagination to test. For a determinist the withdrawing of the Red Sea for the Passover, the dogma of the Immaculate Conception, the genesis of life, all are mythological miracles until he can find a causal explanation for them. For an indeterminist they are just possible results of the play of chance. And if an event with a nonnull probability has never been observed, that is all right, too: we have not waited long enough for it. Either way, you cannot lose. But as a man of science you cannot gain anything either. Indeterminism, of the hard type, has no recourse against Broglie's verdict that with it physics runs "the danger of remaining stuck in purely statistical interpretations and thus become completely sterile."[18] We need only compare the program of action implicit in Planck's thoughts on his own great discovery—"I firmly believe, in company with most physicists, that the

[16] *Ibid.*, p. 290. My italics. And in an informal dialogue, Planck said that above the gate of the temple of science it is written "*Ye must have faith,*" in the principle of causality, that is. Stenographic notes in Max Planck, *Where Is Science Going?* (New York, 1932), p. 214.

[17] Max Born, *Physics in My Generation* (London, 1956), p. 203.

[18] Louis de Broglie, *New Perspectives in Physics* (New York, 1962), p. 106.

quantum hypothesis will eventually find its exact expression in certain equations which will be a more exact formulation of the law of causality"[19]—with the ideas broached by David Bohm on subquantum phenomena, on the one hand, and with the Neumann "dead-end" theorem, on the other.[20] We can then understand why Planck, after recognizing that causality is only a heuristic principle, let his inner convictions sweep him away and without any ado proclaimed that causality is "an almighty law which governs the world."[21]

4. Causality and Free Will. We would be mistaken, however, in believing that the case of determinism versus indeterminism may be closed at this point. However hard natural scientists *and* philosophers have tried to limit the battleground of this controversy to the world of physics, they have not been able to keep it that way. Formidable questions of all kinds kept striking them from other fields. The most celebrated is the freedom of the human will, which with the triumph of the Laplacean mechanistic picture of the world was declared dead and buried. The justification (still heard occasionally) used to run as follows. If one believes in free will and as a result is a busybody, while another is a fatalist placidly accepting things just as they come, everything is as it should be according to the initial conditions that predestined both to be what they are. Neither can the believer in free will alter the deterministic march of things, nor does the fatalist help things along their predestined path.

But the discoveries in quantum physics resurrected the issue. They led most physicists and some philosophers to admit that the individual's *feeling* of being free in his actions is not subject to question. We cannot deny this feeling, they argue, without implicitly denying that consciousness is the highest authority for understanding the world.[22] I fully share this conviction, but not because as a social scientist I see no other basis from which man's actions may be significantly approached. My reason (and the reason of many others) is that I can conceive of no basis for deciding whether, say, a star is single or double other than what our consciousness tells us. The telescope or the photographic plate by themselves cannot decide the issue any more than a computer can establish the truth value of $e^{i\pi} = -1$ or even of $2 + 2 = 5$. For this reason, all appeals, however impressive in form, to the human consciousness to deny its authority, nay, its existence, are logically equivalent to the story of the man who wanted to do away with the branch which was supporting him.

[19] Planck, *The New Science*, p. 98.

[20] See note 52 of Chapter II, above.

[21] Planck, *The New Science*, p. 110 and *passim*.

[22] Cf. Planck, pp, 59, 65 f and *passim*; A. S. Eddington, *New Pathways in Science* (Ann Arbor, 1959), pp. 3 ff.

Now, physicists of all persuasions have rightly cautioned us that from the "free" jumping of the electron we cannot conclude the existence of the freedom of the will. As Margenau put it beautifully, quantum phenomena may prove "*randomness* of action, but never freedom."[23] On the other hand, the presence of random phenomena at the quantum level does take the sting out of the argument that man cannot will freely because the material world is governed by determinism. Clearly, a completely deterministic world and a man with an absolutely free will are incompatible conditions. In such a world, the answer to when an atom of plutonium will explode depends on whether man decides to produce it, to include it in an atom bomb, and to detonate the bomb. Therefore, the issue of the free will arises *if and only if we insist that determinism is the password in the material world*. The point is that in affirming that man's will is free we are no longer oppressed today by "the old classical determinism of Hobbes and Laplace."[24] A strong defender of indeterminism, such as Eddington, goes even further and argues that quantum physics has completely cleared the issue: man is free to will "*in virtue of that nature which physics leaves undetermined and undeterminable*."[25]

The quintessence of the argument should not escape us: man is free in his will because *physics* cannot predict what an individual will do next. If this way of looking at the problem is adopted, then life for a defender of determinism is really hard. It behooves him to prove that physics can predict what any individual will do next. From all we know, this is out of the question. The most a neodeterminist can do is to seek other arguments in support of the thesis that man's actions are no less subject to causality than the material world. Planck's argument, one of the ablest, may fall outside the immediate interest of a student of matter, but its instructive value for the student of life phenomena is, in my opinion, outstanding.

Planck's point of departure can hardly be disputed: Free will can only mean that "the individual feels himself to be free, and whether he does so in fact can be known only to himself."[26] He claims, however, that this story from the inside, the subjective feeling, reflects mainly the fact that *ex post* the individual imagines that he could have acted differently. What psychologists are able to ascertain in some cases must be true in general: *every action follows from a definite motive*. Complete causality is at work here exactly as in the case of a ray of light which conceivably may reach the observer along infinitely many paths but in actuality follows only

[23] H. Margenau, *Open Vistas* (New Haven, 1961), p. 199. See also Niels Bohr, *Atomic Physics and Human Knowledge* (New York, 1958), p. 22; Broglie, *New Perspectives in Physics*, p. viii; Eddington, *New Pathways*, pp. 74, 86.

[24] H. Weyl, *The Open World* (New Haven, 1932), p. 55.

[25] A. S. Eddington, *The Nature of the Physical World* (New York, 1943), p. 260.

[26] Planck, *The New Science*, p. 287.

one—the path of the quickest arrival.[27] If we cannot show that no difference exists between the two cases it is only because "the profound depths of thought cannot be penetrated by the ordinary intellect."[28] However, nothing pleads against the existence of an intellect standing in the same relation with ours "as ours is above the protozoa." A demon with such a mind could follow "even the most fleeting moment of mortal thought, as well as the most delicate vibration in the ganglia of the human brain" and thus see that every simple act of will is completely determined by "the interplay of mutually reinforced or contradicting motives, which [are at work] partly in the conscious and partially also in the unconscious sphere."[29] The illusion of the free will arises from the fact that no individual is aware of this causal connection. Should an individual become aware of it, he would cease that very moment to feel free.[30] But this cannot possibly happen: no eye can see itself, no runner can overtake himself[31]—or, as we would say in general, no action is reflexive. An individual who would nevertheless try to psychoanalyze himself would be drawn into an infinite regress because "the knowledge of any motive or of any activity of will is an inner experience, from which a fresh motive may spring," so that he will never reach "a motive which is definitely decisive for any future action."[32]

In evaluating Planck's argument we should first note that it differs fundamentally from Laplace's. Laplace did not demand from his demon to find out for him how things are related in the universe. On the contrary, Laplace provided the demon with all the theoretical knowledge necessary to accomplish its task of water boy, so to speak. For this is what Laplace's demon is, a fantastically more efficient water boy than any human, but still a water boy, not the headman who planned the fabulous expedition. In contrast with Laplace's, Planck's demon must take over all the duties of the scientific expedition—not only to measure all that is measurable at the atomic level and beyond, but also to frame the right questions

[27] *Ibid.*, p. 73.

[28] *Ibid.*, pp. 60 f.

[29] *Ibid.*, pp. 60, 107, 111.

[30] *Ibid.*, pp. 215 f. This position should be contrasted with that of Kant (summarized presently), on the one hand, and with that of Hegel, on the other. Hegel, by his famous dictum "This truth of necessity is . . . *Freedom*" (*The Logic of Hegel*, tr. W. Wallace, 2nd edn., London, 1904, p. 282), meant that a mind is free if and only if it no longer is the slave of contingency, i.e., if it is not ruled by Nature, but is self-determined (pp. 110, 264 f). In Marx's materialist inversion of Hegel the dictum is interpreted, however, in the reverse: to wit, Socrates died "free" because he accepted the contingent necessity.

[31] Planck, *The New Science*, pp. 62, 116, 216. On the face value of this argument even God was not aware of His motive for creating the world. It remains for us to find it out.

[32] *Ibid.*, pp. 216, 288.

about "motives," and, above all, to discover the strict causal relationships between a human's thoughts (fleeting or abiding), his motives, and the vibrations of his ganglia (delicate or violent), on the one hand, and the apparent or repressed manifestations of the individual's will, on the other. Planck does not tell his demon either what to measure, or how to find out motives, or, especially, what the laws governing the territory under expedition look like. He steps aside, as it were, and waits for the demon to return and tell him, or you and me, what these laws are. But what if the returning demon tells us that the supposed laws do not exist? This is the main logical flaw in Planck's refutation of free will.

It is instructive to note also that the infinite regress which, according to Planck, would bar any self-examination constitutes, on the contrary, an excellent description of conscious life as an alternating concatenation of motives and actions. In actuality, however, actions as well as the emergence of new motives after one action is accomplished require *duration*. Without this requirement man would indeed be stuck in an infinite regress that would freeze his life. As it happens, man's consciousness only develops along what might become an infinite regress if death did not put an arbitrary end to the process. And, in fact, the best argument for the concatenation of motive-action is that every one of us can vouch that we act from motives even though we would generally be embarrassed to make a complete list of our motives each time. In my opinion, the main cause of this embarrassment is the hysteresis of our mind. Between the time of the decision to act and the time when we look back at our initial motives we acquire new knowledge—some knowledge comes with almost every accomplished action. New motives also emerge in our consciousness. All this may contribute to the illusion that we may have acted *in the past* differently than we actually did. The only thing that constitutes a real puzzle for every ego is not the *springing of actions* from motives but the *emergence of new motives*. If man can *will* his motives freely, then man is free in spite of the fact that all actions follow with necessity from motives. That, I believe, is the only issue of the freedom of the will.

5. *The Categorical Imperative.* The surprising denouement of Planck's argumentation shows that even if he may have had some inkling of this issue he failed to allow for it. The denouement is that, because the cause of man's willing what he wills must remain forever beyond the reach of man's comprehension however demiurgic his intelligence may become, mankind needs a substitute guide in life. "The law of causation is the guiding rule of science; but the categorical imperative—that is to say, the dictate of duty—is the guiding rule of life."[33] Repeatedly though Planck

[33] Planck, *The New Science*, p. 120; also pp. 216, 254, 288.

belabors this idea, his thoughts are not made explicit. For if we accept his claim that actions follow from motives and motives from actions with an objective necessity—i.e., with a necessity that can be ascertained by another mind than that of the doer—why should one need another guiding principle? It could be only for deluding himself in exactly the same way in which the infant in the back seat imagines that he is driving the automobile with his toy driving wheel. This is the old determinist position. Alternatively, Planck may have wanted to invoke Kant's teaching.

To recall, Kant distinguishes between acting according to duty, for the sake of duty, and according to moral law—a distinction which presents no difficulty, but which seems overlooked by Planck. As to moral law—the categorical imperative—Kant defined it by the principle that a maxim (a rule of conduct) is moral if and only if one "*could at the same time will that [his] maxim should become a universal law*."[34] A maxim that is not moral "would destroy itself as soon as it got to be a universal law."[35] The condition is analogous to that which we would like logical principles to satisfy, namely, not to lead to a contradiction ever. But just as we are not certain whether a contradiction-free logic exists, so we do not know whether there is a maxim that is not self-destroying. Kant gives numerous examples of self-destroying maxims but confesses his inability to supply one of the other kind.[36] The difficulty of finding a maxim fulfilling the categorical imperative is illustrated by one of Bernard Shaw's quips: Do not do unto others as you would like them to do unto you—the others may have other tastes.[37] Economic historians know only too well the controversy over what economic system would fulfill the condition of not being self-destructive.

Discussions of the free will often confuse two distinct issues. For example, Planck begins by saying that his thesis is that the human *will* cannot be "subject to the sway of mere blind chance."[38] Yet his whole argumentation is about the issue of whether *human actions are causally determined by motives*. Should he have said that *human actions* are not determined by the sway of chance, his argumentation would have been in order. For hardly anyone would disagree with this modified thesis. Kant, for instance, prefaces his discussion of the will with numerous examples intended to clarify the idea that man acts from a motive and to mark the

[34] Immanuel Kant, *The Fundamental Principles of the Metaphysic of Ethics*, tr. O. Manthey-Zorn (New York, 1938), p. 17. It is important to note that in another place (p. 38), Kant has "*a universal law of nature*."

[35] *Ibid.*, p. 19.

[36] *Ibid.*, pp. 37 ff and, above all, the closing statement of the work, p. 84.

[37] Kant, too, does not think that the Golden Rule satisfies the categorical imperative; to be sure, his reasons are different. *Ibid.*, p. 48n.

[38] Planck, *The New Science*, p. 59.

distinction between the broad categories of motives. But Kant also insists that the problem of categorical imperative has nothing to do with "the *particular constitution of human nature* or the chance circumstances in which [man] is placed,"[39] that is, with man's desires, instincts, and impulses. That our actions are determined by our will is not denied by any philosopher who, like Kant or Hegel, has discussed the problem of the freedom of the will. On the contrary, this fact is their point of departure. Without it, there would be no sense at all in having ethical preoccupations.

The only issue raised by the categorical imperative is whether man can will a law for his will independently of physical causality, in other words, *whether man can will his will.* A self-determining will—that is what the freedom or the autonomy of the will means.[40] For the economist the difference between the two issues—the first concerning the relationship between will and actions, the second, the reflexive power of the will—should recall the equivalent opposition in his own field. The tastes of an individual being given, his actions on the market—utility theory teaches us—are completely determined. But as some economists (including myself) claim, with this result we have not exhausted the consumer problem. More important, perhaps, is the question of what determines the tastes—or, better, the wants—of a person. So, the parallel is: can a human being want his own wants?

To repeat, the greatest mystery that confronts anyone who believes with Kant in a completely causal world of *phenomena* is whether or not man's will is free in the sense just described. For his solution Kant proposed to split the nature of man into two, a *phenomenal* man who is part of the causal order of nature and a *noumenal* (unknowable phenomenally) who is free. The first is subject to the necessity characterizing physical phenomena; the latter is free by virtue of a different "kind of causality" which is applicable to the will of rational beings.[41] The question of whether the noumenal man can be one with the phenomenal man still awaits an answer: it is the old Mind and Body problem under a different form. To be sure, the indeterminists believe they hold the solution. Will, they argue, means the power of the mind "to tamper with the odds on atomic behavior." This means that the mind acts like a cheater who by a swift touch of the finger turns around a tossed die so as to get three sixes every time he gets only two. In doing so the mind would violate no physical law since the number of sixes is not determined by a strictly causal law.

[39] Kant, *Fundamental Principles*, p. 61 and *passim.*

[40] *Ibid.*, p. 65. See also note 30 above.

[41] For greater details as well as for Kant's own cautions about what he has proved, see Immanuel Kant, *Critique of Pure Reason* (Everyman's Library edn., New York, 1934), pp. 316–334.

But such a far-fetched idea is not palatable even to all indeterminists.[42]

6. Physical Parochialism and the Notion of Cause. Nowadays, few appreciate the attempts of philosophers who, like Kant, refused to discuss the problem of man's free will otherwise than on a transcendental, metaphysical level without any contact with physics, hyperphysics, or hypophysics—in Kant's words. Yet some of the greatest physicists of our time vindicate Kant's position. Niels Bohr, for instance, insists that the freedom of the will is "a feature of *conscious* life which corresponds to functions of the organism that not only evade a causal mechanical description but resist even physical analysis carried to the extent required for an unambiguous application of the statistical laws of atomic mechanics."[43] The opposite plan, favored by other physicists and philosophers who have tackled this and cognate problems, is to explain everything only in terms of physical phenomena. But let us not be mistaken; this plan too is based on a metaphysical belief. The only difference is that this last belief is apt to fetter, nay, to misguide, the imagination of the student of life phenomena. The result is the parochialism of what now passes as philosophy of science (to which I have referred at the beginning of Chapters IV and V). And this parochialism is harmful both for him who works in a laboratory and for him who indulges in paper-and-pencil constructions. It pervades Planck's proclamation that "the goal of investigation has not been reached until each instance of a statistical law has been analyzed into one or more *dynamic* laws."[44] And even a defender of indeterminism, such as Eddington, turns only to physics: indeterminism comes from the fact that physics "deals with probabilities from the outset."[45] But the clearest indication of this mechanistic temper is the edict issued by the French mathematician Paul Painlevé: "The idea that one must know the entire past of a physical system in order to predict its future is the denial of *science* itself."[46] This, it should be noted, is not a theorem but an implicit definition of science. No one, I think, would deny that if the future of anything depends on its entire past we may not be able to predict that future if the past begins too far back, say, at $t = -\infty$. But why should one deny in the teeth of evidence the existence of hysteresis

[42] Cf. Eddington, *Nature of Physical World*, pp. 311–314.

[43] Niels Bohr, *Atomic Physics and Human Knowledge*, p. 11. My italics.

[44] Planck, *The New Science*, p. 214. My italics.

[45] Eddington, *New Pathways*, p. 105.

[46] Paul Painlevé, *Les axiomes de la mécanique* (Paris, 1922), p. 40 (my translation and italics). As reported by Eddington, *Nature of Physical World*, pp. 228 f, Heisenberg also said that "the question whether from a complete knowledge of the past we can predict the future, does not arise because a complete knowledge of the past involves a self-contradiction." I can see the actual impossibility, but not the self-contradiction.

phenomena or, if one admits their existence, why should he deny that they may form the object of science?

Surely, science should study the phenomena that can be described by a system of ordinary differential equations. Surely, science should study the camel. But what about the dromedary? Or should we, like the zoo visitor who in the story was puzzled by the dromedary, say "this animal simply does not exist" and walk away completely satisfied? Peace of mind at this price may befit a camel boy, but not a servant of science. If one narrows down science and, implicitly, all that is for man to know to a dynamical matrix, then one must swear by determinism. About this kind of determinism—and only about it—I would join Noüy in saying that "it is an essentially restful doctrine for a mind deprived of curiosity."[47] And the same is true for the opposite pole, the full-fledged indeterminism which claims that everything consists of jumping electrons.

Things in the world are what they are; some fit into a dynamical matrix, others not. If we want to get in mental *analytical* contact with the world, the inner and the external, on as broad a front as possible, we must abandon physical parochialism. For physical parochialism forces on us Planck's conclusion that there is a point "beyond which science [read 'physics'] cannot go. . . . This point is the individual ego." This means that we should renounce, as Bohr advises us, any thought of explaining "our own conscious activity."[48] To be sure, physical parochialism has not impeded (one may argue that, on the contrary, it has aided) progress in biochemistry, microbiology, and medicine. But the question remains: why not study the dromedary too and its relation with the camel? It is strange, therefore, to see Planck making the above admission while proceeding with an inquiry of the freedom of the will from evidence provided only by physics. Yet in his discussion of causality Planck felt the need to call in evidence from outside physics as he asked what else the historian or the sociologist do if not look for past causes for present conditions.[49] In my opinion the causes for which a social scientist or a biologist (at times, even a chemist) look are not conceptually the same as the cause of the physicist, which is the sublimation of the mechanical push or pull.

The situation is not as simple as in physics, although "simple" is hardly the right word. I believe that it is not proper to transplant the physical concept of cause even in chemistry, that is, everywhere in chemistry. We need a different meaning of "cause" to express the fact that, although every time we combine hydrogen and oxygen in a definite ratio we get

[47] P. Lecomte du Noüy, *The Road to Reason* (New York, 1948), p. 179.

[48] Planck, *The New Science*, pp. 114 f; Bohr, *Atomic Physics and Human Knowledge*, p. 11.

[49] Planck, *The New Science*, p. 105.

water, some brute facts—that water is a liquid when its temperature is 60 degrees, for example—cannot be traced back to any properties of the components. Chemists would actually tell us that although water is an ultrafamiliar substance its *known* properties are enveloped in more mysteries than those of any other compound. Nor would it be proper, in my opinion, to say that because the structural properties of matter in bulk are not reducible to those of elementary matter they are outside the domain of cause. Rational phenomena of the third order, which abound in the organic and superorganic domain, complicate further the issue of what we mean by "cause." They suggest that the same "cause" may have various "effects." Of course, it would be improper to attribute this variation to a random factor.

The retort to the foregoing observations is a familiar refrain. The phenomena mentioned above reflect only the fact that at present we are ignorant of some causal factors or laws in the sense these terms have in mechanics; when these will become known the peculiarity will vanish. The argument obviously claims as its authority a historical trend, namely, that new factors and new laws have been discovered day after day. Personally, I believe that for our knowledge historical trends as clear-cut as the one just mentioned are by no means less respectable authorities than the *analytical* regularities observed in nature. In fact, these regularities are nothing but a special, limiting instance of historical trend. The difficulty of the "you-will-see" argument comes from another direction. History also teaches us that with each discovery a multiple number of new queries spring up in a snowballing manner. Compare, for example, the situation of physics at the time of Laplace with that of today. Yes, have not the last discoveries of one kind of elementary particle after another increased the number of facts which now are awaiting a "causal" explanation? "An addition to knowledge," as Eddington pointed out, "is won at the expense of an addition to ignorance."[50] If, as I have already submitted, the modes of being of nature are infinite in number, then even if we would assume that the number of outstanding queries in relation to that of the problems already solved decreases with time, our struggle with nature will not come to an actual end. The human mind, I am sure, will never become a divine mind.[51]

As we conquer one hill after another in science, we should first of all collect our ideas and on this basis draw a picture of the phenomenal world at each stage. To assume in such a census taking that we know what we in fact do not and, as a consequence, to proclaim that we must move

[50] Eddington, *Nature of Physical World*, p. 229.

[51] The discussion of the fallacy concerning the process of limit (Appendix A in this volume), should prove handy to those who may disagree with this statement.

on only in one particular direction is to make an already difficult battle still more difficult. Some may not be happy with the viewpoint of scholars such as Bohr, Broglie, or Delbrück, namely, that life is irreducible to further physico-chemical analysis because its characteristic manifestations are far beyond any experience we know about inanimate matter.[52] The existence of life must be regarded as a basic postulate of biology just as the existence of energy is of physics. To hold the contrary view implies that an idea or a sensation reduces to configurations of atoms in the brain—a view no one has been able to prove but against which there are some substantial arguments. Eddington,[53] for instance, wonders amusingly whether it would make any sense at all to say that the brain manufactures sugar when the idea $7 \times 8 = 56$ passes through it and some noxious substance if the idea is $7 \times 8 = 65$. The economist who speaks of utility as being grounded in pleasure and disutility in pain should perhaps wonder, too, whether any biologist (or physico-chemist) has been able to show why working beyond a certain point is accompanied by an unpleasant sensation and consuming by a pleasant one. Has anyone made even the slightest suggestion of why contemplating a work of art gives a sensation of pleasure to some people?

Let us also note that the physical impulses from the sounds of the words "five" and "*cinque*" undoubtedly produce a different atomic matrix on the receiving brain. Yet the Englishman hearing the first and the Italian hearing the second will think of exactly the same thing. Brute facts such as these cannot be reduced to purely physiological laws, all the less to purely physico-chemical ones. Physiologists are prone to denying the existence of mind, consciousness, or will on the ground that they cannot find such things in their extensive search of the human body. Yet a consummate brain surgeon, W. Penfield, reports a striking physiological experiment. He asked some patients undergoing brain surgery to resist moving their arm when he applied an electrode to the proper place of their cerebral cortex. The patients invariably used their other arm to hold down the arm receiving the impulse of the electrode. And this led Penfield to conclude: one arm moved because of the electrode, the other because of the patient's will.[54] This is one great mystery of our mental phenomena. For if our mental states, including the will, are not reducible to physical

[52] Bohr, *Atomic Physics and Human Knowledge*, pp. 15, 21, 76; Louis de Broglie, *Physics and Microphysics* (London, 1955), p. 139. For Delbrück, see Chapter V, Section 1, above.

[53] Eddington, *Nature of Physical World*, p. 345.

[54] Cited in Arthur Koestler, *The Ghost in the Machine* (New York, 1967), p. 203. See also Wilder Penfield, "The Physiological Basis of the Mind," in *Control of the Mind*, eds., S. M. Farber and R. H. L. Wilson (New York, 1961), pp. 3–17.

configurations, how can we explain that a motive, which has only a mental existence, may lead to outward actions of the individual? I know that the view that all is at bottom a purely physico-chemical affair invokes the case of a man running amok under the influence of some drug. The question is whether this evidence has any value for a normal man, that is, for a man who has not lost by some accident his human quality of "rationality." In my opinion (and, I am sure, in the opinion of every physicist), we cannot answer the question of how a radium atom behaves by extrapolating the behavior of its degenerated form, the atom of lead.

The point I wish to make in concluding this section is that although we are directly aware of the connection between our motives and our actions we would only confuse ourselves if we would—as in fact most of us do—use for this relationship the word "cause" with the same meaning it has in the physical domain. Physical parochialism, by spreading the gospel that physics must be the rockbed of any tenable philosophy,[55] has caused to our way of thinking enough harm. It does not matter whether or not the harm came through philosophy: I do not believe in such a thing as an aphilosophical science, at least as far as the highest levels of scientific endeavor are concerned. We have taken a sheer delight in pounding at the notion of cause by exposing its imperfections—some of which are undeniable, to be sure—until we have convinced ourselves that we had better renounce such a hoary, mystic idea. But if we renounce physical parochialism and endeavor to find the most appropriate way to get in mental contact with each phenomenal domain separated by our analysis of the Whole, we will find that, on the contrary, we need, as Aristotle taught, four forms of causes. Planck, himself, is of the opinion that in order to bring our picture of the world in closer accord with the facts the concept of causality must be refined and enlarged as well.[56]

Certainly, everyone wishes that we could do with only one form of cause. Monism, in spite of its repeated disappointments, will never cease to fascinate our minds. But the truth is that we do not possess, in the words of Hermann Weyl, a unified picture of the "interwoven texture of Matter, Life and Soul,"[57] and the possibility of arriving at it even in thousands of years from now—as he hopes—seems to me to be a false hope.

The picture we have now is that the physical domain is dominated by the efficient and the formal causes. In the chemical domain there appears the material cause as well. As we have seen earlier, neither oxygen nor

[55] Cf. Planck, *The New Science*, p. 235.

[56] Max Planck, *Where Is Science Going?* (New York, 1932), p. 221.

[57] Weyl, *The Open World*, p. 55.

hydrogen is the efficient cause of water; they are, though, the material cause of it. Clearly, it would be absurd to say that nature in its physico-chemical modes of being has a purpose. In the other domains, however, we are confronted on a large front with the final cause, whether we consider the equifinality of the biological organisms or, especially, man and society running after their purposes. For let one point be made clear. The motive exists in man's mind; so it is correct for every one of us to say "my motive *is* such and such" or "my motive *has been* such and such." But where are our goals, our purposes at the time when we speak about them as such, that is, before they become through our actions accomplished facts? That is why we should rather say "my purpose *will be* such and such," to show clearly that man is also moved by his vision of the future, by things that are not actual, not only by the pain that causes him to withdraw reflexively his hand from the burning stove.

7. *Entropy and Purposive Activity.* Among the various ideas surrounding the antinomy between physical causality and freedom is that of the inexorability of the physical laws. Properly understood, this idea is that man cannot defeat the physical laws in the sense of preventing their working. The law of gravitation, for instance, is at work even in the case of a flying aircraft. The Entropy Law of Classical thermodynamics is no exception to this rule. Heat is dissipated even when we refrigerate a warehouse, because more heat is "degraded" in the rest of the universe than that which is "upgraded" in the warehouse. The result is that bound heat-energy in the universe has increased, as the law requires. Refrigeration is an exception only to the crude law that heat cannot flow from the colder to the hotter body but not to the law proper which says that heat cannot do so *by itself*. However, the probabilistic formulation of the Entropy Law, based on the idea that heat is merely one manifestation of the irregular motion of particles, raised in some physicists' minds doubts as to the inexorability of that law.

This view is related to a piquant fable of J. Clerk Maxwell's. He imagined a minuscule demon posted near a microscopic swinging door in a wall separating two gases, A and B, of equal temperature. The demon is instructed to open and close the door "so as to allow only the swifter molecules to pass from A to B, and only the slower ones to pass from B to A." Clearly, the demon can in this way make the gas in B hotter than in A. This means that it can unbind bound energy and, hence, defeat the Entropy Law of statistical thermodynamics.[58]

[58] J. Clerk Maxwell, *Theory of Heat* (10th edn., London, 1921), pp. 338 f. We know from Boltzmann (*Populäre Schriften*, p. 231), that J. Loschmidt thought up the same fable long before Maxwell. The coincidence is proof of how deeply significant the issue is.

Ever since Maxwell wrote it (1871), the fable has been the object of a controversy which, I submit, is empty. Taken on its face value, the fable reveals a conflict between the tenet that physical laws are inexorable and the statistical explanation of thermodynamic phenomena. In this perspective, Maxwell's own point corresponds to eliminating the conflict by upholding the tenet and indicting the explanation. But one may equally well accept the statistical explanation and reject the tenet. This second alternative corresponds to the argument enthusiastically supported by all vitalists that a living being—as proved by Maxwell's demon—possesses the power of defeating the laws of matter. It is because of this last argument that the fable acquired a sweeping significance. However, like many other paradoxes, Maxwell's is still an intellectual riddle. Like all paradoxes, Maxwell's can only enlighten our thoughts but cannot become a basis for settling the very issue it raises.

The main line of the arguments aimed at disposing of the paradox descends from Boltzmann, who argued that "if all differences of temperature would disappear, no intelligent being could emerge either."[59] The point has ever since been repeated in various forms by Einstein, Eddington, and many others. The issue was given a more explicit turn by L. Szilard.[60] He argued that the demon cannot act without getting some information about the motions of the particles. This idea paved the way for equating entropy with deficiency of information and led to a series of exercises on the physical limitations of the demon. Their main point is that since a milieu in thermodynamic equilibrium is a black body it is impossible for the demon to see the particles. Should it be provided with some physical device for obtaining the needed information—say, a torch—it still could not unshuffle more energy than that consumed by the device.[61] All these exercises, however, do not dispose of the paradox; they merely assume it away.[62] Their very basis is that the Entropy Law prevents a physical device from performing more work than that warranted by the free energy it receives. Clearly, if this is the premise, the conclusion can only be the absurdity of the fable.

A more familiar argument, instead of providing the demon with a

[59] Boltzmann, *Populäre Schriften*, p. 231. My translation.

[60] L. Szilard, "Über die Entropieverminderung in einem thermodynamischen System bei Eingriffen intelligenter Wesen," *Zeitschrift für Physik*, LIII (1929), 840–856.

[61] For these arguments and the basic references, see L. Brillouin, *Science and Information Theory* (2nd edn., New York, 1962), ch. 13.

[62] It is often pointed out that because of the Heisenberg Indeterminacy the demon can in no case determine both the positions and the velocities of the particle. But Eddington (*New Pathways*, p. 70) believes that the demon will nonetheless succeed *on the average*.

physical device, "exorcises" it, i.e., transforms it into an intelligent being in flesh and blood. It first observes that such a being must consume some free energy in order to survive, and then it asserts that if the being were able to unshuffle a greater amount it would contradict the Entropy Law.[63] This line of reasoning, therefore, is vitiated by the same circularity as that of the preceding paragraph. It has nevertheless the advantage of bringing to the forefront the most important implication of the fable. In the words of Helmholtz, it is the issue of whether the transformation of the disordered heat motion into free energy "is also impossible for the delicate structures of the organic living tissues."[64] More exactly, if all is aimless motion (as statistical thermodynamics contends), we should expect the constituent particles of any organism to disintegrate promptly into a chaotic state just as the aimlessly running mice of G. N. Lewis' metaphor supposedly do.[65] Indeed, the probability that a living organism would not disintegrate promptly is fantastically small. According to the teachings of Boltzmann and of every advocate of the probabilistic approach, the event should never happen. Yet the "miracle" has happened over and over again on a fantastic scale. The miracle, therefore, needs an explanation. As Poincaré put it, it is precisely because according to the laws of physics all things tend toward death "that life is an exception which it is necessary to explain."[66]

It is along this line of thought that Eddington argued that besides random there must be an opposite factor at work in nature: the anti-chance. "We have," he said, "swept away the anti-chance from the field of our current physical problems, but we have not got rid of it."[67] By this he may have meant that we have done away with strict causality and now we need the anti-chance to oppose mere chance in all those countless cases where the rule of chance is contradicted by enduring ordered structures. To be sure, similar suggestions for explaining the contradiction had been made long before by others—by Georg Hirth, for

[63] Interesting discussions of this argument are found in P. W. Bridgman, *The Nature of Thermodynamics* (Cambridge, Mass., 1941), pp. 155 ff, 208 ff, and Norbert Wiener, *Cybernetics* (2nd edn., New York, 1961), pp. 57–59.

[64] Hermann Helmholtz, "Die Thermodynamik chemischer Vorgänge," in *Wissenschaftliche Abhandlungen* (2 vols., Leipzig, 1882–1883), II, 972n. My translation.

[65] To recall, G. N. Lewis, "The Symmetry of Time in Physics," *Science*, June 6, 1930, p. 571, illustrated the position of statistical thermodynamics by inviting us to imagine some aimlessly running mice crowded in a corner of a box hinged down on its center: soon, he argued, the mice will disperse themselves in such a manner as to hold the box in horizontal equilibrium. I wonder, however, why Lewis brought an organic factor into his metaphor. Would some aimlessly thrown balls not do instead?

[66] Henri Poincaré, *Mathematics and Science: Last Essays* (New York, 1963), p. 8.

[67] Eddington, *New Pathways*, p. 60.

instance.[68] By now, practically every thinker feels that "something [a new principle] has to be added to the laws of physics and chemistry before the biological phenomena can be completely understood."[69] Suggestions such as Hirth's and Eddington's have only an indirect value which, moreover, calls for a great deal of conceding. But the alternative position—to maintain that there are no principles in nature other than those manifested in the test tube or on the photographic plate—amounts to a glorification of the fallacy of the puzzled zoo visitor mentioned a while ago. Certainly, as Wiener noted, "it is simpler to repel the question posed by the Maxwell demon than to answer it."[70]

Yet Maxwell's demon was not to remain without glory. The fable had a decisive influence upon the orientation of the biological sciences. To begin with, it compelled us all to recognize the categorical difference between *shuffling* and *sorting*. In thermodynamics we do not ask ourselves whence comes the energy for the shuffling of the universe, even though we know only too well that it takes some work to beat an egg or to shuffle cards. The shuffling in the universe—like that of the gas molecules surrounding the demon—goes on by itself: it is automatic. But not so with sorting: Maxwell invented a demon, not a mechanical device, for this job. "Sorting is the prerogative of mind or instinct," observed Eddington, and hardly anyone would disagree with him nowadays.[71]

Actually the more deeply biologists have penetrated into the biological transformations the more they have been struck by "the amazing specificity with which elementary biological units pick out of the building materials available just the 'right ones' and annex them just at the right places."[72] Irrespective of their philosophical penchant, all recognize that such orderly processes, which are "much more complex and much more perfect than any automatic device known to technology at the present time," occur only in life-bearing structures.[73] This peculiar activity of living organisms is typified most transparently by Maxwell's demon,

[68] Georg Hirth, in his *Entropie der Keimsysteme and erbliche Entlastung* (Munich, 1900), coined the word "ektropy" to denote the principle that opposes the entropy principle of degradation in the life-bearing structures. Hirth's idea and terminology were taken up by Felix Auerbach, *Ektropismum oder die physikalische Theorie des Lebens* (Leipzig, 1910). Hirth's argument is pervaded by confusing mysticism. But there are extenuating circumstances for this: he was an art expert. Unfortunately, Auerbach, although a physicist of some distinction, did not do better, and the notion of ektropy made absolutely no history.

[69] Werner Heisenberg, *Physics and Philsophy* (New York, 1958), pp. 102 f.

[70] Wiener, *Cybernetics*, p. 57.

[71] Eddington, *Nature of Physical World*, p. 93.

[72] L. von Bertalanffy, *Problems of Life* (New York, 1952), p. 29.

[73] Ilya M. Frank, "Polymers, Automation and Phenomena of Life," *Izvestia*, Sept. 11, 1959. English translation in *Soviet Highlights*, I (1959), no. 3.

which from its highly chaotic environment selects and directs the gas particles for some definite *purpose*.

Purpose is, of course, a concept alien to physics. But from what has been said in the preceding section, this point should not bother us. Physicists, in opposition to the positivist sociologists, have one after another admitted that purpose is a legitimate element of life activities, where the final cause is in its proper right, and that it leads to no logical contradiction if one accepts complementarity instead of monism.[74] Eddington, as we have seen, goes even further. For although he argues that the "nonrandom feature of the world might possibly be identified with purpose or design, [noncommittally with] anti-chance," he does not suggest that anti-chance is absent from the physical world. "Being a *sorting* agent, [Maxwell's demon] is the embodiment of anti-chance."[75] Norbert Wiener, too, sees no reason for supposing that Maxwell demons do not in fact exist hidden behind some complex structures, as it were. As the metastable properties of enzymes suggest, they may operate, not by separating fast and slow molecules, "but by some other equivalent process."[76] It is not surprising therefore that thermodynamics and biology have drawn continuously closer and that entropy now occupies a prominent place in the explanation of biological processes.[77]

Unfortunately, most students of life phenomena now shun the use of the concept of purpose. In all probability, this proclivity reflects the fear of being mocked as a vitalist more than anything else. As a result, only few students pay attention to the fact—a physico-chemical marvel in itself—that life-bearing structures are as a rule able to attain their individual purpose over unforeseen obstacles of all sorts or, as Bergson strikingly put it, to secure "the constancy of the *effect* even when there is some wavering in the *causes*."[78] I should hasten to add that by emphasizing the legitimate place of purpose in life phenomena I do not intend to vindicate the ultravitalist position that living structures can defeat the laws of *elementary matter*. These laws are inexorable. However, this very argument uncovers the real issue of the vitalist controversy. Given that even a simple cell is a highly ordered structure, how is it possible for such a structure to avoid being thrown into disorder instantly by the inexorable Entropy Law? The answer of modern science has a definite economic flavor: a living organism is a *steady-going concern* which maintains its

[74] E.g., Bohr, *Atomic Physics and Human Knowledge*, pp. 10, 92, 100.

[75] Eddington, *Nature of Physical World*, pp. 60, 69.

[76] Wiener, *Cybernetics*, p. 58.

[77] This intimate connection is admirably and with unique insight explained in a great little book already quoted: Erwin Schrödinger, *What Is Life?* (Cambridge, Eng., 1944).

[78] Henri Bergson, *Creative Evolution* (New York, 1913), pp. 225 f. My italics.

highly ordered structure by sucking low entropy from the environment so as to compensate for the entropic degradation to which it is continuously subject. Surprising though it may appear to common sense, life does not feed on mere matter and mere energy but—as Schrödinger aptly explained—on low entropy.[79]

Sorting, however, is not a natural process. That is, no law of elementary matter states that there is any sorting going on by itself in nature; on the contrary, we know that shuffling is the universal law of elementary matter. On the other hand, no law prohibits sorting at a higher level of organization. Hence, the apparent contradiction between physical laws and the distinctive faculty of life-bearing structures.[80]

Whether we study the internal biochemistry of a living organism or its outward behavior, we see that it continuously sorts. It is by this peculiar activity that living matter maintains its own level of entropy, although the *individual* organism ultimately succumbs to the Entropy Law. There is then nothing wrong in saying that life is characterized by the struggle against the entropic degradation of mere matter.[81] But it would be a gross mistake to interpret this statement in the sense that life can prevent the degradation of the entire system, including the environment. The entropy of the whole system must increase, life or no life.

Although the point in all its details is quite involved, its gist is relatively simple if we bear in mind a few things. The first is that the Entropy Law applies only *to an isolated system as a whole*. The second is that an isolated system in entropic equilibrium (in a chaotic state) is homogeneous *in itself* and also has no free energy *relative to itself*. Let us consider an isolated system U that is not in entropic equilibrium, and let U_1 be a subsystem of it. We may refer to the complement of U_1, denoted by U_2, as the environment of U_1. Let S_1^n and S_2^n be the entropies of U_1 and U_2 at some moment T_n. All that the Entropy Law says is that, if T_1 is later than T_0, the entropy of U is greater at T_1 than at T_0: $S_1^1 + S_2^1 > S_1^0 + S_2^0$. Consequently, the law is not violated if $S_1^1 \leq S_1^0$, provided that this decrease in the entropy of U_1 is more than compensated by an increase in the en-

[79] Schrödinger, *What Is Life?* chap. vi. The seed of this idea goes back to Ludwig Boltzmann who was first to point out that free energy is the object of the struggle for life. See his 1886 essay "Der zweite Hauptsatz der mechanischen Wärmetheorie" in *Populäre Schriften*, pp. 25–50.

[80] Joseph Needham, "Contributions of Chemical Physiology to the Problem of Reversibility in Evolution," *Biological Reviews*, XIII (1938), 248 f.

[81] Bergson, *Creative Evolution*, pp. 245 f, is known for having presented this view more articulately and more insistently than any other philosopher. The multifarious accusations of mysticism directed against him are no longer in order, if they ever were.

tropy of the environment U_2, i.e., provided that $S_2^1 - S_2^0 > S_1^0 - S_1^1 \geq 0$. True, the entropy of the environment cannot increase unless the environment contains part of the free energy of U at T_0. But this condition pertains to the initial distribution of free energy and, therefore, is not subject to any constraint. The subsystem U_1 can then maintain, even decrease, its entropy by sucking, as it were, free energy (alternatively, low entropy) from its environment.[82] This is precisely what a living organism does. Of course, if U is in entropic equilibrium at T_0, we can no longer distinguish qualitatively a subsystem of it. In the case of a chaotic state, we can only speak of its qualityless parts. The upshot is that the very concept of a living organism is inapplicable in connection with a chaotic universe. In any isolated system life must disappear before the system reaches a chaotic state, i.e., before its entropy attains its maximum. Whether a Maxwell demon, if introduced in such a world, could perform its task is still a moot question. But there is hardly any doubt that in a world whose entropy is still increasing a sorting demon can decrease the entropy of a subsystem. The fact that an exorcised demon, i.e., a living organism, can survive only in a world whose entropy increases has already been pointed out by more than one writer.[83] I should add, however, that life, at least in the form it exists on this planet, is compatible only with a moderate entropy.[84] In an environment of very low entropy, a living organism would not be able to resist the onslaught of the free energy hitting it from all parts. On the other hand, in an environment of very high entropy there would not be enough free energy going around for the sorting to be successful in the short run.

Let me observe that the case, however, is not completely closed by the above remarks. A perhaps even more difficult question confronts us now: is the increase of entropy greater if life is present than if it is not?[85] *For*

[82] In the recent literature, it has become customary to refer to $-S$ (the entropy with the negative sign) as "negentropy" and to say that an organism feeds on negentropy. (Cf. Appendix B in this volume.) I believe, however, that the term "low entropy" conveys more directly the qualitative nature of what is involved.

[83] E.g., L. Boltzmann, "On Certain Questions of the Theory of Gases," *Nature*, LI (1895), p. 415; Paul and Tatiana Ehrenfest, *The Conceptual Foundations of the Statistical Approach in Mechanics* (Ithaca, N.Y., 1959), p. xi.

[84] Naturally, the adepts of statistical thermodynamics maintain, along with Boltzmann (cited in note 69, Chapter VI, above), that since entropy is not increasing everywhere in the universe we are in an exceptional state. E.g., Philipp Frank, "Foundations of Physics," *International Encyclopedia of Unified Science* (Chicago, 1955), II, 452.

[85] Bergson, in *Creative Evolution*, 245 f, maintains that life retards the increase, but offers no evidence in support of this view. The retardation thesis has later been advanced by some natural scientists as well. Cf. A. J. Lotka, "Contribution to the Energetics of Evolution," *Proceedings of the National Academy of Sciences*, VIII (1922), 147–151.

if the presence of life matters, then life does have some effect upon physical laws. Our ordinary knowledge of the change in the material environment brought about by the biosphere seems to bear out the idea that life speeds up the entropic degradation of the whole system. And in fact, a simple laboratory experiment confirms that the entropic evolution of an isolated system is altered if life is introduced in it at a certain moment. All life-bearing structures work toward a purpose—to maintain their entropy intact. They achieve it by consuming the low entropy of the environment, and this fact alone should suffice by itself to justify the belief that life is capable of some physical manifestations that are not derivable from the purely physico-chemical laws of matter. There is, we remember, some freedom left to actuality by the Entropy Law of Classical thermodynamics. And as W. Ostwald, a Nobel laureate for chemistry, noted long ago,[86] it is by virtue of this freedom that a living organism can realize its life purpose and, I should add, that man's economic activity is possible. Another reason why I have dwelt in the preceding chapter on the main flaws ably couched in the reduction of thermodynamics to the law of mechanics should now be obvious: statistical thermodynamics completely denies the possibility of any purposive activity because it claims that everything is completely determined by the laws of mechanics. Accordingly, it would be nonsense to speak of purposive activity and to relate it to some "vitalistic" principle not deducible from those laws. But without such a principle, I contend, we simply turn our backs to a wealth of highly important facts. Actually, if examined closely, many occasional remarks by physicists on the life process tend to show that they too share, however unawares, this "vitalistic" belief.

The fact has a natural explanation. The scholarly mind cannot bear the vacuum left after the Classical concept of cause shared the fate of the mechanistic epistemology. The scholarly mind needs something to stimulate its imagination continuously or, as Planck said, to point in the direction of the most fruitful search. The domain of life-phenomena represents a very special case in this respect. For, as we have seen in this section, life is manifested by an entropic process that, without violating any natural law, cannot be completely derived from these laws—including those of thermodynamics! Between the physico-chemical domain and that of life there is, therefore, a deeper cleavage than even that between mechanics and thermodynamics. No form of cause that may fit other phenomena could do for the sciences of life. The final cause—that is, purpose—is not only in its right place in these sciences but it also con-

[86] W. Ostwald, *Vorlesungen über Naturphilosophie* (Leipzig, 1902), p. 328. Quoted in A. J. Lotka, "Natural Selection as a Physical Principle," *Proceedings of the National Academy of Sciences*, VIII (1922), 151.

stitutes an indispensable and extremely useful tool of analysis. A biologist or a social scientist has to be a "vitalist" and, as a result, to be in the habit of looking for a purpose. It is all right for an economist to rest satisfied with the explanation of a catastrophic crop by some efficient causes triggered by random events. However, the science served by him is ordinarily interested in problems involving human actions. And if an economist wishes to analyze the actions of those who tilled the soil and cast the seeds, or of all those who have been hit by the scarcity produced by the crop failure, he will not arrive at a penetrating understanding if he refuses to look for the purposes that move them. For the truth that cannot be obliterated by the current behavioristic landslide is that we all—the fans of behaviorism included—act from a purpose.

And one complete circle is now closed by recalling that all our important purposes—namely, to stay alive and to keep a place under the social sun—lead to entropic transformations of our neighboring universe. This means that the realization of our purposes sets us on a never-to-return journey.

CHAPTER VIII *Evolution versus Locomotion*

1. Irreversible and Irrevocable Processes. The idea that the life process can be reversed seems so utterly absurd to almost every human mind that it does not appear even as a myth in religion or folklore. The millenary evidence that life goes always in only one direction suffices as proof of the irreversibility of life for the ordinary mind but not for science. If science were to discard a proposition that follows logically from its theoretical foundation, merely because its factual realization has never been observed, most of modern technology would not exist. Impossibility, rightly, is not the password in science. Consequently, if one cornerstone of science is the dogma that all phenomena are governed by mechanical laws, science has to admit that life reversal is feasible. That the admission must cause great intellectual discomfort is evidenced by the fact that, apparently, no scholar of the Classical school made it overtly. Classical thermodynamics, by offering evidence—valid according to the code of scientific court procedure—that even in the physical domain there are irreversible processes, reconciled science's stand with generally shared common sense. However, after statistical mechanics began teaching, with even greater aplomb than Classical mechanics, that all phenomena are virtually reversible, universal reversibility became the object of a prominent controversy. From physics, the controversy spread into biology where the issue is far more crucial.

From the discussion of statistical thermodynamics (Chapter VI), we can expect the controversy to be highly involved. Unfortunately, it has been further entangled by the fact that "reversibility" has been used with different meanings by different authors and, hence, often with another meaning than in mechanics. There, a process is said to be *reversible* if and only if it can follow the same course phase by phase in the reverse order. It is obvious, however, that this is not the sense in which the term is used, for example, in Joseph Needham's argument that biological phenomena are reversible because protein micellae "are continually broken down and built up again."[1] Actually, the process of this illustration is irreversible according to the terminology of mechanics.

One source of this confusion is that only two terms, reversible and irreversible, are commonly used to deal with a situation that really is trichotomous. For the relevant aspects of a process call for the division of nonreversible phenomena—to use the stringent form of logical negation—into two categories.

The first category of "nonreversibility" consists of all processes which, though not reversible, can return to any previously attained phase. The flow of vehicles in a traffic circle comes immediately to mind, but the process of a tree's growing and losing its leaves each year seems a more instructive illustration. Processes such as these are nonreversible but not irrevocable. We may refer to them simply as *irreversible*. No doubt, in the saying "history repeats itself," history is conceived as an irreversible process in this narrow sense.

The second category of "nonreversibility" consists of processes that cannot pass through a given state more than once. Of course, such a process is nonreversible, but *irrevocable* better describes its distinctive property. The entropic degradation of the universe as conceived by Classical thermodynamics is an irrevocable process: the free energy once transformed into latent energy can never be recuperated.

Another source of confusion about reversibility lies in the concept of process itself. Strange though it may seem, the process of the entire universe is a far more translucid concept than that of a single microorganism. The mere thought of a partial process necessarily implies some slits cut into the Whole. This, as we have already seen, raises inextricable problems. But at least we should not lose sight of where we intend the seams to be cut. It matters tremendously whether the process in Needham's illustration includes the life of a single protein micella or of an unlimited number. For in the first case, there are good reasons for regarding the process as irrevocable; however, the second process is unquestionably irreversible.[2]

[1] Joseph Needham, "Contributions of Chemical Physiology to the Problem of Reversibility in Evolution," *Biological Reviews*, XIII (1938), 225.

[2] The argument typified by Needham's article clearly refers to the latter process. Its fault is obvious: from the fact that this process is not irrevocable, it concludes that it is reversible.

2. Evolution, Irrevocability, and Time's Arrow. It is because science began to speak of evolution first in connection with biological phenomena that by evolution we generally understand "*the history of a system undergoing irreversible changes*."[3] (Actually the world should be "irrevocable.") The existence of evolutionary laws in nature depends then upon whether there are irrevocable phenomena: the existence of only irreversible phenomena—in the narrow sense—does not suffice. All the stronger, therefore, is the negation of evolutionary laws by the universal reversibility proclaimed by statistical mechanics. Many a scientist was thus induced to argue that evolution is appearance: a phenomenon may or may not appear evolutionary depending upon the angle from which we view it or upon the extent of our knowledge.

An epitome of this relativist position is Karl Pearson's argument that to an observer traveling away from the earth at a greater speed than light, events on our planet would appear in the reversed order to that in which they have actually occurred *here*.[4] The fact that since Pearson wrote we have learned that the speed of light cannot be exceeded does not destroy the gist of his argument. The gist is that evolution is appearance because any movie can be projected in two ways, "forward" and "backward." If this is all he meant, Pearson did not intend to deprive Time of any order whatsoever. He only challenged the existence of an objective time's arrow. To see clearly the difference, we may note that Pearson's argument does not deny but implies that events have the same structure as the points on an indefinite straight line. They are, therefore, subject only to the triadic relation of betweenness: given three events, one is necessarily between the other two. In this case, if a movie film of nature is cut into its individual frames and the frames shuffled, an imaginary spirit knowing all the *objective* laws of nature but free from the unidirectional illusion of Time will be able to reconstruct exactly the entire film. Pearson's position is tantamount to claiming that this spirit will insist that nothing in nature can determine in which direction it should be projected; hence, if we think that nevertheless one direction is the right representation of nature, we are superimposing our illusion upon the objective nature.

To use a plastic image, we can say that Pearson viewed reality as an endless alley of events out of our anthropomorphic Time. This means that our feeling of Time arises from the fact that, for some unknown reason, we all stroll along the alley in the *same* direction. For this position, the problem of evolution still exists but in a different sense. An evolutionary

[3] Alfred J. Lotka, *Elements of Physical Biology* (Baltimore, 1925), p. 24.

[4] Karl Pearson, *The Grammar of Science* (Everyman's Library edn., London, 1937), pp. 343 f.

law in this sense will have to place an event between two other events. It should provide us with a criterion for placing the emergence of mammals, for instance, between the advent of man and the appearance of the first fish. Only, the notion of an event as the effect of a cause in *the past* will become meaningless. Instead, we may very well say that every event is the result of two cause-events, for we would then need two events to locate its position on the spatialized Time. Nature would be out of Time and the words reversible or irreversible would lose all objective meaning in connection with it. Even the duality of Time would become senseless.

A different view of the duality of Time is that a time's arrow exists always in nature; only, its point is reversed periodically. Nature would go in cycles from A to Z and back from Z to A. Both ways of running the movie would represent the objective mode of being of things. This view of Time is part and parcel of the mechanistic position and, as we have seen, it was defended by Boltzmann in connection with his interpretation of thermodynamical phenomena.[5] But as Eddington caustically remarked, those who cling to this "wholly retrograde" idea of cyclical Time should teach not only evolution but also "anti-evolution"[6] or the laws of a world in which life would start with death and end with birth. Of course, the complete negation of Time as propounded by G. N. Lewis[7] (an unavoidable consequence of the introduction of probability in thermodynamics) has a perfect alibi in this respect. According to that view, even a demiurgic mind could not reconstruct the movie from the unconnected frames. Reality is not even an alley of events out of anthropomorphic Time. Events form a nonlinear scatter in which they appear with various frequencies. Anthropomorphic Time is like a stroll through a forest during which the hiker, naturally, encounters more often the trees that are more frequent there. But nothing prevents him from running constantly into some that have a fantastically small frequency.

The truly unique merit of Classical thermodynamics is that of making perfectly clear the problem of Time in relation to nature. A basis of Time in nature requires this: (1) given two states of the universe, S_1 and S_2, there should be one general attribute which would indicate which state is later than the other, and (2) the temporal order thus established must be the same as that ascertained by a single or collective human consciousness assumed to be contemporary with both S_1 and S_2. It is elementary then that, since the stream of consciousness moves only "forward," the corresponding attribute must reflect an irrevocable process. The alternative position that there is nothing in nature that parallels the advance of

[5] L. Boltzmann, *Lectures on Gas Theory* (Berkeley, 1964), p. 446.

[6] A. S. Eddington, *New Pathways in Science* (Ann Arbor, 1959), pp. 53–59.

[7] Chapter VI, Section 4, above.

consciousness in Time—that the mode of being of nature is an unorganized mass of events—if strictly maintained, would negate most of what we now call science. Science may be a very imperfect product of man's effort to grasp nature with his mind, but the negation of Time would turn it into a fantasy of no operational value in the laboratory or elsewhere. On meeting an Eskimo of fifty in an isolated glacial land, a scientist could no longer conclude that this land had suffered no cataclysm in the last fifty years.

Actually, without a time's arrow even the concept of mechanical reversibility loses all meaning. The tables should, therefore, be turned. It behooves the side claiming that evolution is a relative aspect to show how, if there is no irrevocable process in nature, one can make any sense of ordinary temporal laws. To return to the movie analogy, a movie of a purely mechanical phenomenon—say, the bouncing of a *perfectly elastic* ball—can be run in either direction without anyone's noticing the difference. A biologist, however, will immediately become aware of the mistake if a movie of a colony of protein micellae is run in reverse. And everyone would notice the error if the movie of a plant germinating from seed, growing, and in the end dying, is run in reverse. However, that is not the whole difference. If the frames of each movie are separated and shuffled, only in the last case can we rearrange them in *exactly* the original order. This rearrangement is possible only because the life of a single organism is an irrevocable process. As to the other two processes mentioned, the first is reversible, the second irreversible.

Two important observations should now be made. First, if the movie of the micellae is irreversible it is because the process filmed consists of a series of overlapping irrevocable processes, the lives of the individual micellae. Second, if the first two movies have in the background an irrevocable process—say, that of a single plant—then their individual frames too can be rearranged immediately in the exact original order. The point is that only in relation to an irrevocable process do reversibility and irreversibility acquire a definite meaning.

3. From Part to Whole. An outsider may be surprised to see that the debate concerning the issue of Classical vs. statistical thermodynamics turns around the prediction each theory makes about the fate of the universe. The initiated knows that the reason is that no difference exists between the final formulae of the two theories. Physicists work equally well with either, according to individual preferences: the literature covers both. But since an acid test of any prediction concerning the fate of the entire universe is well beyond our reach, opinions on which of the two theories is more verisimilar have been influenced mainly by the subjective intellectual appeal of each prediction. However, neither the picture of a universe irrevocably racing toward a Heat Death nor that of a universe devoid of temporal order seems particularly attractive. Undoubtedly, it is equally hard to admit that the Gods could create but a finite Existence or that, as Einstein once said, they only play dice continuously.[8]

Law extrapolation is the very soul of cosmology. However, the extrapolation of the Entropy Law—Classical or statistical—to the cosmic scale is particularly vulnerable because very likely the error thus committed is of a qualitative nature. Bridgman, who favors the Classical approach, has set forth some reasons to challenge the cosmic application of the Entropy Law. Moreover, he admitted—just as did Boltzmann, the founder of statistical mechanics—that in some sectors of the universe and for some periods of time entropy may very well decrease.[9] Perhaps still more interesting is one thought of Margenau's. He raised the question of whether even the Conservation Law applies to the entire universe: "If creation of matter-energy takes place . . . all our speculations [about the fate of the universe] are off."[10]

All these thoughts already seem prophetic, for they concur with the recently ventilated hypothesis—mentioned in Chapter III, Section 1—that matter is continuously created and annihilated. From this hypothesis there emerges a universe that neither decays irrevocably nor is deprived of temporal order. It is a universe consisting of a congregation of *individual worlds*, each with an astronomically long but finite life, being born and dying at a constant average rate. The universe is then an everlasting steady state which, like any stationary population, does not evolve.[11] Not only its total energy but also its total entropy must remain constant, or nearly so. This new cosmological conception should not be confused with Boltzmann's speculative view of the universe as being in thermal equilibrium forever, that is, as having a constant entropy.[12] For according to

[8] See note 14 of the preceding chapter.

[9] P. W. Bridgman, *The Nature of Thermodynamics* (Cambridge, Mass., 1941), pp. 148 ff; Bridgman, *Reflections of a Physicist* (2nd edn., New York, 1955), pp. 263 ff. For Boltzmann see note 69 in Chapter VI, above.

[10] H. Margenau, *The Nature of Physical Reality* (New York, 1950), p. 283.

[11] This cosmological hypothesis was first suggested by H. Bondi and T. Gold in England and A. Vorontzov-Velyaminov in the U.S.S.R. and then championed by Fred Hoyle. See F. Hoyle, *The Nature of the Universe* (New York, 1950) and *Astronomy* (New York, 1962); H. Bondi, *The Universe at Large* (Garden City, N.Y., 1960). A highly fascinating discussion of this hypothesis (completed with the annihilation of matter) is offered by Reginald O. Kapp, *Towards a Unified Cosmology* (New York, 1960). For a defense of the older hypothesis, proposed by Abbot G. E. Lemaître, see G. Gamow, *The Creation of the Universe* (New York, 1952), especially pp. 25–28. According to Lemaître, the present universe was created some fifteen billion years ago by a "Big Bang" from a very small nucleus in which the whole matter-energy was concentrated with a density beyond imagination.

[12] L. Boltzmann, "On Certain Questions of the Theory of Gases," *Nature*, LI (1895), 415.

Boltzmann the constancy of the entropy of the universe derives from the fact that the increase in entropy in some parts of the universe, such as that in which we happen to live, is exactly compensated by the decrease of entropy in the remaining parts. In these last parts, heat flows from the colder to the hotter bodies. The square dance moves along, however, and in our part of the universe entropy may at any time start to decrease. Actually, it must do so at some time in the future.[13] According to the view based on the hypothesis of matter creation and annihilation, low entropy is ipso facto continuously created and high entropy annihilated.[14] It is by this process that the entropy of the universe is kept constant. But the *existing* entropy never decreases in any part of the universe. Our solar system, for example, tends definitely toward Heat Death, ultimately toward annihilation. Another solar system may take its place, but not by a pendulum movement of entropy.

In this picture, a time's arrow must come from some individual component if from anything. We are thus back to one of the oldest tenets. What is everlasting cannot evolve (change); evolution is a specific trait of that which is born and dies. In other words, evolution is the process that links birth to death: it is life in the broadest sense of the term. Witness the fact that even the whole universe must have a transient life between Creation and Heat Death if it is to be an evolving entity as pictured by the Classical Entropy Law.

The transparent principle that death is later in time than the birth of the *same* individual—be it a galaxy, a biological species, or a microscopic cell—does not suffice, however, to establish a complete chronology even if we beg such troublesome questions as whether birth and death can be operationally recognized as point-events. For a complete chronology we need a continuous time's arrow of at least one category of individuals the lives of which overlap without interruption.[15] If such a time's arrow can be found, then the cosmic panorama is as simple as our movie of protein micellae: the process of the entire universe is unidirectional, i.e., irreversible, because that of its individual members is irrevocable.

[13] In this connection it is instructive to mention a more specific view advocated by the Swedish chemist, Svante Arrhenius: the universe is a steady-state because "the entropy increases in the suns, but decreases in the nebulae." See H. Poincaré, *Leçons sur les hypothèses cosmogoniques* (Paris, 1911), p. 252, where this and other old cosmological speculations are critically evaluated.

[14] To dispose of a likely disconcerting thought, let me hasten to add that according to the calculations of W. H. McCrea, "The Steady-State Theory of the Expanding Universe," *Endeavour*, IX (1950), 8, the steady-state requires that merely one atom of hydrogen per gallon of space be created once in every 500,000,000 years! Obviously, a phenomenon such as this cannot be caught by any conceivable instrument, all the less by the naked eye.

[15] This condition should be related to the manner in which the historical consciousness is formed, as explained above in Chapter V, Section 5.

4. Evolution: A Dialectical Tangle. Paradoxical though it may seem, the evolution of the simplest microorganism raises far more formidable issues than that of the whole universe. The Whole needs no boundaries to separate it from its Other, for there is no other Whole. And since there is no Other, we need not ask what sameness means for the Whole. A partial process, on the other hand, requires some conceptual cuts in the Whole. Cutting the Whole, as I have observed earlier, creates endless difficulties.

To begin with, across any boundary we may draw in space, time, or society, there is some traffic between the two domains thus separated. Hence, we get three partial processes instead of two, a contrariety to which little attention has been paid. The widespread practice is to ignore completely the processes one initially intended to separate and to reduce the whole picture to the traffic across the boundary. This flow-complex, as I have called it (Chapter IV, Section 4), clearly throws away the baby with the bath water.[16]

The preceding remarks are borne out by the fact that the *isolated* system has ultimately become the unique reference for all propositions of theoretical physics. Of course, this manner of circumventing the difficulties of a partial process was made possible only because a physical universe can be reproduced in miniature and with some satisfactory approximation in the laboratory. Other disciplines are not as fortunate. Biologists too have experimented with isolated processes containing some organisms together with a part of environment. However, the great difference is that such an isolated process is far from being a miniature simulation of the actual process.

Experimenting with isolated systems in biology has reconfirmed—if reconfirmation was needed—that the evolution of the biosphere necessarily implies the evolution of the environment. To speak of biological or social evolution in a nonevolutionary environment is a contradiction in terms. *Ceteris paribus*—the indispensable ingredient of every physical law—is poison to any science concerned with evolutionary phenomena. Evolutionary changes cannot be seen except in an isolated, at least quasi-isolated, system. Perhaps in some domains it might be unscientific to experiment with wholes because, as Popper argues, we cannot thus impute effects to individual causes.[17] That does not apply, however, to evolution which is inseparable from the Whole. Witness the fact that the only case in which we were able to formulate an evolutionary law is that of the whole universe.

[16] More on this in Chapter IX, below.

[17] Karl R. Popper, *The Poverty of Historicism* (Boston, 1957), p. 89. The idea is that you cannot find out who drinks your whiskey—your butler or your cat—if your experiment includes both.

All efforts to discover a time's arrow in the life (evolution) of a single organism or a species considered in isolation have remained vain. Beyond the intricate qualitative aspects of such lives, biology has found only a dualist principle: growth and decay, or anabolism and catabolism. To be sure, both anabolism and catabolism consist of physico-chemical processes, but the dualism comes from the fact that the two phases are not governed by the same category of laws.[18] And though we know that during growth anabolism exceeds catabolism and that the reverse happens during decay, there is no purely physico-chemical explanation of the reversal.[19] As pointed out recently by Medawar, even death is a physico-chemical puzzle.[20] Man's intuition in all times and places seems right then in feeling that death is so much more mysterious than life. Perhaps this is so because man is aware of death, but no consciousness—that ultimate authority of knowledge—can bear witness to what death is.

According to the explanation which was outlined in the last section of the preceding chapter (and which now seems generally accepted), an organism *căn* maintain itself in a steady state by sorting and sucking low entropy from the environment without violating any physico-chemical law. Why should then an organism age and ultimately die? We can understand why a species may die: the environment may no longer be fit for it. But this explanation does not apply to a single organism living in an environment which still can provide an ample amount of low entropy of the right kind. To explain the phenomenon of death by merely proclaiming that *entropy* must always be victorious in its battle with *ektropy*, as Auerbach did,[21] is to turn around verbally a query into an explanation. Besides, even if taken at its face value the principle is refuted by the case of a protozoa which seems to be able to thwart the victory of entropy. A protozoa perpetuates itself eternally (i.e., as long as it is not cut off from its adequate environment) by mere asexual division into two. Also, a gene perpetuates itself in unadulterated form as long as it is not hit by a mutating factor. And it would not do to hedge the issue of death by arguing that an amoeba, as an individual, dies through division and two new individuals are thereby born.[22]

[18] The burning of sugar in a biological structure is, no doubt, a physico-chemical process; yet only in such a structure can it take place without burning the whole structure at the same time. Moreover, some biochemical processes go in the "wrong" direction of the reaction. Cf. H. F. Blum, *Time's Arrow and Evolution* (Princeton, 1951), p. 33 and *passim*; L. von Bertalanffy, *Problems of Life* (New York, 1952), pp. 13 f. More on this in Appendix G in this volume.

[19] Bertalanffy, *Problems of Life*, pp. 136 f.

[20] P. B. Medawar, *The Uniqueness of the Individual* (New York, 1958), chaps. i–ii.

[21] Felix Auerbach, *Die Grundbegriffe der modernen Naturlehre* (3rd edn., Leipzig, 1910), p. 149.

[22] As G. G. Simpson argues in *The Meaning of Evolution* (New Haven, 1949), p. 192n. By the same token, we could say that even in the sexual reproduction the mother after birth is not the same individual as before.

Aging and death cannot be taken for granted once we have recognized that an organism is an entropic steady-state. On the other hand, the issue does not involve the accidental death by physical, chemical, or biological onslaught. Glasses—the analogy used by Medawar—"die" by physical accidents. The issue is whether glasses as well as living organisms are also prone to natural death, a death that would come about inevitably even in the absence of any accidents. Medawar, together with most biologists who have dealt with this problem, is of the opinion that all deaths are at bottom accidental. Potentially, all organisms are immortal. But he adds a highly interesting detail that describes what aging actually is. The same accident is more likely to cause the death of an organism grown in years than of a young one.[23] The explanation is that every accident that does not bring about the death of the organism leaves on it scars that increase its vulnerability at the next accident. One may survive one heart attack, occasionally two or three, but hardly one score. Every frowning adds new invisible wrinkles and deepens the older ones. It thus diminishes the efficiency of the functions of the skin. Aging is to grow more and deeper "wrinkles" as the organism is continuously exposed to accidents. Chance, we could then say, gradually undermines anti-chance until it ultimately prevails over it in the death of the organism.[24] Aging is nothing but the cumulative effect of causes acting in Time; in other words, it is a hysteresis process. So, we may join the Jain dialectical philosopher in saying that "man begins to die at birth." Whatever we do, the explanation of aging presupposes the existence of an objective Time in its unidirectional essence. Otherwise, death should precede or follow birth indifferently.

As expected, entropy does enter into the picture but not as a time's arrow: it decreases during growth and increases during decay. Therefore, even if we were able to determine the entropy level of an organism we still could not say which of two states is earlier. We would also have to know whether entropy is increasing or decreasing in each situation. But this knowledge already presupposes a time's arrow.

The number of biochemical phenomena expressed by numerical formulae is continually increasing, but none of these formulae offers a basis for a biological time's arrow.[25] This, without much doubt, is why no description of an individual or collective organism is complete and meaningful unless these quantitative results are, first, related to the stream

[23] This proposition should not be confused with the obvious truth that *even without aging* the probability at birth that an organism should reach age x decreases with x.

[24] Time and again, one explanation begets additional queries. Very likely, those who share Medawar's view would say that if the above law does not apply to the amoeba it is because the amoeba gets rid of all the scars by a sexual rebirth before they become too portentous. But now the problem is to find a reason for the exception, if there is one. To acknowledge the brute fact would not suffice.

[25] For a very instructive—by necessity somewhat technical—analysis of this problem see Blum, *Time's Arrow and Evolution*.

of consciousness and, then, cemented into a single picture by an immense dose of quality. For biology, and even more for a social science, to excommunicate dialectical concepts would therefore be tantamount to self-imposed paralysis.

5. Evolution Is Not a Mystical Idea. The preceding analysis was intended only to pinpoint the epistemological difficulties of the concept of evolution and their reflection upon the study of evolutionary processes. Nothing is further from my thought than to suggest thereby that evolution is a mystical concept. To make this point clear, let me return to the picture of the universe as a steady population of evolving individual worlds, a picture which, I believe, is intellectually far more satisfying than its alternatives.

Certainly, this picture no longer compels us to believe in *absolute* novelty. For in a steady state nothing fundamentally new can happen: essentially the same story is told over and over again by each transient world. In such a universe there is nevertheless evolution, but in a different sense than the term has for the biologist. The tenet in biology is that only an aggregate of similar but not identical individuals, i.e., a species, can evolve; an individual never evolves, it only comes into existence, lives, and dies. In the sense proposed here evolution is reflected in the life of *any* individual part of the universe, be it a galaxy, a species, or a minuscule worm. It is the process that links a birth to a death in each of the countless cases of generation and annihilation that occur continuously in nature. And if an analogy may help clarification, the steady-going universe may be likened to a vast but isolated traffic circle which, while remaining on the whole identical to itself, harbors enough hustle and bustle to dazzle man's intellect. For a nonevolving universe we need no longer assume that the laws of nature change over Time, some applying only before ylem turned into matter, others only thereafter. Complete knowledge no longer constitutes the exclusive privilege of a divine mind capable of discerning in the protogalactic ylem the distant emergence of man, nay, of superman. A demon having only an *ordinary* mind deprived of any clairvoyance, but lasting millions of eons and capable of moving from one galaxy to another, should be able to acquire a complete knowledge of every transient process, just as a biologist can arrive at a description of the typical life of a new strain of bacteria after observing a large number passing from birth to death. The principle "what holds on the average for one holds for all" would apply in both cases. However, if the laws of nature are infinite in number—as I contend they are—even our imaginary demon could not perform the super-task of learning them all in a finite, albeit vast, stretch of time. We would have to fall back on a divine mind.

But, perhaps, the exceptional properties with which we have endowed our demon violate other (unknown) laws of nature, so that its existence is confined to our paper-and-pencil operations. Be this as it may, even the most optimistic expectations do not justify the hope that mankind might in the end fulfill the exceptional conditions with which we have endowed our demon. With a life span amounting to no more than a blink of a galaxy and restricted within a speck of space, mankind is in the same situation as a pupa destined never to witness a caterpillar crawling or a butterfly flying. The difference, however, is that the human mind wonders what is beyond mankind's chrysalis, what happened in the past and, especially, what will happen in the future. The greatness of the human mind is that it wonders: he "who can no longer pause to wonder and stand rapt in awe"—as Einstein beautifully put it—"is as good as dead."[26] The weakness of the human mind is the worshiping of the divine mind, with the inner hope that it may become almost as clairvoyant and, hence, extend its knowledge beyond what its own condition allows it to observe repeatedly.

It is understandable then why the phenomena that man can repeatedly observe exercise such an irresistible fascination for our minds while the idea of a unique event causes intellectual discomfort and is often assailed as wholly nonsensical. Understandable also is the peculiar attraction which, with "the tenacity of original sin" (as Bridgman put it), the scientific mind has felt over the years for all strains of mechanistic dogmas:[27] there is solace in the belief that in nature there is no other category of phenomena than those we know best of all. And, of course, if change consisted of locomotion alone, then evolution would be a mystical notion without place in scientific knowledge. However, as we have seen through some of the preceding pages, it is far more mystical to believe that whatever happens around or within us is told by the old nursery rhyme:

> Oh, the brave old Duke of York
> He had ten thousand men;
> He marched them up to the top of the hill,
> And he marched them down again.
> And when they were up, they were up,
> And when they were down, they were down,
> And when they were only half-way up,
> They were neither up nor down.[28]

[26] Quoted in *The Great Design*, ed. F. Mason (New York, 1936), p. 237.

[27] P. W. Bridgman, *The Logic of Modern Physics* (New York, 1928), p. 47.

[28] *The Oxford Dictionary of Nursery Rhymes* (Oxford, 1951), p. 442. My initial source is A. S. Eddington, *The Nature of the Physical World* (New York, 1943), p. 70.

If evolution of large organizations and, especially, of our own species seems somewhat of a mystery, it is only for two reasons: first, not all natural phenomena follow the pattern of the rhyme, and second, the condition of mankind is such that we can observe nature only once, or more exactly, only in part. This is the only fundamental reason why evolution is "the Problem of Problems" as G. G. Simpson labels it—and why men can hope only to grasp it in a very imperfect way. Even set in this modest manner (as it should be), the task is no less than titanic. No wonder then that man has approached it with a variety of tools different from those used for the study of phenomena he can observe over and over again. Whether in biology or sociology, students of evolution have generally looked for historical trends. Admittedly, the existence of historical trends raises some intricate problems, a reason why they are rejected not just by logical positivists. But, to recall a point made earlier, the regularity imputed to nonevolutionary phenomena (which passes as the indisputable cornerstone of objective science) shares the same epistemological basis with all historical trends. Is there any weighty reason for raising the *horizontal* historical trend on such a high pedestal while refusing to grant others any recognition? I know of none and, myself, cannot conceive of any. Besides, the discrimination is logically self-defeating. For, once we accept the validity of horizontal trends, we must extend it to any linear trend: the trend of the slope of a linear trend is a horizontal trend. Next, we must accept as valid any trend represented by a second-degree parabola because its slope has a linear trend. The logic of this algorithm compels us to accept all trends that are represented by an *analytical* function.

It is a highly plausible surmise that most of the adversaries of historical trends would ultimately concede the existence of analytical but not of nonanalytical trends. In support of this position they may even invoke the characteristic property of an analytical function, which is that the function can be completely determined from the knowledge of its values over a finite interval *however small*. Consequently, they may say, the analytical trend has an objective existence because we can determine it completely by observing the corresponding phenomena during only one second (conceivably). This is, to be sure, an objective position. But, as is usually the case with such impeccable dictions, there is a stumbling block, a question that should disturb any alert student of statistics: how can one ascertain that the trend of an evolving phenomenon is analytical throughout? Or to cite a subsidiary question: do we have any reasons for assuming *ex ante* that there is a category of evolutionary phenomena that follow an analytical trend? There is an obvious kinship between the property of analytical functions and Georges Cuvier's famous tenet that an individual animal is so constructed that we can reconstruct it from the knowledge of a single vertebra. But Cuvier had studied thousands of *complete* animals before he arrived at and used his tenet. Unlike Cuvier, we have only meager and fragmentary records of evolving entities both in biology and social sciences. Besides, biologists know nowadays that the tenet is not as faultless as Cuvier thought. Astronomers are still struggling with the physical evolution of a single nebula. After the numberless fiascos in predicting evolutionary phenomena (some as simple as the evolution of nations' populations or of the value of money) by analytical formulae, the sensible conclusion is to accept the postulate that evolutionary laws are not amenable to analytical expressions.

Nature may be such that all its evolutionary laws are laws of Change, one for each type of phenomena and expressed as a mere succession of qualities (some measurable, some not), such as is exemplified by the manner a biologist describes the life-pattern of, say, an insect. The biologist's predicament of being unable to reduce this pattern to a chain of ordinary causal laws of the kind used by the physicist reflects, I believe, the normal situation in nature, rather than the exception. There is a great deal to be said in favor of the point (made in the preceding chapter) that our inability to discover an efficient cause for everything does not prove that nature is lawless. But it does suggest that nature is not governed by analytical laws in all respects. The result is crystallized in Simpson's informed conclusion that "evolution is neither wholly orderly nor wholly disorderly."[29] Moreover, out of all his consummate argument one point detaches itself with complete clarity: the evolution is disorderly because of the repeated instances in which "A follows B" is actual only for one category of individuals. In such a situation, the most we can do is to bend our efforts to discover historical trends in spite of the difficulty and uncertainty of the task. The key to the Law of Existence may be likened to a number of infinitely many decimals and of a complex nature. All means are good as long as they hold out some reasonable hope that they may enable us to discover the values of at least some decimals of that miraculous number. But the belief that the laws of locomotion alone will lead us to discover all the decimals is both inept and foolhardy.

There are, finally, some who have seen an irreducible paradox of infinite regression in the problem of evolution. The study of the evolution of human society, it is argued, includes the study of that study itself.[30] That there is a contradiction in any self-evolution study is beyond question. But in the absence of absolute novelty the concept of evolution involves no paradox, as can be easily seen from the fact that any human

[29] Simpson, *Meaning of Evolution*, p. 185.

[30] E.g., Popper, *Poverty of Historicism*, p. 80 and *passim*.

can learn a lot about his own life by observing other humans during various phases of the same life pattern. The predicament of any evolutionary science derives from the fact that mankind has no access to observing other "mankinds"—of which there must be a multitude at all times in nature if the universe is a steady-going concern subject to Timeless laws.[31]

[31] At the time this chapter was originally written as a part of AE (in 1963), Fred Hoyle had not renounced the cosmological hypothesis of a steady-state universe. In spite of his renunciation—which in a sharp form came only in his "Recent Developments in Cosmology," *Nature*, CCVIII (October 1965), 113—I have not deemed necessary to alter the argument of this section. My reasons are several. First, my main point that in the universe there is evolution in my own sense and that this evolution is not reducible to locomotion is independent of whether the universe is in a steady state, or an expanding Whole born by a "Big Bang," or a closed system oscillating between one "Big Bang" and one "Sudden Fsss." Second, only in the hypothesis of one single "Big Bang" would my imaginary demon be unable to discover the evolutionary laws still unfolded and evolution would remain a "mystery" for him just as it is for a human. But this hypothesis does not go well with the Einsteinian notion of a finite universe. Thus, the only hypothesis competing with that of a steady state is that the universe is an oscillating system. On the other hand, from all that physicists know, oscillations must damp out; hence, the universe should ultimately come to rest "in an intermediary static state," as Hoyle now maintains ("Recent Developments in Cosmology"). Third, a quick perusal of H. Poincaré, *Leçons sur les hypothèses cosmogoniques*, should convince anyone that cosmological hypotheses are prone to having only very short vogues. (See also the pessimistic remarks in Fred Hoyle, *Galaxies, Nuclei, and Quasars*, New York, 1965, p. 24.) The hypothesis of a nonevolving universe had other spells of vogue in the past when it was supported by other arguments—we may recall the hypothesis of Arrhenius mentioned earlier. I venture the thought that it is our cosmological hypotheses that surely oscillate and that we shall again return to that of a steady-state universe. There is a major reason for this: the hypothesis of a steady state, as I have observed earlier, is intellectually the most satisfying of all. [A recent survey article, which came to my attention only after these pages had been set in type, suggests that the situation is in general as I had envisioned it. See G. de Vaucouleurs, "The Case for a Hierarchical Cosmology," *Science*, February 27, 1970, pp. 1203–1213.]

CHAPTER IX *The Analytical Representation of Process and the Economics of Production*

1. The Partial Process and Its Boundary. Occasionally, the use of a term spreads through the scientific literature with amazing swiftness but without a valid birth certificate, that is, without having been defined in some precise manner. Actually, the swifter the spreading, the greater is everyone's confidence that the meaning of the term is perfectly clear and well understood by all. One of the most glaring examples of this state of affairs is supplied by "process." It must be admitted, though, that process is a particularly baffling concept, for process is Change or is nothing at all. And as we have seen in Chapter III the intricate issues surrounding the idea of Change have divided philosophers into opposing schools of thought, one holding that there is only Being, the other that there is only Becoming. Science, however, can follow neither of these teachings. Nor can it follow the dialectical synthesis of the two into Hegel's tenet that "Being is Becoming." Science can embrace only the so-called vulgar philosophy according to which there is both Being and Becoming, for by its very nature it must distinguish between object and event. In other words, science must try to remain analytical throughout, even though, as I have argued earlier, it cannot succeed in this forever. The upshot is that science must have a clear idea of how to represent a process *analytically*. Failure to do so before the game starts is apt to become a source of important errors. In physics, we may remember, the opposition between particle and wave in quantum phenomena compelled the physicists to become more careful in interpreting observed processes. In social sciences—especially in economics where the paper-and-pencil arguments generally have only a remote contact with actual data—"process" is an abused term: it is used to denote almost anything one pleases. Witness the variety of mathematical formulae by which such a basic element of economic theory as the production process is represented. Witness, too, the practically total lack of concern for what the symbol-letters of these formulae stand for in actual terms.[1]

In approaching the problem of how to describe a process analytically, we should note that we must go along with the dialectics of Change on at least one point: Change cannot be conceived otherwise than as a relation between one thing and "its other" (to use Hegel's convenient terminology). To explain: in viewing a tree as a process we oppose it in our thought to everything that is not that tree even though we may not be fully conscious of this opposition all the time. Only for the absolute totality—the entire universe in its eternity—Change has no meaning; nothing corresponds to "its other." There certainly is Change within such a totality, but in order to discover it we must get inside, so to speak. More exactly, we must divide the totality into parts, into partial processes. The notion of a partial process necessarily implies some slits cut into the seamless Whole with which Anaxagoras identified actuality.[2] It is at this point that the dialectical thorns of the idea of partial process come to be appreciated even if we do not wish to go too deep into dialectics. Hardly anyone would deny that a living organism is a partial process; most would leave it at that. Yet, as Bohr reminds us, it is nigh impossible to say in every case whether a particular atom of the totality belongs to the organism in question or to "its other."[3] Economists, too, should be aware of the difficulty in deciding whether a truck hired by company A from company B and riding on some highway loaded with goods for company C is part of the activity of A, of B, or of C. Or to cite a still more intricate case: is the hired worker in a capitalist system in essence owned by the capitalist, as Marx argued? We are here confronted with the same issue that opposes dialectical (in my own meaning) to arithmomorphic notions. Analysis cannot accept a penumbra between one individual process and "its other." For if it does, it must set it as another partial process ana then it ends with three partial processes instead of two. We would thus be drawn into an infinite regress.

One obvious conclusion of the foregoing observations is that analysis must, in this case as in all others, proceed by some heroic simplifications

[1] This imbroglio is exposed together with its symbiotic fallacies in my paper "Chamberlin's New Economics and the Unit of Production," chap. ii in *Monopolistic Competition Theory: Studies in Impact*, ed. R. E. Kuenne (New York, 1967), pp. 38–44.

[2] See Chapter III, note 28.

[3] Niels Bohr, *Atomic Physics and Human Knowledge* (New York, 1958), p. 10.

and totally ignore their consequences. The first such step is to assume that actuality can be divided into two slices—one representing the partial process determined by the topical interest, the second, its environment (as we may say)—separated by an analytical boundary consisting of an arithmomorphic void. In this way, everything that goes on in actuality at any time is a part either of the process in point or of its environment. The first element, therefore, that the analytical picture of a process must necessarily include is the analytical boundary. *No analytical boundary, no analytical process.* The point deserves emphasis because often we may catch ourselves in the act of speaking about a process without having the faintest idea where its boundary should be drawn. On such occasions we are simply abusing the term "process."

Precisely because the Whole has no seams, where to draw the *analytical* boundary of a partial process—briefly, of a process—is not a simple problem. Plato to the contrary, there are not even joints in actuality to guide our carving.[4] One may slice actuality anywhere one pleases. This does not mean that any boundary cut by mere whim determines a process that has some significance for science. Analysis has already compartmented the study of actuality into special fields, each one with its own *purpose*. So, every special science draws process boundaries where it suits its special purpose. Without an intimate knowledge of the phenomenal domain of chemistry, for instance, one would not know where to draw a compatible boundary. In other words, a relevant analytical process cannot be divorced from purpose and, consequently, is itself a primary notion—that is, a notion that may be clarified by discussion and examples but never reduced to other notions by a formal definition.

If we consider further the nature of the boundary of a process, one point should arrest our attention: such a boundary must necessarily consist of two distinct analytical components. One component sets the process against its "environment" at any point of time. For lack of a better term, we may refer to this component as the *frontier* of the process. We should be careful, however, not to let this term mislead us into believing that the frontier of a process is geographical, i.e., spatial. Thought itself is a partial process; yet one can hardly say that it is enclosed within a definite space. The same is true of numerous sociological or political processes. Nor should we lose sight of another difficulty: the process may be such that it alters its own frontier. But this difficulty is not insuperable provided that we grant the analyst the faculty of perceiving that an oak and the acorn from which it grew belong to the *same* process. And we could not possibly deny him this faculty without denying all articulation to knowledge in general.

[4] See Chapter III, note 29.

The boundary must also contain a temporal component, the *duration* of the process. We must specify the time moments at which the analytical process we have in mind begins and ends. In view of the fact that it is for the sake of science that nature is sliced into partial processes, the temporal component of any such process must necessarily be a finite time interval. It must begin at some $t_0 > -\infty$ and end at some $t_1 < +\infty$. For if $t_0 = -\infty$ we would not know all that has gone into the process and if $t_1 = +\infty$ all that it does. Extrapolation may be in order in some special cases, but to walk on firm ground we must start with a finite duration. For the same reason, the case of $t_0 = t_1$ should also be excluded from the category of analytical processes proper. To recall Whitehead's dictum, a durationless process, an event at an instant of time as a primary fact of nature, is nonsense. Like the everlasting process, the point-process is an analytical abstraction of the second order and, like it, can be reached only by approximation.

A process involves, above all, some happening. How to represent this happening analytically is our next problem. But two observations are necessary before we tackle this new task.

The first is that by deciding to identify a process by its boundary we have implicitly given up any thought of describing what happens within that boundary, that is, inside the process. Should we wish to learn something about what happens inside, we must draw *another* boundary across the process and thus divide it into *two* processes to be studied separately. These processes could not have been part of our analytical picture before the new boundary was drawn because of the simple principle "no boundary, no process." Conversely, if for some reason or another we need to focus our attention only on the process obtained by subsuming two processes into one, we must remove from the analytical picture the boundary separating them and *also everything connected with it*. Should we aim at a complete description of everything that happens inside a process, we shall be drawn into an infinite regress whose resolution uncovers the inherent vice of any plan to represent actuality by an analytical framework. Indeed, there is no end to the division of nature by one analytical boundary after another. The limit of this algorithm is an abstract matrix in which every process is reduced to a point-instant of the space-time. All partial processes will thus vanish from our ambitious portrait of actuality. In other words, analysis, after starting from the position that there is both Being and Becoming, is in the end saddled with a matrix in which neither Being nor Becoming exists any more. It is because of this paradox of analysis that we may rest assured that physics, whose aim is to get further and further inside matter, will always cling to the idea that matter is made of atomic, i.e., indivisible yet sizable, particles.

The second observation is that in saying that the duration of the process *begins* at t_0 and *ends* at t_1 we must take the underscored words in their strictest sense. At $t < t_0$ or $t > t_1$ the *analytical* process is out of existence. By this I do not mean that outside the duration we have chosen for an analytical process the corresponding part of actuality is inexistent. What I mean is that we must abstract from what may have happened in actuality before t_0 and from what will happen after t_1. The corresponding mental operation should be clear: an analytical process should be viewed in itself as a hyphen between one *tabula rasa* and another.

2. *The Analytical Coordinates of a Partial Process.* Because analysis must renounce the idea of including in the description of a process what happens either inside or outside it, the problem of describing the happening associated with a process reduces to recording only what crosses the boundary. For convenience, we may refer to any element crossing the boundary from the environment into the process as an *input* and to any element crossing it in the opposite direction as an *output*.[5]

At this juncture, analysis must make some additional heroic steps all aimed at assuming away dialectical quality. Discretely distinct qualities are still admitted into the picture as long as their number is finite and each one is cardinally measurable. If we denote the elements that may cross the boundary of a given process by $C_1, C_2, \ldots, C_m$, the analytical description is *complete* if for *every* C_i we have determined two nondecreasing functions $F_i(t)$ and $G_i(t)$, the first showing the cumulative input, the second, the cumulative output of C_i up to the time t. Naturally, these functions must be defined over the entire duration of the process which may be always represented by a closed time interval such as $[0, T]$.

The question of whether this analytical model is operational outside paper-and-pencil operations cannot be decided without an examination of the nature of the elements usually found in actual processes. Such an examination reveals that there always exists numerous elements for which either $F_i(t)$ or $G_i(t)$ is identically null for the entire duration of the process. Solar energy is a typical example of an element which is only an input for any terrestrial process. The various materials ordinarily covered by the term "waste" are clear examples of elements which are only outputs. In all these cases, we may simplify the analytical picture by representing each element by one coordinate only, namely, by

$$E_i(t) = G_i(t) - F_i(t). \tag{1}$$

For an output element, $E_i(t) = G_i(t) \geq 0$; for an input element, $E_i(t) =$

[5] In the above context the terms have a precise meaning, a fact that contrasts with the current practice in economics where they are used so loosely that we see them applied to services of capital and labor as well.

$-F_i(t) \leq 0$. The sign of $E_i(t)$ suffices to tell which is actually the case.

A second category of elements is typified by the Ricardian land, i.e., by land viewed only in its "original and indestructible powers." If we refer to the simple case of a process consisting of growing corn from seed on an acre of land, the coordinates of the Ricardian land are

$$(2) \qquad \begin{aligned} &F_\alpha(t) = 1 \quad \text{for } 0 \leq t \leq T; \\ &G_\alpha(t) = 0 \quad \text{for } 0 \leq t < T, \qquad G_\alpha(T) = 1. \end{aligned}$$

In the same example, we find that corn, too, belongs to this ambivalent category. As seed, corn is an input; as crop, it is an output. Thus, assuming that one bag of corn is used as seed and the crop is ten bags, we have

$$(3) \qquad \begin{aligned} &F_\beta(t) = 0 \quad \text{for } 0 \leq t < t', \qquad F_\beta(t) = 1 \quad \text{for } t' \leq t \leq T; \\ &G_\beta(t) = 0 \quad \text{for } 0 \leq t < T, \qquad G_\beta(T) = 10; \end{aligned}$$

where t' is the time of seeding.[6] One may cite numerous cases of the same nature. One particular example, which I shall use often later on, is supplied by the hammers used in hammering additional hammers. With the aid of some analytical refinements we may represent each element of this category, too, by one single coordinate, $E(t)$. However, for reasons to become apparent in Section 4, below, it is preferable to abide by the more direct representation such as (2) and (3).

A third (and last) category of elements, which is illustrated by workers and tools, poses a special problem. A worker is a *rested* man when he goes into the process but comes out a *tired* man. A tool may be *new* when it enters the process but it is *used* when it comes out. In view of the analytical condition of discrete distinctness between the elements C_i, the "same" worker must be split into two distinct entities, one representing the worker when rested, the other, when tired. On the surface, this point may seem to be of a practical order only. In fact, it is a palpable symptom of the difficulty of separating the proper notion of process from that of qualitative change, a difficulty on which I have insisted in Chapter III. The elimination of qualitative change, we see, forces us to bar such a basic notion as that of sameness from our analytical picture of a process. Needless to add, from the formal viewpoint nothing pleads against representing a rested worker (or a new tool) by one C_k and the same worker when tired (or the same tool when used) by a different C_j. That is, the "same" worker may be represented by one input and one output coordinate:

$$(4) \qquad \begin{aligned} &E_k(t) = 0 \quad \text{for } 0 \leq t < t', \qquad E_k(t) = -1 \quad \text{for } t' \leq t \leq T; \\ &E_j(t) = 0 \quad \text{for } 0 \leq t < t'', \qquad E_j(t) = 1 \quad \text{for } t'' \leq t \leq T; \end{aligned}$$

[6] Seeding and harvesting, being processes, have durations. But the simplification involved in (3) has no effect on the point discussed here.

where t' and t'', $t' < t''$, are the times when the worker enters and leaves the process, respectively. Rested and tired workers and new and used tools may thus be included in the same category as the other ordinary inputs and outputs such as solar energy, waste, raw materials, etc.

The analytical description of a process is thus complete. We may associate it with a point in an abstract space of functions and write it symbolically as follows:

$$(5) \qquad [\overset{T}{\underset{0}{E_i(t)}};\ \overset{T}{\underset{0}{F_\alpha(t)}},\ \overset{T}{\underset{0}{G_\alpha(t)}}].$$

In this expression the subscript i covers all elements that are only inputs or only outputs, the subscript α, those that are both inputs and outputs. And a point we should not fail to note: the representation (5) keeps in permanent focus the fact that every process has a duration T.[7] Alternatively, the same representation can be laid out in a less abstract form as a series of graphs, each graph representing one of the functions involved in (5).[8]

An analytical picture in which the same worker (or the same tool) is split into two elements would undoubtedly complicate matters beyond description. The reason why these complications have not upset the various other analytical models currently used in natural or social sciences is that the issue of qualitative change has been written off *ab initio* by various artifices.[9] For example, the chemist usually draws the boundary of a chemical process in such a manner that the material structure—say, the test tube—inside which a reaction takes place is not listed as an element of the process. Perhaps he is justified in abstracting the test tube—chemical reactions may also occur in *open* space. However, even a chemist would mention the use of a catalyst (when necessary) even though a catalyst, like the Ricardian land, is not transformed by the process. On the other hand, a chemical engineer must, under heavy penalty, not lose sight of the fact that a dyeing vat deteriorates with use. All the more then we should expect an economist to make room in his analytical representation of a production process for this important economic factor—

[7] In the case of a production process we may use instead Marx's convenient term "time of production." Karl Marx, *Capital* (3 vols., Chicago, 1932–33), II, 272 f.

[8] For which see Fig. 1 of my article "Process in Farming vs. Process in Manufacturing: A Problem of Balanced Development," in *Economic Problems of Agriculture in Industrial Societies*, Proceedings of a Conference held by the International Economic Association at Rome (1965), eds. Ugo Papi and Charles Nunn (New York, 1969), pp. 497–528.

[9] One notable exception, to which I shall refer more than once, is Marx's analysis of the worker's participation in the productive process, an analysis which occupies a prominent place in the first volume of *Capital* and which, its shortcomings notwithstanding, is distinctly superior to everything else I have been able to come across in the literature.

the wear and tear. This he does, at times, explicitly. But in doing so he resorts to evaluating depreciation in money terms according to one of the conventional rules set up by bookkeepers. The solution is not only arbitrary, but also logically circuitous: it presupposes that prices and the interest rate, which in fact are influenced by production, are independent of it.

An inspection of the basic models of production (in real terms) reveals however, that none includes the tired worker or the used tool among their coordinates. In addition to the formal complications already mentioned, there are other reasons which command the economist to avoid the inclusion of these elements in his analytical representations of a process. The economist is interested first and last in *commodities*. To wit, no economist would nowadays draw the boundary of a process so that melted glass, for instance, should be an output or an input element. Melted glass, no doubt, is an indispensable factor in the production of glass wares; it is not something one would throw away—in a sense, it has economic value. But it is not a commodity under the present technology. The notion of commodity reflects not only the dialectical individuality of human wants but also (and especially) the fact that production under any state of the arts is carried out by fairly well individualized processes. At any one time, therefore, the spectrum of commodities is determined by the prevailing technology. Until recently, half-baked bread or ready-mixed cement were not commodities any more than melted glass is today. At any one time, however, the boundaries of the processes in which the economist is interested are drawn where the circulation of commodities can be observed, i.e., where they pass from one production unit to another or from one production unit to a consumption unit.

Even though there is no fast and general rule for determining what is and what is not a commodity, by no stretch of the imagination could we say that tired workers and used tools are commodities. They certainly are *outputs* in every process, yet the aim of economic production is not to produce tired workers and worn-out equipment. Also, with a few exceptions—used automobiles and used dwellings are the most conspicuous ones—no used equipment has a market in the proper sense of the word and, hence, no "market price." Moreover, to include tired workers and used tools among the products of industry would invite us to attribute a cost of production to such peculiar commodities. Of course, the suggestion is nonsense. Economics cannot abandon its commodity fetishism any more than physics can renounce its fetishism of elementary particle or chemistry can renounce that of molecule.

The conclusion is that at least for the purpose of microanalysis the representation of an economic process in the form to which the considerations of this section have led us is highly cumbersome, to say the least.

The question before us is whether there is some other mode of describing analytically a process, a mode that is both manageable and adequate in the sense that it does not leave out any essential factor. And the wear and tear, this work of the Entropy Law, is such a factor.

3. Stocks and Flows. The analytical models currently used in economics for representing a production process fall into two main categories, each category being related to an entirely different viewpoint. Although opposite to each other, the two views fared side by side in economics long before the era of mathematical models. One view, which began its great vogue with the advent of the Leontief static input-output system, is that a process is completely described by its flow coordinates, explicitly, by "the rate of flow per unit of time of each of the N *commodities* involved."[10] Assumingly, the flow rates are determined on the boundary identifying the process (although the idea of a boundary is never mentioned in the related works). The complex that characterizes this approach—and which is apparent from the arguments and applications of the flow models—is that the process is viewed as a continuously going affair which is approached by the observer at any time he may please *but only from the outside.* That is, during his tour of duty the observer is supposed to record only the flows that cross the frontier of such a going on process. What was already inside that process when he arrived on the scene and what remained inside it when he left are no concern of his. In its strict form, a flow model does not start with a *tabula rasa* nor ends with one.

The other type of analytical representation of a process reflects the diametrically opposite view: a complete representation of a process consists of two snapshots, as it were, one at the time when the observer comes on the scene, the other when he leaves. Or to put it differently, the observer takes two censuses, one at the beginning of his period of observation and one at the end. He pays no attention whatsoever to *what crosses the frontier* at any time. According to this viewpoint, a process is represented analytically by a two-row matrix

$$\begin{bmatrix} A'_1, A'_2, \cdots, A'_n \\ A''_1, A''_2, \cdots, A''_n \end{bmatrix}, \tag{6}$$

where the vectors (A') and (A'') represent the stocks of *commodities* inside

[10] T. C. Koopmans, "Analysis of Production as an Efficient Combination of Activities," in *Activity Analysis of Production and Allocation*, ed. T. C. Koopmans (New York, 1951), p. 36 (my italics). As hinted above, this conception of a process had already been advocated in nonmathematical quarters; e.g., G. Stigler, *The Theory of Competitive Price* (New York, 1942), p. 109. That the same conception is the analytical cornerstone of Leontief's input–output system is obvious from the statements which stud his major contribution, W. W. Leontief, *The Structure of the American Economy: 1919–1939* (2nd edn., New York, 1951), especially pp. 12 f, 27.

the boundary at two time instants $t' < t''$, respectively.[11]

One point, which I made some years ago, should be abundantly clear from the preceding remarks: each of the two types of models tells only one different part of the whole story.[12] For an incisive example, let us consider the case in which $(A') = (A'')$. Unless the frontier of the process includes the entire universe—alternatively, unless we know that we are dealing with an isolated system—it is impossible for us to say whether (6) represents a stationary state (in which something does happen) or a frozen conglomerate (in which nothing happens). Turning to the flow models, let us take the case of two processes having exactly the same flow coordinates. In this situation, we have no way of knowing whether they are identical or one is more efficient (in some particular sense) than the other. The observer being supposed to approach the process from the outside as the process is going on, the flow representation of an agricultural process should not include the Ricardian land.[13] Nor could the tools already in use be included in such a model if its rationale is strictly followed. Should the observer be by chance meticulous indeed, he may, at most, record the output rate of scrap. But no model builder yet seems to have been meticulous to that extent.

The opposition between the two types of models brings to mind the famous antinomy between flow and stock. For if both the flow and the stock models offer an adequate representation of a process—as each model claims for itself—the antinomy between flow and stock should be fictitious. As it happens, the two models are neither equivalent nor contradictory. We may be thus tempted to conclude that, after all, the concepts of flow and stock are not strictly antinomic. The antinomy is nonetheless as irreducible as antinomy can be.

No alert economist would nowadays make the same kind of statement as that by which Adam Smith opened his magnum opus: "The *annual* labor of every nation is the *fund* which originally supplies it with all the necessaries and conveniences of life."[14] That much is certain after we have

[11] John von Neumann, "A Model of General Economic Equilibrium," *Review of Economic Studies*, XIII (1945), 2. For a sample of the numerous works in which the stock-process conception has been advocated, see A. L. Bowley, *The Mathematical Groundwork of Economics* (Oxford, 1924), pp. 28 f; J. R. Hicks, *The Theory of Wages* (London, 1932), p. 237; Paul A. Samuelson, *Foundations of Economic Analysis* (Cambridge, Mass., 1948), p. 57.

[12] See my article "The Aggregate Linear Production Function and Its Applications to von Neumann's Economic Model," in *Activity Analysis of Production and Allocation*, ed. Koopmans, pp. 100 f.

[13] But see note 30 below.

[14] Adam Smith, *The Wealth of Nations*, ed. E. Cannan (2 vols., 5th edn., London, 1930), I, 1 (my italics). But the acme of surprise is that Léon Walras—a mathematics aspirant in his youth—associates income with stock. See his *Elements of Pure Economics* (Homewood, Ill., 1954), pp. 212 f. The same thought echoes in J. A. Schumpeter, *The Theory of Economic Development* (Cambridge, Mass., 1934), p. 46: "the reservoirs which we call income."

been repeatedly instructed not to confuse what flows with what stands still.[15] The oft-quoted dictum on this issue is Irving Fisher's: "Stock relates to a *point* of time, flow to a *stretch* of time."[16]

Like all tersely formulated principles, this rule has walked the rounds so swiftly that practically everyone's mind felt satisfied without looking into the thoughts behind it. It was thus very easy for the modern tide of formalism to bury the antinomy under a trite formula which now walks the rounds with still greater ease. The formula could not possibly refurbish such a fundamental concept as that of stock. Stock continued to be conceived as a qualityless entity—we would say—which exists as a quantum in a definite "place" and has a cardinal measure at any instant during the time interval in focus. Flow, however, came to be defined simply as the difference between two instances of a stock at two different instants of time. The idea is crystallized in the tautological formula

$$\Delta S = S(t_1) - S(t_0), \tag{7}$$

where $S(t_0)$ and $S(t_1)$ are the measures of the correlative stock at the instants $t_0 < t_1$. That this approach hides away the antinomy is beyond question. The difference between two quanta of, say, wheat is also a quantum of wheat whether the two quanta refer to the same storehouse at two different instants or to two storehouses at the same instant. It is because of this truism that we are apt to commit the error of confusing stock with flow. According to formula (7) both an income over any period and a bank balance consist of dollars indistinguishable from each other. Why should we then treat income and wealth as two different essences?

One answer—on which Fisher himself fell back—is that what is after all opposed to stock is not ΔS but $\Delta S/(t_1 - t_0)$, that is, the flow *rate*.[17] A flow rate, certainly, is not of the same essence as a stock. But the relation between this difference and the old antinomy is only superficial. To wit, an instantaneous flow rate also refers to a point of time. When driving, I can read on the panel instruments both the speed of the car and the mileage driven from home at any chosen instant. But if I do not know what speed actually means, I am apt to tell the policeman who stops me for driving sixty miles per hour in the center of the town,

[15] To recall, S. Newcomb, in his *Principles of Political Economy* (New York, 1886), p. 316 and *passim*, was first to draw the attention of economists to the error of the crude Wage Fund doctrine which confused—as Adam Smith did—an annual flow with a fund.

[16] Irving Fisher, "What Is Capital?" *Economic Journal*, VI (1896), 514. The caution comes up repeatedly in most of Fisher's later writings down to his *The Nature of Capital and Income* (New York, 1919), chap. iv.

[17] Fisher, "What Is Capital?" pp. 514 f.

"Officer, it cannot possibly be so, I have not driven sixty miles since I left home." True, I may avoid committing such an error if my attention is drawn to the fact that ΔS and $\Delta S/\Delta t$ do not have the same dimensionality. According to this line of thought, the only reason against confusing the monthly rate of income with a monthly bank balance is the general principle that concepts of different dimensionalities must be kept separate in our minds and in our operations. The upshot is that in the particular case under discussion the role of time becomes accidental. Fisher's dictum would convey nothing more than countless other rules of the same form, say, the rule that "height relates to a *point* in space, slope to a *stretch* of space." The answer mentioned at the beginning of the paragraph misses the point that the antinomy between flow and stock does not involve only the difference between the dimensionality of flow rate and stock.

Even though most economic models nowadays use formula (7) by rote in order to pass from stock to flow coordinates and vice versa, they offer no occasion for the reader to sense the antinomy that has intrigued Fisher and many other careful analysts. Actually, this antinomy is implicitly but unmistakably denied by the argument that the stock model is the more comprehensive of the two because the flow coordinates can be derived by (7) from stock data but the stocks can be determined from flows only beyond an arbitrary constant (or only if the stocks are known at some instant). One should, the argument concludes, prefer the stock model to the other.[18]

The advice harbors the fallacy, manifest in one model after another, that a census taker must come out with exactly the same list of elements as the custom official who records only what crosses the frontier. In other words, the list of the elements C_i must be identically the same in the stock and in the flow representations of the same process.[19] This is the natural consequence of settling the issue of flow by (7). For if that equation

[18] Among the authors that I could cite in support of the judgment expressed in this paragraph are authorities such as John Hicks, for instance. In his recent *Capital and Growth* (New York, 1965), p. 85, he tells us explicitly that "We do not need to distinguish between stocks and flows; for stocks and flows enter into the determination of equilibrium in exactly the same way." In this volume, just as in the earlier *Value and Capital* (Oxford, 1939), Hicks adopts the idea expressed by (7) to derive the flows from the stock coordinates by which he prefers to represent a process.

[19] We may cite here the case of the dynamic Leontief system where the lists of current and capital input–outputs are identical. Cf. W. Leontief *et al.*, *Studies in the Structure of the American Economy* (New York, 1953), pp. 55–58. True, in a formal model one may use the same list for the flow and the stock items and let some of the coordinates be set to zero in the concrete applications of the model. But such a procedure is likely to conceal from view a very important feature of process. See Section 9, below.

is accepted as the only definition of a flow we cannot avoid the conclusion that whenever there is a flow there must be a stock, and conversely. Clearly, if one side of a definitional formula has a meaning, so must the other. A few simple counter-examples suffice to show that between the lists of flow and the stock elements of the same process there does not exist even a relation of inclusion: normally, the lists overlap. A census must include the land of a country, its roads, its river dams, its factories, etc., etc.—items never found in any import-export statistics. On the other hand, most private homes use a flow of electricity; yet a census taker may find no stock of electricity in it. But even if we take an item such as "raw rubber"—which is both a stock and a flow coordinate of the United States viewed as one partial process—we shall find that the stocks and the flow coordinates do not as a rule satisfy (7).

The crux of the issue under discussion is that *a flow does not necessarily represent either a decrease or an increase in a stock of the same substance.* The melted glass that flows into the rolling machines does not decrease the stock of melted glass in the furnace. In the ultimate analysis, it decreases the stocks of sand, coal, etc.—that is, the stocks of other substances in nature. The flow of food consumed by mankind since its origin has not come out from a stock in existence at the time of Creation. But, for an analogy that should make the point crystal clear, there is the fact that Time always flows but never exists as a stock.

The position that formula (7) takes perfect care of the notion of flow because every flow comes from one stock and goes into another stock can be traced back to the epistemological fallacy which I have endeavored to confute in some of the preceding chapters. The fallacy is that Change consists of locomotion and nothing else. As a result, the intricate notion of flow, which is intimately connected with qualitative change, is reduced to motion from one slice of actuality to another. There are, no doubt, cases in which formula (7) expresses directly the connection between *two* stocks and *one* flow. Still, for the overwhelming number of the relevant cases the true connection is between *one* stock and *one* flow. For a simple illustration, let us consider the flow of melted glass that pours from the furnace into the rolling machines. We may simply visualize the stock of melted glass that would have accumulated during some interval if it had not been almost instantaneously transformed into glass plate. Or we may visualize the stock of wheat that would be accumulated by now if, say, all the wheat produced since 1900 had not been consumed in step with every harvest.

The moral of these illustrations is plain: *a flow is a stock spread out over a time interval.* The stock to which this definition refers may have an analytical existence only—as in the case of the last examples—or an actual existence, in which case it corresponds to ΔS of formula (7). The definition, I believe, is far more incisive than Fisher's dictum.

Whether the flow comes out of a stock or goes into one, or whether it is of the nature of an event, it can be represented analytically by a coordinate such as $E(t)$ of relation (1) in Section 2 above, *defined over an appropriate time interval.*[20] Often, we may be satisfied with a less sharp description and simply say that a flow of ten tons of melted glass *occurred* during five hours. In this case, the analytical representation is the *pair* (S, T), where S is a stock and T is a stretch of time. The explicit mention of the corresponding stock *and* of the duration is indispensable. And in fact, this is done in every statistical table of production data, for example. Only, the time component is separated from S and included in the title of the table, which may read "The *Yearly* Production of the Steel Industry." But the data in the body of the table are stocks, as said above. To say *only* that the rate of flow was on the average two tons per hour—i.e., to replace the pair (S, T) by a single coordinate S/T—does not constitute a complete description of the flow even in the simplified form.

4. Funds and Services. The main point of the preceding section—that a flow does not necessarily come out or go into an actual stock—is connected with the plain fact that products are *created*. If the boundary of a process that produces automobiles, for instance, is appropriately drawn, we will find no stock out of which the product flows. Conversely, a boundary of a process may be drawn in such a way that many input flows are *annihilated* the instant they enter the process. In economic jargon, they are consumed. The inputs falling in this category are characterized by one interesting feature. Although when we settle the final accounts we see that the completion of a process requires a definite amount of such an input, this amount is not needed all at once, but only as a flow spread over time in some specific manner. Think, for instance, of the amount of solar energy or the amount of rainfall necessary to bring to completion a process of raising corn. A painter, also, does not need to buy all the paint for a job at once. If material constraints arising from discontinuous units were not present, we could visualize him buying a continuous flow of paint.

What we have just said about solar energy, rainfall, and paint does not apply to all inputs. These other inputs are characterized by two correlated

[20] Obviously, the essence of $E(t)$ is that of stock in all cases. At times, $E(t)$ cannot be determined otherwise than by an instrumental measure of the instantaneous flow rate, $e(t) = E'(t)$. In this case, we must not lose sight of the fact that a flow always consists of some substance in the broad sense of the term. Otherwise, we may find ourselves speaking of a stock of voltage, if we read the wrong instrument.

features. First, they are not consumed in the process; instead they come out of the process, albeit with some scars. The ladder of a painter is a good illustration. But the most stringent example of this category is the Ricardian land which comes out in exactly the same amount and quality. Most of the inputs of this category exist only in some indivisible physical units. They are typified by any tool that outlasts the process in which it participates as well as by a worker. But in all these cases, we speak of land, of tools, and workers as being *used* in, not *consumed* by, the process. And we are right in making this distinction.

The point is not new. The way it has often been presented is that the distinction arises from the fact that some things can be consumed at once but others are durable because their consumption requires duration.[21] As expected, positivist arguments have assailed this position on the ground that no event is durationless and no fast line can be drawn to separate durable from nondurable factors of production.[22] The fault of the position, however, is that it claims—as is apparent from the literature—that any object can be classified as durable or nondurable independently of the process in which it is an input. The sin is similar to that of the general dichotomy of commodities into consumer and producer goods. Analysis may abstract from a dialectical penumbra but not if the penumbra happens to cover almost the entire spectrum of discourse.

Inputs can be classified into nondurable and durable in a manner that meets the requisites of analysis if we adopt a relative criterion. In relation to a *given* process an input is only used (but not consumed) if it can be connected with an output element by reason of identity of substance—like the clover seed in growing clover seed—or sameness of object—like the painter's ladder. If this is not the case, the input is consumed in the process. The classification is, of course, dialectical because we find no tool in the positivist paraphernalia for recognizing sameness. A few extreme illustrations may be in order for additional clarification. A space rocket would at present be classified as a consumable input; yet in the technology of tomorrow it may become a durable input used successively in several space flights. Also, we may conceive processes with no durable input besides mere space and some raw form of matter-energy—the evolution of the universe from the Big Bang to the present, for instance. A completely tragic expedition in the Sahara is another example. Finally, let us note that the economic process of mankind from its inception to this

[21] E.g., Léon Walras, *Elements of Pure Economics*, p. 212.

[22] Time and again, the oily inconsistency of the positivist dogma comes up to the surface. We may recall that on other occasions the same dogma finds nothing wrong with the idea of an event at an instant of time. Cf. Chapter III, Sections 4 and 5, above.

day has no durable input of human or technical nature and only a few of other nature. For such processes the above classification of inputs into consumable and durable, although still workable, may not be relevant. They raise some issues that must be handled differently. So, let us make abstraction of them for the time being and concentrate on the overwhelming number of processes for which the distinction is highly enlightening.

About a durable input—a machine, for instance—economists say not only that it can be *used* in a production process, but also that it can be *decumulated*. They also speak of capital *accumulation* when a new factory is built. We should note, however, that in these expressions the meanings of "accumulation" and "decumulation" differ profoundly from those in saying that a flow accumulates into a stock or a stock decumulates into a flow. In the last cases "accumulation" and "decumulation" represent some mechanical operations akin to locomotion. Because the difference thus screened is of paramount analytical importance, the ambiguous usage has served as a hotbed of idle controversy and a source of grave errors—one of which will presently have our attention.

There can be no doubt that the decumulation of a machine is not a mechanical spreading in time of the machine as is the case with the stock of provisions of an explorer, for instance. When we "decumulate" a machine we do not separate it into pieces and use the pieces one after another as inputs until all parts are consumed. Instead, the machine is used over and over again in a temporal sequence of tasks until it becomes waste and has to be thrown away. A machine is a material stock, to be sure, but not in the sense the word has in "a stock of coal." If we insist on retaining the word, we may say that a machine is a stock of services (uses). But a more discriminating (and hence safer) way of describing a machine is to say that it is a *fund of services*.

The difference between the concept of stock and that of fund should be carefully marked, lest the hard facts of economic life be distorted at everyone's expense. If the count shows that a box contains twenty candies, we can make twenty youngsters happy now or tomorrow, or some today and others tomorrow, and so on. But if an engineer tells us that one hotel room will probably last one thousand days more, we cannot make one thousand roomless tourists happy *now*. We can only make one happy today, a second tomorrow, and so on, until the room collapses. Take also the case of an electric bulb which lasts five hundred hours. We cannot use it to light five hundred rooms for an hour *now*. The use of a fund (i.e., its "decumulation") requires a duration. Moreover, this duration is determined within very narrow limits by the physical structure of the fund. We can vary it only little, if at all. If one wishes to "decumulate" a pair of shoes, there is only one way open to him: to walk until they become waste.[23] In contrast with this, the decumulation of a stock may, conceivably, take place in one single instant, if we wish so. And to put the dots on all significant i's, let us also observe that the "accumulation" of a fund, too, differs from the accumulation of a stock. A machine does not come into existence by the accumulation of the services it provides as a fund: it is not obtained by storing these services one after another as one stores winter provisions in the cellar. Services cannot be accumulated as the dollars in a saving account or the stamps in a collection can. They can only be used or wasted.

Nothing more need be said to prove that also the use of the term "flow" in connection with the services of a fund is improper if "flow" is defined as a stock spread over time. In fact, the generally used expression "the flow of services" tends to blur—at times, it has blurred—the important differences between two mechanisms, that by which the prices of services and that by which the prices of material objects are determined. The inevitable trap of this ambiguous use of "flow" is that, because a flow can be stored up, we find it perfectly normal to reason that services are "embodied" in the product.[24] Only the materials that flow into a production process can be embodied in the product. The services of the tailor's needle, for example, cannot possibly be embodied in the coat—and if one finds the needle itself embodied there it is certainly a regrettable accident. The fact that in certain circumstances the value of services passes into the value of the product is to be explained otherwise than by simply regarding a machine as a stock of services that are shifted one after another into the product.

The difference between flow and service is so fundamental that it separates even the dimensionalities of the two concepts. For this reason alone, physicists would not have tolerated the confusion for long. The amount of a flow is expressed in units appropriate to substances (in the broad sense)—say pounds, quarts, feet, etc. The rate of flow, on the other hand, has a mixed dimensionality, (substance)/(time). The situation is entirely reversed in the case of services. The amount of services has a mixed dimensionality in which time enters as a factor, (substance) $\times$ (time). If a plant uses one hundred workers during a working day (eight

[23] Of course, one may sell the shoes. But this would mean decumulation of the shoes as a stock, not decumulation of the shoes as a fund of services. Besides, selling the shoes implies a buyer who presumably is interested in using them himself. The elementary fact that funds cannot be decumulated except by use over a fairly determined duration accounts not only for the economic ills of recession but also for the structural locks of many Latin American economies. Cf. my article "O Estrangulamento: Inflação Estrutural e o Crescimento Econômico," *Revista Brasileira de Economia*, XXII (March 1968), 5–14.

[24] E.g., A. C. Pigou, *The Economics of Stationary States* (London, 1935), pp. 20, 117.

hours), the total of the services employed is eight hundred *man* × *hours*. If by analogy with the rate of flow we would like to determine the rate of service for the same situation, by simple algebra the answer is that this rate is one hundred *men*, period. The rate of service is simply the size of the fund that provides the service and consequently is expressed in elemental units in which the time factor does not intervene. A rate with respect to time that is independent of time is, no doubt, a curiosity. It was all the more necessary to point out that it exists and to show the reason why it exists.

5. *A Flow–Fund Model.* As manifested by the standard of numbers, the present temper in economics is to jump directly to tackling only the "big" problems, of growth or of development. But, especially among the rank and file, not all economists who write on development or who are engaged in planning seem to heed one elementary object lesson of mechanics, which is that one cannot speak of accelerated motion otherwise than as a passage from one *uniform* motion to another such motion. For, just like the accelerated motion, growth cannot be conceived otherwise than as a passage from one stationary state to another. The study of growth must begin with the study of the stationary state and develop up from this basis if it is to be a well-planned scientific enterprise.[25] The view—expressed quite often, albeit sotto voce rather than solemnly—that the concept of a stationary state constitutes only a textbook cumber is therefore inept. Actually, the reverse is true: ordinarily, writers do not pay enough attention to clarifying the concept.[26] A complement of the same mistaken view is that the concept of a stationary state is in addition factually irrelevant. This reflects both a superficial knowledge of facts and a misunderstanding of what "factually relevant" means in science. Even a practicing mechanical engineer, who is interested only in facts as they are, would not say that uniform motion is factually irrelevant for him. And just as there are actual motions that are *almost* uniform and, hence, can be treated as being uniform, so in the history of mankind we do find cases after cases of almost stationary economic states. From the dawn of man's economic evolution to this day only the present interlude constitutes an exception to the rule that human society has advanced at such a slow speed that the change becomes visible only in the perspective of centuries or even millennia. On a lower level, what is a normally functioning factory if not a quasi stationary state or a steady-going concern, if you wish?

[25] For an example of this procedure at its best the reader is invited to look up Part III of Leontief, *Structure of the American Economy*.

[26] One notable exception standing in a class by itself is the masterly analysis by Pigou in his *Economics of Stationary States*, now almost completely buried by oblivion.

The quality of being stationary may be defined in several equivalent ways. The direction from which Karl Marx approached the problem appears to suit best the scope of this chapter.[27] A system is stationary if whatever it does can be repeated identically over and over again. "Stationary state" and Marx's "simple reproduction" are therefore perfectly synonymous terms. But in order that a partial process be capable of being repeated after its conclusion, it is imperative that the fund factors involved in it should not come out degraded. From what we have seen already in this essay, this condition leads to an impasse.

However, the impasse can be resolved and the solution comes straight out of the economic literature of older vintage. It is the idea of capital equipment *being kept as a constant fund by the very process in which it participates*.[28] Strictly interpreted, this idea is a fiction. A process by which something would remain indefinitely outside the influence of the Entropy Law is factually absurd. But the merits of the fiction are beyond question. Like the notion of uniform motion (i.e., a motion without entropic friction), that of a process which maintains its equipment constant is not as remote from actuality as it may at first seem. We need only look around in almost any factory or home to convince ourselves that normally efforts are constantly directed toward keeping every piece of equipment in good working condition. For let us not fail to note that "maintaining capital constant" does not imply that a piece of capital is an indestructible monolith. All it means is that the *specific* efficiency of every piece of capital is kept constant. It matters not that a machine looks old, is scratched, dented, out of fashion, etc., as long as it is as efficient as when it was new. In places that the jet planes cannot yet reach we see hundreds of DC-3 planes, some twenty years old, doing now as good a job as when they were new and flying between the metropolises of the world. There is, though, a snag in the idea of capital's being maintained constant, but the snag pertains to analytical, not factual, considerations.

To keep a spade in good working condition, a farming process needs, among other things, a file. The file, being now a necessary element of the process, must also be kept in good order and, hence, it calls in turn for another tool (say, a wire brush); this tool calls for another, and so forth. We are thus drawn into a regress which might not end until we have included in the process in question a very large part of the entire produc-

[27] See Marx, *Capital*, I, 619 f.

[28] The reason given by Marx (*Capital*, I, 221 f) for his choice of the term "constant capital" to denote the material means of production is that the value of these means passes unaltered into the value of the product. To emphasize in this manner the main tenet of the labor doctrine of value was natural for him. But his analysis of the diagram of simple reproduction (*ibid.*, II, 459 f) clearly suggests that he had in mind mainly the idea mentioned in the text.

tion sector of the economy. And that is not all. Since workers are funds, they must be kept "in good working condition," too. The initial process has then to be expanded until the household process of almost every worker and practically every production line in the world are included in it. This conclusion is a glaring example of the ways in which the seamless actuality resists being divided into arithmomorphic parts. If we insist on connecting a process with some enduring entity, some form of Being, we are forced to go back to the Whole. Such a broad viewpoint may have its merits in other respects—as we shall see in time—but it forbids us from dealing with microprocesses, a plant or even an industry, for instance. Some sort of compromise is necessary in order to circumvent the difficulty. It consists of admitting that maintenance may be achieved in part also through services brought in from outside and ignoring the daily wear and tear of the worker (which in fact is always restored outside, in the household). The dividends of this compromise are paid by a clearer picture of the practical implications of a production process.

The factors of production can now be divided into two categories: the fund elements, which represent the *agents* of the process, and the flow elements, which are *used* or *acted upon* by the agents. Each flow element continues to be represented by one coordinate $E_i(t)$ as defined in Section 2 above. But in view of the fact that a fund element enters and leaves the process with its efficiency intact, its analytical representation can be greatly simplified. Specifically, we can represent the participation of a fund C_α by a single function $S_\alpha(t)$ showing the amount of services of C_α up to the time t, $0 \leq t \leq T$.[29] We still need to refer to a point in an abstract space for the analytical representation of the process, but this representation is much simpler than (5) of Section 2:

$$[\overset{T}{\underset{0}{E_i(t)}};\ \overset{T}{\underset{0}{S_\alpha(t)}}]. \tag{8}$$

Formal results such as the one just reached tend to screen issues and points that must nonetheless be continuously borne in mind lest we turn into symbol spinners. Most important of all is to remember that the question whether a factor is classified as a fund or as a flow element in the

[29] Alternatively, C_α can be represented by a function $U_\alpha(t)$ showing how much of the fund is participating in the process at t, with the convention that a fund is *in* at the instant it enters and *out* at the instant it leaves the process. This convention is the symmetrical aspect of the fact that an input in a partial process is an output of the environment, and vice versa. The graph of $U_\alpha(t)$ looks like a skyline of a city and has the advantage of bringing into focus the periods when C_α is idle, i.e., when it is not needed by the process. See Fig. 1 in my paper "Process in Farming vs. Process in Manufacturing" (cited in note 8, above).

analytical representation of an actual process depends upon the duration of that process. If the process in which, say, an automobile is used is relatively short, the efficiency of the automobile may be maintained by replacing now and then the spark plugs or the tires, for instance. If it is longer, we may have to replace the motor, the chassis, or the body parts. The flow elements will not be the same in the two cases. Conceivably, any automobile may be maintained forever through flows of all its constituent parts. In the long run, the automobile will have only the name in common with the initial one. But this fact need not bother the analyst: the process needs the services of an automobile of a definite type, not those of a particular automobile identified by the serial number. Cost is one reason why in actuality automobiles and other pieces of equipment are not maintained forever but are discarded from time to time. But even if cost would not be an impediment, obsolescence would ultimately bring about the same result. Novelty, therefore, is the main cause why an automobile, a machine, a bridge, or a highway are discarded and replaced in the long run. Of course, in the very, very long run it is the work of the Entropy Law that prevents anything from lasting forever. The limitations of the flow–fund model as an analytical representation of actual processes should not, therefore, be ignored. But neither should the merits of the model in casting a great deal of light on many analytical points be belittled.

Another important point to be borne in mind is that the division of factors into flow and fund elements does not mean that the same item cannot appear both as a flow and as a fund. Let us recall one illustration of Section 2, namely, the process in which hammers are used to hammer hammers. It should be obvious that in this case the item "hammer" is an output flow—and as such must be represented in (8) by one $E_i(t)$—and also a fund—to be represented by one $S_\alpha(t)$. Yet the point has often been missed. And it is not a point of minor importance. One notable illustration is provided by the analytical difficulties into which Marx got himself by failing to distinguish in his diagram of simple reproduction between the fund-hammers and the flow-hammers. The problem has many instructive facets and I shall come back to it (Section 14, below).

6. Further Reflections on the Flow–Fund Model. The general expression (8) can be made more telling by bringing into it the broad categories into which the fund and the flow elements may be classified according to their specific nature or role in the process. For funds we may take our cue from the Classical division of production factors and distinguish them into Ricardian land (L), capital proper (K), and labor power (H). Among flow elements we may distinguish first the inputs of the so-called natural resources (R)—the solar energy, the rainfall, the "natural" chemicals in the air and the soil, the coal-in-the-ground, etc. Second, there are the current input flows (I) of the materials which are normally transformed into products and which come from other production processes—the lumber in a furniture factory, the coke in a foundry, etc. Third, there are the input flows needed for maintaining capital equipment intact, (M)—lubricating oil, paint, parts, etc. Fourth, there is the output flow of products (Q). And, finally, there is the output flow of waste (W). With the correlative notations, expression (8) may be replaced by

$$[\overset{T}{\underset{0}{R}}(t), \overset{T}{\underset{0}{I}}(t), \overset{T}{\underset{0}{M}}(t), \overset{T}{\underset{0}{Q}}(t), \overset{T}{\underset{0}{W}}(t); \overset{T}{\underset{0}{L}}(t), \overset{T}{\underset{0}{K}}(t), \overset{T}{\underset{0}{H}}(t)]. \tag{9}$$

The natural factors of production have always offered a matter of disagreement to economists. The reason why my classification differs from that of the Classical school, which includes all these factors under "land," should by now be obvious. One may, though, object that, in view of the fact that I have associated the concept of fund with that of agent, it is inconsistent to place the Ricardian land—an inert element—in the fund category. However, I submit that the Ricardian land *is* an agent in the true sense of the term. Just as a net catches fish even if left by itself in the sea, the Ricardian land catches rainfall and, above all, solar radiation. Moreover, it is the only net that can do this. Had our planet a radius twice as great, we could catch four times as much of this most vital energy for man's existence. By and large, the scarcity of land derives from this role; the role of providing mere space is secondary. Conceivably, we could get more land-space by building acres on top of acres, but only those on the very top would be green acres.

One may also count on the objection that the inclusion of both the Ricardian land and the solar energy among the factors of production constitutes a double-counting. The point that the rain and the solar energy flow by themselves and hence are "free gifts of nature" is a familiar leitmotiv of all major doctrines of economic value. This is no reason, however, for omitting the natural factors from our scientific report of a process. The issue of what has and what has not value must not be prejudiced—as has generally been—by a trimmed-off representation of a process in *real* terms.[30] A glaring illustration of the danger of simplifying

[30] Among the authors of the main models of production, Koopmans ("Analysis of Production," pp. 37–39) is perhaps the only exception in that he lists the flow of resources from nature and the flow of land services among the coordinates of a productive process. The general trend follows the pattern of both the static and the dynamic input–output systems of Leontief in ignoring all natural factors. This is all the more curious since these systems are intended as instruments of *material* planning of production rather than abstract foundations for an analysis of value.

this representation is, again, provided by Marx. It is from Marx, I believe, that we have inherited the heresy that if the maintenance flow of, say, a bridge is included as a factor in the representation of a process using the bridge, then the inclusion of the bridge itself constitutes a double-counting that serves the interest of the capitalist exploiters. So eager was Marx to avoid the slightest suggestion of the idea that the services of capital proper may contribute to the value of the product something more than the value of the maintenance flow, that he painstakingly avoided any reference to services even in the case of the laborer. Instead, he used such veiling expressions as the work performed by a machine or the life activity of the worker.[31]

In the light of our flow–fund model Marx's tour de force lets itself be seen in detail and admired as well. It is beyond question that Marx started by viewing the worker as a fund.[32] Labor power—one of the many useful terms introduced by Marx—means "the aggregate of those mental and physical capabilities existing in a human being, which he *exercises* whenever he produces a use-value of any description."[33] In a plain incontrovertible manner the same idea is expressed by Engels: "Labor power exists in the form of the living worker who *requires* a definite amount of means of subsistence for his existence as well as for the maintenance of his family."[34] Marx, we should note, did not say in the cited phrase that the worker *consumes* his capabilities in production. Nor did Engels say that the services of labor power are in some precise sense *equivalent* to the maintenance flow required by the worker. Yet Marx, as he comes to the crux of his argument, suddenly introduces the equivalence by reducing the participation of the worker in a production process to a "definite quantity of human muscle, nerve, brain, etc., [which] is wasted" during work.[35] By this equivalence Marx simply covered up the plain fact that the worker participates in the production process with his entire stock of muscle, nerve, brain, etc. Nature is such that no instructor can discharge his duty by sending to class only that part of his nervous or muscular energy he usually spends during a lecture. And the reason for the im-

[31] E.g., Marx, *Capital*, I, 589, and his "Wage, Labor and Capital" in K. Marx and F. Engels, *Selected Works* (2 vols., Moscow, 1958), I, 82. In one place, *A Contribution to the Critique of Political Economy* (Chicago, 1904), p. 34, Marx did use the term "service" in arguing that the question in the exchange value "is not as to the service which it renders, but as to the service which it has been rendered in its production," only to follow this remark by a sneer at J. B. Say and F. Bastiat. The remark, obviously, gives him away.

[32] Cf. Marx, *Capital*, I, 189 f and, especially, 622.

[33] *Ibid.*, 186. My italics.

[34] Engels, "Marx's Capital," in *Selected Works*, I, 464. My italics. See also Marx, *Capital*, I, 189.

[35] Marx, *Capital*, I, 190.

possibility is elementary. To teach, an instructor must be present in class with all his labor power, i.e., with the aggregate of all his "mental and physical capabilities." A service must not be confused with a partial decumulation of one's stock of energy even if one insists on considering only the material factors of an economic process. If it were true that one can cross a river on the maintenance flow of a bridge or drive a maintenance flow of an automobile on the maintenance flow of a highway, there would be practically no financial problem in saturating the world with all the river crossings and automotive facilities. Economic development itself could be brought about everywhere almost instantaneously. These are the well-concealed implications of Marx's doctrine in which the main agent of the economic process—the human being—is degraded to a mere stock of energy for the sole purpose that the material means of production may also be denied the quality of agent.

7. *The Production Function.* During the foregoing discussion of how a process may be represented analytically, one question should have brewed up gradually. It is this: why is a production process represented in Neoclassical economics by an ordinary vector (in which every coordinate is a number) if, as I have argued, each coordinate in the analytical representation of a process is a function of time? The opening remarks of this chapter contain the only explanation of the discrepancy: economists, more so than other scientists, have treated the concept of process in a cavalier manner. The tone was set by P. H. Wicksteed as he sought to improve on Walras' treatment of production by introducing the general concept of production function: "*The product being a function of the factors of production we have* $P = f(a, b, c, \cdots)$."[36] Numberless others after him have made and still make the same swift passage from "function" understood in the broadest sense to the "point function" of mathematics. In addition, Wicksteed's presentation leaves us completely in the dark on what process means and why a process is represented by an ordinary vector $(P, a, b, c, \cdots)$. The situation even worsened after the vapid terms of input and output spread throughout the economic literature. At their best, the modern works liken the description of a process to a recipe from a cookbook, which in itself is a good starting point. But the sequel is rather a regress. According to his cookbook—we read—an iron manufacturer knows that if he "mixes so much ore, so much lime, so much coke, and so much heat for so many hours, [he will get so] much iron."[37] One is thus invited to read only the list of ingredients, which in cookbooks is usually

[36] Philip H. Wicksteed, *The Co-ordination of the Laws of Distribution*, (London, 1894), p. 4. Reprinted as No. 12 of the Scarce Tracts in Economic and Political Science.

[37] Kenneth E. Boulding, *Economic Analysis* (3rd edn., New York, 1955), pp. 585 f.

printed above the recipe proper, and ignore the rest. Obviously, the recipe being reduced to "that much of this" and "that much of that," the description of the process, too, is reduced to a list of quantities.

Once this result is reached, albeit surreptitiously, the concept of production function encounters no difficulties. As Samuelson views it,[38] the production function is a catalog of all recipes found in the cookbook of the prevailing state of arts for obtaining a *given* product out of *given* factors. And since each recipe now tells us only that we can obtain the quantity z of product by using the quantities $x, y, \cdots$ of this and that factor, the catalog itself is reduced to a point function.

$$z = f(x, y, \cdots). \tag{10}$$

To quote Boulding again, a "basic transformation function of an enterprise is its *production function*, which shows what quantities of inputs (factors) can be transformed into what quantities of output (product)."[39] In this short sentence, there is packed almost every misleading notion that surrounds the conception of process in the economic literature.

Yet Boulding's idea that the description of a process is a recipe is, as I have already said, a very fortunate one. Let us start again from it. First, we should clarify our thoughts on one point. One may speak vaguely of a recipe for making, say, tables. But there is a host of such recipes. Tables are made in the shops of cabinet makers; they are also made in small-scale or large-scale industrial plants. At times, they are made out of dressed lumber and wood panels, at others out of raw lumber, and at others out of living trees. Whatever the case, I propose to consider that recipe which describes the partial process by which one table considered by itself is produced in each particular system of production.[40] I shall refer to such a partial process as an *elementary process*, on the ground that every production system of any type whatsoever is a system of elementary processes. In the shop of a cabinet maker the elementary process by which a piece of furniture is produced develops unclouded in front of our eyes. But even in a more complex system, it can be isolated if one follows the rules outlined earlier for drawing the boundary and recording the analytical coordinates of a partial process. The point is that the concept of elementary process is well defined in every system of production. In fact, it should not

[38] Samuelson, *Foundations*, p. 57.

[39] Boulding, *Economic Analysis*, p. 585.

[40] For products such as "gasoline" or "steel," the elementary process may be associated with a molecule or, better, with a "batch" appropriately chosen to fit the concrete conditions in each case. Even for bread, we may associate the elementary process with a batch of loaves, the number of loaves being determined by the capacity of the oven, for example.

be difficult to reconstruct it by an attentive examination of all the orders issued by the production manager.

Nothing need be added now to what has been said in some previous sections in order to see that precisely the process described by a cookbook recipe cannot be completely represented by an ordinary vector. Only an expression such as (9) can represent it completely. A catalog of all *feasible* and *nonwasteful* recipes then consists of a set of points in an abstract space, as opposed to Euclidean space. The set may be regarded as a variety within the abstract space and, hence, represented by a relation of the form

$$\overset{T}{\underset{0}{Q(t)}} = \mathscr{F}[\overset{T}{\underset{0}{R(t)}}, \overset{T}{\underset{0}{I(t)}}, \overset{T}{\underset{0}{M(t)}}, \overset{T}{\underset{0}{W(t)}}; \overset{T}{\underset{0}{L(t)}}, \overset{T}{\underset{0}{K(t)}}, \overset{T}{\underset{0}{H(t)}}], \tag{11}$$

which in mathematical jargon is called a *functional*.[41] This is a relation from *a set of functions to one function*. Consequently, (11) is a far cry from the Neoclassical production function (10), which is a point function, i.e., a relation from *a set of numbers to one number*. Yet there is a connection between (11) and (10). To unravel it and to make it explicit is our next task.

8. The Economics of Production. All elementary processes have one important feature in common. In relation to any given elementary process most of the fund factors involved in it must remain idle during a great part of the production time. This idleness, it should be emphasized, is not the result of our own fault or wish. It is an unavoidable consequence of the material conditions of the process itself. A superficial observation of a cabinet maker at work should suffice to convince us of the general validity of this truth. The saw, the plane, the sander, etc., are never used simultaneously in the production of a table considered by itself. Every tool is used by turns; in the meantime it lies idle. Should there be specialized workers—say, one specialized in operating the saw, another in applying varnish, etc.—they, too, would be idle by turns in relation to every elementary process. Moreover, all tools and all workers are idle (in the same sense) during the time when the varnished table is set out to dry. During this phase, nature is the silent partner of man, its forces operating through some flow elements included under (R). A flow input of oxygen from the air oxidizes the varnish while the varnish solvent evaporates as an output flow. All these facts are even more conspicuous in a farming

[41] The fact that the functional does not exist for every point $R, I, \ldots, H$ may well be ignored at this juncture. But we should note that, since the functional represents an elementary process, we have $Q(t) = 0$ for $0 \leq t < T$ and $Q(T) = 1$ with unity standing for the unit of product associated with the elementary process.

process, but they are part and parcel of any elementary process, be it in manufacturing, mining, construction, or transportation.

Another important observation is that if the flow of demand is such that only one table is demanded during an interval equal to or greater than the time of production, the production of tables has to be carried out by partial processes arranged *in series*, i.e., in such a way that no process overlaps with another in time. This was the case of every craft shop in older times and is now the case for canals, bridges, large maritime ships, and so forth. New plants also are ordinarily produced in series. The point to be retained is that a low intensity of demand imposes on most fund elements long periods of idleness. The human factor can find employment only by shifting periodically to other lines of production—as thousands of peasants do by seeking employment in the cities during the idle periods on the farm. But this seasonal employment also falls back on the existence of some demand. Besides, not all partial processes include sufficiently long periods of idleness to make the shift operative. We can understand now the reason why, as long as the demand for most manufactured goods remained at a very low level, specialization of tools and especially of labor was uneconomical. The craftsman of the Middle Ages, for instance, had to know how to perform all the tasks required by the elementary process of his trade. Otherwise, he would have had to remain idle part of the time and share with others the revenue accruing to labor. Under such conditions, specialization was *uneconomical*.

The case in which more than one table is demanded during an interval equal to the duration of the elementary process leads to two alternatives.

First, production may be carried out by the appropriate number, n, of elementary processes set *in parallel*, i.e., started at the same time and repeated after they are completed. In many cases the resulting system is a typical case of processes that are added externally.[42] To describe it we need only multiply every coordinate of the elementary process in point by n. The corresponding production function is then easily derived from (11):

$$[nQ(t)]_0^T = \mathscr{F}\{[nR(t)]_0^T, \cdots, [nW(t)]_0^T; (nL(t)]_0^T, \cdots\}. \tag{12}$$

The point that deserves to be stressed is that the arrangement in parallel offers little or no economic gain. Most kinds of fund factors are now needed in an amount n times as great as in the elementary process. In addition, the idleness of each such fund factor is *ipso facto* amplified by n. The only exceptions are the fund factors that—like a large bread oven, for

[42] For which see Chapter IV, Section 5.

instance—may accommodate several elementary processes simultaneously. But even though the capacity of such a fund factor would be more fully utilized, its idleness period would remain the same.

The second alternative is to arrange the appropriate number of processes *in line*. In this system, the time of production is divided into equal intervals and one elementary process (or a batch of such processes) is started at each division point. In more familiar language, the elementary processes are uniformly staggered in time. There is no need to go here over the mathematical proof—which, in fact, is quite simple—of the following proposition:

If the number of the elementary processes is sufficiently large and all periods during which each fund factor renders service are commensurable with the time of production, then there is a minimum number of elementary processes that can be arranged in line so that every fund factor is continuously employed.[43]

In plain words, the proposition says that if the demand for a product is sufficiently large, then production may be arranged so that no fund factor employed in it is ever idle. Obviously, this arrangement represents the factory system, where every tool and every worker shifts from one elementary process to the next as soon as they have performed their services in the first. No tool and no worker is thus idle during the time when the process of the whole factory goes on.

9. The Factory System and Its Production Function. To arrive at the analytical representation of the process consisting of a factory system we have simply to follow our basic rule of starting with one *tabula rasa* and ending with another and observe the distinction between flow and fund elements. The duration of the process to be described may be chosen arbitrarily: a factory system once set in order is a steady state in which all funds are kept in good working conditions at all times. However, to simplify the notational scaffold I shall make the perennial assumption of continuity, i.e., I shall assume that a batch of elementary processes is started at each instant of time and, by necessity, all flow elements are continuous entities. In this case, it is straightforward that the flow coordinates are homogeneous linear functions of t:

[43] For a diagrammatical illustration of this proposition, see Fig. 2 in my article "Process in Farming vs. Process in Manufacturing," cited in note 8, above. But because the point is related to some aspects of size, it deserves to be made more explicit. The number of elementary processes to be started at each division point is the smallest common multiple of the numbers of such processes that can be accommodated at the same time by each unit of the various funds. The intervals between two consecutive batches is T/d, where d is the greatest common divisor of T and of the intervals during which the various kinds of funds are needed in an elementary process.

$$(13) \quad R(t) = rt, \qquad I(t) = it, \qquad M(t) = mt, \qquad Q(t) = qt, \qquad W(t) = wt.$$

The same is true of the fund coordinates. But to render our representation more discriminating we need to set apart two new categories of capital funds. The first includes the stores of commodities, the inventories in the narrow sense, which are related to some (ordinarily, all) flow elements included under I, M, Q, and W. The real role of these stores is to dampen the irregular fluctuations in the number of accidents in the process of production and in the rhythm of sales. One must have on hand a certain number of fuses so that even if many fuses happen to blow at the same time they can be replaced without delay. A certain quantity of each product must also be stored in order to take care of any fortuitous concentration of orders.[44] Let us denote this category, generically, by S.

The second new category of capital funds corresponds to the familiar term of "goods in process." But "goods" is here a patent misnomer: melted glass, half-tanned hides, half-wired radio sets, for example, can hardly fit the term. It is nevertheless true that at any time there exists inside the factory system a *process-fund*, $\mathscr{C}$, in which is mirrored the entire transformation of the material inputs into the final products. The time of production of, say, an aircraft may be several months or even a couple of years. But in the process-fund of a factory producing such an aircraft there must exist at any time at least one "aircraft" in each phase of its transformation. If we take one photograph of each successive phase on the same film roll, and then project the film as if it were a movie, we will indeed see a movie—a movie showing how one aircraft is made out of metal sheets, motor parts, cables, etc. The whole qualitative change—a Becoming—is thus frozen, as it were, into a time-less Being—the process-fund. This fact explains my choice of the term.

Let us also note that without the *process-fund* no factory is complete. The role of the process-fund is fundamental. It can be likened to the water in the vertical pipe of a hand pump. Unless the water fills that pipe, the pump is not primed; we must work the pump for some time before we can get any water. If, on the contrary, the pump is primed, water starts to flow the minute we move the pump's handle. In a factory, too, the outputs included under Q and W begin to flow out the moment the factory opens in the morning and the inputs R, I, M begin to flow in. This is possible only because a factory at closing time is left in a primed state, with its $\mathscr{C}$ intact. The continuous maintenance of tools and buildings requires, we remember, some special assumptions. By contrast, the process-

[44] It is clear then that speculative inventories are left out of account—as they should be in a description of the purely material process of production.

fund is maintained by the very manner in which the elementary processes are arranged in a factory system.[45]

Looking at a factory from the outside, as the flow complex does, one would certainly see only the flow coordinates (13). Moreover, one may very well say that production is instantaneous, i.e., that a batch of input materials is instantaneously transformed into a batch of products. What happens in fact is rather similar to what happens when we push the end of a perfectly inelastic rod: the other end moves at the very same instant. In the case of a factory, the process-fund plays the role of such a rod. In all probability, it is this peculiar property of a factory system that has led some economists to argue that, since production is instantaneous, wages are always paid out of the product, never out of capital. The waiting doctrine of capital would thus be baseless. Of course, once a factory is built and primed, there is no longer any waiting. But both to build and prime a factory require duration. Only to prime an aircraft factory, for instance, we may have to wait several months.

The fund coordinates of a factory system being

$$(14) \quad L(t) = Lt, \quad K(t) = Kt, \quad S(t) = St, \quad \mathscr{C}(t) = \mathscr{C}t, \quad H(t) = Ht,$$

its analytical representation is now complete. And according to what has been said earlier, the production function of a factory process—the catalog of all factory processes by which the *same* products can be obtained from the *same* factors—is a functional involving all functions listed in (13) and (14):

$$(15) \qquad \overset{T}{\underset{0}{(qt)}} = \mathscr{G}[\overset{T}{\underset{0}{(rt)}}, \cdots, \overset{T}{\underset{0}{(wt)}}; \overset{T}{\underset{0}{(Lt)}}, \cdots, \overset{T}{\underset{0}{(Ht)}}].$$

In contrast with the functionals (11) and (12), where T is a physical coordinate determined by the nature of the elementary process, in this last functional T is a freely varying parameter. The consequence of this fact is that the relation expressed by (15) boils down to a relation only between the coefficients of t in the functions (13) and (14). In other words, the functional in this case *degenerates* into an ordinary point function, namely, into

$$(16) \qquad q = F(r, i, m, w; L, K, S, \mathscr{C}, H).$$

Alternatively, the same functional may be replaced by a point function

[45] A factory system is like a music box, which starts to play the moment it is opened and stops playing the moment it is closed. Of course, if laid idle for a long period of time, any factory would need some additional work to remove the damage done by the Entropy Law.

between the amounts of flows and services over an arbitrary time interval t. *But in this function t must appear as an explicit variable*:

$$qt = \Phi(rt, \cdots, wt; Lt, \cdots, Ht; t). \tag{17}$$

We should not fail to note that, in contrast with the function F of (16), Φ is a homogeneous function of the first degree with respect to *all* its variables, that is, including t.[46] Obviously, we have the identity

$$\Phi \equiv tF.$$

We can see now the thought which, possibly, may have guided the Neoclassical economists who, in the past as well as now, represent any production process by the jejune formula (10) about which we are told only that the dependent variable stands for "output" and all other symbols stand for "inputs". No wonder then that economists took liberties with the interpretation of these terms—some defining the production function as a relation between the quantity of product and the quantities of inputs, others as a relation between the output of products per unit of time and the input of factors per unit of time. Some have even adopted the two definitions within the same work. As the analysis developed in the foregoing sections clearly shows, once the production function is defined as a catalog of recipes, its formula cannot be decreed by our whims—reasonable though they may seem on the surface.[47] The production function is always a functional, either (11), or (12), or (15), according to the system in question.

That in the case of a factory we should prefer (16) to the pseudo functional (15) is perfectly natural. Yet (16) looks like a black sheep amidst the flock of other functionals: in contrast with them, it does not involve the time element. In the process of passing from the degenerate functional to the point function (16), we have let the time element slip through our analytical fingers. As a result, the production function (16) does not tell us what the corresponding system *does*, but only what it *may do*. The variables involved in it consist only of the rates of the flow factors and the sizes of the fund factors. They describe the process in the same manner in which the inscription "40 watts, 110 volts" on an electric bulb or "B.S. in Chemical Engineering" on a diploma describe the bulb or the engineer. Neither description informs us how long the bulb burnt

[46] This homogeneity expresses the trivial truth that the flows and the services of any factory during, say, eight hours are eight times as great as during one hour. Clearly, it has absolutely no bearing on whether there is an optimum size of the unit of production or not. See also note 48, below.

[47] For further details, some apparently so surprising that they were denounced as false on the spot when I first presented them, see my articles cited in notes 1 and 8 above.

yesterday or how many hours the engineer worked last week. Similarly, (16) may tell us that a man with a 100 hp tractor which uses three gallons of gasoline and one quart of oil per hour can plow two acres per hour.

It stands to reason then that what a factory is capable of doing is a function of its purely technical structure alone. The point is that a competent person should be able to determine from the blueprint of a factory what the factory can do and also what it needs for this. Consequently, the production function (16) may be decomposed into several elements which together constitute a more faithful picture of the factory process. The first two elements are

$$(18) \qquad q^* = G(L, K), \qquad H^* = H(L, K),$$

where q^* represents the maximum rate of product flow of which the factory is capable if properly manned with H^*. However, the human element being as variable as it is in actuality, q^* is rather an unattainable limit. To take into account that the actual rate of product flow depends on the quality as well as the size of the personnel employed, we have to replace (18) by

$$(19) \qquad q = f(L, K, H) \leq q^*.$$

This relation should not, however, be confused with the form currently used in theoretical and applied works. As defined by (19), if $q < q^*$, q need not (and usually does not) decrease if either L and K are decreased while H is kept constant.

The other fund factors, S and $\mathscr{C}$, are also determined by the same basic funds, L, K, H. Hence, we have

$$(20) \qquad S = S(L, K, H), \qquad \mathscr{C} = C(L, K, H).$$

There remains to examine the relations binding the other flow elements. The case of the maintenance flow is easily settled. Its rate must be a function of the capital to be maintained and of the labor fund. Moreover, by virtue of the Conservation Law of matter and energy, it must be equal to the flow rate of wear-and-tear waste, w_1. We are thus led to put

$$(21) \qquad m = m(K, H), \qquad w_1 = m.$$

These relations take care also of the fact that the capital proper may be more intensively or less intensively used according to the size of the manpower employed.

According to the same Conservation Law of matter and energy, there must exist in each case some relation between the other input and output flows:

$$(22) \qquad qt = g(rt, it, w_2t),$$

where w_2 denotes the flow rate of that waste which arises only from transformation. Since (22) must be true for any positive value of t, the function g must be homogeneous of the first degree. This result may be reached also by a familiar argument: double the amounts of timber, of impregnating material, *and of waste*, and the amount of railroad ties will necessarily double, too.[48] However, to double q, we need another factory, i.e., another fund combination (L, K, H). With this new combination, the amount of waste may not be (and usually is not) doubled. This is precisely one meaning of the statement that one technical recipe is more efficient than another: the value of w_2 is smaller for the more efficient recipe. We are thus led to put

$$w_2 = w_2(L, K, H). \tag{23}$$

Relation (22) then becomes

$$q = g[r, i, w_2(L, K, H)]. \tag{24}$$

To sum up, the catalog of all factory systems that produce the same products with the same factors (flow or fund) consists not of one, but of seven basic functions, listed as (19), (20), (21), (23), and (24) in this section. There are therefore some definite limitationalities inherent to the structure of production by the factory system. Technical features peculiar to each process may introduce additional binding relations between factors. We may recall the customary examples of gold in the production of wedding rings and of a tractor needing only one driver. But these special cases apart, we must not jump to the conclusion that the factors included in any of the point functions representing the catalog of the factory recipes for a given product are substitutable in the sense assumed by the current theory of production. To recall, in these point functions K represents generically capital equipments of various qualities, K_i meaning a certain amount of capital of "quality i." The same applies to L and H. Moreover, there is not necessarily a process corresponding exactly to every possible combination (L, K, H). A more capital intensive process normally requires a different type of capital. Therefore, if we consider a given process, there may be no process corresponding to the substitution of more of K_i for a decrease in H_j. Substitution means rather that K_c and L_d are used instead of K_a and L_c. And if this is the case, substitution

[48] It may be well to point out that this argument does not imply that there is no optimum size of a factory, although those who have argued against the existence of the optimum size may have been influenced by it. The absence of the optimum size requires that the functions in (18) be homogeneous of the first degree. Cf. Chapter IV, Section 4, above, and my article "Chamberlin's New Economics and the Unit of Production," cited in note 1, above.

cannot be represented in terms of two coordinates—one representing "capital," the other "labor"—as is done in the familiar map of isoquants. Neoclassical economists, after censuring Marx for his idea that every concrete labor is a congealed form of general abstract labor, returned to their own shop to outdo him in this very respect by assuming that concrete capital, too, is congealed abstract capital.

As a highly abstract simile, the standard form of the Neoclassical production function—as a function of K, the cardinal measure of homogenous "capital," and H, the cardinal measure of homogeneous "labor"—is not completely useless. But, in sharp contrast with the ophelimity function (where substitutability is a result of the individual's subjective weighing), the value of the standard form of the production function as a blueprint of reality is nil. It is absurd therefore to hold on to it in practical applications—as is the case with the numberless attempts at deriving it from cross-section statistical data. The K_i in these data are not all qualitatively identical and, hence, have no common measure. For the same reason, there is no sense of speaking of the elasticity of substitution between homogeneous capital and homogeneous labor. Marginal productivity, too, comes out as an empty word. True, capital and labor may be rendered homogeneous but only if they are measured in money. All this shows that the theorems which adorn the theory of marginal pricing are in essence misleading analytical ornaments. In fact, to explain the adaptation of production to prices, whether in the case of a factory or any other arrangement of elementary processes, we do not require the existence of either Neoclassical substitutability or marginal physical productivity. Such an adaptation is secured regardless of the number and the nature of the limitationalities a production function may contain.[49] Cost is the only element that counts in this problem. And in cost, all qualitative differences between factors vanish into one homogeneous entity, money. The only role the production function (as developed above) has in this particular case is to enable us to know what factors, and in what amounts, enter into the cost of every possible factory process. As I have argued elsewhere, E. H. Chamberlin's "idea of analyzing the problem of optimum scale with the aid of a diagram of a family of average cost curves seems far more promising than using the production function and its isoquants—however more respectable the latter approach may be."[50]

10. Production and the Time Factor. I have already underscored the

[49] For which see my article reprinted as Essay 7 in AE.

[50] See my article "Measure, Quality, and Optimum Scale" in *Essays on Econometrics and Planning Presented to Professor P. C. Mahalanobis* (Oxford, 1964), p. 255.

fact that the basic relation (19) does not tell us what a factory does. To describe what a factory did yesterday or what it does every day, we need an additional coordinate, not included in (19). This coordinate is the time during which the factory works each day. We may refer to this time interval as the working day of the factory and denote it by δ. If the ordinary day is taken as time unit, then $\delta \leq 1$. The daily output of a factory, $Q = \delta q$, follows straightforward from (19):

$$Q = \delta f(L, K, H). \tag{25}$$

This formula is again a far cry from the Neoclassical production function (10), which does not contain the time factor as an explicit variable. I can foresee that this statement may be questioned on the ground that in the Neoclassical formula the symbols stand for *quantities* of flows and services, and thus the time factor is not ignored. Many economists have indeed proclaimed on intuitive grounds that the production function is a relation between quantities.[51] But their intuition has failed to perceive one point which is made so obvious by the analysis in the foregoing section. The relation between the quantity of product and the quantities of flows and services must include time as an explicit variable—as in (17). The conclusion is that no matter what position we consider—whether the symbols in (10) represent rates of flows and services or represent amounts of flows and services—the Neoclassical mode of representing the production function ignores the time factor.

This is a regrettable, albeit understandable, regress from Marx's analysis of the production process in which the time factor—whether as the time of production of what I have called an elementary process or as the working day of the worker—occupies a quite prominent place.[52] Marx looked for every analytical element that may evolve historically. The Neoclassical school, on the contrary, planned to ignore the march of history. Indeed, the most favorable excuse for the omission of the working day from the formula intended to describe what factories do is that the Neoclassical economists regard δ as a given social coordinate. Being a given coefficient, δ does not have to appear explicitly in a general formula any more than any other physical coefficients.

This excuse does not alter the fact that the consequences of the omission of the factory's working time from the standard analytical apparatus are more complicated than one would like to think. Some are aggravated by

[51] See note 39, above.

[52] Cf. Marx, *Capital*, vol. I, ch. x. Incidentally, formula (25) lends support to one dearest tenet of Marx's, namely, that labor time, though it has no value itself, is a measure of value. *Ibid.*, pp. 45, 588.

another fault of the same apparatus—the confusion of (19) with (18). This confusion is tantamount to another omission, that of neglecting the intensity of capacity utilization, which is measured either by q/q^* or by H/H^*. Both omissions seriously vitiate the studies in which the argument involves the capital-output or the capital-labor ratio and which have been rendered highly popular by some of the highest economic authorities as well as by such respectable institutions as the National Bureau of Economic Research.

In the light of the preceding analysis, it is clear that an objective definition of capital intensiveness in a factory process must be grounded in relations (18). Hence, K/q^*—alternatively, K/Q^* where Q^* corresponds to q^* and $\delta = 1$—and K/H^* constitute the only objective measures of capital intensiveness. The point deserves unparsimonious emphasis: capital intensiveness is essentially a coordinate of the factory's blueprint, not of what a factory happens to do. It would be a gross mistake to measure capital intensiveness by the ratio $K/Q = K/(\delta q)$: the daily (or the annual) production, Q, varies both with δ and the intensity of capacity utilization. The same applies to the capital-labor ratio measured by K/N, where N is the *average* number of employees (or of the production workers) over the year: N varies with both the intensity of capacity utilization and the number of shifts. Clearly, if *ceteris paribus* a factory passes from using one shift to using two shifts of the same size, K/N would be halved, even though the capital intensiveness of the process has not changed. The ratios K/Q and K/N, therefore, are affected by the working day of the factory, the number of shifts, and the intensity of capacity utilization. These coordinates, in turn, fluctuate according to the momentary business outlook in the corresponding line of activity. The moral is that any comparisons of the ratios K/Q or K/N, either between one year and another for the same industry or between two industries for the same year, do not necessarily reflect a change in capital intensiveness. This is especially true of interindustrial comparisons.

Yet, to my knowledge, all studies concerned with capital intensiveness use these last measures of capital-output and capital-labor ratios. And even though one finds occasional mention of some possible reason for the noncomparability of these measures, I know of no author to insist on all the implications of the problem of measuring capital intensiveness. The curious thing is that, had anybody seen that the correct measures are K/q^* and K/H^*, he would have not been able to arrive at a statistical estimation of these ratios. The reason is that even the most sophisticated statistical agencies do not include in their censuses of manufactures the data required for deriving these last ratios from the ordinary data on production, capital, and employment. Nothing more normal for a statistical bureau than to orient its data collection according to the inventory of the tool box of the analytical social scientist.[53]

Now, the consequences of the fact that the elements mentioned in the preceding paragraphs have been omitted from the analytical tools of the Neoclassical economist are not confined to purely academic matters. The omission of the length of the working day, δ, is responsible, I believe, for the strange fact that no Neoclassical planning expert seems to realize that, as correctly assessed by Marx and confirmed by (25), one of the "secrets" by which the advanced economics have achieved their spectacular economic development is a long working day.[54] The length of the working day, although an economic lever that can be used directly and without delay, is not a coordinate in any Neoclassical model of economic development found in the general literature and, probably, in any other. In view of our loudly proclaimed aims, to help the underdeveloped economies not only to make progress but to make *rapid* progress, the legal regimen of the eight-hour day in such economies (even in those where overpopulation brings about unwanted leisure) is a patent incongruity, if not a planned anachronism as well.

Were we in the situation in which there were enough manpower to keep all factories working around the clock by four, six, or even twelve shifts, there would be no economic objection (besides the cost of changing shifts) to have a six-hour, a four-hour, or a two-hour day. But what underdeveloped economy, nay, what economy is in this position? The basic shortage in underdeveloped economies—as we have finally come to realize recently[55]—is capital in both its forms: *machines and skilled labor*. The two go together simply because skilled labor is a package of labor *and* skill and because skill is akin to capital: it takes time to acquire it.[56]

[53] An excellent example is supplied by the epochal impact the Leontief system had on the collection of statistical data pertaining to interindustrial transactions.

[54] We need not rely only on the relation by F. Engels in his *The Condition of the Working Class in England in 1844* (London, 1892). According to W. S. Woytinsky and Associates, *Employment and Wages in the United States* (New York, 1953), p. 98, in the United States as late as 1850 the average working week was seventy hours. The first attempt to limit the work of children under twelve to a ten-hour day was made only in 1842 by the Commonwealth of Massachusetts. The ten-hour day did not become a widespread rule for the other workers until 1860. See Philip S. Foner, *History of the Labor Movement in the United States* (4 vols., New York, 1947), I, 218, and G. Gunton, *Wealth and Progress* (New York, 1887), pp. 250 f.

[55] E.g., Theodore W. Schultz, *The Economic Value of Education* (New York, 1963).

[56] Strangely, this last point has been long ignored by those who opposed the idea that in many countries overpopulation is a reality which requires an economic handling different from that prescribed by Neoclassical economics. Cf. my article "Economic Theory and Agrarian Economics" (1960), reprinted in AE, pp. 372–374.

Ordinarily, the shortage of skilled labor in underdeveloped economies is so acute that many a factory cannot be worked around the clock. In this case, the idleness of the inert factors would not be inherent to the physical nature of the process itself—as in the case of an elementary process—but to the shortage of their human companions in work. To set the same legal limit to the working day as in the advanced economies—where, thanks to the abundance of capital, leisure has economic value—is tantamount to decreeing an unnecessarily high amount of idleness and a cut in the potential income of the country. For the same reason, any factory built in an underdeveloped country in addition to any other producing the same product and operating with only one or two shifts of eight hours each is a waste of resources. If there is already, say, a shoe factory which works with only one shift, it makes no economic sense to build another shoe factory also operated by one shift. The two shifts can produce the same output (practically) with the old factory, and the additional capital can be invested in another line to support further growth.

"Economic Development Takes Time" would make a very appropriate inscription above the entrance of every economic planning agency, so that the passers-by be continuously reminded of the bare truth, however disappointing. But inside every office the inscription should read "Do not make this time longer by unnecessary idleness." For unnecessary idleness results in a waste of time. I am convinced that all economic plans harbor, in a larger or smaller measure, idleness unconsciously planned. No wonder we feel or even recognize occasionally that most plans of economic development have not been speedy enough. Perhaps all this could be avoided if in planning economic development we would bear in mind the economic object lesson of the factory system.

11. The Factory: Its Advantages and Limitations. A factory is such a familiar object in the industrialized world in which most economists have been reared that we seem to have lost sight of two important facts.

The first fact is that the factory system is one of the greatest *economic* inventions in the history of mankind—comparable only to the invention of money but just as anonymous in origin. The word "economic" should indeed be underscored, because the advantages of the factory system are independent of technology and also above it. We may be told that the factory system was a creation of the industrial revolution, that is, of the mass of technological innovations of the eighteenth century and thereafter. In my opinion, the causal relationship is the reverse: the factory system, which had already begun to be practiced in the old craft shops because of an increased demand, was one of the main factors that spurred the technological innovations.

The factory system, as the preceding sections amply attest, is superior to all other arrangements of the elementary processes, not because it increases the power of a tool or the command of man over natural forces, but because it does away with the idleness of the fund factors which is inherent to any recipe. And the gain is availing whatever the technology may be: cloth could be produced by a factory system using the technique of the Egyptians in the time of the Pharaohs. Whether we can take advantage of this gain depends, not on the technology available, but on the level of the demand for the product under consideration. To wit, if transoceanic Queens are not produced under a factory system, i.e., in line, it is only because they are not demanded fast enough in relation to the time of production. Strange though it may seem, if the technology in shipbuilding were still that of a hundred years ago, we might be building Queens in line provided the demand for them would still be what it is today. In some cases, therefore, technological progress may work against the factory system if the demand does not increase in step with it.

The upshot is that the intimate connection which undoubtedly exists between the factory system and technological progress involves mainly the work of demand. Just as a low intensity of demand renders uneconomical any specialization, so an increase in demand paves the way for further specialization. The point is easily proved by observing that if a particular task of an agent in an elementary process is divided into several distinct tasks, the number of elementary processes needed for an arrangement in line without any idleness generally increases (and rather sharply). The output flow, therefore, must also increase. If the demand flow does not increase in the same proportion, specialization would only result in costly idleness.[57]

The role of demand as a stimulant of technological innovation is seen even in those cases in which, for some reason or other, the elementary processes have to be set in parallel. To wit, as the demand for bread in a small community increases, the baking industry may find it economical to replace the ovens used daily in parallel by a larger oven instead of adding more ovens of the same size. Actually, technological progress has always consisted of a blend of specialization *and* concentration of several tools into one unit of a larger but more efficient capacity. In both cases, the result has been an increase in the size of the unit of production. The limits beyond which this size cannot go are set by the laws of matter, as

[57] As we all know, it was Adam Smith who first argued that "the division of labor is limited by the extent of the market." *The Wealth of Nations* (ed. Cannan), I, 19. But the analysis of the factory system in Section 9 and especially the theorem of note 43, above, set this proposition on a clear foundation and also extend it to the specialization of capital equipment as well.

we have seen in Chapter IV, Section 4, but what stimulates the increase is a growing demand alone.[58]

The second fact of which we often lose sight is that the factory system cannot be applied to everything man needs or wants to produce. We have already seen that one obstacle is a low demand for some commodities. Another, more subtle, reason is related to the fact that normally *we produce not only commodities but also processes.* Only in a stationary economy is production confined to commodities. Because in such an economy every extant process maintains itself, none needs to be produced. But in a changing world we must also produce new processes, in addition to those that exist or in place of those that have become obsolete. And it stands to reason that it is impossible to produce all these processes by factory systems. At least the factory producing a new type of factory must be produced anew, that is, not by an existing factory. A third reason, the most relevant of all for the actual world, has its roots in the conditions of human life on this planet.

12. The Factory and the Farm. In order to arrange the elementary processes in line uninterruptedly, it is necessary that we should be able to start an elementary process at any moment in Time we may please. In a great number of cases we can do so. A hobbyist, for instance, is free to start his project of making a desk at three o'clock in the morning, on a Monday or a Friday, in December or August. Without this freedom the production of furniture, automobiles, coke, etc., could not go uninterruptedly around the clock throughout the year in factory systems. By contrast, unless one uses a well-equipped greenhouse one cannot start an elementary process of growing corn whenever he may please. Outside a few spots around the equator, for every region on the globe there is only a relatively short period of the year when corn can be sown in the fields if one wants a corn crop. This period is determined in each place by the local climatic conditions. These, in turn, are determined by the position of our planet and its rotation in relation to the sun as well as by the geographical distribution of land and water on the surface of the globe. So vital is the dependence of terrestrial life on the energy received from the sun that the cyclic rhythm in which this energy reaches each region on the earth has gradually built itself through natural selection into the reproductive pattern of almost every species, vegetal or animal. Thus, lambs are born in the spring, chickens hatch in early spring, calves are

[58] Because of this connection between demand and the technological recipes, I take exception to the view, shared by many of my fellow economists, that for the economic theorist the production functions are given data "taken from disciplines such as engineering and industrial chemistry." Stigler, *Theory of Competitive Price*, pp. 109 f. See also Pigou, *Economics of Stationary States*, p. 142; Samuelson, *Foundations*, p. 57; J. R. Hicks, *Theory of Wages*, p. 237.

born in the fall, and even a fish such as the turbot is not worth eating unless caught in April or May. So, in husbandry too an elementary process cannot be started except during one specific period dictated by nature.

These facts are commonplace. Yet the general tenor among economists has been to deny any substantial difference between the structures of agricultural and industrial productive activities. In the socialist literature of the past this fact was unmistakably reflected in the claim that under socialism the backward farms will be replaced by "open-air factories." In the Neoclassical literature the production function (10) is used regardless of whether the problem at hand refers to agricultural or industrial activity.[59] The elementary processes in agricultural production, however, cannot be arranged in line without interruption. True, if we view a corn plant as a unit of product, the elementary processes are arranged in line as the plowing and the sowing go on. The rub is that this line cannot go on forever: there is a point in time after which no seed sown will mature properly into a plant. In order that all the corn fields in a climatic region be cultivated in time, farmers have to work their fields *in parallel*. In view of the short length of time during which the field of a single farm is plowed, seeded, weeded, or harvested, it is quite safe to describe the production system of each individual farm by assuming that all elementary processes are started at the same time. With this convenient simplification, the production function of a farm system is the *nondegenerate* functional (12) of Section 8, above.[60]

Again, the difference between this production function and that of the factory—the point function (16) or (17)—is not a mere academic nicety. On the contrary, it teaches us some important economic questions. Long ago, Adam Smith argued that "the improvement of the productive powers of labor [in agriculture] does not always keep pace with their improvement in manufactures."[61] The proposition led to the controversy over the difference of returns in agriculture and industry and thus failed to be retained in modern economic thought. However, the foregoing analysis

[59] Actually, in no other economic field are so many studies confined to merely fitting a production function—usually, the perennial Cobb-Douglas type—to some particular product in a particular region as in agricultural economics.

[60] For completion, I may add that there are other activities besides agriculture which are subject to the rhythm of the climate: hostelry in tourist resorts comes immediately to mind, and so does construction. Most of what can be said about cost of production in agriculture applies *mutatis mutandis* to such activities, too. Thus, if you happen to arrive in Oslo and find no room to your liking, do not blame the Norwegians for not building more or bigger hotels for tourists. Such hotels would be idle during ten months each year, so short is the tourist season there. Only a millionaire can afford the waste of a villa on the French Riviera which he occupies only a few days each year, if at all.

[61] *The Wealth of Nations*, I, 8.

reveals one of the deep-seated reasons why the proposition is true even in a stronger form. One reason why technological progress has, by and large, proceeded at a slower rate in agriculture is that agricultural elementary processes cannot be arranged in line.[62] Curiously, the association of agricultural activity with an appreciable amount of labor unemployment is a fact accepted even by those who challenge Adam Smith's proposition. But our analysis not only shows why this association is inevitable, but also brings to the surface some interesting aspects of it.

There is one important difference between industrial and agricultural unemployment. An idle industrial worker is free to take a job and stay with it. A farmer even when idle is still tied to his job. If he accepts a regular job elsewhere, he creates a vacancy on the farm. Only in the case of overpopulation are there villagers unemployed in the strict sense of the term. But the inherent *idleness* is present wherever agricultural production is a system of processes in parallel—overpopulation or no overpopulation.

To do away with unemployment proper is a difficult but not an intricate task. However, to do away with agricultural idleness is a well-nigh insoluble problem if one stops to think about it in detail. For should we try to find different agricultural activities which, if spliced, would completely eliminate the idleness of the farmer *and* his implements, we will discover an insuperable obstacle. Nature, as the silent partner of man, not only dictates to man when he should start an agricultural process, but also forbids him stopping the process until it is completed. In industry we can interrupt and start again almost any process whenever we please, but not so in agriculture. For this reason, trying to find agricultural processes that would fit exactly in the idleness periods of one another is a hopeless enterprise. The "romantic" Agrarians had their feet on the ground after all as they insisted on the beneficial role of the cottage *industry* as a complementary activity in underdeveloped agricultural economies. But even with cottage industries that would splice perfectly with the idleness periods of the human capital employed in agricultural activities, the capital proper would still remain idle over large intervals of time. The conclusion may be surprising, but it is inescapable. The predicament of agriculture as an economic activity is *overcapitalization*. Nothing need be added to see that this predicament holds the key to a rational economic policy for any underdeveloped agricultural economy.[63]

[62] An even more important reason for this difference will be discussed in Chapter X, Section 3, below.

[63] In such economies, overcapitalization is often aggravated by a land distribution such that the size of most farms is smaller than the optimum. Cf. my article "Economic Theory and Agrarian Economics," reprinted in AE, p. 394.

Two exceptions to the rule that the production function of a farm system is a functional such as (12) will help bring to the surface other important differences between the economy of the farm and that of the factory.

Take the case of the Island of Bali where, because the climate is practically uniform throughout the year, one can see all the activities (plowing, seeding, weeding, harvesting) performed at the same time on various fields. On a spot such as this, certainly, nothing stands in the way of growing rice by elementary processes arranged in line, by an open-air factory. The proper number of buffaloes, plows, sickles, flails, and villagers operating them could move over the entire field of a village, plowing, seeding, weeding, and so on, without any interruption, i.e., without any agent—land, capital, and labor—being idle at any time. The advantages of the factory system can in this case be easily pinpointed. First, the villagers would eat each day the rice sown that very day, as it were, because in a factory system, we remember, production is instantaneous. There would be no longer any need for the community to bear the specific burden of the loans for agricultural working capital which constitute everywhere the farmer's major headache. The overcapitalization of which I have just spoken will now appear as a palpable excess of capital to be used in other activities. For, as we would try to implement the factory system, we would be left with a residual of superfluous implements (and superfluous men) even if the older units of production were of the optimum size.

How tremendous the impact of the conversion from farm to factory on the cost of production may be is made crystal clear by the second exception. The exception is the system by which chickens are nowadays produced in the United States on practically every farm. With the use of the incubator, chickens are no longer produced in parallel as in the old system dictated by nature. A crop of chicken is ready for the market practically every day of the year, be it in August or in December. "Chicken farm" has thus become a misnomer: the situation calls for replacing it by "chicken factory." Because of the new system, a pound of chicken sells in the United States for less than a pound of any other kind of meat, while in the rest of the world, where the old system still prevails, chicken continues to be "the Sunday dinner." The famous "chicken war" of yesteryear would not have come about if the difference between the farm and the factory system in producing chickens had not been so great as to cover the shipping cost and the differential labor cost between the United States and Europe.

13. Internal Flows and Analysis. The analytical decomposition of a partial process into flow and fund coordinates bears on an incongruity associated with the Leontief input–output table. In fact, the incongruity goes back to Karl Marx—the first user of such a table. Thanks to Leontief's contribution, the input–output table no longer needs any introduction: it is now one of the most popular articles of the economist's trade. However, the point I wish to bring home requires that the relation between the input–output table and the ideas developed in this chapter should first be made as clear as possible.

TABLE 3
Economy E Represented in Process Form

	P_1	P_2	P_3	N	P_4
		Flow Coordinates			
C_1	x_1^*	$-x_{12}^*$	$-x_{13}^*$	*	$-x_{14}^*$
C_2	$-x_{21}^*$	x_2^*	$-x_{23}^*$	*	$-x_{24}^*$
C_3	$-x_{31}^*$	$-x_{32}^*$	x_3^*	*	$-x_{34}^*$
R	$-r_1^*$	$-r_2^*$	$-r_3^*$	r^*	$-r_4^*$
W	w_1^*	w_2^*	w_3^*	$-w^*$	w_4^*
		Fund Coordinates			
C_1	X_{11}^*	X_{12}^*	X_{13}^*	*	X_{14}^*
C_2	X_{21}^*	X_{22}^*	X_{23}^*	*	X_{24}^*
C_3	X_{31}^*	X_{32}^*	X_{33}^*	*	X_{34}^*
$\mathscr{C}$	$\mathscr{C}_1^*$	$\mathscr{C}_2^*$	$\mathscr{C}_3^*$	*	*
L	L_1^*	L_2^*	L_3^*	*	L_4^*
H	H_1^*	H_2^*	H_3^*	*	H^*

A very simple illustration will serve this purpose much better than the general structure commonly used in the studies of the Leontief input–output system. Thus, let E be a stationary economy surrounded by its natural environment N and consisting of three production sectors P_1, P_2, P_3 and one consumption sector P_4. To remain within the rationale of Leontief's own system, let us assume that each productive process P_i produces only one commodity C_i and that there is only one quality of natural resources R, of waste W, and of labor power H. For the same reason, each process will be represented by its flows and services over one year. The notations being the same as in the foregoing sections, this means that the coordinates are now $R(T = 1)$ and $H(T = 1)$, for instance, instead of $R(t)$ and $H(t)$—quantities instead of functions.[64] If for the convenience of diction, we use a star to show that a notation stands for the annual flow or service, the analytical representation of the five

[64] For the particular purposes intended by Leontief for his input–output system the fact that the seasonal rhythm of some processes is ignored in this simplified representation does not matter.

processes into which we have decomposed the whole actuality is laid out in Table 3.[65]

This table involves an algebraic key. Because of the tautological truth that every output flow of a process is an input flow of some other process(es)—and vice versa—every row of the flow matrix must add up to zero. For example, we must have $x_1^* = x_{12}^* + x_{13}^* + x_{14}^*$. We may therefore delete the elements $x_1^*, x_2^*, x_3^*, r^*, -w^*$ without discarding any information. However, for facility in reading the table we may write them in an additional column. And if, in addition, we change the sign of the other flow coordinates, we have simply transformed the flow matrix of our table into the input–output form shown in Table 4.[66]

TABLE 4
The Input–Output Table of Flows in Economy E

	P_1	P_2	P_3	N	P_4	Totals
C_1	*	x_{12}^*	x_{13}^*	*	x_{14}^*	x_1^*
C_2	x_{21}^*	*	x_{23}^*	*	x_{24}^*	x_2^*
C_3	x_{31}^*	x_{32}^*	*	*	x_{34}^*	x_3^*
R	r_1^*	r_2^*	r_3^*	*	r_4^*	r^*
W	$-w_1^*$	$-w_2^*$	$-w_3^*$	*	$-w_4^*$	$-w^*$

Two obvious points should be stressed now. The first is that an input–output table is only a scrambled form (according to some definite rules) of the corresponding flow matrix of the process representation. Consequently, the flow matrix and the input–output table are two completely equivalent forms. Given one of these forms, we can derive the other straightforwardly. The second point is that, because of the particular rules of scrambling, some boxes in any input–output table must always be empty. Such are the first four diagonal boxes of Table 4.[67]

[65] At this time, there is no need to separate each x_{ik}^* into a current input and a maintenance flow or each X_{ik}^* into the services of a store and of an equipment fund. Nor do we need to concern ourselves with the *stocks* of R and W in nature, except to note that because of the Entropy Law they have decreased and increased during the year by more than r^* and w^*.

[66] Leontief includes in the input–output table the "flow" of labor services which he regards as the "output" of the consumption sector. See Leontief, *The Structure of the American Economy: 1919–1939*, pp. 41 f and *passim*; Leontief *et al.*, *Studies in the Structure of the American Economy*, pp. 23, 55. I prefer to abide by the fundamental difference between flow (as a substance that crosses a boundary) and service (as an action performed by a fund element inside the boundary). Also in Table 3, H^* of column P_4 stands for the consumption activity of the entire population of E—which I believe to be the correct analytical representation of that process.

[67] These diagonal boxes correspond to *product* coordinates.

Many writers believe that a greater degree of generality is nevertheless reached if we fill these boxes with some elements.[68] The difficulty with this position is the question of what precisely corresponds to these diagonal elements in actuality. No one, to my knowledge, has put forward the thesis that an input–output table represents an entirely new conception of how a process may be represented analytically. This being so, the burning question is what place we should assign to the diagonal elements of an input–output table when we rescramble it into the matrix flow of the process form. If we add them to the marginal totals and treat such a sum as the product flow of the corresponding process, we are simply admitting that we did not follow the scrambling rules to the letter. The fact remains that no one seems to have thought of the issue raised by the diagonal elements in relation to the equivalence of the two forms. The sparse justifications offered for the input–output table in which the diagonal boxes are not necessarily empty have approached the issue from some side line.

The issue arose in connection with consolidation. Because the problem at hand does not always require that all production processes be explicitly distinguished in the analytical framework, the economist often consolidates several processes into a single process. This operation by itself raises no difficulty whatsoever. All we need to do in order to consolidate P_1 and P_2 into P_0 is to remove from our analytical picture the boundary that separates them. The effect on Table 3 is straightforward: the columns P_1 and P_2 are added horizontally into a new column P_0 that replaces the others.[69] This introduces, however, one dissonant feature: the consolidated process P_0 has *two* products C_1 and C_2,[70] a reason why economists generally do not stop here. We prefer to pair each process with one product only. In a sense, it seems natural that if we have consolidated several processes into one "metallurgical" industry, we should also aggregate

[68] E.g., O. Eckstein, "The Input–Output System: Its Nature and Its Uses," in *Economic Activity Analysis*, ed. O. Morgenstern (New York, 1954), pp. 45 ff; M. A. Woodbury, "Properties of Leontief-Type Input–Output Matrices" in that same volume, pp. 341 ff.

[69] This rule sounds like the ultrafamiliar rule for the addition of vectors. However, there is more to it. For instance, the flow coordinates x_1^* and $-x_{12}^*$ must be replaced by their sum $x_1^0 = x_1^* - x_{12}^*$ because the sum of the corresponding row must, as we have noted earlier, add up to zero. The reason why the fund coordinates X_{11}^* and X_{12}^* must also be replaced by their sum $X_{10}^* = X_{11}^* + X_{12}^*$ is, however, different: when a boundary that separates two processes is removed, the actions of the corresponding fund factors are obviously pooled together.

[70] The flow matrix of the new representation can nevertheless be transformed into an input–output table. Only, this table has one column less than Table 4. The point supplies an additional clarification of the relation between an input–output table and the representation in process form.

their products into one "metallurgical" product.[71] That is why in economics "consolidation" means *consolidation* of processes and *aggregation* of the corresponding products.

TABLE 5
The Consolidated Form of Table 3

	P_0	P_3	N	P_4
	Flow Coordinates			
C_0	x_0^* $(x_1^* + x_2^* - x_{12}^* - x_{21}^*)$	$-x_{03}^*$ $(-x_{13}^* - x_{23}^*)$	*	$-x_{04}^*$ $(-x_{14}^* - x_{24}^*)$
C_3	$-x_{30}^*$ $(-x_{31}^* - x_{32}^*)$	x_3^*	*	$-x_{34}^*$
R	$-r_0^*$ $(-r_1^* - r_2^*)$	r_3^*	r^*	r_4^*
W	w_0^* $(w_1^* + w_2^*)$	w_3^*	$-w^*$	w_4^*
	Fund Coordinates			
C_0	X_{00}^* $(X_{11}^* + X_{12}^* + X_{21}^* + X_{22}^*)$	X_{03}^* $(X_{13}^* + X_{23}^*)$	*	X_{04}^* $(X_{14}^* + X_{24}^*)$
C_3	X_{30}^*	X_{33}^*	*	X_{34}^*
$\mathscr{C}$	$\mathscr{C}_0^*$ $(\mathscr{C}_1^* + \mathscr{C}_2^*)$	$\mathscr{C}_3^*$	*	*
L	L_0^* $(L_1^* + L_2^*)$	L_3^*	*	L_4^*
H	H_0^* $(H_1^* + H_2^*)$	H_3^*	*	H^*

If we denote the aggregate commodity of C_1 and C_2 by C_0, the effect of the consolidation (in the above sense) of P_1 and P_2 is shown by Table 5. The rule is simple: we add the columns P_1 and P_2—as already explained—

[71] As I have said, the consolidation of P_1 and P_2 is a simple operation free from any snags. The opposite is true for the aggregation of several quanta into a single quantum. But this problem, the knottiest of all in economic analysis and especially in the applications of the input–output system, may be begged by the present argument without any risk.

TABLE 6
The Correct Consolidated Form of Table 4

	P_0	P_3	N	P_4	Totals
C_0	*	x_{03}^*	*	x_{04}^*	x_0^*
C_3	x_{30}^*	*	*	x_{34}^*	x_3^*
R	r_0^*	r_3^*	*	r_4^*	r^*
W	$-w_0^*$	$-w_3^*$	*	w_4^*	$-w^*$

and also the rows C_1 and C_2 in both the flow and the fund matrices of Table 3. An obvious, but crucial, point is that consolidation cannot destroy the algebraic key of the flow matrix: each row still adds up to zero. Consequently, we can transform the matrix flow of Table 5 into an input–output table by the same scrambling rules as before. The result, shown in Table 6, makes it abundantly clear why even after consolidation the proper diagonal boxes in an input–output table must still be empty. This vindicates the rule outlined by Leontief for the consolidation of an input–output table: after the addition of the corresponding columns and rows, the resulting diagonal element (if nonnull) must be suppressed and the row total modified accordingly.[72]

TABLE 7
The Incorrect Consolidated Form of Table 4

	P_0	P_3	N	P_4	Totals
C_0	$x_{12}^* + x_{21}^*$	x_{03}^*	*	x_{04}^*	$x_1^* + x_2^*$
C_3	x_{30}^*	*	*	x_{34}^*	x_3^*
R	r_0^*	r_3^*	*	r_4^*	r^*
W	$-w_0^*$	$-w_3^*$	*	$-w_4^*$	$-w^*$

Some economists, however, take exception to this rule and simply add the pertinent columns and rows without suppressing the diagonal element. They obtain Table 7 instead of Table 6. Perhaps this view is a faint echo of the rule for the addition of vectors which, as we have seen, works perfectly in the case of a process form representation. But if this is the case, the view ignores the essential fact that an input–output table is a scrambled arrangement of the other. Apparently, only one explicit reason has been offered in support of maintaining the diagonal elements after

[72] Leontief, *The Structure*, pp. 15 f, 189. Curiously, Leontief broke this rule himself. See note 76, below.

consolidation, namely, that the algebra works better if they are not suppressed.[73] About this, there can be no question: in algebra, terms may cancel each other but they are never just suppressed. Besides, if all flows are measured in money terms (as is often the case in applications), the grand total of the input–output table does not have to be changed. But the rub is that the algebra which works splendidly on a scrambled matrix is apt to be itself scrambled algebra in relation to the unscrambled, basic matrix.

To say only that "there is no difficulty connected with the definition of $[x_1^* + x_2^*]$ and no need to eliminate items of the type $[x_{12}^* + x_{21}^*]$"[74] does not suffice to justify the form of Table 7. We need to know what corresponds to the item $x_{12}^* + x_{21}^*$ in actuality when we conceive the entire economy subdivided only into the processes listed in the consolidated input–output table. Analytical frameworks should not be superimposed in a confusing mesh. To explain, $x_1^* + x_2^*$ represents indeed the combined product output of P_1 and P_2 but only in a framework which includes these processes explicitly. If they are consolidated into a single process P_0, there is no room in the resulting picture except for the product output of that process, namely, for $x_0^* = x_1^* + x_2^* - x_{12}^* - x_{21}^*$—as shown by both Tables 5 and 6.

The point seems so simple that one can only wonder how it was possible to be set aside. I recall that the late League of Nations used to publish the foreign trade data for all countries in the world in the form of an input–output table identical in all respects with that made later famous by Leontief.[75] Of course, all the diagonal boxes were empty. Had there appeared a figure in the box corresponding to the export of Italy to Italy, everyone would have been certain that it was a typographical error! And let us think of such a statistical table consolidated so as to show the export between the continents of the world. Should we not consider it a typographical error if a figure would appear in the diagonal box for the export of Europe to Europe? The point is that in consolidating the table from countries into continents, the export between the European countries has to be suppressed. Clearly, such a consolidated table cannot include the "internal" European export any more than the export of the United States can include interstate commerce.

One is nevertheless greatly tempted to argue that we should place in

[73] R. Dorfman, P. A. Samuelson, and R. M. Solow, *Linear Programming and Economic Analysis* (New York, 1958), chaps. ix and x.

[74] *Ibid.*, p. 240. The expressions between square brackets are my apropos substitutions.

[75] See, for instance, *Memorandum on Balance of Payments and Foreign Trade Balances, 1910–1923*, League of Nations (2 vols., Geneva, 1924), I, 70 ff.

the diagonal box Europe-to-Europe the internal European export and that, similarly, we should regard the diagonal element $x_{12}^{*} + x_{21}^{*}$ in Table 7 as representing the *internal flow* of the consolidated process P_0. So great is this temptation that even Leontief, soon after insisting on the suppression of the diagonal elements, included such an element in one of his tables to represent an internal flow—"payments from business to business."[76] According to the analytical view of a process, however, flows are the elements that are especially associated with a crossing of a boundary. Consequently, once we have removed from our analytical picture the boundaries between the European countries or the boundary between P_1 and P_2, gone also must be the flows associated with them. Analytically, therefore, the term "internal flow" is a mismatch. Yet the use of the concept—under this or some other name—is so widespread that a direct proof of the analytical incongruity involved in it should be in order.

TABLE 8
The Input–Output Table of a Subdivided Canal

From/To	N	P_1	P_2	$\cdots$	P_{n-1}	P_n	Totals
N	w_0	w	*	$\cdots$	*	*	$w_0 + w$
P_1	*	w_1	w	$\cdots$	*	*	$w_1 + w$
P_2	*	*	w_2	$\cdots$	*	*	$w_2 + w$
.	.	.	.	$\cdots$	.	.	$\cdots$
P_{n-1}	*	*	*	$\cdots$	w_{n-1}	w	$w_{n-1} + w$
P_n	w	*	*	$\cdots$	*	w_n	$w_n + w$

Let us visualize a canal P through which water flows at a constant speed and let us decompose it into n partial canals by analytical boundaries drawn without any plan. Let $P_1, P_2, \cdots, P_n$ denote the partial canals and N the environment. The input–output table of the system is given by Table 8. On purpose, no assumption is made concerning the values of the coordinates w_i. If we now consolidate the P_i's back into P and do not suppress the diagonal elements, we obtain Table 9. And since we can take n as large as we may wish and since the value of w is independent of the number of subcanals, it follows that the internal flow of P—that is, $\sum_1^n w_i + (n - 1)w$—may exceed any value we please. The internal flow should, therefore, be infinite. It is obvious that the same absurd conclusion obtains for any other process.

Another justification for the inclusion of diagonal elements invokes the common distinction between *gross* and *net* output flow. According to this view, the diagonal element $x_{12}^{*} + x_{21}^{*}$ of Table 7 is supposed to represent

[76] Leontief, *The Structure*, p. 18.

the difference between the gross output flow of P_0, $y_0^* = x_1^* + x_2^*$, and the net output flow, $x_0^* = x_{03}^* + x_{04}^*$. In explicit terms, that diagonal element represents the part of the flow of C_0 which is used by P_0 itself.[77] This interpretation thus takes us back to the same position—that a diagonal element stands for an internal flow.

TABLE 9
The Consolidated Form of Table 8

From/To	N	P	Totals
N	w_0	w	$w_0 + w$
P	w	$\sum_1^n w_i + (n - 1)w$	$\sum_1^n w_i + nw$

Of course, *inside* any process there is something going on at all times, something flowing in the broad sense of the term. Inside a factory producing glass from sand, for instance, there is a continuous "flow" of sand, melted glass, rolled glass, etc. But this internal flow, as we have seen, is a fund category and, hence, is represented in the analytical picture of the factory by the process-fund $\mathscr{C}$, not by a flow coordinate. There is also a "flow" of clover seed in the process by which clover seed is produced or one of hammers in the process producing hammers. These, too, are funds that must be represented by a fund coordinate such as X_{11}^* of Table 3.[78] Perhaps, by insisting—as the flow complex does—on the inclusion of internal flows in an input–output table, we unwittingly seek to make room for such fund factors in a framework which seems so convenient but which normally includes only pure flows. That is, we seek to smuggle funds into a flow structure. In the end, we will find ourselves adding or subtracting flow and fund coordinates which, as we saw in Section 4, are heterogeneous elements. Unfortunately, the algebra will nonetheless work well most of the time—a cunning coincidence which should not be taken at its face value. Algebra cannot give us any warning signals on such matters. That is why the harm done by smuggling funds into the flow category is not likely to manifest itself on the surface. But below the skin of algebra, things may be distorted substantially.

[77] This viewpoint appears in Leontief, *The Structure*, Tables 5, 6, and 24 (pocket), where several diagonal boxes are filled with data. See also the tables in his "The Structure of Development," *Scientific American*, September 1963, pp. 148–166.

[78] I feel it necessary to go on and point out that the clover seed used in producing clover fodder is, on the contrary, a flow, not a fund element. If the difference may seem perplexing, it is undoubtedly because of our money fetishism—a harmful fetishism this is—of thinking of every economic variable in money terms by preference. But the puzzle should disappear if we observe that to repeat the process of raising clover fodder a farmer has to exchange some fodder for seed, that is, he must go through another process—the market for seed and fodder.

The most convincing illustration is supplied by Marx's endeavor to explain the pricing system in the capitalist system by his labor doctrine of value. The crucial element in his argument is the simple "diagram" by which he represented the economic process analytically, and which, at bottom, is an input–output table. The source of Marx's well-known predicament, I contend, is the internal flow by which he represented the hammers used to hammer hammers—to use again my metaphor. But since Marx was completely sold on the idea that economics must be a *dialectical* science (in the strict sense), it was in order for him not to distinguish between flow and fund and, hence, to substitute an internal flow for an analytical fund. In a strictly dialectical approach of any strain, Being is Becoming. However, for his diagram of simple reproduction Marx turned to analysis and, at that point, he mixed dialectics with analysis—a fact of which he was not aware, apparently. The object lesson of the difficulties he encountered thereafter is clear. If one decides to make dialectics his intellectual companion, one must also be careful not to mix dialectics with analysis. The stern commandments of analysis can be neither circumvented nor disobeyed.

14. Marx's Diagram of Simple Reproduction versus a Flow–Fund Model. As we may recall, in Marx's analytical diagram the economy is divided into two departments, P_1 and P_2, producing capital goods and consumer goods, respectively, and two consumption sectors, P_3 and P_4, of the workers and of the capitalists.[79] The notations used in Table 10 are the

TABLE 10
The Input–Output Table of Marx's Diagram of Reproduction

	P_1	P_2	P_3	P_4	Totals
G_1	c_1	$c_2 = v_1 + s_1$	*	*	$\omega_1 = c_1 + v_1 + s_1$
G_2	*	*	$v_1 + v_2$	$s_1 + s_2$	$\omega_2 = c_2 + v_2 + s_2$
H	v_1	v_2	*	*	$v_1 + v_2$

familiar ones: v_i and s_i represent the *flow* of consumption goods accruing to the workers and the capitalists associated with department P_i. The term c_2 stands for the maintenance *flow* of G_1 necessary to keep the capital of P_2 constant; and c_1, the troublesome item, stands for the *internal flow* of capital goods in P_1, i.e., the flow of capital goods consumed in the

[79] It may be worth pointing out here that the current practice of putting all households in the same analytical bag is a regrettable regress from Marx's analysis which, by separating the households of the capitalists from those of the workers, kept the social dimension in the center of economic analysis. Economics has indeed drifted away from political economy to become almost entirely a science of management.

production of capital goods. All terms are expressed in labor value terms.[80] If one still needs to expose the heterogeneity of the terms composing ω_i, one may cite Sweezy's cacophony in explaining that the total value is obtained by adding "the constant capital engaged [in production with] the income of the capitalist [and] the income of the worker."[81] Marx also took it that the total capital of the capitalist consists only of constant capital and variable capital. Clearly, in a steady-going *industrial* process wages as well as the current input and maintenance flows are paid out of the simultaneous product flow. There is thus no need for assuming that the capitalist owns also some working capital, unless we wish to make the diagram more realistic and bring in a store fund of money to take care of irregular fluctuations in the operations (Section 9). But such a fund does not necessarily stand in the same ratio with every category of payments. There is one way, however, to make some analytical sense of Marx's diagram, which is strongly suggested by countless elaborations in *Capital*. In all probability, the process Marx had in mind in setting up his diagram was an agricultural, not an industrial process. For we should not forget that he borrowed his diagram from François Quesnay, who by his famous *Tableau économique* sought to depict the economics of agricultural production.[82] In this alternative, c_1 is analogous to the corn used as seed at the beginning of the process and ω_1 is the gross output of corn at the end of the process. The diagram would thus represent a system of elementary processes which are arranged *in series* and in which there is no durable fund, whereas the industrial system which Marx wanted to analyze is a system *in line* in which capital is a self-maintaining fund at all times. But we must pass on.

Marx's basic tenets are well-known: (1) competition brings about the equality of values and prices in the sense that quantities of equal value sell for equal amounts of money; (2) the workers are paid their value, i.e., their standard of subsistence, regardless of how many hours the capitalists may force them to work each day;[83] (3) competition also equalizes in all departments the rate of labor exploitation

[80] Marx, *Capital*, II, 458–460, and Paul Sweezy, *The Theory of Capitalist Development* (New York, 1942), pp. 75–79. I have included the row H in Table 10 because, like Leontief, Marx treated the services of labor as a flow category (although, as I have pointed out earlier, he steadily avoided the term service).

[81] Sweezy, *Theory*, pp. 76 f.

[82] Cf. K. Marx and F. Engels, *Correspondence, 1846–1895* (New York, 1935), pp. 153–156. As I have pointed out in my "Economic Theory and Agrarian Economics," reprinted in AE, p. 384, even Marx's law of surplus value—relation (26), below—reflects the tithe system in agriculture.

[83] Implicit in these two tenets is the principle mentioned in Section 6 above, that the value of a fund's service is completely taken care of by the maintenance flow of that fund.

$$(26) \qquad s_1/v_1 = s_2/v_2.$$

On this basis, Marx claimed to be able to explain the pricing mechanism by which the rates of profit of all departments are equalized in the *capitalist system*. But as he in the end found out, if (26) is true, then the equality of the rates of profit,

$$(27) \qquad s_1/(c_1 + v_1) = s_2/(c_2 + v_2),$$

does not obtain unless the organic composition of capital is the same in both departments, i.e., unless

$$(28) \qquad v_1/c_1 = v_2/c_2.$$

This relation expresses in fact a general technological law which cannot possibly be accepted.[84] As a result, Marx was compelled to admit that prices cannot reflect values and proposed a rule for determining "the prices of production" corresponding to a given diagram. The rule consists of redistributing the total surplus value, $s = s_1 + s_2$, between the two departments in such a way as to bring about the equality of the rates of profit. But Marx offered no economic explanation of why and how the production prices would be brought about. The same is true of the numerous rescuers of latter days who tried to conjure away the analytical impasse by far-fetched reinterpretations and, often, highly complicated algebra.[85] But turning in circles is inevitable as long as we cling to Marx's flow complex. Let us then abandon this complex and see what we may be able to do if, instead, we use our flow–fund model for probing Marx's argument about value.

Table 11 represents in process form the same structure as that which Marx had in mind. It assumes that the working day, δ, is the same in both departments, that the working class receives only its daily standard subsistence V, and that the scales of production are adjusted so that the product flow of P_1 is just sufficient for the maintenance of the capital fund K_2 of P_2. The other notations are self-explanatory: K_1 is the capital fund of P_1, and n_1, n_2 are the numbers of homogeneous workers employed in the two departments, $n = n_1 + n_2$. We can always choose the unit of G_2 in such a way that δx_2 be equal to the total labor time δn, in which case the labor value of that unit is unity. This convention yields $x_2 = n$.

[84] Marx himself denounced it in *Capital*, vol. III, chap. viii. See also Sweezy, *Theory*, pp. 69 f.

[85] To my knowledge, all these solutions are concerned only with the flow diagram. For Marx's rule see *Capital*, vol. III, chap. ix, and Sweezy, *Theory*, pp. 107–115. One of the highly praised alternative solutions, by L. von Bortkiewicz, is presented in Sweezy, pp. 115–125.

There remain only two unknowns to be determined: δ_0, the length of the "normal" working day (the necessary labor, in Marx's terminology), and p_0, the *value* of G_1. Not to depart from Marx's line of reasoning, we must compute p_0 in the absence of any labor exploitation.

TABLE 11
A Two-Department Economy

	P_1	P_2	P_3	P_4
		Flow Coordinates		
G_1	δx_1	$-\delta x_1$	*	*
G_2	*	δx_2	$-V$	$-(s_1 + s_2)$
		Fund Coordinates		
G_1	δK_1	δK_2	*	*
H	δn_1	δn_2	*	*

If there is no exploitation—by which, with Marx, we must mean that $s_1 = s_2 = 0$—from the last flow row of Table 11 we obtain the normal working day,

$$\delta_0 = nv/x_2 = v, \tag{29}$$

where $v = V/n$ is the daily wage of the worker. If δ^* is the greatest numbers of hours a worker can work daily without impairing his biological existence, the last relation shows that the workability of the system represented by Table 11 requires that $\delta^* - v \geq 0$. The fact that labor is productive, in the sense that under any circumstances it can produce more than its standard subsistence, invites us to assume that $\delta_0 < \delta^*$ and, hence,

$$\delta - v > 0 \tag{30}$$

for any δ, $\delta_0 < \delta < \delta^*$. The equality between price and cost (with no share for the services of capital) yields for each department

$$\delta_0 x_1 p_0 = n_1 v, \qquad \delta_0 x_2 = \delta_0 x_1 p_0 + n_2 v. \tag{31}$$

By (29), from the first of these conditions we obtain

$$p_0 = n_1/x_1, \tag{32}$$

a value which satisfies the second condition as well, since $x_2 = n$.

Next, let us assume—also with Marx—that the capitalists can impose a working day δ, $\delta_0 < \delta \leq \delta^*$, and still pay the workers the same *daily* wages.[86] In this case, from the cost equations (with G_1 priced at p_0) we obtain

$$(33) \qquad s_1^0 = n_1(\delta - \delta_0), \qquad s_2^0 = n_2(\delta - \delta_0).$$

Per worker, therefore, the rate of exploitation is the same, $(\delta - \delta_0)$, in both departments, and Marx's law of surplus value (26) is vindicated. However, the rates of profit in the two departments being $r_1^0 = n_1(\delta - \delta_0)/p_0K_1$ and $r_2^0 = n_2(\delta - \delta_0)/p_0K_2$, they, again, cannot be equal unless the fund factors are combined in the same proportion in both departments, i.e., unless

$$(34) \qquad n_1/K_1 = n_2/K_2,$$

which is tantamount to Marx's (28).

One factual element should be now brought into our abstract analysis. Capital goods are produced in the same manner as biological species. Occasionally, one "species" of capital goods evolves from another such species. That is, new capital species are produced by mutations. The first stone hammer was produced only by labor out of some materials supplied by the environment; the first bronze hammer was produced by labor aided by a substantial number of stone hammers. But in a stationary economy there can be no mutation: hammers (or machines) are reproduced by the same kind of hammers (or machines). Now, the role of capital is not only to save labor but also to amplify man's meager physical power. It stands to reason, then, that *on the whole* it takes more machines per man to make machines than to use these last machines in producing consumer goods. The fact, I contend, is fairly transparent and within a stationary two-sector economy perhaps an *a priori* synthetic judgment.[87]

[86] The notion that the *wage rate* should be set so as to allow the worker only his daily maintenance at the "regular" working day was very old by Marx's time: "for if you allow double, then he works but half so much as he could have done, and otherwise would; which is a loss to the *Publick* of the fruit of so much labor." *The Economic Writings of Sir William Petty*, ed. C. H. Hull (2 vols., Cambridge, Eng., 1899), I, 87 (my italics). This idea, found also in the works of François Quesnay, implies a unit elasticity of the supply of hours of work and, clearly, differs from Marx's own explanation. For what may bring the workers, of that and later times, to have such a supply schedule, see my article "Economic Theory and Agrarian Economics" (1960), reprinted in AE, p. 383.

[87] Whether the same judgment is true for *any* capital goods industry compared with *any* consumer goods industry constitutes an entirely different issue. To decide it, we need an accurate estimation of every K/H^* (in our case, K_i/n_i). But, for the reasons explained in Section 10, above, the best available censuses of manufactures do not provide us with the necessary data. Nor is the usual classification of industries suitable for this particular purpose. If the nineteen basic manufacturing industries (of the United States classification) are ranked according to the following *brute* capital-labor ratios—fixed capital per worker, capital invested per production worker, horsepower per worker, and fixed capital per wage and salary dollar—the rankings display no striking parallelism. For whatever significance it might have, I should add that the industries of apparel, textiles, leather, furniture, and printing usually are at the bottom of every ranking. Only the food industry tends to be slightly above the median.

This means that

$$(35) \qquad n_1/K_1 < n_2/K_2$$

is the only case in actuality. Hence, always $r_1^0 < r_2^0$. Consequently, as long as the capital goods sell at their value p_0, the owners of the means of production will certainly shift their capital from P_1 to P_2. As a result, the decreased production of P_1 will no longer suffice to maintain the increased capital fund of P_2 constant. Ultimately, *the whole fund of constant capital of the economy will dwindle away.*[88]

But before this would come about, the capitalists of the department P_2 will naturally compete for the increasingly scarce maintenance flow of G_1. Competition—which, we may remember, is a fundamental condition in Marx's argument—must necessarily bring an increase in the price of the capital goods. This increase may put an end to the flight of capital from P_1 to P_2 and, ipso facto, to the gradual shrinking of the capital fund of the economy. To check this conclusion by algebra, let p be the money price of G_1 at which there would be no incentive for any shift of means of production from one department to the other. This price must obviously bring about the equality of the two rates of profit, $s_1/pK_1 = s_2/pK_2$. After some algebraic manipulations, this condition yields

$$(36) \qquad p = p_0 + \frac{(\delta - \delta_0)(n_2K_1 - n_1K_2)}{\delta x_1(K_1 + K_2)}.$$

In view of (30) and (35), this formula shows that, while everything else continues to sell at its labor value (in Marx's sense), capital goods must sell at more than their labor value.[89] The only exception is the case of $\delta = \delta_0$, which entails $p = p_0$ and $s_1 = s_2 = 0$. But in this case the capitalists would eat up their capital anyhow.[90] Of course, if $\delta > \delta_0$ and

[88] Because of (35), any shift of capital from P_1 to P_2 calls for an increase in employment. It would seem therefore that there should be also an increase in the total wage bill. However, if we interpret analytically Marx's assumption of the reserve army combined with the idea that the working class receives exactly its standard subsistence, the wage rate is not a datum. Instead, it is determined by the historically determined constant V and the size of the employment, $v = V/n$. Cf. my article "Mathematical Proofs of the Breakdown of Capitalism," reprinted in AE, p. 400.

[89] Because px_1 represents a money transfer from department P_2 to P_1, we should expect the total surplus value $s = s_1 + s_2$ to remain the same for any value of p—a fact which is easily checked by algebra. Also, my solution, in contrast with that by Bortkiewicz, does not require a reevaluation of the wage bills; hence it is much more in the spirit of Marx's.

[90] To avoid a possible misunderstanding, I may note that this statement does not contradict the proposition that a zero rate of interest is compatible with any trend of capital accumulation. In the model considered here the working class cannot save, because it receives only its standard subsistence. (This, again, does not preclude that each member of the worker class may save for old age at zero interest within that class.) The point is that, in this situation, a title to the means of production could not possibly find a buyer among the income earners: its market value would be zero, smaller than that of a piece of scrap paper.

the inequality (35) is reversed, capital goods should sell at *less* than their value. Were they to sell at p_0, all capital would move into the producer goods industries and the economy would die because machines would be used to make only machines. The fact that this reversed world, in which the consumer goods industries are more capital intensive than the others, can exist only on paper sharpens the general conclusion of this section.

Needless to insist, within a scheme of simple reproduction in which the capital fund is a datum we cannot entertain the question of how and why capital has been accumulated. The only problem that we can entertain is how that fund can be maintained. If the means of production are not owned by some individuals, then it is tautological that the whole production flow of consumer goods must accrue to the workers (provided no other institutional claim exists on it). The normal working day is, in this case, determined by the preferences of the whole population between leisure and real income at the prevailing technical rate v/x_2. As a price of account, G_1 must be reckoned at p_0. The system can then go on reproducing itself indefinitely. If, on the contrary, the means of production are owned by some individuals who, as we have seen, can only transform them into a flow of consumer goods, the maintenance of the capital fund requires that the working day should be longer than the normal working day. Otherwise the owners would eat up their capital (alternatively, the other institutional claimants would starve). A further condition for the reproduction of the system is that the share of the flow of consumer goods accruing to the owners must be proportional to the value of the capital invested in each line of production. In turn, this condition brings in some hard facts of technology, namely, that in the sector in which capital goods are reproduced they participate in a higher ratio to labor than in the sector in which consumer goods are produced.[91] This is the ultimate reason why capital goods must sell at a higher price than their labor value established according to Marx's own rationale.

15. Commodities, Processes, and Growth. We have thus far considered only the analytical representation of steady-going processes, that is, of processes that *reproduce* themselves. We have not touched the question of how such a process may come into existence. Were we concerned with steady-going mechanical systems involving only locomotion, we could dispose of this question either by assuming—as Aristotle did—a Prime

[91] Of course, a stationary economy without ownership of capital could go on indefinitely even in the reversed world. An unsuspected difficulty emerges, however, if instead of a stationary we consider a dynamic system: the "normal" world is dynamically unstable and the "reversed" world stable! See my paper "Relaxation Phenomena in Linear Dynamic Models" (1951), reprinted in AE, pp. 310 f, for an analysis of each case according to whether $n_1K_2 - n_2K_1$ is greater than, equal to, or less than 0.

Mover which set them into motion at the beginning of Time or by simply acknowledging their existence—as Newton did—through the Law of Inertia (Newton's First Law). But in economics we cannot dodge the question in this manner. Economic processes, even the steady-going ones, are set in motion and kept so by man. More pointedly, *economic processes are produced just as commodities are*. Think of a factory. Is not a textile factory, for instance, just as much the "product" of man's economic activity as an ell of cloth is? Ever since the economic evolution of mankind reached the phase in which man used commodities to produce commodities, the production of more commodities has had to be preceded by the production of additional processes. On the other hand, to produce an additional process implies the use of some commodities already available. In a down-to-earth view, *investment* is the production of additional processes, and *saving* is the allocation of already available commodities to this production.

Needless to say, none of the analytical representations considered in the preceding sections offer room for this important side of man's economic activity: the production of processes. These representations describe reproductive processes already produced. But the fact which I wish to bring to the reader's attention is that, as far as one may search the economic literature, all dynamic models (including those concerned with *growth*) allow for the production of commodities but not for that of processes. The omission is not inconsequential, be it for the theoretical understanding of the economic process or for the relevance of these models as guides for economic planning. For one thing, the omission is responsible for the quasi explosive feature which is ingrained in all current models of dynamic economics—as I shall show in a while.

But there is another reason why—the literature of economic dynamics notwithstanding—a dynamic model is useless for throwing any light on the problem of how growth comes about, which includes the problem of how growth itself may grow faster.[92] Just as a stationary model by itself implies a Prime Mover at minus infinity on the time scale, so a dynamic model implicitly assumes a Prime Planner which set the system growing at the origin of Time. A parallel from mechanics will set in sharp focus the issue as I see it. Let us imagine a ball moving (without friction) on a horizontal table according to the Law of Inertia, i.e., in a linear uniform motion. According to the same law, this system cannot change *by itself* its reproductive manner of moving. Only an *external* force—say, the gravitational force that comes into play as soon as the ball reaches the edge of the

[92] As J. R. Hicks, "A 'Value and Capital' Growth Model," *Review of Economic Studies*, XXVI (1959), 173, indicted the dynamic models, they allow only the selection of the starting point on a pre-selected growth path.

table—can cause its motion to become accelerated. By contrast, an economic steady-going system has *within itself* the power to move faster, in a word, to grow. A second (and far more important) difference is this: the ball does not have to move slower for a while in order to acquire a greater velocity under the influence of the gravitational force. A steady-going economic process, on the contrary, must, like the jumper, back up some distance in order to be able to jump. And my point is that in a dynamic model this backing up is thrown to minus infinity on the time scale.

To illustrate in detail the preceding remarks, I shall refer to that dynamic system which, in my opinion, is the most explicitly outlined of all, the Leontief system. The simplicity of its framework will also keep irrelevant issues from cluttering the argument. For the same reason, I wish to consider the simplest case, namely, that of a system consisting of two productive processes P_1 and P_2 producing commodities C_1 and C_2, respectively. With the notations of Table 3 (Section 13), the characteristic assumption of all Leontief systems (static or dynamic) is that for every process that may produce C_i, the input coefficients

$$a_{ki} = x^*_{ki}/x^*_i, \qquad B_{ki} = X^*_{ki}/x^*_i, \tag{37}$$

are constant.[93] To render this assumption more explicit, we may write

$$\begin{aligned} x^*_1 &= x_1\delta_1 a_{11}, & x^*_{21} &= x_1\delta_1 a_{21}, & x^*_2 &= x_2\delta_2 a_{22}, & x^*_{12} &= x_2\delta_2 a_{12}, \\ X^*_{11} &= x_1\delta_1 B_{11}, & X^*_{21} &= x_1\delta_1 B_{21}, & X^*_{12} &= x_2\delta_2 B_{12}, & X^*_{22} &= x_2\delta_2 B_{22}, \end{aligned} \tag{38}$$

where δ_i is the working day of P_i and x_i is a pure number measuring the *scale* of P_i in relation to the corresponding unit-scale process. The unit-scale processes are:

$$\begin{aligned} &P^0_1\,(a_{11} = 1,\ -a_{21};\ B_{11},\ B_{21}), \\ &P^0_2\,(-a_{12},\ a_{22} = 1;\ B_{12},\ B_{22}). \end{aligned} \tag{39}$$

P^0_1, for instance, describes the process *capable* of producing a flow rate of one unit of C_1 per unit of time.[94] Consequently, a_{ik} is a *flow rate*, and B_{ik}

[93] Leontief, *Studies in the Structure of the American Economy*, pp. 18, 56. In his dynamic system, Leontief leaves out even the labor input. The reason is, perhaps, the same as that which led me to write the basic relations of the production function in the form of (18) in Section 9, above.

[94] As it should be clear by now, I take exception to Leontief's view—*The Structure*, p. 211, *Studies*, p. 12—that static analysis or short-run analysis may completely disregard the fund coordinates B_{ik}. True, in the short run the extant funds are supposed to remain fixed. Now, if the constancy of coefficients (37) is assumed, the short-run variations can come only from a change in the δ_i's or the capacity utilized (which is tantamount to a change in the x_i's). Hence, it is important to know what each P_i can produce at full and continuous utilization of the extant capacity (which is determined by the extant funds, not by the observed flow coefficients, a_{ik}). This maximum capacity cannot be exceeded no matter how much labor power we transfer to P_i—a point which is generally ignored in the practical applications of the Leontief static system.

a *fund*. Since the δ_i's do not appear explicitly in Leontief's presentation, we may assume that, like every Neoclassical economist, he took it that they have the same and invariable value.[95] For the following argument, it does not matter if we take the same position and, in addition, assume $\delta_1 = \delta_2 = 1$.

Given the scale x_i, the flow rate of net product (y_1, y_2) which the system is capable of producing is determined by the well-known system of relations[96]

$$a_{11}x_1 - a_{12}x_2 = y_1, \qquad -a_{21}x_1 + a_{22}x_2 = y_2, \tag{40}$$

which is subject to the indispensable condition

$$a = a_{11}a_{22} - a_{12}a_{21} > 0. \tag{41}$$

Let us now assume that we plan for the increases in the flow rates of net product

$$\Delta y_1 \geq 0, \qquad \Delta y_2 \geq 0, \qquad \Delta y_1 + \Delta y_2 > 0. \tag{42}$$

These increases require the increases Δx_1 and Δx_2 in the *scales* of P_1 and P_2. They are determined by the system

$$a_{11}\Delta x_1 - a_{12}\Delta x_2 = \Delta y_1, \qquad -a_{21}\Delta x_1 + a_{22}\Delta x_2 = \Delta y_2. \tag{43}$$

These last increases, in turn, require some increases in the existing funds $B_1 = x_1B_{11} + x_2B_{12}$, $B_2 = x_1B_{21} + x_2B_{22}$, namely,

$$\Delta B_1 = B_{11}\Delta x_1 + B_{12}\Delta x_2, \qquad \Delta B_2 = B_{21}\Delta x_1 + B_{22}\Delta x_2. \tag{44}$$

To accumulate these additional funds, a part of the flow of net product must be accumulated (instead of consumed) over some period Δt. During this period, therefore, the flow rate of net product available for consumption is

$$z_1 = y_1 - \frac{\Delta B_1}{\Delta t}, \qquad z_2 = y_2 - \frac{\Delta B_2}{\Delta t}. \tag{45}$$

[95] In one place, *The Structure of the American Economy*, p. 160, Leontief does allude to the possibility of the working day to vary, but only from one industry to another. I should also mention that in the decomposition (38) a_{ik} is not a time-free coordinate, but B_{ik} is so. Of course, the a_{ik}'s are *numerically* equal to a time-free coordinate, namely, to "the physical amounts of $[C_i]$ absorbed by industry $[P_k]$ per unit of its own output"—as Leontief has it in *The Structure*, pp. 188 f, and *Studies*, p. 18.

[96] In these relations $a_{11} = 1$, $a_{22} = 1$. Because a_{11} and a_{22} are dimensional coefficients (not pure numbers), I included them explicitly so as to allow us to check at a glance the dimensional homogeneity of these and the subsequent relations.

If Δx_1 and Δx_2 are eliminated from (43) and (44), the last relations become

$$\text{(46)}\qquad \begin{aligned} z_1 &= y_1 - \frac{a_{22}B_{11} + a_{21}B_{12}}{a}\left(\frac{\Delta y_1}{\Delta t}\right) - \frac{a_{11}B_{12} + a_{12}B_{11}}{a}\left(\frac{\Delta y_2}{\Delta t}\right), \\ z_2 &= y_2 - \frac{a_{22}B_{21} + a_{21}B_{22}}{a}\left(\frac{\Delta y_1}{\Delta t}\right) - \frac{a_{11}B_{22} + a_{12}B_{21}}{a}\left(\frac{\Delta y_2}{\Delta t}\right), \end{aligned}$$

or briefly,

$$\text{(47)}\qquad \begin{aligned} z_1 &= y_1 - M_{11}\left(\frac{\Delta y_1}{\Delta t}\right) - M_{12}\left(\frac{\Delta y_2}{\Delta t}\right), \\ z_2 &= y_2 - M_{21}\left(\frac{\Delta y_1}{\Delta t}\right) - M_{22}\left(\frac{\Delta y_2}{\Delta t}\right). \end{aligned}$$

This system shows, first, that once we have chosen Δy_1, Δy_2, there is a lower limit to Δt, i.e., to how quickly we may reach the chosen level $y_1^1 = y_1 + \Delta y_1$, $y_2^1 = y_2 + \Delta y_2$. Conversely, if Δt is chosen there is an upper limit to Δy_1 and Δy_2. Secondly, (47) shows that no matter how small Δy_1 and Δy_2 and how large Δt are chosen, the system must drop to a lower level of consumption before pulling itself up to a higher level. Obviously, we may diminish this drop by using the additional funds as they are accumulating, but we cannot avoid it.

Let us then consider a succession of periods Δt and assume that the funds saved during each period are invested at the end of the period. For each period there obtains a system analogous to

$$\text{(48)}\qquad \begin{aligned} z_1^i &= y_1^i - M_{11}\left(\frac{\Delta y_1^i}{\Delta t}\right) - M_{12}\left(\frac{\Delta y_2^i}{\Delta t}\right), \\ z_2^i &= y_2^i - M_{21}\left(\frac{\Delta y_1^i}{\Delta t}\right) - M_{22}\left(\frac{\Delta y_2^i}{\Delta t}\right), \end{aligned}$$

where $y_k^{i+1} = y_k^i + \Delta y_k^i$, $y_k^0 = y_k$. The systems (48) allow us to determine step by step the sequences $[y_k^i]$ from appropriately chosen sequences $[z_k^i]$, and conversely. The picture of how a steady-going process may become a growing process is thus clear.

If we pass to the limit by choosing an increasingly smaller Δt, (48) becomes

$$\text{(49)}\qquad \begin{aligned} z_1(t) &= y_1(t) - M_{11}\dot{y}_1(t) - M_{12}\dot{y}_2(t), \\ z_2(t) &= y_2(t) - M_{21}\dot{y}_1(t) - M_{22}\dot{y}_2(t), \end{aligned}$$

where the dot indicates the derivative with respect to t.[97] In this case,

[97] The standard form used by Leontief (*Studies*, pp. 56 f) can be derived from (49) if y_1 and y_2 are replaced by their values given by (40). My preference for (49) is that it directly compares the net product with the consumption level.

too, we can determine the functions $z_k(t)$ if $y_1(t)$ and $y_2(t)$ are given; this is simple enough. But the main application of (49) or of any other dynamic system concerns the case in which we choose arbitrarily the z_k's and use the system to determine the y_k's.[98] Calculus teaches us that given the z_k's, the general solutions of (49) involve two arbitrary constants. These constants, as Leontief advises us, can be determined by the initial conditions $y_1(0) = y_1^0$ and $y_2(0) = y_2^0$. This advice is fully correct provided that at the chosen origin, $t = 0$, the actual process was *already* an accelerated one, i.e., a dynamic process. In case the process comes from the past as a stationary one, there are some restrictions on the choice of the z_k's, the most important being that $z_k(0) < y_k(0)$—to allow for the "drop" of which I have spoken above.[99]

As it should be clear from the foregoing analysis, the dynamic models involve a peculiar assumption to which practically no attention has been paid. The assumption is that as soon as the necessary funds have been saved the level of net product *instantaneously* jumps to $(y_1 + \Delta y, y_2 + \Delta y_2)$. As a result, the net product starts to increase the very moment the old level of consumption is decreased. This is the quasi explosive feature of the dynamic models to which I have alluded earlier. Indeed, if this assumption were true in actuality, we could bring about a fantastic growth of any economy by merely decreeing, say, one day of the week during which no commodities should flow into the consumption sector (all other things being kept as before). The reason why we cannot achieve this tour de force is that an increase in the product flow requires that some additional *processes* be first created. Also, as we have seen in Section 9, above, a process can start producing a product flow only after it is primed, i.e., only after its process-fund $\mathscr{C}$ is completed. And both to build a process out of commodities and to prime it require some duration in addition to the time necessary for the accumulation of the funds ΔB_1 and ΔB_2. Specifically, after we have accumulated the additional funds $B_{11}\Delta x_1$ and $B_{21}\Delta x_1$ during the interval Δt, we must wait an additional

[98] *Ibid.*, pp. 57–60.

[99] Dynamic systems such as (49) conceal unpleasant surprises. This is why even the condition just mentioned is not always sufficient to sustain growth continuously. The point is simply illustrated by a system involving only one commodity, in which case (49) reduces to $z(t) = y - M\dot{y}$ or to $z(t) = y - \dot{y}$ if M is chosen as the unit of time. The solution that transforms a steady-going system y^0 into a growing one is

$$y(t) = y^0e^t - e^t \int_0^t e^{-t}z(t)\,dt,$$

for $t \geq 0$. The necessary and sufficient condition that y should be always increasing is that $y(t) > z(t)$. Let us also note that, in contrast with the movement of the ball of our earlier example, $\dot{y}(t)$ cannot have the same value for $t = 0$ as the speed of the previous system up to that point.

time interval τ_1 before the additional product flow of P_1 becomes available. And, a point which deserves stressing, τ_1 covers the time needed to build and prime the new processes, just as the necessary savings $B_{11}\Delta x_1$, $B_{21}\Delta x_1$ must include not only the ordinary equipment of that process but also its process-fund $\mathscr{C}_1$. The consequence is that in the chain of systems (48) we can no longer write $y_k^{i+1} = y_k^i + \Delta y_k^i$. That is not all. Accumulation of stocks may be regarded as locomotion, which goes on continuously in time. But building a process is an event which cannot be reduced to a point in time. Consequently, though nothing stands in the way of making Δt tend toward zero in the modified system (48), it would mess up things completely if we were to make τ_1 and τ_2, too, tend toward zero. These lags, therefore, must appear explicitly in the new system, which is now better expressed in terms of $x_1(t)$ and $x_2(t)$:

$$(50)\qquad \begin{aligned} z_1(t) &= \quad a_{11}x_1(t) - a_{12}x_2(t) - B_{11}\dot{x}_1(t-\tau_1) - B_{12}\dot{x}_2(t-\tau_2), \\ z_2(t) &= -a_{21}x_1(t) + a_{22}x_2(t) - B_{21}\dot{x}_1(t-\tau_1) - B_{22}\dot{x}_2(t-\tau_2). \end{aligned}$$

The quasi explosive feature of the Leontief dynamic system (49) *as a planning tool* is thus eliminated. In particular, if we apply (50) to changing a steady-going economic process into a growing process or to increasing the growth of an already growing system, the solution will be such that no increase in the output of P_1 or P_2 will appear before some time interval (the smaller of τ_1 and τ_2) has elapsed after the beginning of the new saving.[100]

But even in a *growing* process there need not necessarily be any waiting for growth. A lag between accumulation and the increased output exists because each additional process, too, is the product of an elementary process and because the completion of an elementary process requires duration—the time of production. The reason for the lag is, therefore, the same as that which we have found to work in the case of small-shop production, namely, a low rate of demand.[101] However, with economic development an economy may reach the point when it finds advantageous the building of a system Π_1 that produces processes P_1 and P_2 *in line* just as a factory produces commodities in line. Once the process Π_1 is built, the economy can produce processes P_1 and P_2 *without any waiting*. What is true for a factory producing commodities "instantaneously" must hold

[100] The analytical advantages of the lag systems over the purely dynamical ones have been repeatedly stressed in the literature: e.g., Leontief, *Studies*, pp. 82 f; J. D. Sargan, "The Instability of the Leontief Dynamic Model," *Econometrica* XXVI (1958), 381–392. But the fact that their solutions do not possess the analytical simplicity of the purely dynamic systems has made their study less profitable and has deterred their use in concrete applications. On the issue of the stability of the Leontief dynamic system see also my paper cited in note 91, above.

[101] Cf. Sections 7 and 11, above.

for a factory producing processes. The economy can therefore grow at a constant speed which is determined by the scale of Π_1. There will be waiting only if the economy wants to grow at a higher speed. To grow at a higher speed requires an increase in the scale of Π_1 which can be achieved only by elementary processes in series, unless the economy includes a process Π_2 that produces processes Π_1 in line. Should this be the case, the economy can grow at a constant acceleration (constantly increasing speed) without waiting. On paper, there is no limit to this analytical algorithm.

The world of facts, however, does not seem to quite fit into this Π-model. Even in the most advanced economies we do not find factories that build factories that build factories that build factories.... However, in these economies we find a complex and extensive net of enterprises that are continuously engaged in building factories not quite in line but almost so. They are the general contracting firms, the building enterprises, the construction firms, and so on. Because of the necessity of dispersing their activity over a large territory, these enterprises do not possess a factory in the narrow sense of the term. Yet these organizations operate severally or in association essentially like a factory—a flexible factory, but still a factory.

In conclusion, I wish to submit that it is this Π-sector that constitutes the fountainhead of the growth and further growth which seems to come about as by magic in the developed economies and which, precisely for this reason, has intrigued economists and puzzled the planners of developing economies. By a now popular metaphor, we speak of the "take-off" of a developing economy as that moment when the economy has succeeded in creating within itself the motive-power of its further growth. In light of the foregoing analysis, an economy can "take off" when and only when it has succeeded in developing a Π-sector. It is high time, I believe, for us to recognize that the essence of development consists of the organizational and flexible power to create new processes rather than the power to produce commodities by materially crystallized plants. Ipso facto, we should revise our economics of economic development for the sake of our profession as a pure and practical art.

CHAPTER X *Entropy, Value, and Development*

1. Entropy and Economic Value. One point of the agitated history of thermodynamics seems to have escaped notice altogether. It is the fact that thermodynamics was born thanks to a revolutionary change in the scientific outlook at the beginning of the last century. It was then that men of science ceased to be preoccupied almost exclusively with celestial affairs and turned their attention also to some earthly problems.

The most prominent product of this revolution is the memoir by Sadi Carnot on the efficiency of steam engines—of which I spoke earlier.[1] In retrospect it is obvious that the nature of the problem in which Carnot was interested is economic: to determine the conditions under which one could obtain the highest output of mechanical work from a given input of free heat. Carnot, therefore, may very well be hailed as the first econometrician. But the fact that his memoir, the first spade work in thermodynamics, had an economic scaffold is not a mere accident. Every subsequent development in thermodynamics has added new proof of the bond between the economic process and thermodynamic principles. Extravagant though this thesis may seem *prima facie*, thermodynamics is largely a physics of economic value, as Carnot unwittingly set it going.

A leading symptom is that purists maintain that thermodynamics is not a legitimate chapter of physics. Pure science, they say, must abide by the dogma that natural laws are independent of man's own nature, whereas thermodynamics smacks of anthropomorphism. And that it does so smack is beyond question. But the idea that man can think of nature

[1] Chapter V, Section 4.

in wholly nonanthropomorphic terms is a patent contradiction in terms.[2] Actually, force, attraction, waves, particles, and, especially, *interpreted* equations, all are man-made notions. Nevertheless, in the case of thermodynamics the purist viewpoint is not entirely baseless: of all physical concepts only those of thermodynamics have their roots in economic value and, hence, could make absolutely no sense to a nonanthropomorphic intellect.

A nonanthropomorphic mind could not possibly understand the concept of order–entropy which, as we have seen, cannot be divorced from the intuitive grasping of human purposes. For the same reason such a mind could not conceive why we distinguish between free and latent energy, should it see the difference at all. All it could perceive is that energy shifts around without increasing or decreasing. It may object that even we, the humans, cannot distinguish between free and latent energy at the level of a single particle where normally all concepts ought to be initially elucidated.

No doubt, the only reason why thermodynamics initially differentiated between the heat contained in the ocean waters and that inside a ship's furnace is that *we can use the latter but not the former*. But the kinship between economics and thermodynamics is more intimate than that. Apt though we are to lose sight of the fact, the primary objective of economic activity is the self-preservation of the human species. Self-preservation in turn requires the satisfaction of some basic needs—which are nevertheless subject to evolution. The almost fabulous comfort, let alone the extravagant luxury, attained by many past and present societies has caused us to forget the most elementary fact of economic life, namely, that of all necessaries for life only the purely biological ones are absolutely indispensable for survival. The poor have had no reason to forget it.[3] And since biological life feeds on low entropy, we come across the first important indication of the connection between low entropy and economic value. For I see no reason why one root of economic value existing at the time when mankind was able to satisfy hardly any nonbiological need should have dried out later on.

Casual observation suffices now to prove that *our whole economic life feeds on low entropy*, to wit, cloth, lumber, china, copper, etc., all of which are highly ordered structures. But this discovery should not surprise us. It is the natural consequence of the fact that thermodynamics developed

[2] Cf. Chapter XI, Section 4, below.

[3] The point is related to a consequence of the hierarchy of wants: what is always in focus for any individual is not the most vitally important; rather, it is the least urgent needs he can just attain. An illustration is the slogan, "what this country needs is a good five-cent cigar." Cf. Section V of my article, "Choice, Expectations, and Measurability" (1954), reprinted in AE.

from an economic problem and consequently could not avoid defining order so as to distinguish between, say, a piece of electrolytic copper—which is useful to us—and the same copper molecules when diffused so as to be of no use to us.[4] We may then take it as a brute fact that low entropy is a *necessary* condition for a thing to be useful.

But usefulness by itself is not accepted as a cause of economic value even by the discriminating economists who do not confuse economic value with price. Witness the keen arguments advanced in the old controversy over whether Ricardian land has any economic value. It is again thermodynamics which explains why the things that are useful have also an economic value—not to be confused with price. For example, land, although it cannot be consumed, derives its economic value from two facts: first, land is the only net with which we can catch the most vital form of low entropy for us, and second, the size of the net is immutable.[5] Other things are *scarce* in a sense that does not apply to land, because, first, the amount of low entropy within our environment (at least) decreases continuously and irrevocably, and second, *a given amount of low entropy can be used by us only once.*

Clearly, both scarcities are at work in the economic process, but it is the last one that outweighs the other. For if it were possible, say, to burn the same piece of coal over and over again *ad infinitum*, or if any piece of metal lasted forever, then low entropy would belong to the same economic category as land. That is, it could have only a scarcity value and only after all environmental supply will have been brought under use. Then, every economic accumulation would be everlasting. A country provided with as poor an environment as Japan, for instance, would not have to keep importing raw materials year after year, unless it wanted to grow in population or in income per capita. The people from the Asian steppes would not have been forced by the exhaustion of the fertilizing elements in the pasture soil to embark on the Great Migration. Historians and anthropologists, I am sure, could supply other, less known, examples of "entropy-migration."

Now, the explanation by Classical thermodynamics of why we cannot use the same amount of free energy twice and, hence, why the immense heat energy of the ocean waters has no economic value, is sufficiently transparent so as to be accepted by all of us. However, statistical thermodynamics—undoubtedly because of its ambiguous rationale—has failed

[4] By now the reader should know better than to suspect that by the last remark I wish to imply that the Entropy Law is nothing but a mere verbal convention. It is a miracle, though, that an anthropomorphically conceived order fits also the fact that coal turns into ashes in the same direction, from past to future, for all humans.

[5] Chapter IX, Section 6, above.

to convince everyone that high order–entropy too is irremediably useless. Bridgman tells of some younger physicists who in his time tried to convince the others that one could fill "his pockets by bootlegging entropy,"[6] that is, by reversing high into low entropy. The issue illustrates most vividly the thesis that thermodynamics is a blend of physics and economics.

Let us take the history of a copper sheet as a basis for discussion. What goes into the making of such a sheet is common knowledge: copper ore, certain other materials, and mechanical work (performed by machine or man). But all these items ultimately resolve into either free energy or some orderly structures of primary materials, in short, to *environmental* low entropy and nothing else. To be sure, the degree of order represented by a copper sheet is appreciably higher than that of the ore from which we have obtained the finished product. But, as should be clear from our previous discussions, we have not thereby bootlegged any entropy. Like a Maxwell demon, we have merely sorted the copper molecules from all others, but in order to achieve this result we have *used up irrevocably a greater amount of low entropy than the difference between the entropy of the finished product and that of the copper ore.* The free energy used in production to deliver mechanical work—by humans or machines—or to heat the ore is irrevocably lost.

It would be a gross error, therefore, to compare the copper sheet with the copper ore and conclude: Lo! Man can create low from high entropy. The analysis of the preceding paragraph proves that, on the contrary, production represents a deficit in entropy terms: it increases total entropy by a greater amount than that which would result from the automatic shuffling in the absence of any productive activity. Indeed, it seems unreasonable to admit that our burning a piece of coal does not mean a speedier diffusion of its free energy than if the same coal were left to its own fate.[7] Only in consumption proper is there no entropy deficit in this sense. After the copper sheet has entered into the consumption sector the automatic shuffling takes over the job of gradually spreading its molecules to the four winds. So, the popular economic maxim "you cannot get something for nothing" should be replaced by "you cannot get anything but at a far greater cost in low entropy."

But, one may ask, why do we not sort out again the same molecules to reconstitute the copper sheet? The operation is not inconceivable, but in entropy terms no other project could be as fantastically unprofitable.

[6] P. W. Bridgman, *Reflections of a Physicist* (2nd edn., New York, 1955), p. 244.

[7] According to the Entropy Law (Chapter V, Section 4, and Chapter VI, Section 1, above), the entire free energy incorporated in the coal-in-the-ground will ultimately dissipate into useless energy even if left in the ground.

This is what the promoters of entropy bootlegging fail to understand. To be sure, one can cite numberless scrap campaigns aimed at saving low entropy by sorting waste. They have been successful only because in the *given circumstances* the sorting of, say, scrap copper required a smaller consumption of low entropy than any alternative way of obtaining the same amount of metal. It is equally true that the advance of technological knowledge may change the balance sheet of any scrap campaign, although history shows that past progress has benefited ordinary production rather than scrap saving. However, to sort out the copper molecules scattered all over land and the bottom of the seas would require such a long time that the entire low entropy of our environment would not suffice to keep alive the numberless generations of Maxwell's demons needed for the completion of the project. This may be a new way of pinpointing the economic implications of the Entropy Law. But common sense caught the essence of the idea in the parable of the needle in the haystack long before thermodynamics came to the scene of the accident.

Economists' vision has reacted to the discovery of the first law of thermodynamics, i.e., the principle of conservation of matter–energy. Some careful writers have even emphasized the point that man can create neither matter nor energy.[8] But—a fact hard to explain—loud though the noise caused by the Entropy Law has been in physics and the philosophy of science, economists have failed to pay attention to this law, the most economic of all physical laws. Actually, modern economic thought has gradually moved away even from William Petty's old tenet that labor is the father and nature the mother of value, and nowadays a student learns of this tenet only as a museum piece.[9] The literature on economic development proves beyond doubt that most economists profess a belief tantamount to thinking that even entropy bootlegging is unnecessary: the economic process can go on, even grow, without being continuously fed low entropy.

The symptoms are plainly conspicuous in policy proposals as well as in analytical writings. For only such a belief can lead to the negation of the phenomenon of overpopulation, to the recent fad that mere school education of the masses is a cure-all, or to the argument that all a country —say, Somaliland—has to do to boost its economy is to shift its economic activity to more profitable lines. One cannot help wondering then why Spain takes the trouble to train skilled workers only to export them to

[8] E.g., A. Marshall, *Principles of Economics* (8th edn., New York, 1924), p. 63.

[9] "Hands being the Father, as Lands are the Mother and Womb of Wealth." *The Economic Writings of Sir William Petty*, ed. C. H. Hull (2 vols., Cambridge, Eng., 1899), II, 377.

other West European countries,[10] or what stops us from curing the economic ills of West Virginia by shifting its activity to more profitable *local* lines.

The corresponding symptoms in analytical studies are even more definite. First, there is the general practice of representing the material side of the economic process by a *closed system*, that is, by a mathematical model in which the continuous inflow of low entropy from the environment is completely ignored.[11] But even this symptom of modern econometrics was preceded by a more common one: the notion that the economic process is wholly *circular*. Special terms such as roundabout process or circular flow have been coined in order to adapt the economic jargon to this view. One need only thumb through an ordinary textbook to come across the typical diagram by which its author seeks to impress upon the mind of the student the circularity of the economic process.

The mechanistic epistemology, to which analytical economics has clung ever since its birth, is solely responsible for the conception of the economic process as a closed system or circular flow. As I hope to have shown by the argument developed in this essay, no other conception could be further from a correct interpretation of facts. Even if only the physical facet of the economic process is taken into consideration, this process is not circular, but *unidirectional*. As far as this facet alone is concerned, the economic process consists of a continuous transformation of low entropy into high entropy, that is, into *irrevocable waste* or, with a topical term, into pollution. The identity of this formula with that proposed by Schrödinger for the biological process of a living cell or organism vindicates those economists who, like Marshall, have been fond of biological analogies and have even contended that economics "is a branch of biology broadly interpreted."[12]

The conclusion is that, from the purely physical viewpoint, the economic process is entropic: it neither creates nor consumes matter or energy, but only transforms low into high entropy. But the whole physical process of the material environment is entropic too. What distinguishes then the first process from the second? The differences are two in number and by now they should not be difficult to determine.

To begin with, the entropic process of the material environment is *automatic* in the sense that it goes on by itself. The economic process, on the contrary, is dependent on the *activity* of human individuals who, like

[10] The above was written in 1963. But as any tourist knows, even nowadays in many West European countries the menial jobs in hotels are filled by temporary emigrants from Italy and Spain (at least). The same situation prevails, however, for the manual labor in mining and road building.

[11] See Chapter IX, note 30, above.

[12] Marshall, *Principles*, p. 772.

the demon of Maxwell, sort and direct environmental low entropy according to some definite rules—although these rules may vary with time and place. The first difference, therefore, is that while in the material environment there is only shuffling, in the economic process there is also sorting, or rather, a sorting activity.

And, since sorting is not a law of elementary matter, the sorting activity must feed on low entropy. Hence, the economic process actually is more efficient than automatic shuffling in producing higher entropy, i.e., waste.[13] What could then be the *raison d'être* of such a process? The answer is that the true "output" of the economic process is not a physical outflow of waste, but the *enjoyment of life*. This point represents the second difference between this process and the entropic march of the material environment. Without recognizing this fact and without introducing the concept of enjoyment of life into our analytical armamentarium we are not in the economic world. Nor can we discover the real source of economic value which is the value that life has for every life-bearing individual.

It is thus seen that we cannot arrive at a completely intelligible description of the economic process as long as we limit ourselves to purely physical concepts. Without the concepts of *purposive activity* and *enjoyment of life* we cannot be in the economic world. And neither of these concepts corresponds to an attribute of elementary matter or is expressible in terms of physical variables.

Low entropy, as I have stated earlier, is a *necessary* condition for a thing to have value. This condition, however, is not also *sufficient*. The relation between economic value and low entropy is of the same type as that between price and economic value. Although nothing could have a price without having an economic value, things may have an economic value and yet no price. For the parallelism, it suffices to mention the case of poisonous mushrooms which, although they contain low entropy, have no economic value.[14] We should not fail to mention also another common instance—that of an omelette, for instance—when man prefers a higher entropy (the beaten egg) to a lower entropy (the intact egg). But even for beating an egg, just as for shuffling the cards at bridge, man necessarily degrades some available energy. The economic process, to be sure, is entropic in each of its fibers, but the paths along which it is woven are traced by the category of utility to man. It would therefore be utterly wrong to *equate* the economic process with a vast thermodynamic system and, as a result, to claim that it can be described by an equally vast

[13] Cf. Chapter VII, Section 7.

[14] Of course, even poisonous mushrooms might be indirectly useful to us through a divine order, *die göttliche Ordnung* of Johann Süssmilch. But that does not concern our problem.

number of equations patterned after those of thermodynamics which allow no discrimination between the economic value of an edible mushroom and that of a poisonous one. Economic value distinguishes even between the heat produced by burning coal, or gas, or wood in a fireplace. All this, however, does not affect the thesis that I have endeavored to develop in this book, namely, that the basic nature of the economic process is entropic and that the Entropy Law rules supreme over this process and over its evolution.

There have been sporadic suggestions that all economic values can be reduced to a common denominator of low entropy. Apparently, the first writer to argue that money constitutes the economic equivalent of low entropy is the German physicist G. Helm (1887).[15] We find the same idea expanded later by L. Winiarski: "Thus, the prices of commodities (whether we take Jevons' definition, as the ratio of pleasures, or of Ricardo—as the ratio of labors—which comes to the same thing) represent nothing but the various conversation coefficients of the biological energy." The conclusion is just as astonishing: "*Gold* is therefore the general social equivalent, the pure personification and the incarnation of the socio-biological energy."[16] Other writers tried to improve somewhat upon Helm's and Winiarski's position by arguing that, although there is no direct equivalence between low entropy and economic value, there is in each case a *conversion* factor of the former into the latter. "Just as one particular slot machine will always deliver a certain package of chocolate, so a certain social organization under similar conditions will render (approximately) the same amount of selected form of energy in return for a stated sum of money."[17] The suggestion to determine all individual conversion factors—even if it were achievable—would still not be of much help to the economist. He would only be saddled with a new and wholly idle task—to explain why these coefficients differ from the corresponding price ratios.

2. The General Equation of Value. The preceding remarks become immediately obvious if we look at the whole economic process as one partial process and if, in addition, we consider this partial process over a sufficiently short interval of time. Since over such an interval, any growth or development may safely be neglected, the process comes very close to a stationary one. Its analytical description in material terms needs no elaboration. The funds—land, capital proper, and the entire population—go into the

[15] G. Helm, *Die Lehre von der Energie* (Leipzig, 1887), pp. 72 ff.

[16] L. Winiarski, "Essai sur la mécanique sociale: L'énergie sociale et ses mensurations," Part II, *Revue Philosophique*, XLIX (1900), 265, 287. My translation and italics.

[17] Alfred J. Lotka, *Elements of Physical Biology* (Baltimore, 1925), p. 356. It is not irrelevant to note that Lotka, too, could not get rid of money fetishism in discussing the role of low entropy in the economic process.

process and come out of it intact (in the particular sense we have attributed to the term "intact" in the preceding chapter). There are only two flows: an input flow of low entropy and an output flow of high entropy, i.e., of waste. Were we to set the balance sheet of value on the basis of these inputs and outputs, we would arrive at the absurd conclusion that the value of the low entropy flow on which the maintenance of life itself depends is equal to the value of the flow of waste, that is, to zero. The apparent paradox vanishes if we acknowledge the fact that the true "product" of the economic process is not a material *flow*, but a psychic *flux*—the enjoyment of life by every member of the population. It is this psychic flux which, as Frank Fetter and Irving Fisher insisted,[18] constitutes the pertinent notion of income in economic analysis. The fact that their voices were heard but not followed should not prevent us from recognizing, belatedly, that they were right.

Like any flow, the flux of life enjoyment has an intensity at each instant of time. But in contrast with a material flow, it cannot accumulate in a stock. Of all the past enjoyment of life an individual preserves only a memory of varying vividness. A millionaire who has lost all his fortune in a stock market crash cannot draw on the reservoir of accumulated life enjoyment of his former good years because there simply is no such reservoir. Nor can a retired worker who has saved for old age say in any sense that he now depletes the stock of his accumulated life enjoyment. His money savings are only the instrument by which such a person is able to achieve a desired intensity of life enjoyment at each moment of his life, not that enjoyment itself. Yet, just as in the case of a service, we may think of the flux of life enjoyment over a *stretch of time*. The only difficulty raised by this thought is that the intensity of this flux at an instant of time does not seem to be a measurable entity, not even in the ordinal sense. This is in fact the only issue with which utility theorists have continuously, but vainly, struggled ever since economists turned to utility for an explanation of economic value.[19] However, over a short interval—as we have now in view—the intensity of life enjoyment does not change much. Consequently, we may represent the total life enjoyment symbolically by the product of its intensity and the length of that interval. This is all the more legitimate since I do not intend to perform any arithmetical operations with this pseudo measure in the subsequent argument.

Another elementary fact is that the enjoyment of life depends on three

[18] F. A. Fetter, *The Principles of Economics* (New York, 1905), chap. vi; Irving Fisher, *The Theory of Interest* (New York, 1930), p. 3.

[19] Cf. my articles "Choice, Expectations, and Measurability" (1954), reprinted in AE, and "Utility," *International Encyclopedia of the Social Sciences* (New York, 1968), XVI, 236–267.

factors, two favorable and one unfavorable. The daily life enjoyment is enhanced by an increase in the flow of consumer goods one can consume daily as well as by a longer leisure time.[20] On the other hand, the enjoyment of life is diminished if one has to work longer hours or at a more demanding task. One point that at present calls for some special emphasis is that the negative effect of work on the daily life enjoyment does not consist only of a decrease of leisure time. To expend a manual or mental effort diminishes indeed the leisure time, but in addition burdens life enjoyment with the disutility of work.[21] Consequently, all the three factors that together determine the daily life enjoyment must be kept separate in a preliminary analytical representation. If e stands for the daily life enjoyment of a given individual, we may write symbolically:

(1) $e =$ Consumption Enjoyment + Leisure Enjoyment − Work Drudgery.

I say "symbolically" because in this equation (as well as in those of the same nature that I shall write hereafter) the mathematical signs are not taken in the strict sense, but rather as convenient signs for summarizing the imponderable elements that enter, in a positive or negative way, into the entity represented on the left of the equality sign. With this idea in mind we may write (1) in a more detailed form as follows:

$$e = (i_1 \times 1) + [i_2 \times (1 - \delta)] - (j \times \delta). \tag{2}$$

Here, i_1 is the intensity of consumption enjoyment; i_2 is the intensity of leisure enjoyment; j is the intensity of work disutility; and δ is the working day (which need not be a positive number for every individual). The fact that the intensity of consumption is multiplied by unity (the full day) should be easily understood. Consumption is a process that goes on uninterruptedly with the flow of Time. We must eat, wear clothes, be sheltered, etc. every day round the clock. The consumption day, in contrast with the working day, is not determined by our will or our institutions; it is dictated by the fact that the process of life cannot be interrupted and retaken (as a factory process can).

To relate the preceding observations to the economic process, it is

[20] Actually the individual also enjoys the services of some funds—of some durable consumer goods. For the sake of avoiding complications irrelevant to the theme of this section, I propose to set aside this element.

[21] The currently prevailing thesis is that leisure "means freedom from the burden of work; and the satisfaction it yields is the enjoyment of not working." T. Scitovsky, *Welfare and Competition* (Chicago, 1951), p. 105. The thesis is rooted in Walras' approach which ignores the disutility of labor. The Gossen-Jevons approach, on the other hand, takes into consideration only the disutility of labor and pays no attention to the utility of leisure. For further details on the difference between these two incomplete approaches see my article, "Utility," pp. 248 f.

immediately apparent that we have to divide this process into two processes—the traditional division into the production process P_1 and the consumption process P_2. With the elimination of irrelevant details, the system obtained is represented by Table 12, where C stands for a composite consumer good, K for a composite capital good (including inventories and process-funds), n is the number of labor shifts, and $\Delta = n\delta \leq 1$.[22] The symbol E^* stands for the life enjoyment afforded by the consumption sector, i.e., for the consumption enjoyment and the leisure enjoyment of the entire population. Symbolically, it can be broken down as follows

$$(3) \qquad E^* = (H_0 \times I_1) + (H' \times I_2') + [H \times I_2 \times (1 - \delta)],$$

where H_0 is the size of the population, $H' = H_0 - H$ is the size of the "kept-up class" (young and old as well as the rentiers, if any), I_1 is the "average" daily consumption enjoyment, and I_2' and I_2 are the "average" intensities of leisure enjoyment for H' and H.[23] To complete the picture, let E be the total daily life enjoyment of the population when the disutility of work is taken into account. We have

TABLE 12.
A Schematic Representation of the Economic Process

	P_1	P_2
	Flow Coordinates	
C	$\Delta \times c$	$-\Delta \times c$
R	$-\Delta \times r$	*
W	$\Delta \times w_1$	w_2
	Fund Coordinates	
Land	$\Delta \times L$	*
Capital	$\Delta \times K$	*
Labor Power	$\delta \times H$	E^*

$$(4) \qquad E = E^* - (H \times J \times \delta),$$

[22] Cf. Table 3 of Chapter IX, above. For a more faithful representation, the production sector should be divided into an agricultural sector (in which there can be no labor shifts properly speaking) and a manufacturing sector. However, such a division would only complicate unnecessarily the argument to follow.

[23] It may be well to repeat that the symbolism should not be interpreted in the arithmetic sense. By using the compact diction $H_0 \times I_1$, for instance, I do not imply that the individual enjoyments may be added together into a significant coordinate. Nothing could be more opposed to my own thoughts on the measurability of utility.

where J is the "average" intensity of the discomfort produced by work.

I now wish to submit that everything that supports life enjoyment directly or indirectly belongs to the category of economic value. And, to recall, this category does not have a measure in the strict sense of the term. Nor is it identical to the notion of price. Prices are only a parochial reflection of values. They depend, first, on whether or not the objects in question can be "possessed" in the sense that their use can be denied to some members of the community. Solar radiation, as I have repeatedly stressed, is the most valuable element for life; yet it can have no price because its use cannot be controlled except through the control of land. But in some institutional setups even land may have no price in money—as was the case in many a society of older times, such as unadulterated feudalism, and is now the case in communist states. Prices are also influenced by another, more common, institutional factor—the fiscal power of public administration. By contrast, value is a category which can change only with the advance of knowledge and which can be projected only on a dialectical scale of order of importance.

The flow of consumer goods $\Delta \times c$ has value because without it there would be no consumption enjoyment, in fact, no human life. And everything that is needed to produce this flow also has value by virtue of the principle of imputation. We can then write a first equation of value

$$\text{(5)} \quad \begin{aligned} \text{Value}\,(H_0 \times I_1) &= \text{Value}\,(\Delta \times c) \\ &= \text{Value}\,(\Delta \times r) + \text{Value}\,(\Delta \times L) \\ &\qquad + \text{Value}\,(\Delta \times K) + \text{Value}\,(\delta \times H). \end{aligned}$$

Naturally, since nothing that has value is thrown out of the economic process, the value of waste is zero and does not have to appear in this equation—except perhaps as a negative term in some cases.

The circle between the production process and the enjoyment of life is closed by a second equation which relates the value of labor services to the disutility of work:

$$\text{(6)} \qquad \text{Value}\,(\delta \times H) = \text{Value}\,(H \times J \times \delta).$$

With the aid of the last two equations equation (4) can be written

$$\text{(G)} \quad \begin{aligned} \text{Value}\,E = \text{Value}\,(\Delta \times r) + \text{Value}\,(\Delta \times L) + \text{Value}\,(\Delta \times K) \\ + \text{Value}\,(H' \times I_2') + \text{Value}\,[H \times I_2 \times (1 - \delta)]. \end{aligned}$$

I propose to refer to this equation as the general equation of value for the reason that every major doctrine of value can be shown to be a particular case of it. It enables us to delineate the basic differences between these doctrines against the same background.

Spelled out in price terms and income categories, (G) becomes

(G1) Income = Royalties + Rent + Interest + Leisure Income,

or by (5),

(G2) Income = Net Product + Leisure Income − Wages.

We may consider first the Ricardian conception of *net income*:[24]

(R) Income = Royalties + Rent + Interest = Net Product − Wages.

If compared with (G1) and (G2), this relation shows that implicit in Ricardo's conception there is the idea that leisure has no value. The thought that, since leisure is not a direct product of labor, it would be inconsistent for a labor theory of value to attribute any value to leisure could hardly have occurred to Ricardo. In any case, we have no indication that it did. He introduced the concept of net revenue in relation with the problem of what should bear a tax. Leisure, obviously, does not constitute a tangible basis for taxation. However, he used net revenue also as an index of welfare. Consequently, we should ask why, even in this case, Ricardo clung to the equation (R). The answer is hinted at by Ricardo himself in his *Notes on Malthus*: "I limited my [welfare] proposition to the case when wages were too low to afford [the laborer] any surplus beyond absolute necessaries."[25] This means that he had continuously in mind a situation of such an intensive exploitation of labor that no leisure proper is left to the laborer. No wonder then that subsequently he had to make amends concerning the net revenue as an index of welfare. In parrying some of Malthus' criticism, Ricardo admitted that "wages may be such as to give to the laborers a part of the neat revenue."[26] In fact, he goes as far as to speak of leisure and also of the worker's enjoyment of consumption in excess of their disutility of working: "if the laborer's wages were high he might do as he pleased—he might prefer indolence or luxuries," or "the situation of the laborer would be improved, if he could produce more necessaries in the same time, and with the same labor."[27]

Second in chronological line there is Marx's conception of income which is based on the well-known tenet that nothing can have value if it is not due to human labor.[28] From this tenet it follows that

(7) $\quad$ Value $(\Delta \times r) = 0$, $\quad$ Value $(\Delta \times L) = 0$, $\quad$ Value $(\Delta \times K) = 0$,

[24] David Ricardo, *On the Principles of Political Economy and Taxation* in *The Works and Correspondence of David Ricardo*, ed. P. Sraffa (10 vols., Cambridge, Eng., 1951–1955), I, chap. xxvi.

[25] Ricardo, *Works*, II, 381.

[26] *Ibid.*

[27] *Ibid.*, II, 332, 334.

[28] Karl Marx, *Capital* (3 vols., Chicago, 1932–1933), I, 47.

i.e., that things supplied by nature "gratis" and the *services* of capital proper have no value—as Marx explicitly and repeatedly argued. But it also follows that

$$\text{Value of Leisure} = 0, \tag{8}$$

a point that does not appear explicitly in Marx's writings. On the basis of (7) and (8), equation (5) becomes

$$\text{Value } (\Delta \times c) = \text{Value } (\delta \times H), \tag{9}$$

which is the cornerstone of Marx's doctrine. The net income of Ricardo (in money terms) represents the "surplus value" which capitalists obtain by forcing the workers to produce more than their own necessities of life. However, there is no way of introducing this additional disutility of labor into Marx's framework: such an attempt would create an asymmetry with respect to (8).

Because equation (9) reduces value only to the labor of man, one may be tempted to say—some have indeed said—that Marx's doctrine is idealistic rather than materialistic. Nothing could be further from truth. For if we take account of (7) and (8), equation (G) becomes

$$\text{Value } E = 0. \tag{M}$$

This means that the enjoyment of life itself—which according to my contention is the only basis from which value springs—has no value whatsoever. The full-fledged materialism of Marx's *economics*, even if viewed separately from the doctrine of historical materialism, need not therefore raise any doubts in our minds.[29]

On the other hand, the preceding analysis shows that, contrary to the generally held opinion, Marx's doctrine of value is not a close relative of Ricardo's. According to Marx, all the terms of Ricardo's equation (R) are zero. And the fact that for Ricardo, too, the ultimate determination of the prices of commodities *produced by man* is the amount of labor, should not mislead us. For Ricardo, both "land" and the services of capital (beyond and above maintenance) have a price which, obviously, cannot be determined by the labor formula. Yet the two economic approaches have one important feature in common. To establish the relation between

[29] The idea that life has no value is a tenet strongly defended by Marxist exegetes. When asked why he does not then commit suicide on the spot, one such exegete reportedly answered by the sophistry: precisely because there is no difference between being alive or dead. Persisting questions such as this prompted the Polish philosopher, Adam Schaff, to denounce the "Marxist prejudices" in a highly interesting article "On the Philosophy of Man," a translation of which appeared in *East Europe*, X (April 1961), 8–12, 43–45. At the time, Schaff was the dean of the Polish Marxists and a member of the Central Committee of the Communist Party.

prices of commodities and prices of labor, Ricardo goes to the margin of cultivation, i.e., where the whole product goes to labor. Marx, too, goes to a margin, the margin of economic history where capital does not yet exist and labor is the only production agent. Marx was thus just as much of a "marginalist" as Ricardo. The main examples used in the introductory chapters of the first volume of *Capital* to illustrate his doctrine of labor value leave little doubt about this. Marx is certainly right if we take the case of the first stone hammer ever produced from some stone picked up from a creek's bed: that stone hammer was produced only by labor out of something readily supplied by nature. But what Marx ignored is that the next stone hammer was produced with the help of the first, actually at a reproduction rate greater than one to one.[30]

There remains the Neoclassical conception of income, which is the only one used currently in the standard literature. In this conception, income is simply identified with the value of the product. It amounts to striking out the last two items in equation (G2). Alternatively, the formula may be written in the popular form

(NC) Income = Royalties + Rent + Interest + Wages.

Like Ricardo's, this approach denies any economic value to leisure, but unlike it, it does not deduct from income the disutility of work. Obviously, the approach reflects the businessman's viewpoint: wages are a part of his cost but do not represent a cost counterpart in the life enjoyment of the worker.[31]

The opinion that the theoretical scaffold of Marx's doctrine of value—taken by itself and without regard for the factual validity of the assumptions—is to be admired for its logical consistency has been expressed by many an authority in economics.[32] The foregoing analysis offers no reason against that opinion. By comparison, the philosophical bareness of the Neoclassical school becomes all the more conspicuous. Perhaps a pronounced pragmatical bent is responsible for the fact that this school, while paying attention to that part of a worker's time sold for wages, has completely ignored the value of the leisure time. Whether one equates

[30] On the change thus introduced, whether in technology or in mathematics, see Chapter XI, Section 3, below.

[31] Since the difference between equations (G1) and (NC) is confined to that of the last item of each sum, they would not differ if the disutility of work would just be compensated by the utility of leisure. With sufficient largesse one may however read (G1) into Walras' system by connecting his general remarks on revenue with his idea that the workers in fact sell the utility of part of their leisure time. See Léon Walras, *Elements of Pure Economics* (Homewood, Ill., 1954), pp. 257–260, 378–380.

[32] E.g., Thorstein Veblen, "The Socialist Economics of Karl Marx and His Followers," *Quarterly Journal of Economics*, XX (1906), 575; J. A. Schumpeter, *Ten Great Economists from Marx to Keynes* (New York, 1951), p. 25.

prices with value or considers prices as the only indirect means of measuring values, the problem of the value of leisure time involves a practical difficulty. There is no directly observable price for leisure time. Yet this difficulty has a very simple solution within the Neoclassical apparatus itself. In relation to the labor market, leisure time represents reserve demand. Just as the eggs that a peasant chooses to keep for his own consumption must be valued at the price of the sold eggs, so should leisure time be valued at the prevailing wage rate for labor time. Of course, this is a simplification which does away with the essential difference between the disutility of labor and the enjoyment of leisure. However, without this heroic simplification there is no way of arriving at a reasonable pseudo measure of the welfare level of a community that would not be vitiated by the omission of an important component.

The necessity of including the value of leisure time (under some form or another) into the pseudo measure of welfare becomes all the more imperative in the case of international comparisons and in that of comparing the situations of the same community at distant points in time.[33] It is beyond doubt that, if a person can choose between living in country U or in country S—both countries having the same income per capita—he will choose U if *ceteris paribus* its economic situation requires shorter working hours and less hard work. There is, however, one pitfall of which we should be aware in substituting a price evaluation of the leisure in equation (G2). In overpopulated countries leisure is so abundant that by valuing it at the wage rate we run the risk of placing India's welfare higher than, perhaps, that of the United States. The point to bear in mind is that, in contrast with a developed economy, in overpopulated countries most of the leisure is *unwanted* leisure. In this situation, the argument of the reserve demand breaks down—and for two correlated reasons. The first is that if our peasant has no alternative use for the eggs with which he is forced against his will to return from the market, we cannot speak of a reserve demand in the proper sense. Second, an excessive abundance of eggs may bring the price of eggs down almost to zero. But the same law does not apply to labor: wages cannot fall below a certain minimum even if there exists an abundant excess supply of labor and, in many sectors, labor is used to the point where its marginal productivity is zero. Consequently, in the pseudo measure of welfare for any country where leisure is unwanted, leisure should simply be attributed a null price.[34]

[33] To my knowledge, the first author to insist on and defend this view is Simon Kuznets, "Long-term Changes in the National Income of the United States of America since 1870" in *Income and Wealth*, Series II, ed. Simon Kuznets (Cambridge, Eng., 1952), pp. 63 ff.

[34] Cf. my article "Economic Theory and Agrarian Economics" (1960), reprinted in AE, pp. 387 f.

3. Entropy and Development. The fact that the economic process consists of a continuous and irrevocable transformation of low into high entropy has some important consequences which should be obvious to anyone willing to descend for a moment from the higher spheres of elucubrated growth models down to the level of elementary facts.

To begin with, let us observe that the manufacturing sector is completely tributary to the other two processes—agriculture and mining—in the sense that without the current input flows received from them it would have nothing to manufacture into industrial products. True, these other two sectors, in turn, are tributary to the industrial sectors for the tools they use and, implicitly, for a large measure of their technical progress. But this mutual dependence should not cause us to lose sight of the fact that it is the pace at which low entropy is pumped from the environment into the economic process that limits the pace of this process, nor of the specific causal order that relates the three sectors into which, for good reasons, economists have divided man's productive activity.

That man must first satisfy his biological needs before he can devote any time and energy to produce commodities that satisfy other kind of needs is a commonplace. Yet we seem now to ignore, often to deny, the priority which the production of food must thus have over the production of other consumer goods. But the fact is that man was *homo agricola* before becoming also *homo faber*. For long ages agriculture was, as Xenophon noted, "the mother and the nurse of all the other arts."[35] It was their mother because the earliest technical innovations came from agriculture. Think of the practice of manuring, of crop rotation, and above all of the plow, which even nowadays is made on the same "blueprint" of its inventors, some anonymous peasants. Agriculture was, and still is, the nurse of all other arts for the simple reason that as long as Robinson Crusoe and Friday could not subsist on the food gathered by only one of them neither could devote his entire time to any other art. If agriculture had not been able to develop *by itself* to the level at which it could feed both the tillers of the soil and those engaged in other activities, mankind would still be living in wilderness.

Even if we consider the problem from a more sophisticated viewpoint, all advanced economies of the world climbed to the height of their present economic development on the broad basis of a developed agriculture. True, nowadays a few countries—Kuwait, for one—may find a source of development in their mineral resources alone. But this is only because these resources can *now* be used by the already developed economies. The singular case of Japan is particularly instructive for pinpointing these preliminary remarks.

[35] *The Economist of Xenophon*, ed. John Ruskin (London, 1876), V. 17.

We usually speak of Japan's economic miracle in relation to the spectacular recovery and the equally exceptional growth rate achieved by that country after World War II. In my judgment, the miracle, if we should use this term, is what happened after the Meiji Restoration (1886). Japan's geographical conditions are rather inhospitable: the soil contains no mineral resources to speak of; the topography is such that eighteen percent of the entire area (the present figure) is practically all that can be brought under the plow. The Japanese, however, went out to plow the high seas for protein food and turned to a highly labor intensive cultivation for the rest. With practically no royalty income from her own territory, Japan had already a developed economy by World War I. The miracle is that Japan's economy "took off" on the back of a silk moth. Other nations had the silkworm, but missed the same opportunity. The explanation must be sought in the differences in cultural attitudes. They account also for the fact that Japan can now operate an impressive industry by paying royalties to the nations from which she imports the low entropy materials. She can pay and still thrive because of her formidable human assets: a highly efficient and easily trainable labor as well as a very imaginative technological talent.

There is, however, a question about which the Japanese economists ought to start thinking instead of devoting their time, as most do, to esoteric mathematical models having only a vocabulary connection with the economic actuality. How long can Japan continue to operate her economy in this manner? Money royalties have a tendency to increase if a local industry develops. Will Japan still be able to absorb the cost of increased royalties after the countries from which she now imports raw materials have developed, as we must assume, their own industry fully? In other words, can her present competitive advantages—a relatively cheap and efficient labor and the quasi-rent of new technological ideas—continue to prevail against increasing odds? A miracle that lasts too long is a super-miracle.

I mention this question not only for accentuating the role of low entropy in our general economic activity, but also for the following reason. As a result of the modern economist's belief that industrialization is a cure-all, every economically underdeveloped country aims at becoming industrialized to its teeth without stopping to consider whether or not it possesses the necessary natural resources within its own territory. When I raised this last issue with the planning agencies of some countries known for their meager mineral resources, I was invariably served the case of Japan as a justification for their plan to build even a heavy industry. But should a demon be able to implement overnight the long-range economic plans (perhaps even the short-range ones) of every country in the world, the following day we would discover, I am sure, that we have in fact been planning for an immense industrial capacity which must remain largely idle because of insufficient mineral resources. As these plans will be gradually realized in the near future, the planned duplication of industrial capacity will necessarily cry out in our face. In a few instances the symptoms of this excess planning have already begun to embarrass the planners. I venture to think that sooner or later some coordination of all national plans will have to be introduced through some international agency for the purpose of avoiding wasteful duplication. The thought presupposes that we will also abandon many of the ideas to which we now cling in matters of economic development and replace them by a broader perspective of what economic development means in terms of entropy transformation.

If divested of all the obstructive garb donned on it by the growth models now in vogue, economic development boils down to only two elements: *development proper*, i.e., the innovation of finer sieves for the sifting of low entropy so as to diminish the proportion of it that inevitably slips into waste, and *pure growth*, i.e., the expansion of the sifting process with the extant sieves. The economic history of mankind leaves no doubt about this entropic struggle of man.[36] This struggle, however, is subject to some laws, some deriving from the physical properties of matter, some from the nature of man himself. Some may be commonplaces, some not. For our understanding, however, only an integrated picture of these laws counts.

Earlier, I have observed that in the manufacturing process, where the factory system now predominates, the product flow is proportional to the time during which the productive capacity is in operation daily.[37] The observation needs to be qualified by the condition that the other two sectors should be able to support the increased activity. The same is true for the expansion of the *scale* of the manufacturing sector, and this time regardless of whether this sector operates by the factory system. The point is that in the end the issue of returns boils down to that of returns in mining and in agriculture. There is, though, a difference between returns in mining and returns in agriculture. In mining, we tap the *stocks* of various forms of low entropy contained in the crust of the planet on which we live; in agriculture, we tap primarily the *flow* of low entropy that reaches the earth as solar radiation.

There is no external reason, generally speaking, why the product flow of a mine operated with given installations should not be roughly propor-

[36] I hasten to add that to innovate and expand is not an end in itself. The only reason for this hustle and bustle is a greater life enjoyment.

[37] Chapter IX, Section 10.

tional to the time of daily operation. Moreover, we can increase the flow of mined resources by opening additional mines. The only restriction is set by the amount of resources within the reach of the mine, in the first case, and the total reserves in the bowels of the earth, in the second. Conceivably, we may mine the entire stock of coal-in-the-ground within a short period, one year, for example. But to do so we would have to reach deeper into the earth's crust and also for poorer and poorer lodes. *At any one time*, therefore, a substantial increase in the tapping of mineral resources can be achieved at an increased unitary cost in terms of low entropy. Conceivably also, after mining all the coal-in-the-ground we may burn it within one year. Of course, there are great obstacles to mining and burning all coal reserves within such a short interval.[38] But the purpose of my imaginary examples is to bring home the point that the rate at which we may use our mineral reserves is largely a matter of *our own decision*.

Jevons tried to lay bare the implication of some of these points in his first economic work, *The Coal Question*, but was met with almost general disapproval. "In round numbers," he noted, "the population [of Great Britain] has about quadrupled since the beginning of the nineteenth century, but the consumption of coal has increased sixteen fold, and more."[39] What alarmed Jevons was the fact that, as he and others correctly assessed, England's economic superiority at the time was based on her abundant coal mines.[40] For he foresaw—and in this he was later proved correct—that the still more abundant coal reserves in the United States and possibly in other countries would eventually tip the balance in the opposite direction.[41]

But Jevons laid himself open to the criticism of his contemporaries because he also took the firm position that man will not find another substitute for coal as a source of free energy.[42] History has blatantly refuted him on this count. However, if we reinterpret his basic point of

[38] As pointed out by Lord Kelvin in "On the Fuel Supply and the Air Supply of the Earth," *Report of the British Association for the Advancement of Science*, 1897, pp. 553 f, to burn all the coal reserves within one or even ten years may require a greater amount of oxygen than is available in the atmosphere. The idea foreshadowed one factor in today's air pollution—the intemperate speed with which we transform the oxygen of the atmosphere into carbon dioxide (and monoxide).

[39] W. Stanley Jevons, *The Coal Question*, ed. A. W. Flux (3rd edn., London, 1906), p. 196. In judging Jevon's concern over the imminent exhaustion of the coal reserves one should consider the fact that a natural scientist of first rank, Svante Arrhenius, predicted in 1923 that by 1950 there would be no more petroleum available! Yet the failure of predictions such as these does not prove the inexhaustibility of the earth's resources.

[40] *Ibid.*, pp. 3, 321.

[41] *Ibid.*, chap. xiv. And he was correct because the balance began tipping before oil came to replace coal on a significant scale.

[42] *Ibid.*, pp. 8, 183.

departure in the light of some of his side remarks, we find it now vindicated by the principles of thermodynamics. Had Jevons referred to the reserves of low entropy in the earth's crust instead of coal in speaking of "a certain absolute and inexorable limit, uncertain and indefinable that limit may be,"[43] and had he also added that free energy cannot be used more than once, he would have presented us with a clear picture of one side of man's struggle with the limited dowry of mankind's existence on earth. In this perspective, the conclusion is far stronger than that which Jevons reached for coal: *even with a constant population and a constant flow per capita of mined resources, mankind's dowry will ultimately be exhausted if the career of the human species is not brought to an end earlier by other factors.*

Unlike most economists of later days, Jevons has a perfect excuse for ignoring the Entropy Law: this law was formulated by Clausius the very year Jevons' book came out of the printing press (1865).[44] For the same reason we may absolve Jevons for another statement and also his contemporaries for not objecting to it. The statement is that "A farm, however far pushed, will under proper cultivation continue to yield forever a constant crop. But in a mine there is no reproduction; the produce once pushed to the utmost will soon begin to fail and sink towards zero."[45] Curiously, the same idea, even in a stronger form, still enjoys great currency not only among economists but also among agronomists. In a recent collaboration of experts on agriculture and population we read: "Properly used, [the plants of the earth] can by their reproductive powers supply us *indefinitely* with the food, the wood, and the other natural products we require."[46] Apparently, we have not yet learned what Malthus wanted to say any more than Jevons did. Perhaps Malthus said it badly because in his days he could not possibly disentangle the fundamental differences between agriculture and mining, the bases of the economic process.

The scarcity of the low entropy that man can use does not suffice by itself to explain the peculiar balance and the general direction of economic development. There is a conflict that steadily, albeit imperceptibly, permeates this development and has its origin in the asymmetry of the two sources of low entropy: the sun's radiation and the earth's own deposits. As we have already seen, the difference in the location and the

[43] *Ibid.*, p. 195.

[44] See Chapter V, Section 4, above.

[45] Jevons, *Coal Question*, p. 201.

[46] M. Cépède, F. Houtart, and L. Grond, *Population and Food* (New York, 1964), p. 309. My italics. Most agronomists share the same fallacy. For another example, see Q. M. West, "The World Food Supply: Progress and Promise" in *Food: One Tool in International Economic Development*, Iowa State University Center for Agricultural and Economic Adjustment (Ames, Iowa, 1962), p. 103.

nature of these two sources is responsible for some aspects of this asymmetry. Another, equally important, reason for the asymmetry is the fact that each source of low entropy is associated with one of the main categories of man's productive endeavors. Solar radiation is associated primarily with husbandry, the mineral low entropy with industry. This division accentuates the asymmetry of scarcity because of the undeniable fact that, although nature is the partner of man in every productive activity, this partnership is more stringent and more subtle in husbandry than in all other sectors.

The partnership is more stringent in husbandry because, first, nature dictates the time when an agricultural elementary process must be started if it is to be successful at all. This, we remember, generally denies the use of the factory system in agriculture. The flow of agricultural products can certainly be increased (within some limits) by more intensive work or by longer labor services. But it is equally certain that the statement "doubling the working hours with the same material funds doubles the product flow" can rarely, if ever, apply to agriculture.[47] Granted the industrial capacity and the necessary mineral resources, a community can increase the production of Cadillacs by twenty-five percent by simply working ten instead of eight hours per day. The fact that the same legerdemain is unavailing in agriculture constitutes one irreducible obstacle in man's struggle to nourish himself.

The second reason why the same partnership is more stringent in agriculture is that we cannot mine the stock of solar energy at a rate to suit our desires of the moment. We can use only that part of the sun's energy that reaches the globe at the rate determined by its position in the solar system. With the stocks of low entropy in the earth's crust we may be impatient and, as a result, we may be impatient—as indeed we are—with their transformation into commodities that satisfy some of the most extravagant human wants. But not so with the stock of sun's energy. Agriculture teaches, nay, obliges man to be patient—a reason why peasants have a philosophical attitude in life pronouncedly different from that of industrial communities.

But the most decisive element of the asymmetry of scarcity and, implicitly, of the difference between agriculture and industry is the extremely subtle way in which nature helps the husbandman. When we use the chemical energy of some dynamite or the kinetic energy of a waterfall,

[47] Cf. Chapter IX, Section 9, above, especially note 46. Jevons, *Coal Question*, p. 195, thought of opposing mining and industry to agriculture by insisting that if "we want to double the produce of a field we cannot get it simply by doubling the laborers." Unless he meant "doubling the laborers by using two shifts instead of one," the remark misses the target: doubling the workers of a full shift will not double the produce of a mine or a factory either.

for example, the result sought is the statistical average of the uncertain effects at the microlevel. Even the production of the most sensitive instruments does not have to follow exactly a specific atomic structure. A tolerable approximation of the macroblueprint suffices for all practical purposes. In bulk, even chemical substances do not have to be produced with absolute purity. To grow a plant from seed, however, is not a matter of average mass effect. On the contrary, every cell, nay, every part of a cell must develop exactly after a complicated but absolutely rigid blueprint of the atomic, even subatomic, structure.

Particularly nowadays, the idea of a fundamental difference between what man can do in husbandry and what he can do in mining and in industry is likely to be dismissed unceremoniously as romantic vitalism. At most, one may be reminded that biology has recently achieved some spectacular results. And indeed, these discoveries have been advertised as the forebodings of the approaching biological millennium not only by journalistic humbugs but even by a few authorities who have apparently succumbed to the temptation of writing a somewhat jocose utopia or to an exaggerated enthusiasm for one's own wares.[48] Nothing is further from my thought than to deny—or even to belittle—the achievements of any science, including the various branches of biology. I also am perfectly confident that the biological knowledge available in the distant future would make us awestruck if it were suddenly revealed to us now. But even if it were true—as Joshua Lederberg judges—that predictions about the achievements of molecular biology have recently been too conservative,[49] it does not follow that the trend is going to be everlasting or that there is no limit to what biology can do.

The position that the differences between the economics of husbandry and the economics of mining and industry are not likely to disappear in the future does not deny the numerous achievements of biology that are worthy of the highest praise. Nor do such glamorous achievements constitute a refutation of that position. The position is justified by the resilient difficulties of understanding, predicting, and especially manipulating life processes. A survey of these difficulties—offered in Appendix G, below—reveals that the lasting obstacle to man's manipulating living matter as efficaciously as inert matter resides in two limitations inherent in man's nature.

First, man cannot reach into the cosmic dimension of space and time.

[48] Salient examples of the last two categories are the pundit J. B. S. Haldane and the Nobelite Joshua Lederberg. On this problem more will be said in Appendix G, below.

[49] Joshua Lederberg, "Biological Future of Man," in *Man and His Future*, ed. G. Wolstenholme (Boston, 1963), p. 266.

That is why man is denied the cosmic lever and fulcrum demanded by Archimedes. Not being able to reach too far into space and time, man also cannot handle those numbers that Émile Borel appropriately termed "inaccessible." And biology abounds in such numbers at every turn. For example, the distinct human genotypes are so numerous that it is impossible for all to appear during the entire life of mankind.

The second limitation is not as immediately obvious, but in no way less inexorable. Although we may look at a star hundreds of light-years away, we know that no earthling will ever reach it. Similarly, although we can hold in the cup of our hand billions of billions of atoms, we cannot pick up one single atom. Man cannot reach too deep into the microcosm either. What matters for our present discussion is that we cannot construct an individual cell or even a molecule part by part, in the same simple and direct manner in which we put together electronic contraptions or skyscrapers, for example. Even if we had the fantastically complex and immense blueprint of a cell—which in fact is "inaccessible"—we would still be lacking nanotweezers and nanoscoops with which to pick up or scoop out atoms and ions and place each one of them in the mathematically exact position it must occupy. For the cells and the biomolecules are such that a difference of only a few atoms separates, for example, the normal hemoglobin from that responsible for sickle-cell anemia.[50] Naturally, the impossibility of a nanotweeze bars us from the less formidable project of remodeling an individual cell or even a macromolecule.

The basis of the limitation pertaining to the microcosm is, obviously, the Principle of Indeterminacy. Actually, before Heisenberg's discovery it would not have been absurd to believe that a molecule and, time permitting, even a cell could eventually be put together atom by atom. And if, as I contend, the Heisenberg Principle expresses an inherent limitation of our senses and their instrumental extensions rather than an objective law of matter, there can be no question of its refutation. So, a modern Archimedes may exclaim "Give me a submolecular tweeze and I will be able to build a living cell from scratch."

The only way man can handle matter is in bulk, chemical reactions not excepted. The same is true of the various remodeling techniques in which some free energy is used to trigger some changes in the chemical structure of cells. But given the immensity of a cell's chemical structure, the probability of obtaining a right "hit" is extremely small. The probability of getting a lethal hit is, on the contrary, very high. To use a topical term, we may say that every one of these atomic disturbances is very "unclean." One is thus obliged to use an immense number of cells if one wishes to have a fair chance of a right hit. This means that the cost and waste of

[50] C. H. Waddington, *The Nature of Life* (New York, 1962), pp. 41 f.

any of these procedures are so high that it would be antieconomical to apply them to organisms other than bacteria, insects, and some plants. From all we may judge, in the case of higher animals the cost would be prohibitive even for experimental purposes.[51]

Ever since the infusion of chemistry into biology, many biologists seem to have acquired a superiority complex reminiscent of that of the Classical physicists and, certainly, destined to the same fate. But for the time being, we are dangerously exposed to their exaggerated claims and exalted visions. That is why Medawar found it necessary to tell his lay audience that "it may surprise you to know that there is still no comprehensive theory of the improvement of livestock animals by selection."[52] This says a great deal about what we still cannot do in husbandry. But other consummate biologists have also recognized that progress has proceeded more slowly in biology than in physics and chemistry. Biology, they note, has been and still is tributary to these other sciences.[53] However, as the arguments presented here suggest, the difference between biology and physicochemistry is deeper than mere lag would justify.

The whole history of man's technological achievements points clearly in the same direction. In the world of inert matter we have mastered one source of energy after another. Also, our imagination has been able to ride over most technical obstacles. The result is that nowadays we can weave an ell of cloth a thousand times faster and better than in the times of the Pharaohs. The other day we threw a boomerang, as it were, around the moon with three men riding on it. Yet it takes us just about the same time as in ancient Egypt to grow a rice plant from a rice seed. The gestation period of domestic animals also has not been shortened by an iota. And little, if anything, has been achieved in shortening the time necessary to bring such an animal to maturity. Whatever progress we have made in husbandry, it has been the result of simply waiting for mutations to happen and imitating thereafter the work of natural selection. Naturally, the innovations in artifacts, being more impressive, have enslaved our imagination and, ipso facto, our thoughts on what man can achieve.

Timid though they were, the mechanical inventions of the seventeenth century did impress so highly the learned society that one writer after another came to hold that there is no limit to what man can do with the industrial arts. An effervescent industrial development (especially in England) close to areas where virgin lands were still available furthered the thought that the New Jerusalem was within reach if only there were enough hands around for the industrial activities. The problem of food

[51] For details of the preceding analysis, see Appendix G, below.

[52] P. B. Medawar, *The Future of Man* (New York, 1960), p. 62.

[53] Waddington, *Nature of Life*, p. 17.

production for a growing population came thus to be considered as implicitly solved by the industrial progress and, as a result, to be written off. William Petty, while viewing nature as the mother of wealth, insisted that "Fewness of people, is real poverty; and a Nation wherein are Eight Millions of people, are more then twice as rich as the same scope of Land wherein are but Four."[54]

We are indeed susceptible of intellectual arrogance when we come to appraise the power of man's inventions. An example as good as many others is the claim of Lewis H. Morgan: "Mankind are the only beings who may be said to have gained an absolute control over the production of food."[55] The position is part and parcel of the Marxist dogma that overpopulation can exist only in a relative sense, more precisely, that the production of food "can keep pace with [human] population whatever that might be."[56] Yet even Engels in quoting Morgan found it appropriate to insert "almost" to tone down "absolute." The evidence now before us—of a world which can produce automobiles, television sets, etc., at a greater speed than the increase in population but is simultaneously menaced by mass starvation—is disturbing. It is beyond question that, as some careful studies have shown, much of this menace is due to the inequality of the distribution of population in relation to the fertile land as well as to the misallocation of land uses. It is also beyond question that the world food production can still be increased by the dissemination of the most efficient methods of cultivation known to agronomy.[57] But viewing the matter in this way—as all these studies have apparently done—is glossing over the real problem.

The real problem has two dimensions which, although not strictly independent, must be kept separate and analyzed as such. Only one of these dimensions is acknowledged by the question, now topical, of *how large* a population could be fed properly in case the barriers listed above are removed and other favorable changes in diet and methods of production occur before A.D. 2000.[58] The ignored dimension is *how long* this

[54] Petty (note 9, above), I, 34. That the dearth of industrial labor sharpened the conflict of interests between the capitalists and the landlords which ended with the victory of the former—the abolition of feudal relations in agriculture—is a well-known fact. In my opinion, the same excess demand prompted the British economists to see in labor the only source of value, an idea which was aired by some even before Petty.

[55] Lewis H. Morgan, *Ancient Society, or Researches in the Lines of Human Progress from Savagery Through Barbarism and Civilization* (New York, 1878), p. 19. Quoted with addition in F. Engels, *The Origin of the Family, Private Property, and the State* (4th edn., New York, 1942), p. 19.

[56] For some old and recent statements to this effect, see Cépède *et al.*, *Population and Food*, pp. 64–66.

[57] Cf. Cépède *et al.*, pp. 441–461.

[58] According to the best projections, the world population will reach seven billions by the year 2000. See *World Population Prospects as Assessed in 1963*, United Nations, Population Studies, No. 41, 1966, pp. 134–137.

population could be so fed thereafter. This dimension is implicitly blotted out if one sides with the view of Jevons—and others quoted above—about the indefinite reproduction of the crop on the same piece of land.

Jonathan Swift, the merciless critic of William Petty's thesis on population,[59] was on the good road for his own time in maintaining that "whoever could make two ears of corn, or two blades of grass, to grow upon a spot of ground where only one grew before, would deserve better of mankind."[60] As the Entropy Law now teaches us, even to make *one* blade of grass grow on the same spot year after year on end would be a miracle! The fact that the decrease in the rate of solar energy that reaches the globe is imperceptible at our scale of Time and the predominant role this energy has in the agricultural production should not deter us from recognizing the crucial importance of the entropic degradation of the soil through continuous cultivation. To ascertain the degradation of the soil we need not observe nature over astronomically long periods of time. The early tillers who discovered the advantages of manuring knew only too well that raising a crop means to mine, in part, the soil.

It would be a mistake, however, to believe that the practice of manuring can defeat the Entropy Law and transform the production of food into a pendulum motion. For let us visualize a herd of buffaloes living in wilderness on a tract of grazing land. Even if their number would not increase, there would necessarily be a constant degradation of the soil. The low entropy on which life feeds includes not only the low entropy transmitted by the sun but that of the terrestrial environment as well. Otherwise, the paradise for living creatures would be in the sunny Sahara. The degradation of the soil with manuring is certainly slower than without it, so much slower that it may not strike us immediately. But this is no reason for ignoring this factor in a broader perspective of what has happened and what will happen in the future in the production of food. I have mentioned earlier the case of the Great Migration. Let me turn now to a recent, and as yet undiscerned, event which pertains to water buffaloes, oxen, horses, and manure.

The fact that labor is a negative factor in the enjoyment of life explains why thousands of years ago man sought to domesticate and use draft animals in agriculture and transportation. The substitution formula worked splendidly as long as there still was plenty of land to feed both the people and the animals without great exertion of the powers of the soil. As population grew and the scarcity of land began to make itself felt, crop rotation and manuring came to relieve the pressure for food. Ultimately,

[59] See *The Works of Jonathan Swift*, ed. Walter Scott (12 vols., Edinburgh, 1814), VII, 454–466.

[60] *Ibid.*, XII, 176.

the pinch reached the point when man came to realize that "horses eat people"—as a Romanian peasant saying wittily puts it.[61] In many parts of the world, the burden of the farmer became unbearable: on an insufficient size of land degraded through millenary use, he had to raise some food for himself and some for the town (through taxes) and also enough fodder for his animals. The scene is now set for the inevitable next act: the elimination of the draft animals as a source of draft power and manure.

The mechanization of agriculture, even if it had no influence on increasing the yield per acre, would have to go on in every part of the world. To arrive at a clear picture of this necessity we should observe, first, that the mechanical buffalo is made of iron ore and coal (primarily) and feeds on oil; second, that the manure of the departed water buffaloes must necessarily be replaced by chemical fertilizers. The consequence should be plain: since the power and the vivifying elements no longer come from the flow of sun's radiation through the draft animals, they must be obtained by an additional tapping of the stock of mineral resources in the earth's crust. This shift in low entropy from one source to another has an important bearing on the problem of how long a given population can be fed by this globe.

A thorough and well-planned mechanization of agriculture all over the world may possibly enable mankind to feed a population even greater than seven billions by A.D. 2000. The advantages of mechanization are undeniable but only from an opportunistic viewpoint. For, contrary to what some enthusiasts believe and preach, these advantages are not without a price. We can obtain them only by eating more quickly into the "capital" of low entropy with which our planet is endowed. That, indeed, is the price we have paid and still pay not only for the mechanization of agriculture but for every technical progress. Think, for instance, of the replacement of the wood plow by the iron plow centuries ago, of the replacement of charcoal by coal in smelting iron in the less remote past, and of the replacement of other natural materials by metals and synthetic products in the contemporary era.

In a broad perspective we may say that mankind disposes of two sources of wealth: first, the finite stock of mineral resources in the earth's crust which within certain limits we can decumulate into a flow almost at will, and second, a flow of solar radiation the rate of which is not subject to our control. In terms of low entropy, the stock of mineral resources is only a very small fraction of the solar energy received by the globe within a single

[61] Most interestingly, the same thought occurred to Sir Thomas More, *Utopia with the 'Dialogue of Comfort'* (London, 1913), p. 23: "your shepe that were wont to be so meke and tame, and so smal eaters, now [are] so great devowerers and so wylde, that they eate up, and swallow downe the very men them selfes."

year. More precisely, the highest estimate of terrestrial energy resources does not exceed the amount of free energy received from the sun during *four days*![62] In addition, the flow of the sun's radiation will continue with the same intensity (practically) for a long time to come. For these reasons and because the low entropy received from the sun cannot be converted into matter in bulk, it is not the sun's finite stock of energy that sets a limit to how long the human species may survive. Instead, it is the meager stock of the earth's resources that constitutes the crucial scarcity. Let S be this stock and r the average rate at which it may be decumulated. Clearly, $S = r \times t$, where t stands for the corresponding duration of the human species. This elementary formula shows that the quicker we decide to decumulate S, the shorter is t. Now, r may increase for two reasons. First, the population may increase. Second, for the same size of population we may speed up the decumulation of the natural resources for satisfying man-made wants, usually extravagant wants.

The conclusion is straightforward. If we stampede over details, we can say that every baby born now means one human life less in the future. But also every Cadillac produced at any time means fewer lives in the future. Up to this day, the price of technological progress has meant a shift from the more abundant source of low entropy—the solar radiation—to the less abundant one—the earth's mineral resources. True, without this progress some of these resources would not have come to have any economic value. But this point does not make the balance outlined here less pertinent. Population pressure and technological progress bring *ceteris paribus* the career of the human species nearer to its end only because both factors cause a speedier decumulation of its dowry. The sun will continue to shine on the earth, perhaps, almost as bright as today even after the extinction of mankind and will feed with low entropy other species, those with no ambition whatsoever. For we must not doubt that, man's nature being what it is, the destiny of the human species is to choose a truly great but brief, not a long and dull, career. "Civilization is *the economy of power* [low entropy]," as Justus von Liebig said long ago,[63] but the word economy must be understood as applying rather to the problems of the moment, not to the entire life span of mankind. Confronted, in the distant future, with the impending exhaustion of mineral resources (which caused Jevons to become alarmed about the coal reserves), mankind—one might try to reassure us—will retrace its steps. The thought ignores that, evolution being irrevocable, steps cannot be retraced in history.

Uncertainty being what it is, it would be a sign of arrogance to try to

[62] Eugene Ayres, "Power from the Sun," *Scientific American* CLXXXIII (August 1950), 16.
[63] Quoted in Jevons, *Coal Question*, p. 142.

predict the problems that may arise in the evolution of mankind and the manner man will deal with them. Yet I feel that the broad analysis of man's entropic conditions presented in this chapter delineates the material forces that have a very slow but continuous effect on that evolution. And forces such as these are, generally, more important than those which act with conspicuous speed. Every human being ages because of the entropic factors which begin to work at birth, nay, before it, and work slowly but cumulatively. They are, as any biologist would tell us, the most important element in man's biological life. Accidents, such as a death by pneumonia or by a fall in mountain climbing, may catch our eye more easily because in these cases the causes work their effect so much more quickly. However, the greater risk of death by accidental causes for an older person is the product of the slow-acting aging causes. By the same token, it is the degradation of man's dowry of low entropy as a result of his own ambitious activity that determines both what man can and cannot do. On the basis of this general picture, one may thus venture to assess some trends for the near future at least.

We may be pretty sure that there will be some reversal—not retraced steps—in the use of free energy. Already, the waterfalls—an energy produced indirectly by the sun's radiation—are being increasingly used as a source of free energy in the form of electric power. This trend is certain to become more accentuated. If the scattered efforts to use directly the solar radiation as a source of power succeed in making the idea operational, perhaps we shall not be as astounded as when we learned, in a macabre way, of man's harnessing the power of the atom. But in view of what I have said in this section, such a success would represent a far greater, because more lasting, benefit to mankind. For the same reasons, I believe that it is bound to come under the pressure of necessity.

Necessity also will cause some revision of our present impatience with the use of mineral resources in producing free energy and synthetic substitutes. The necessity derives from the increasing baneful effect of waste. As we have seen in the preceding pages, from the purely material viewpoint the economic process merely transforms low entropy into waste. The faster the economic process goes, the faster the noxious waste accumulates. For the earth as a whole there is no disposal process of waste. Baneful waste once produced is there to stay, unless we use some free energy to dispose of it in some way or other. We have long known this from the old practice of garbage collection. But recently other forms of waste began to interfere with our life and the cost of getting rid of them is no longer unimportant. There is a vicious circle in burning coal for industrial processes and then having to use more coal to produce the energy necessary to blow the smog away. There is a vicious circle in using detergents for economy of resources and labor and afterwards having to use costly procedures to restore to normal life lakes and river banks. At least, the industrial energy we derive or may derive from solar radiation does not produce by itself noxious waste. Automobiles driven by batteries charged by the sun's energy are cheaper both in terms of scarce low entropy and healthy conditions—a reason why I believe they must, sooner or later, come about.

Finally, let me say that, although the problem of feeding a population of seven or more billions by a reorganization of the world's agriculture from top to bottom may look solved on paper, its actual solution raises a truly staggering issue—how to organize such an immense mass of people first. We should not ignore the fact that the scale of political organization, too, is subject to limitations for the good reason that it cannot exist without a material scaffold. Nor should we ignore the fact that the thorough reorganization of agriculture as proposed requires that a fantastic amount of resources now allocated to the production of durable consumer goods be reallocated to the production of mechanical buffaloes and artificial manure. This reallocation, in turn, demands that the town should abdicate its traditional economic privileges. In view of the basis of these privileges and of the unholy human nature, such an abdication is well-nigh impossible. The present biological spasm of the human species—for spasm it is—is bound to have an impact on our future political organization. The shooting wars and the political upheavals that have studded the globe with an appalling frequency during recent history are only the first political symptoms of this spasm. But we have no reason to believe that the outcome—which is anybody's guess—would do away with the social conflict that, under one form or other, has been thus far the flywheel of political history.

4. From the Struggle for Entropy to Social Conflict. My reason for the last statement is that, like Marx, I believe that the social conflict is not a mere creation of man without any root in material human conditions. But unlike Marx, I consider that, precisely because the conflict has such a basis, it can be eliminated neither by man's decision to do so nor by the social evolution of mankind. The Marxist dogma in its comprehensive form has often been hailed as a new religion. In one respect, the thought is correct: like all religions, the dogma proclaims that there is an eternal state of bliss in man's future. The only difference is that Marxism promises such a state here on earth: once the means of production are socialized by the advent of Communism, that will be the end of all social change. As in Heaven, man will live forever thereafter without the sin of social hatred and struggle. This tenet seems to me to be as unscientific as any religion known to man. The end of the social conflict implies a radical change in man's nature, nay, in his biological nature.[64] More precisely, it requires that man should by some evolutionary reversal be degraded to the status of other animals—an utterly absurd eventuality. It may seem curious, but it actually is natural, that a consummate biologist, Alfred Lotka, put the finger on the crucial difference between man's entropic struggle and that of other living creatures.

All living beings, in their role as Maxwellian demons sorting low entropy for the purpose of enjoying and preserving their lives, use their biological organs. These organs vary from species to species, their form even from variety to variety, but they are characterized by the fact that *each individual is born with them.* Alfred Lotka calls them *endosomatic instruments.* If a few marginal exceptions are ignored, man is the only living being that uses in his activity also "organs" which are not part of his *biological constitution.* We economists call them capital equipment, but Lotka's term, *exosomatic instruments,* is more enlightening.[65] Indeed, this terminology emphasizes the fact that broadly interpreted the economic process is a continuation of the biological one. At the same time it pinpoints the *differentia specifica* between the two kinds of instruments which together form one genus. Broadly speaking, endosomatic evolution can be described as a progress of the entropic efficiency of life-bearing structures. The same applies to the exosomatic evolution of mankind. Exosomatic instruments enable man to obtain the same amount of low entropy with less expenditure of his own free energy than if he used only his endosomatic organs.[66]

As already explained, the struggle for life which we observe over the entire biological domain is a natural consequence of the Entropy Law. It goes on between species as well as between the individuals of the same species, but only in the case of the human species has the struggle taken also the form of a social conflict. To observe that social conflict is an outgrowth of the struggle of man with his environment is to recognize a fairly obvious fact, but not to explain it. And since the explanation is of particular import for any social scientist, I shall attempt to sketch one here.

A bird, to take a common illustration, flies after an insect with *its own* wings and catches it with *its own* bill, i.e., with endosomatic instruments which by nature are the bird's individual property. The same is certainly

[64] Cf. Chapter XI, Section 5, below.

[65] Alfred J. Lotka, "The Law of Evolution as a Maximal Principle," *Human Biology,* XVII (1945), 188.

[66] The question why the expenditure of man's own free energy, even if continuously replaced, should be accompanied by a feeling of unpleasantness is, I think, a moot question. But without this feeling, man probably would not have come to invent exosomatic instruments, to enslave other men, or to domesticate animals of burden.

true of the primitive exosomatic instruments used during the earliest phase of human organization, the primitive communism as Marx calls it. Then each familial clan lived by what *its own* bow and arrow could kill or *its own* fishing net could catch, and nothing stood in the way of all clan members' sharing the product more or less according to their basic needs.

But man's instincts, of workmanship and of idle curiosity, gradually devised exosomatic instruments capable of producing more than a familial clan needed. In addition, these new instruments, say, a large fishing boat or a flour mill, required more hands both for being constructed and for being operated than a single familial clan could provide.[67] It was at that time that production took the form of a *social* instead of a *clannish* activity.

Still more important is to observe that only then did the difference between exosomatic and endosomatic instruments become operative. Exosomatic instruments not being a natural, indissoluble property of the individual person, the advantage derived from their perfection became the basis of inequality between the various members of the human species as well as between different communities. Distribution of the communal income—income being understood as a composite coordinate of real income and leisure time[68]—thus turned into a social problem, the importance of which has never ceased to grow. And, as I shall presently submit, it will last as a center of social conflict as long as there will be any human society.

The perennial root of the social conflict over the distribution of income lies in the fact that our exosomatic evolution has turned production into a social undertaking. Socialization of the means of production, clearly, could not change this fact. Only if mankind returned to the situation where every family (or clan) is a self-sufficient economic unit would men cease to struggle over their anonymous share of the total income. But mankind could never reverse its exosomatic any more than its endosomatic evolution.

Nor does socialization of the means of production implicitly warrant—as Marx asserted—a rational solution of the distributive conflict. Our habitual views on the matter may find it hard to accept, but the fact is that *communal ownership of the means of production is, in all probability, the only regime compatible with any distributive pattern.* A most glaring example is provided by feudalism, for we must not forget that land passed into private ownership only with the dissolution of the feudal estates, when not only the serfs but also the former lords legally became private

[67] Cf. Karl Kautsky, *The Economic Doctrines of Karl Marx* (New York, 1936), pp. 8 ff.

[68] Cf. Section 2 of this chapter.

owners of land. Besides, it is becoming increasingly obvious that social ownership of means of production is compatible even with some individuals' having an income which for all practical purposes is limitless in some, if not all, directions.[69]

There is, however, another reason why the conflict between individuals over their share of the social income inevitably precipitates a class conflict in any society save primitive communism. Social production and its corollary, social organization, require a specific category of services without which they cannot possibly function. This category comprises the services of supervisors, coordinators, decision makers, legislators, preachers, teachers, newsmen, and so on. What distinguishes these services from those of a bricklayer, a weaver, or a mailman is that they do not possess an objective measure as the latter do. Labeling the former *unproductive* and the latter *productive*—as in the tradition of Adam Smith—is, however, a misleading way of differentiating between the two categories: production needs both.

Now, even if the entire social product were obtained only with the aid of services having an objective measure, the problem of the income distribution would be sufficiently baffling. But the fact that society needs also services which have no objective measure adds a new dimensional freedom to the patterns of distribution. Economists know this from their lack of success in finding a measure for entrepreneurship. This difficulty, however, does not matter practically; an entrepreneur is supposed (at least in principle) to be satisfied with receiving for his "unproductive" services the residual profit which may be a gain or a loss according to how well or how poorly inspired his venture has been. What does really matter is that there is absolutely no way of measuring objectively the other "unproductive" services. Organized society can hardly apply to these services the same rule of remuneration as for entrepreneurs. All these "managers" must be paid a contractual income, that is, an income established *before they are hired*. What is the proper level of income for services that do not produce a palpable result constitutes the perennial taproot of the social conflict in any organized society.

An intellect from another world, if ignorant of the political history of

[69] For a few glaring examples from some countries leaning heavily toward socialism: in Indonesia scores of luxurious villas have been built in the most attractive spots for the use of the president, who cannot visit them all during one year; in Bombay, scarce though the medical resources are all over India, the best equipped clinic has been earmarked by a 1963 law for the *exclusive* use of the families of the members of the local government and legislature. Expressions such as "the bāns of Communism" or "the barons of science," which are indigenous to some socialist countries of Eastern Europe where they enjoy a relatively large currency, tell a lot under the circumstances.

mankind and free from our intellectual biases, would certainly reason that those who perform unproductive services on this planet receive only an income at the discretion of the productive workers. In other words, it would expect mankind to live under a genuine dictatorship of the workers. The logic of that intellect should be obvious to any economist: given the impossibility for a person performing unproductive service to show a tangible result of his activity, the class as a whole must necessarily be in an inferior bargaining position. One can easily understand the difficult position of such a person in claiming that "certificate from society to the effect that he has done *such and such a quantity of work*," on the basis of which, as Lenin imagined, every member of the community should recieve "from the public warehouses . . . a corresponding quantity of products."[70] In view of his predicament, a performer of unproductive services should be quite happy if the prevailing rules would be those advocated by Marx and Lenin: that the productive workers should "hire their own technicians, managers, bookkeepers, and pay them *all*, as, indeed, every 'state' official, with the usual workers' wage."[71]

But should the same intellect know also the human nature and all its biases and frailties, it would immediately see that the weak position of the class which performs unproductive services can be turned—as it has been—into a most formidable and everlasting weapon in the social conflict. Indeed, only what does not have a tangible measure can easily be exaggerated in importance. This is the basic reason why the privileged elite in every society has always consisted—and, I submit, will always consist—of members who perform unproductive services under one form or another. Whatever the title under which this elite may receive its share, this share will never be that of worker's wage—even if, as is possible, it may be called by that name.

Pareto explained how every elite is overthrown by a jealous minority which stirs the masses by denouncing the abuses of the establishment and finally replaces it.[72] Elites, as he said, circulate. Naturally, their names and the rationalizations of their privileges change. But it is important to note also that each elite inspires a new socio-political mythology by which the new situation is interpreted for the occasion. Yet the same leitmotiv runs through all these self-glorifications: "where would the people be if it were not for our services?" In ancient Egypt, the elite of high priests claimed to help the welfare of the people by reading the future in the stars; the consuls and the generals of the Roman empire boasted of furthering the cause of progress by extending *Pax Romana* over the rest of the world;

[70] V. I. Lenin, *State and Revolution* (New York, 1932), p. 76. My italics.
[71] *Ibid.*, p. 43 and *passim*.
[72] Vilfredo Pareto, *Les systèmes socialistes* (2 vols., 2nd edn., Paris, 1926), I, 30 ff.

later, each feudal baron presented himself as the defender of his subjects against the neighboring barons; more recently, the captains of industry and finance claimed that it was they who provided the working masses with a livelihood. The now rising elite seems to say that without its services people could not prosper economically. Quite recently and in unmistakable terms, a distinguished economist set a far stronger claim for the intelligentsia: "Since it is their role to interpret values in all fields of culture, the intellectuals are very well placed for identifying the aspirations that express the deepest trends in social feeling."[73] Ergo, they must take over the control of everything.

Undoubtedly, there is a large dose of truth in every one of these claims in relation to its own epoch. Every elite performs a useful job the nature of which derives from the exosomatic evolution of mankind and is continually changed by it. Even the reading of the stars, for instance, was extremely useful for regulating the agricultural activities in antiquity. But the fact that every elite performs services which do not produce a palpable, measurable result leads not only to economic privileges, as I have argued above, but also to abuses of all kinds. The political power of any ruling elite offers the elite the possibility of extolling the value of its services in the eyes of the masses and thus making any increase in its privileges appear "logical." Moreover, where services do not produce a palpable result featherbedding grows by itself simply because it cannot be demonstrated in any objective manner. The economic implications of the abuses inherent to an elite retained the attention of Adam Smith, who with his characteristic meticulosity described those which prevailed in his own time.[74] Later, in the *Communist Manifesto,* Marx and Engels went further and admitted that all social movements until then (1848) have been accomplished by minorities for the profit of minorities.[75] They, of course, believed and preached that the Communist revolution will be an exception to this rule. By now, we know that it is not: a new privileged class is steadily crystallizing itself under every Communist regime.[76] History has not yet disproved Pareto's thesis of the perennial circulation of the elites. And if the argument of this section is correct, only in the late twilight of the human species, when human society will very likely disintegrate into

[73] Celso Furtado, *Diagnosis of the Brazilian Crisis* (Berkeley, 1965), p. 37.

[74] Adam Smith, *The Wealth of Nations,* ed. E. Cannan (2 vols., 5th edn., London, 1930), I, 324–326.

[75] *The Communist Manifesto of Karl Marx and Friedrich Engels,* ed. D. Ryazanoff (London, 1930), p. 40.

[76] It may be superfluous to mention, in support of the above statements, Milovan Djilas' classic *The New Class: An Analysis of the Communist System* (New York, 1957). But one passage (p. 39) is worth citing: "The new class may be said to be made up of those who have special privileges and economic preference because of the administrative monopoly they hold." See also note 69 above.

small packs of humans, will the social factors which produce the circulation of elites fade away, too.

It is important also to know where elites arise from. Georges Sorel's opinion that every revolution means "the replacement of some intellectuals by other intellectuals"[77] is certainly off the mark. It does nonetheless open our eyes to one fact. From the beginning of civilization every elite has included some literati, in the strict sense of the word; in the broadest sense, it has consisted only of literati. Briefly, no elite has ever consisted only of persons performing productive services, whether workers or peasants. Besides, whenever such people have been included in a revolutionary committee, it has been only for display purposes. As a rule, those that had once been peasants or workers were no longer so at the time. In relation to its own epoch, every elite both before coming into power and thereafter consisted of people with enough general education to be in a position to claim that they could manage the affairs of the community with superior efficiency. In fact, a large majority of every elite has always been capable of doing so.

It should be obvious then why every elite has emerged from and has remained associated with the town community. The countryside is hardly the place for the development of those arts which, as Xenophon said, are sustained by agriculture. The progress of these arts requires the commercial and intellectual intercourse that only a busy place such as the town can provide. So, no sooner did agriculture reach the point where it could feed more souls than it needed on the fields than the other arts abandoned the countryside to found quarters of their own. The power-motive of society—to use one of Marx's expressions—thus concentrated into town has never since ceased to maintain from there a firm hold on the rural community in spite of being tributary to it for the means of biological existence.

Even during the early Middle Ages—a period that may come to the reader's mind at this point—the old cities of the fallen Roman empire retained their ascendency and others kept growing in step with the political power. Also, the lower spearheads of the feudal elite lived in some important burg of their fiefs. What prominent member of any elite, of any epoch, would have preferred or would prefer to live permanently in a rural hamlet? That would be a technical impossibility, to begin with.

"The unloving separation between country and town life," which Ruskin denounced as "a modern barbarism,"[78] is, instead, the natural consequence of the exosomatic evolution of mankind and of the difference this evolution has created between the process in agriculture and the

[77] Cited in Vilfredo Pareto, *Manuel d'économie politique* (2nd edn., Paris, 1927), p. 474. My translation.

[78] Preface to *The Economist of Xenophon*, p. xii.

process in industry. Marx was thus inexact in denouncing the bourgeoisie for having "subjected the countryside to the rule of the town."[79] This subjection goes back to the beginning of man's civilization through the development of industrial arts as a separate activity. Yet on another occasion Marx noted in passing, "It may be said, that the whole economical history of society is summed up in the movement of this antithesis"—the opposition between the industrial town and the agricultural countryside.[80] It must have been a slip of the pen, for although he added "we pass it over, however, for the present," he never came back to it. Naturally, to do so would have compelled him to admit that there is in organized society a line of conflict which is not only left out by the call "Proletarians of all lands, unite!" but also accentuated by it.[81] The issue, perhaps, prompted Engels to preach that the Communist state will erect "palatial dwellings . . . where communities of citizens shall live together for the carrying on of industry and agriculture."[82] Curiously, it was an "utopian" socialist who, long ago, struck the right nail on the head: "it is cheap food that maintains a low wage rate at the cost of the suffering of the [peasant] farmer."[83] And nowadays nobody doubts any more that, even in the countries where the town–countryside conflict is greatly attenuated by some special conditions, the average personal income of the farmer is lower not only than that in urban activities as a whole but also than that of the industrial workers.

To change this situation would require that the privileged elites divorce their own interests from those of the urban population. Such a divorce, however, is out of the question and the reason lies in the hierarchy of human wants. The enjoyment of a higher and higher income necessarily implies consuming more and also newer industrial goods—hence the interest of all elites in promoting the industrial arts which in turn requires "cheap bread."[84] All this is not alien to the present infatuation with the idea that industrialization per se automatically brings about economic development. The magic feat of the demon, of which I spoke in the preced-

[79] *The Communist Manifesto*, p. 31.

[80] Marx, *Capital*, I, 387.

[81] As it soon became apparent, the slogan actually meant "unite, first, against the capitalists, ultimately, against the peasants." See my paper cited in note 34 above, pp. 364–367.

[82] F. Engels, "Principles of Communism," reprinted in *The Communist Manifesto*, ed. Ryazanoff, p. 332.

[83] J. C. L. Simonde de Sismondi, *Nouveaux principes d'économie politique* (2 vols., Paris, 1819), I, 346. See also Max Weber, "The Relations of the Rural Community to Other Branches of Social Science," *International Congress of Arts and Science* (St. Louis, 1904), VII, 727.

[84] One piece of recent history is highly instructive in this respect. The Agrarian political parties of Eastern Europe failed to rally the necessary support of the educated class precisely because their platform, at bottom, called for "dear bread" and fewer luxuries for the town.

ing section, would reveal not only that current economic plans lead to an excess capacity of heavy industry but also that many of them aim at providing lavish luxuries rather than wage goods and, still less, at abating hunger. But we need not conjure a demon in order to convince ourselves of this bias: some cases speak amply for themselves. In some countries—such as Egypt or India, for example—where the food problem cries for immediate action, an impressive proportion of resources has been nonetheless invested in the production of consumer goods inaccessible to rural masses, to urban masses as well. There is a foolish extravagance, if one pauses to think about the fact, in pushing persistently the industries of automobiles, refrigerators, television sets, and other similar consumer goods, in countries whose annual income per capita amounts to a couple of hundred dollars.[85] The economic experts who defend inflation as the only sensible means for economic development in Latin America completely overlook one important element, namely, that the effect of the prescription is to increase almost exclusively the income of the upper classes and, consequently, to push continuously the growth of luxury goods industries. Development by inflation not only sharpens the social conflict—which is public knowledge—but also creates a structural lock which is self-aggravating.[86] At the national level extravagance does not make more economic sense than at the level of an individual; actually, it is much worse.

At the end of a highly enlightening essay, Gerschenkron expressed the hope that "in drafting the maps of their own industrial progress [the underdeveloped countries] will be eager to select those paths along which they will be able to keep down the cost and to increase the yield in terms of *human welfare and human happiness*."[87] Many underdeveloped countries are far from fulfilling this hope. Their economic plans and policies which claim for themselves the virtue of bringing economic progress through industrialization are, more often than not, rationalizations of the ulterior motives of the present elite. Inflation in Latin America—an economic expert claims—answers "the aspiration of the masses to improve their

[85] In problems of this sort we usually overlook the fact that even this meager average income provides a rosy picture of the actual situation. In a strongly skew distribution—as the income distribution is likely to be in these countries—the arithmetic mean leaves below it a very large majority of the population. It is this statistical feature that makes it so easy to stir the masses by the promise to sack the rich.

[86] See my articles "O Estrangulamento: Inflação Estrutural e o Crescimento Econômico," *Revista Brasileira de Economia* XXII (March 1968), 5–14, and "Structural Inflation-Lock and Balanced Growth," in *Économie mathématique et Économétrie* (Cahiers de l'I.S.E.A., Paris), IV (1970), 557–605.

[87] A. Gerschenkron, *Economic Backwardness in Historical Perspective* (Cambridge, Mass., 1962), p. 51. My italics.

standard of consumption";[88] in fact, it answers the aspirations of the upper classes for a still more luxurious living. The same lip service to the welfare of the masses conceals the aspirations of the same classes in many a planned economy.

Schumpeter, in one of his clinching lessons, observed that during the era of Queen Elizabeth of England only the queen, probably, could afford silk stockings: nowadays, any factory girl in Great Britain can afford them.[89] The lesson is that without an achievement of this sort economic development would be just an empty word. Some are now inclined to read into the lesson that all we need to do in order to develop an economy is to build silk stocking or automobile factories (even if they can cater only to the internal market). But the economic development of Great Britain did not come about by the expansion of the silk stockings industry. Before the British factory girls could afford silk stockings, the working class of that country was able to satisfy progressively some of the other, more basic, wants. Actually, economic history bears out fully the contention that the broadening of the industry or the trade of wage goods is not a purely ethical requisite of economic development, but an organic condition of it. So, there is no economic sanity in the idea, which in a somewhat different gist goes back to Mandeville's *Fable of the Bees*, that the luxury goods industry alone provides the ignition power for economic development. And if actually followed by policy makers—as is now often the case—the idea is bound to bring the class conflict nearer to the boiling point.

By the preceding observations I do not mean to say that no industry or trade should concern itself with luxury goods. Such a thought would be utopian in view of the fact that man's exosomatic evolution has created the everlasting necessity of a mankind divided into supervisors and supervised, into directors and directed, into leaders and led. The existence of elites, in turn, makes the production of luxury consumer goods inevitable —a simple consequence of the fact that, by definition, such goods satisfy wants that do not come into play before a person's income is well above the average income of the community. In a small nutshell, that is all that the class conflict is about.

The class conflict, therefore, will not be choked forever if one of its phases—say, that where the captains of industry, commerce, and banking claim their income in the name of private property—is dissolved. Nor is there any reason to justify the belief that social and political evolution will come to an end with the next system, whatever this system may be.

[88] Roberto de Oliveira Campos, "Inflation and Balanced Growth," in *Economic Development for Latin America*, Proceedings of a Conference held by the International Economic Association, ed. H. S. Ellis (London, 1962), p. 82.

[89] Joseph A. Schumpeter, *Capitalism, Socialism, and Democracy* (2nd edn., New York, 1947), p.67.

CHAPTER XI *The Economic Science: Some General Conclusions*

1. The Boundaries of the Economic Process. Controversy has been, with a varying degree of relative importance, a continuous stimulus in all spheres of intellectual endeavor, from literary criticism to pure physics. The development of economic thought, in particular, has been dependent on controversy to an extent that may seem exasperating to the uninitiated. It is nevertheless true that the doctrinaire spirit in which some fundamental issues have been approached has harmed the progress of our science. The most eloquent example of this drawback is the controversy over the boundaries of economics or, what comes to the same thing, over the boundaries of the economic process.

The problem was implicitly raised first by the German historical school, but it caused practically no stir until Marx and Engels set forth their doctrine of historical materialism. From that moment on, the proposition that constitutes the first pillar of that doctrine has been the subject of a sustained and misdirected controversy. This proposition is that the economic process is not an isolated system. The non-Marxist economists apparently believe that by proving the existence of some natural boundaries for the economic process they will implicitly expose the absurdity of historical materialism and, hence, its corollary: scientific socialism. However, whatever one may have to say about the other pillars of Marxism, one can hardly think of a plainer truth than that the economic process is not an isolated system. On the other hand, equally plain is the necessity of delimiting this process in some way: otherwise, there would be no sense at all in speaking of the economic process.

The problem is related to a point which I have endeavored to establish in the course of this book, namely, that the boundaries of actual objects and, especially, events are dialectical penumbras. Precisely because it is impossible to say, for example, where the chemical process ends and where the biological one begins, even natural sciences do not have rigidly fixed and sharply drawn frontiers. There is no reason for economics to constitute an exception in this respect. On the contrary, everything tends to show that the economic domain is surrounded by a dialectical penumbra far wider than that of any natural science.

Within this wide penumbra—as every sophomore knows from the famous riddle of what happens to the national income if a bachelor marries his housekeeper—the economic intertwines with the social and the political. How could we otherwise account for the economic stagnation during the Middle Ages in Europe which spanned a full millennium? How could we otherwise account for the wars fought between the European nations over the control of foreign markets and natural resources as well as for the technological changes induced by this struggle? Or, how could we account for the tremendous difference in economic development between North America and Latin America—given that natural resources are equally abundant in both places—if not by the difference in social and political factors?

Malthus, we remember, argued that there is also an interconnection between the biological growth of the human species and the economic process. Economists in general have rejected his doctrine because until very recently they have failed to see that, in spite of his unfortunate choice of expressing it, Malthus was in essence right. This can be immediately seen from our entropic analysis of the economic process. The fact that biological and economic factors may overlap and interact in some surprising ways, though well established, is little known among economists.

In the past, when in many parts of the world communities lived for centuries without major social upheaval, so that class segregation had time to work out its genetical effect almost to the full, it was a common feature of the upper classes—noticed even by James Cook among some Pacific populations—to have more refined physical features than the others. The phenomenon was explained by the British anatomist W. Lawrence long before the knowledge of heredity advanced from the purely empirical stage. He attributed it to the power of men in the upper classes to attract in marriage the more beautiful women of the land. A similar thesis was advanced later by Francis Galton in his celebrated *Hereditary Genius* (1869). Using complete genealogical data, Galton showed how the desire for wealth—certainly, an economic factor—contributed to the biological extinction of twelve out of the thirty-one original English peerages. He found that peers more often than not married wealthy heiresses and thus introduced the gene of low fertility in their blood lines. Some forty years after Galton's discovery, J. A. Cobb pointed out that the phenomenon is far more general. In a society where personal wealth and social rank are highly correlated—as is the case under the regime of private ownership—the gene of low fertility tends to spread among the rich, and that of high fertility among the poor. On the whole, the family with very few children climbs up the social ladder, and that with more than the average number of offspring descends it. Besides, since the rich usually marry the rich, the poor cannot marry but the poor. The rich thus become richer and the poor poorer because of a little-suspected interplay of economic and biological factors.[1]

The problem of delimiting the sphere of economics, even in a rough way, is therefore full of thorns. In any case, it is not as simple as Pareto urges us to believe through his argument that just as geometry ignores chemistry so can economics ignore by abstraction *homo ethicus*, *homo religiosus*, and all other *homines*.[2] But Pareto is not alone in maintaining that the economic process has definite natural limits. The same position characterizes the school of thought which has followed the attractive paths opened by the early mathematical marginalists and which has come to be commonly referred to as standard economics. A more recent formulation of this position is that the scope of economics is confined to the study of how *given* means are applied to satisfy *given* ends.[3] In more specific terms: at any given instant of time the means at the disposal of every individual as well as his ends over the future are given; given also are the ways (technical and social) in which these means can be used directly or indirectly for the satisfaction of the given ends jointly or severally; the essential object of economics is to determine the allocation of the given means towards the optimal satisfaction of the given ends. It is thus that economics is reduced to "the mechanics of utility and self-interest." Indeed, any system that involves a conservation principle

[1] W. Lawrence, *Lectures on Physiology, Zoology, and the Natural History of Man* (Salem, 1822), pp. 389 f; Francis Galton, *Hereditary Genius* (London, 1869), pp. 132–140. For a masterly discussion of this category of problems—highly instructive for any student of economics—see R. A. Fisher, *The Genetical Theory of Natural Selection* (Oxford, 1930), chaps. x and xi. Also J. B. S. Haldane, *Heredity and Politics* (New York, 1938), pp. 118 ff. Among economists, apparently only A. C. Pigou became aware of the possible interactions between the economic and the biological. See "Eugenics and Some Wage Problems" in his *Essays in Applied Economics* (London, 1924), pp. 80–91.

[2] Vilfredo Pareto, *Manuel d'économie politique* (Paris, 1927), p. 18.

[3] By far the most articulate defense of this restrictive viewpoint is due to Lionel Robbins, *An Essay on the Nature and Significance of Economic Science* (2nd edn., London, 1948), p. 46 and *passim*.

(given means) and a maximization rule (optimal satisfaction) is a mechanical analogue.[4]

Now, the economic nature of allocating given means for the optimal satisfaction of given ends cannot possibly be denied. In its abstract form, such allocation reflects a permanent preoccupation of every individual. Nor can one deny that frequently the problem presents itself in concrete terms and is susceptible of a numerical solution because all necessary data are actually *given*. The recent results achieved in this direction following the pioneering work of T. C. Koopmans deserve the highest praise. Yet, highly valuable though these results are, the new field of engineering (or managerial) economics does not cover the whole economic process any more than husbandry exhausts all that is relevant in the biological domain.

Let me hasten to add that the usual denunciation of standard economics on the sole ground that it treats of "imaginary individuals coming to imaginary markets with ready-made scales of bid and offer prices"[5] is patently inept. Abstraction, even if it ignores Change, is "no exclusive *privilegium odiosum*" of the economic science,[6] for abstraction is the most valuable ladder of any science. In social sciences, as Marx forcefully argued, it is all the more indispensable since there "the force of abstraction" must compensate for the impossibility of using microscopes or chemical reactions.[7] However, the task of science is not to climb up the easiest ladder and remain there forever distilling and redistilling the same pure stuff. Standard economics, by opposing any suggestion that the economic process may consist of something more than a jigsaw puzzle with all its elements given, has identified itself with dogmatism. And this is a *privilegium odiosum* that has dwarfed the understanding of the economic process wherever it has been exercised.

So it is for its dogmatism, not for its use of abstraction, that standard economics is open to valid criticism. Casual observation of what happens in the sphere of economic organizations, or between these organizations and individuals, suffices to reveal phenomena that do not consist of *tâtonnement* with given means towards given ends according to given rules. They show beyond any doubt that in all societies the typical

[4] Cf. Henri Poincaré, *The Foundations of Science* (Lancaster, Pa., 1946), p. 180. For a detailed examination of the strict analogy between the Pareto-Walras system and the Lagrange equations see V. Pareto, "Considerazioni sui principii fondamentali dell' economia politica pura," *Giornale degli economisti*, IV (1892), 409 ff.

[5] Wesley C. Mitchell, "Quantitative Analysis in Economic Theory," *American Economic Review*, XV (1925), 5.

[6] Joseph A. Schumpeter, *Essays*, ed. R. V. Clemence (Cambridge, Mass., 1951), p. 87.

[7] Preface to the first edition of Karl Marx, *Capital* (3 vols., Chicago, 1932–1933), I, 12.

individual continually pursues also an end ignored by the standard framework: the increase of what he can claim as his income according to his current position and distributive norms. It is the pursuit of this end that makes the individual a true agent of the economic process.

Two are the methods by which he can pursue this particular end. First, he may seek ways by which to improve *qualitatively* the means he already possesses. Secondly, he may seek to increase his personal share of the stock or flow of social means, which is tantamount to changing the prevailing distributive relations. It is because even in a socialist society the individual activity is in the long run directed also towards these aims that new means are continually invented, new economic wants created, and new distributive rules introduced.[8]

The question is why a science interested in *economic* means, ends, and distribution should dogmatically refuse to study also the process by which new *economic* means, new *economic* ends, and new *economic* relations are created. Perhaps one would answer that what is to be included in the scope of any special science is a matter of convention or of division of labor. To return to an earlier parallel, is it not true that husbandry constitutes a proper scientific endeavor and a very useful discipline despite the fact that it does not concern itself with biological evolution? There is, however, a very important reason why economics cannot follow the example of husbandry.

The reason is that the evolutionary pace of the economic "species"—that is, of means, ends, and relations—is far more rapid than that of the biological species. The economic "species" are too short-lived for an economic husbandry to offer a relevant picture of the economic reality. Evolutionary elements predominate in every concrete economic phenomenon of some significance—to a greater extent than even in biology.[9] If our scientific net lets these elements slip through it, we are left only with a shadow of the concrete phenomenon. No doubt, a navigator does not need to know the evolution of the seas; actual geography, as Pareto argued, suffices him.[10] But my point is that Pareto's illustration would be of no avail if the earth's geography evolved as rapidly as that of the economic world. It is beyond dispute, therefore, that the sin of standard

[8] The preceding remarks should be compared with those of Frank H. Knight, *Ethics of Competition* (New York, 1935), pp. 58 ff. However, I am not sure whether the particular activity described above coincides with what Knight calls "the institution of sport."

[9] This is not the same thing as saying that the economic material is exposed to numerous disturbances, as Joseph A. Schumpeter says in *Business Cycles* (2 vols., New York, 1939), I, 33. From a mechanistic viewpoint, every concrete phenomenon appears subject to innumerable disturbances.

[10] Pareto, *Manuel*, p. 101.

economics is the fallacy of misplaced concreteness, by which Whitehead understands "neglecting the degree of abstraction involved when an actual entity is considered merely so far as it exemplifies certain [pre-selected] categories of thought."[11]

In retrospect, it appears natural that denunciations of the sterility of the standard armamentarium should have come from men such as Marx and Veblen, who were more interested in distributive relations than in the efficient allocations of means: the fallacy of misplaced concreteness is more conspicuous in the former than in the latter problem. However, although the disciples of Marx or Veblen like to claim the entire glory for their own master,[12] the shortcomings of the static analysis originated by Ricardo were pointed out long before Marx. J. B. Say, for example, in an 1821 letter warned Ricardo's contemporaries that future generations would laugh at the terror with which, because of the Ricardian analysis, they were viewing the effect of technical progress upon the fate of industrial workers.[13] It is nevertheless true that lessons, perhaps the only substantial ones, on how to transcend the static framework effectively have come from Marx, Veblen, and Schumpeter.[14]

One should not fail, however, to recognize also the unique endeavor of Marshall to instill some life into the analytical skeleton of standard economics. Schumpeter, with his tongue in cheek—as it often was—said that Marshall "wanted—strange ambition!—to be 'read by businessmen.'"[15] No doubt *it was* a strange ambition after all for Marshall to insist upon respect for relevance instead of succumbing to the temper of his age. To cite only one from the many eloquent examples: it was Marshall who showed in the most incontrovertible way that even such a basic concept as the supply schedule of an "increasing returns" industry slips through the analytical mesh because "increasing returns" is an essentially evolutionary phenomenon, necessarily irreversible and perhaps irrevocable as well.[16] Marshall expressed his respect for analysis in so many words, but his "thought ran in terms of evolutionary change—in terms of an organic,

[11] Alfred North Whitehead, *Process and Reality: An Essay in Cosmology* (New York, 1929), p. 11.

[12] E.g., Karl Korsch, *Karl Marx* (London, 1938), p. 156; John S. Gambs, *Beyond Supply and Demand* (New York, 1946), p. 10.

[13] Jean-Baptiste Say, *Letters to Mr. Malthus* (New York, 1967), p. 70.

[14] As all economists know, only Schumpeter formed no school. I wish to observe, however, that the American Institutionalists, though hailing Veblen as their prophet, have inherited little from him besides an aggressive scorn for "theory." Be this as it may, Paul T. Homan, in "An Appraisal of Institutional Economics," *American Economic Review*, XXII (1932), 10–17, has completely missed the issue raised by Veblen.

[15] Joseph A. Schumpeter, *Ten Great Economists* (New York, 1951), p. 97.

[16] Alfred Marshall, *Principles of Economics* (8th edn., New York, 1924), p. 808. See also Schumpeter, *Essays*, p. 53*n*2, and Knight, *Ethics of Competition*, pp. 166 f.

irreversible process."[17] But Schumpeter went on to say that Marshall's "vision of the economic process, his methods, his results, are no longer ours."[18] Coming from an economist in whose work evolution occupied a prominent position, this last remark cannot be taken for anything but a veiled lament. Great minds—such as Lionel Robbins—who ultimately awake from "dogmatic slumber,"[19] are, unfortunately, rare exceptions.

As to where the boundary of the economic process should be appropriately set, I know of no better answer than Marshall's definition of economics as the "study of mankind in the ordinary business of life,"[20] provided one does not insist on an arithmomorphic interpretation of every term. The examples and the observations presented in this section should suffice to trace out the dialectical penumbra of this science.

2. *Why Is Economics Not a Theoretical Science?* Everyone uses "theory" in multifarious senses. To wit, in one place Schumpeter uses it to mean a "box" of analytical tools.[21] But in discriminating usage, the term generally denotes a logical edifice. Or, as I have explicitly put it (Chapter I, Section 4, above), theory means a logical filing of *all* extant knowledge in some particular domain such that every known proposition be either contained in the logical foundation or deducible from it. That such a filing has the unique merit of affording *comprehensibility* is a leitmotiv inherited from Aristotle. However, hardly any attention has been paid to the fact that there can be no comprehensibility without the *compressibility* of extant knowledge into only a relatively few ω-propositions. If our knowledge of a certain domain is not compressible, i.e., if its logical filing results in a very great number of ω-propositions, Aristotelian comprehensibility does not obtain. I have illustrated this point in connection with chemistry where, because of the frequency of novelty by combination, any logical foundation must contain far more numerous propositions than the β-class. For this very reason a logical foundation of chemistry would have to be continuously "under construction." A chemical theory, clearly, would serve no purpose whatsoever.[22] The same applies with even greater force to any science concerned with evolution, for the scene of evolution is dominated by novelty.

After what I have said about the scope of economics the answer to the question at the head of this section is so obvious that to dwell further on it might seem superfluous. But since the view that the propositions

[17] Schumpeter, *Ten Great Economists*, p. 101.

[18] *Ibid.*, p. 92.

[19] As Lionel Robbins admits for himself in his *The Economic Problem in Peace and War* (London, 1947), pp. 67 f.

[20] Marshall, *Principles*, p. 1.

[21] Schumpeter, *Essays*, p. 227.

[22] See Chapter V, Section 1, above.

about the economic process can be arranged into a theory is widely shared, it appears instructive to analyze briefly the most salient arguments advanced in its support.

The oldest, and also the most commonly held, argument is that economics must necessarily be a theoretical science because every economic phenomenon follows logically from a handful of elementary principles. The idea goes back to the Classical school, which taught that all economic phenomena are grounded in "the desire for wealth" which characterizes any "sane individual," and are governed by only two general laws. The first is that "a greater gain is preferred to a smaller"; the second is the propensity to obtain "the greatest quantity of wealth with the least labor and self-denial."[23] To these general laws the marginalists added two principles of more substantial content, the principles of decreasing marginal utility and decreasing returns. But economists have continued to argue that the fundamentals of economics are known to us immediately by intuition, and hence their truth can be trusted "more confidently and certainly than . . . any statement about any concrete physical fact or event."[24] Still more important is the claim that because of this special property of its fundamental laws economics is the deductive science *par excellence*. Consequently, all economic propositions are valid in any institutional setting.[25]

No doubt, one can hardly think of a more obvious tautology than the principle "*Each individual acts as he desires.*"[26] Or as the same idea is expressed in modern jargon, everybody acts so as to maximize his satisfaction in every given set of circumstances. Clearly, it is as absurd to think of an individual who prefers being less happy as to imagine a quadrangle with five sides. A life of material austerity and self-negation still represents the greatest happiness for him who has chosen to be a monk. And absolutely nobody can prove that a monk is less happy than the rich *bon vivant* who enjoys all the riches and frivolities in the world. On the other hand, to compare the principle of maximum satisfaction with "any statement about any *concrete* physical fact" is an idle proposal, unless "satisfaction" too is more concretely described.

The last requirement is essential. Even standard theory could not ignore it: its theorectical edifice was not built upon a general and vague concept

[23] John Stuart Mill, *A System of Logic* (8th edn., New York, 1874), pp. 623 ff; Knight, *Ethics of Competition*, pp. 135 ff.

[24] Frank H. Knight, *On the History and Method of Economics* (Chicago, 1956), p. 164. Also, W. Stanley Jevons, *The Theory of Political Economy* (4th edn., London, 1924), p. 18.

[25] Cf. Jevons, *Theory*, p. 19; Knight, *Ethics of Competition*, pp. 137 f and *passim*.

[26] Irving Fisher, *Mathematical Investigations in the Theory of Value and Prices* (New Haven, 1925), p. 11; Pareto, *Manuel*, p. 62.

of satisfaction, but on the specific proposition that *only those goods and services an individual can enjoy personally influence his satisfaction.* Accordingly, in standard theory ophelimity is a function only of the quantities of such goods and services.

This particular formula—as I argued elsewhere[27]—reflects an institutional trait proper (and, perhaps, specific as well) to the large urban communities of industrialized societies. The same is true of another cornerstone of standard theory, namely, the proposition that, for a seller, "gain" is measured solely by money-profit. But—to recall Marx's protest —"the bourgeois reason is [not] the normal human reason."[28] As Marshall carefully pointed out, it is not the general reason even in the bourgeois society.[29] Still less can we expect it to be valid in all institutional settings. Actually, in peasant communities the happiness of the individual depends not only on the quantities of goods and services at his disposal but also on other social variables, and gain depends on other factors besides money-profit.

The statement that the fundamental principles of economics are universally valid, therefore, may be true only as their *form* is concerned. Their *content*, however, is determined by the institutional setting. And without this institutional content, the principles are nothing but "empty boxes," from which we can obtain only empty generalities. This is not to say that standard theory operates with "empty boxes." On the contrary, as we have seen, those boxes are filled with an institutional content distilled from the cultural patterns of a bourgeois society. They may be only partly filled—as is certainly the case. Indeed, many traits of such societies have been left out because they were not quite ripe at the time the foundations of standard theory were laid, others because they cannot be fitted into the arithmomorphic structure a theory necessarily has.[30]

Let me repeat here a point made in the paper entitled "Economic Theory and Agrarian Economics" (1960), reprinted in my *Analytical Economics*. It is precisely because the boxes of standard theory were already filled with a specific institutional content that this theory was rejected by the students of the economic process in noncapitalist settings.

[27] "Economic Theory and Agrarian Economics" (1960), Section III (2), reprinted in AE.

[28] Karl Marx, *A Contribution to the Critique of Political Economy* (Chicago, 1904), p. 93.

[29] Marshall, *Principles*, pp. 762 ff.

[30] The reader should have no difficulty in finding the reason why the preceding conclusions differ fundamentally from those of some well-known discussions of the same topic, such as that of Knight, *Ethics of Competition*, pp. 135 ff, or J. H. Clapham, "Of Empty Economic Boxes," *Economic Journal*, XXXII (1922), 305–314. These authors use "content" in its Paretoan sense, meaning the ensemble of all "standard" ophelimity and production functions.

The most salient examples are those of the historical school in Germany and of *Narodnikism* in Russia. Significant though the point is, it has received not more than casual attention. Marshall is among the few to reproach the standard economists for having worked out "their theories on the tacit supposition that the world was made up of city men."[31] Yet even Marshall's censure does not aim at the real issue.

No economist, even a Ricardo or a Walras, can be blamed for not having constructed a theory both *relevant and valid* for all institutional settings. Society is not an immutable entity, but evolves continuously in endless forms that differ with time and place as well. It is normal, therefore, that every great economist should have filled his analytical boxes with an institutional content inspired by the cultural patterns of the society he knew best: that in which he lived.

The economic profession should accept with immense pride Bridgman's accusation of practical opportunism.[32] Indeed, it would have been most regrettable had no Quesnay been interested in the specific economic problems of eighteenth-century France, had no Keynes studied the economic problems of modern state organizations, or had no contemporary economist been attracted by the problem of how to develop backward economies—which is *the* problem of our age. The standard economist, therefore, cannot be indicted, any more than Marx, for constructing his theory after the model of capitalist society. The egregious sin of the standard economist is of another kind. Because he denies the necessity of paying any attention to the evolutionary aspects of the economic process, he is perforce obliged to preach and practice the dogma that his theory is valid in *all* societies.[33]

The celebrated *Methodenstreit* apparently centered upon methodology. But, as should be clear from the preceding analysis, at bottom the *Streit* (i.e., the fight) was about the claim that it is possible to construct a universally valid economic theory. The adversaries of the Ricardians maintained that there is a Great Antinomy between this claim and the evolutionary nature of the economic process. Standard economists, as we have just seen, entrenched themselves behind the position of the

[31] Marshall, *Principles*, p. 762.

[32] P. W. Bridgman, *Reflections of a Physicist* (2nd edn., New York, 1955), pp. 443 f.

[33] In justice to Marx, I should note that he never endorsed this position. On the contrary, Marx repeatedly emphasized that his analysis pertains only to the capitalist system: e.g., Marx, *Critique*, p. 269. He also was aware of the fact that the differences between French and German economic schools were reflections of the institutional differences between the respective countries. *Ibid.*, p. 56*n*. Yet, in the end, Marx committed the great error of indiscriminately extending the laws of a capitalist society to the economy of a rural, agricultural society. See Section I(2) of my paper reprinted in AE, "Economic Theory and Agrarian Economics" (1960).

directly intuitive basis of the fundamental economic laws. But another signal attempt at resolving the Great Antinomy proceeds from an *objective* basis. In essence, it is a chemical doctrine of society.[34]

A chemical doctrine claims, first, that all forms of societies can be objectively analyzed into a finite number of immutable elements, and second, that a society can possess no properties other than those inherent in its elementary components. The Golden Horde, the medieval city of Florence, twentieth-century Switzerland, therefore, would not be different "animals" each with its specific behavior, but only stronger or weaker cocktails obtainable from a finite list of ingredients.

We owe to Walter Eucken the most cogent elaboration of a chemical doctrine of the economic process. He argues that the perennial ingredients of any economic system fall into three categories: the control (central or plural), the market (with its standard forms), and the monetary conventions (commodity-money, commodity-credit, money-credit).[35] Any economy is nothing but some combination of these ingredients, one from each category. All we need to know is the particular combinative formula in each case under consideration.

To clarify this epistemological position, Eucken resorts to an analogy: the works of composers, though different, have been created "by combining together a limited number of tones which they all used."[36] The choice is, however, most unfortunate, for through this analogy Eucken unwittingly lays bare the basic weakness of all chemical doctrines of society.

Musical scales have evolved and new ones are still in store for us. Besides, music requires instruments; new ones have been invented even during our generation. It is, therefore, patently false to say that *all* music is analyzable into a *given* set of tones and a *given* set of instruments. But that is not the major fault of a chemical doctrine.

From all we know, activity without a controlling agent is inconceivable; the existence of markets goes back to the dawn of history; some forms of capitalistic enterprises and money are found even in ancient societies. The obviousness of the general proposition that every economy consists of control, market, and monetary conventions, however, may be dangerously alluring. For, at least to anyone uncommitted to the fallacy of misplaced concreteness, it is equally obvious that this mixing formula fails to describe even partially the essential aspects of an extant economy.

[34] See above, Chapter V, Section 2. Actually, the term "chemical" is misappropriated, as will presently appear.

[35] Walter Eucken, *The Foundations of Economics* (London, 1950), Part III, chap. ii.

[36] *Ibid.*, pp. 226 f.

As I had occasion to observe earlier, every chemical compound has some properties not possessed by any of its elements; moreover, there is no general principle by which to deduce every property of a compound from its chemical formula. If this were not so, it would be child's play—as P. Green remarked in a different connection—for the modern scientist who can count the protons in the whole universe to find by calculation the color spots on a bird from New Guinea.[37] Given that the "chemical" doctrine fails to work in the chemical domain, it would be foolhardy to count on its success in social sciences, where the number of compounds is almost limitless and quality dominates the scene to an incomparably greater degree than in the domain of elementary matter.

It is highly significant that a modern mathematician, not a medieval mystic, raised the devastating question: how can a naturalist who has studied only the chemical composition of the elephant know anything about the behavior of that animal?[38] But biology, despite the increasing tribute it pays to chemical knowledge, did not wait for the intervention of an outsider to reject the chemical doctrine. As a Nobel laureate instructs us, for modern biology "a gene is known by its performance and not by its substantive properties."[39] This simple statement epitomizes the new biological conception, which has come to be known as the organismic epistemology.[40] It is a belated recognition of the existence of novelty by combination, but free from any vitalist overtones.

The same conception did not fare as well in the social sciences, still less in economics. The job of the economist being that of studying a process which often evolves faster than he can complete his professional training, it is normal for him to thirst more than anyone else for the objectivity of Classical physics. To be sure, such a thirst becomes even more pressing when fed propositions which defy any algebra, such as the tenet that "society is not a sum of individuals." Let us observe, however, that this is a rather unfortunate way of saying that society has properties the individual *by himself* cannot have. It may seem superfluous to some, futile to others, to dwell further on this point which is now crystallized in the philosophy of Gestalt.[41] Curiously, the opposition to this philosophy

[37] P. Green, "Time, Space and Reality," *Philosophy*, IX (1934), 463.

[38] Poincaré, *Foundations of Science*, p. 217.

[39] P. B. Medawar, *The Future of Man* (New York, 1960), p. 119. See also Appendix G in this volume.

[40] The essence of this idea, however, is much older than its propounders seem to realize. See Plato, *Philebus*, 14 ff.

[41] For which see K. Koffka, "Gestalt," *Encyclopedia of Social Sciences* (New York, 1930–1935), VI, 642–646, or *A Source Book of Gestalt Psychology*, ed. Willis D. Ellis (New York, 1938). Solomon E. Asch, "Gestalt Theory," *International Encyclopedia of the Social Sciences* (New York, 1968), VI, 158–175, offers a critical up-to-date appraisal.

is far more spread among social scientists (and, of course, among philosophers of positivistic strain) than among natural scientists. Max Planck, for instance, overtly recognized that "the whole is never equal simply to the sum of its various parts."[42] H. Weyl sided fully with the idea for which H. Driesch fought so dramatically. Even in the inorganic world—quantum physics not excepted—"it is out of the question," Weyl cautions us, "to derive the state of the whole from the state of its parts."[43] Take a melody, the classical example used by C. von Ehrenfels to illustrate what Gestalt means. The critics of Gestalt maintain that a melody is nothing but a sequence of identifiable notes each having an independent existence because whether played in the melody or alone each note sounds always the same. What they inexplicably refuse to see is that the sequence has a quality (the *Gestaltqualität*) that no note by itself possesses: the melody itself.[44] Indeed, we find Gestalt even in mathematics. A number taken by itself is neither rational nor irrational; nor is it continuous or dense. Only a pair of numbers may be rational or irrational. When we say that π, for instance, is irrational, we in fact say that the pair $(1, \pi)$ is so. Also, only a set of numbers *as a whole* may possess the quality of continuity or density. In the notion of continuum, as Leibnitz taught, "the whole precedes the parts." There is no way to reduce the antinomy that analysis creates between the properties of the whole and the properties of the parts when taken in isolation.[45] To turn to some elementary examples from the social domain: although every inch of the devastation left by a mob could be traced back to an act of some particular individual, an individual by himself can never display the peculiar properties of a mob. Nor can a single individual have all the manifestations of a religious sect, nor those we observe at religious revivals. Marx was completely right in ridiculing the economics of Robinson Crusoe,[46] where there are no monopolists, no labor unions, no conflict over the distribution of sacrifice and reward.

On the other hand, we may as well recognize that the reluctance of most of us to part with the tenet that society is a sum of individuals is

[42] Max Planck, *The New Science* (New York, 1959), p. 255. See also A. S. Eddington, *New Pathways in Science* (Ann Arbor, 1959), p. 296.

[43] H. Weyl, *The Open World* (New Haven, 1932), pp. 55 f. Some highly interesting illustrations of Gestalt in electrostatic structures are given by one of the founders of the doctrine: Wolfgang Köhler, "Physical Gestalten" (1920), in *Source Book of Gestalt Psychology*, ed. Ellis, pp. 17–54.

[44] Max Wertheimer, "Gestalt Theory" (1925), in *Source Book* just cited, p. 4.

[45] An exception among positivists, Bertrand Russell, *The Principles of Mathematics* (Cambridge, Eng., 1903), p. 477, explicitly recognizes the antinomy; but he certainly goes too far in saying that it applies even to the compound effect of mechanical forces.

[46] Marx, *Critique*, p. 266.

rooted in a historical condition: the only instance where the tenet is *roughly* true is the bourgeois society in which we have been reared and which is the nearest approximation to Hegel's Civil Society.[47] However, even the bourgeois society evolves, and nowadays it probably no longer fits Hegel's bill of requirements.[48]

Viewed as a theoretical reduction of a phenomenal domain, any chemical doctrine is fallacious from the start—save in the case of those physical phenomena which are indifferent to scale. At most, it can be accepted as a procedural code for morphological analysis. In this role it has proved its usefulness in chemistry, in nuclear physics, and to a lesser extent in the biology of the organism. In all probability that is the limit, considering that as keen an economist as Eucken could reap only a few vague generalities of little value even for morphological analysis. His doctrine leaves the economist as enlightened as a naturalist told only that the common denominator of all organisms is nutrition, defense, and reproduction.

The import of the conclusion that economics cannot be a theoretical and at the same time a pertinent science may seem purely academic. Unfortunately, this is not so. For the tenacity with which we cling to the tenet that standard theory is valid in all institutional settings—either because its principles are universally valid or because all economic systems are mere mixtures of some invariable elements—has far-reaching consequences for the world's efforts to develop the economy of nations which differ in their institutions from the capitalist countries. These consequences may go down in history as the greatest monument to the arrogant self-assurance of some of science's servants.

For example, most of us now swear by the axiom—which actually goes back to Marx—that industrial development is the only road to general economic development, that is, to the development of the agricultural sector as well. As factual evidence we invoke the incontrovertible fact that industrialization did result in the over-all development of the South of the United States. But the ingrained outlook of the standard economist—that what is good for one country is good for any other—prevents us from noting first, that the South is part and parcel of the most advanced capitalist economy, and second, that the American farmer is not institutionally identical (or even comparable) to the Indian or any other *peasant*. In fact, the greater the industrial development achieved by an underdeveloped nation plagued by a predominant, overpopulated, and disorganized agricultural sector, the stronger the evidence such a nation offers of the fallacy of the industrialization axiom. There

[47] *Hegel's Philosophy of Right*, tr. T. M. Knox (Oxford, 1953), pp. 124 ff, 267.

[48] For some brief remarks on this point see Section III(2) of my "Economic Theory and Agrarian Economics" (1960), in AE.

the peasantry is still as poverty-stricken as ever—a passive gloomy onlooker at the increasing well-being of the exclusive circle that delights in the Square Dance of Effective Demand, which alone moves faster and faster with each day. But, for one who believes that distributive relations form the core of the economic process, even this situation has its simple explanation. It is one phase in the evolution of the social conflict.

3. Arithmomorphic Models and Economics. In an often-quoted passage from "In the Neolithic Age" Rudyard Kipling said:

> There are nine and sixty ways of constructing tribal lays
> And-every-single-one-of-them-is-right!

This, however, is not the whole reason why economics cannot be a theoretical science. For even if there were only nine and sixty economic lays we still could not derive their laws from a single logical foundation. The laws of the capitalist society, for instance, are not valid for the feudal system, nor for an agrarian overpopulated economy. That is not all. The number of the economic lays is not even finite; instead, there is a continuous spectrum of forms which slide into each other as the economic process evolves and ultimately become as different as a bird is from a worm. It is then the evolutionary nature of the economic process that precludes a grasping of all its relevant aspects by an arithmomorphic scheme, even by a dynamic one. "The Mecca of the economist," as Marshall insisted, "lies in economic biology rather than in economic dynamics."[49] Yet, as Marshall went on to say, we have no choice but to begin with economic dynamics. What he failed to say is that by economic dynamics we should understand the dynamics of each known species of economic lays, not a general dynamics in which standard economics believes.

One may think then that the first task of economics is to establish some general criteria for classifying all known economic systems into genera, species, and varieties. Unfortunately, our economic knowledge in this direction is so little that even an economic Linnaeus would not be able to design a system of classification. The most we can do at this stage is to observe each economic reality by itself without necessarily looking for taxonomic characteristics. Our aim should be to construct an ideal-type that would make "pragmatically *clear* and *understandable*" the specific features of that particular reality.[50] But without a classificatory code—one may argue—even this lesser task cannot be accomplished. Too many of us hold today that classificatory systems, abstract analytical concepts, and, according to K. Popper, even "theories are prior to observations"[51]

[49] Marshall, *Principles*, p. xiv.

[50] Max Weber, *The Methodology of the Social Sciences* (Glencoe, Ill., 1949), p. 90.

[51] Karl R. Popper, *The Poverty of Historicism* (Boston, 1957), p. 98. Implicitly or explicitly, the idea appears in many writings; e.g., Jevons, *Theory*, p. 22.

as if all these things were found by science ready-made. We seem to forget not only that science emerged from undirected observation but that some pre-scientific thought always precedes the scientific one.[52]

The absence of a classifying code did not prevent the Classical economists—to cite a single example—from discovering the significant features of the capitalist economy. There are some tasks in every science, not only in economics, which require an appreciable dose of "delicacy and sensitiveness of touch."[53]

Once we have arrived at a workable body of descriptive propositions for a given reality, to construct an arithmomorphic model is a relatively simple task. Each economic reality should be provided with such a model as soon as feasible.[54] All the harder to understand is the position that even in the case of a capitalist system "it is premature to theorize."[55] Actually, judging from the immense difficulties encountered by Werner Sombart and other inspired economists we should rather agree with Marshall in saying that economics is not yet ripe for historizing.[56] And if economics seems to be now moving in the opposite direction it is only because modern economists spend most of their time on theorizing, some only on vacuous theorizing.

Arithmomorphic models, whether in physics or any other science, subserve legitimate needs of Understanding and, in my opinion, of Didactics even more. The scientist who would deny that his mind, at least, does not grasp a diagrammatic representation and, if he had some training, a mathematical model more firmly and faster than a verbal analysis of the same situation, is free to step forward any time, if he so wishes. Besides, of all men of science, economists should not let their slip show by opposing the use of the mathematical tool in economic analysis, for this amounts to running counter to the principle of maximum efficiency. But on the same principle we must deplore the exaggerated fondness for mathematics which causes many to use that tool even when a simple diagram would suffice for the problem in its unadulterated form.

Let me add that the position taken by many of my colleagues that "mathematics *is* language"[57] tends rather to obscure the fact that, when-

[52] Albert Einstein, *Ideas and Opinions* (New York, 1954), p. 276.

[53] Marshall, *Principles*, p. 769.

[54] For the loss incurred by not doing so, see Section I(4) of my "Economic Theory and Agrarian Economics," reprinted in AE. No doubt, the analytical tools developed by standard economics could prove themselves handy in many other situations. That is no reason to say with Schumpeter, *Essays*, p. 274n, that a model in which factor prices are not proportional to their marginal productivities is "still marginal-productivity theory." For then Einstein's theory should still be a Newtonian theory: in both theories there is a formula for the addition of velocities.

[55] Gambs, *Beyond Supply and Demand*, p. 64.

[56] *Memorials of Alfred Marshall*, ed. A. C. Pigou (London, 1925), p. 489.

[57] P. A. Samuelson, "Economic Theory and Mathematics—An Appraisal," *Papers and Proceedings, American Economic Review*, XLII (1952), 56.

ever the mathematical tool can be used, the analytical process can be accomplished faster than if carried out by ordinary logic alone. No doubt the mathematical armamentarium, if traced back to its genesis, is the product of ordinary logic, just as capital equipment resolves phylogenetically into labor, and living organisms into elementary matter. However, once these forms emerged from their *causa materialis* they displayed novel qualities that have ever since differentiated them from ordinary logic, labor, and inert matter, respectively. To obtain, say, a horse we do not go back and retrace the evolutionary process by which the horse gradually emerged from lifeless substance. Nor do we produce steel hammers by using stone hammers found accidentally in nature. It is more efficient to take advantage of the fact that we can obtain a horse from a horse and capital equipment with the aid of capital equipment. By the same token, it would be utterly absurd to rely on ordinary logic alone whenever a mathematical tool can be used or every time we want to prove a mathematical proposition. If we do teach mathematics from ABC in schools, it is only because in this way we aim not only to maintain our mathematical capital intact but also to develop the mathematical skill of future generations. It is ghastly to imagine the destruction of all present capital equipment, still ghastlier to think of all men suddenly forgetting all mathematics. But this thought may make us see that qualitatively mathematics is not just language, and though man-made it is not an arbitrary game of signs and rules like, say, chess.

And the immense satisfaction which Understanding derives from arithmomorphic models should not mislead us into believing that their other roles too are the same in both social and natural sciences. In physics a model is also "a *calculating* device, from which we may compute the answer to *any* question regarding the physical behavior of the corresponding physical *system*."[58] The same is true for the models of engineering economics. The specific role of a physical model is better described by remarking that such a model represents an *accurate blueprint* of a particular sector of physical reality. But the point, which I made in "Economic Theory and Agrarian Economics" (reprinted in my *Analytical Economics*), and which I propose to explain now in greater detail, is that an economic model is not an accurate blueprint but an *analytical simile*.

Economists are fond of arguing that since no model, whether in physics or economics, is accurate in an absolute sense we can only choose between a more and a less accurate model. Some point out also that after all how accurate we need to be depends on our immediate purpose: at times the

[58] P. W. Bridgman, *The Nature of Physical Theory* (Princeton, 1936), p. 93. Italics mine.

less accurate model may be the more rational one to use.[59] All this is perfectly true, but it does not support the further contention—explicitly stated by Pareto—that it is irrelevant to point out the inaccuracy of economic models. Such a position ignores an important detail, namely, that in physics a model must be accurate in relation to the sharpest measuring instrument existing at the time. If it is not, the model is discarded. Hence, there is an *objective* sense in which we can say that a physical model is accurate, and this is the sense in which the word is used in "accurate blueprint." In social sciences, however, there is no such objective standard of accuracy. Consequently, there is no acid test for the validity of an economic model. And it is of no avail to echo Aristotle, who taught that a model is "adequate if it achieves that degree of accuracy which belongs to its subject matter."[60] One may always proclaim that his model has the proper degree of accuracy. Besides, the factors responsible for the absence of an objective standard of accuracy also render the *comparison* of accuracy a thorny problem.

To illustrate now the difference between blueprint and simile, let me observe that one does not need to know electronics in order to assemble a radio apparatus he has purchased in kit form. All he needs to do is follow automatically the accompanying blueprint, which constitutes an *operational* representation by symbols of the corresponding mechanism. The fact that no economic model proper can serve as a guide to *automatic action* for the uninitiated, or even for a consummate economist, necessitates no special demonstration. Everyone is familiar with the dissatisfaction the average board member voices after each conference where some economic consultant has presented his "silly theory." Many graduate students too feel greatly frustrated to discover that, in spite of all they have heard, economics cannot supply them with a manual of banking, planning, taxation, and so forth. An economic model, being only a simile, can be a guide only for the initiated who has acquired an analytical insight through some laborious training. Economic excellence cannot dispense with "delicacy and sensitivity of touch"—call it art, if you wish. And it is only too bad if at times the economist lets himself be surpassed in this respect by the layman. The widespread view that the economist's role is to analyze alternative policies, whereas their adoption is the art of statesmanship,[61] is no excuse. An artless analysis cannot subserve an art.

[59] Pareto, *Manuel*, pp. 11, 23, and *passim*; also Milton Friedman, *Essays in Positive Economics* (Chicago, 1953), pp. 3–43.

[60] Aristotle, *Ethica Nicomachea*, 1094^b 12–14.

[61] Cf. Homan (note 14, above), p. 15.

Jevons' hope that economics will ultimately become an exact science has filled the hearts of many great economists. Irving Fisher still nourished it at eighty.[62] And since by exact or genuine science they all understood a science of calculating devices—a definition that goes back to the Enlightenment era[63]—they all endeavored to point out the quantitative nature of the economic domain. Schumpeter even argued that economics is "the most quantitative . . . of *all* sciences" because its observables are "made numerical by life itself"[64]—an argument far more impressive than Jevons'. Some, also like Jevons, went further and argued that even pleasure can be submitted to accurate calculation.[65] But none paid any attention to the fact that natural scientists, who know what measurement and calculation truly are, often smiled at the idea.[66] A few economists, however, gradually came to weakening the classical definition of exact science by distinguishing between *quantitative* and *numerical* devices.[67] An economic model is still exact even if it does not serve as a calculating device, provided that it constitutes a paper-and-pencil representation of reality.

To recall, Pareto argued with his characteristic aggressiveness that Walras had already transformed economics into an exact science. But while firmly holding that we can determine the value of any parameter we choose, he explicitly stated that, in opposition to Walras, he did not believe in the possibility of effectively solving a concrete Walrasian system.[68] Pareto, like Cournot before him, saw in the immensity of equations the only obstacle to economics' being a numerical science, like astronomy.[69]

Many still share the idea that the Walrasian system would be an accurate calculating device for a Laplacean demon. But let us imagine a new demon, which with the speed of thought can make all the necessary observations for determining all ophelimity and production functions, solve the system, and communicate the solution to everyone concerned.

[62] Ragnar Frisch, "Irving Fisher at Eighty," *Econometrica*, XV (1947), 74.

[63] Cf. *The Logic of Hegel*, tr. W. Wallace (2nd edn., London, 1904), p. 186.

[64] Schumpeter, *Essays*, pp. 100 f.

[65] Surprising though it may seem, this very idea is found in Plato: "If you had no power of calculation you would not be able to calculate on future pleasure, and your life would be the life, not of a man, but of an oyster or *pulmo marinus*." *Philebus*, 21.

[66] E.g., Max Planck, *The New Science*, p. 308.

[67] Robbins, *An Essay* (note 3, above), p. 66; Joseph A. Schumpeter, *History of Economic Analysis* (New York, 1954), p. 955.

[68] V. Pareto, "Teoria matematica dei scambi foresteri," *Giornale degli economisti*, VI (1894), 162. I need to add that this source shows that G. Demaria is wrong in saying that Pareto thought that his system would enable economists to make the same kind of predictions as astronomers. See V. Pareto, *Scritti teorici*, ed. G. Demaria (Milan, 1952), p. xix.

[69] A. Cournot, *Researches into the Mathematical Principles of the Theory of Wealth* (New York, 1897), p. 127.

Pareto's position is that everyone will be perfectly happy with the solution and that the economy will remain in equilibrium, if not forever, at least until disturbed by new forces from the *outside*.

This logic ignores a most crucial phenomenon: the very fact that an individual who comes to experience a new economic situation may alter his preferences. *Ex post* he may discover that the answer he gave to our demon was not right. The equilibrium computed by our demon is thus immediately defeated not by the intervention of exogenous factors but by endogenous causes. Consequently, our demon will have to keep on recomputing running-away equilibria, unless by chance he possesses a divine mind capable of writing the whole history of the world before it actually happens. But then it would no longer be a "scientific" demon. Pareto, first among many, would have nothing to do with clairvoyance.

There is at least one additional difficulty into which our demon would certainly run with the Walrasian system. It is the Oedipus effect, which boils down to this: the announcement of an action to be taken changes the evidence upon which each individual bases his expectations and, hence, causes him to revise his previous plans. Preferences too may be subject to an Oedipus effect. One may prefer a Rolls-Royce to a Cadillac but perhaps not if he is told that his neighbor, too, will buy a Rolls-Royce. And the rub is that no process in which the Oedipus effect is at work can be represented by an analytical model. In a very simple form: if you decide to make up your mind only next Saturday, *not before*, on how to spend the weekend, you cannot possibly know *now* what you will do next Sunday. Consequently, no analytical device can allow you (or someone else) to describe the course of your future action and, hence, that of the community of which you are a part.

Edgeworth once said that "to treat *variables* as *constants* is the characteristic vice of the unmathematical economist."[70] But an economist who sticks only to mathematical models is burdened with an even greater vice, that of ignoring altogether the qualitative factors that make for endogenous variability. Bridgman was thus right in reproaching the social scientist for failing to pick up the significant factors in describing social reality.[71]

Time and again, we can see the drawback of importing a gospel from physics into economics and interpreting it in a more catholic way than the consistory of physicists.[72] It is all right for physics to trust only what

[70] F. Y. Edgeworth, *Mathematical Psychics* (London, 1932), p. 127*n*.

[71] Bridgman, *Reflections*, pp. 447 f.

[72] Some economists would not accept arithmomorphic models at all in economics. E.g., F. A. Hayek, "The Use of Knowledge in Society," *American Economic Review*, XXXV (1945), 519–530. That is an extreme position which, as should be clear from the foregoing remarks, I do not share. Marshall, I maintain, was entirely right on this point. See his *Principles*, Appendix D.

is amenable to sense-perception, i.e., only observables, because that is the sole contact we have with the outside world. It is equally understandable for physics to treat as fiction and view with mistrust the unobservables it had to invent in order to unify into one picture disparate observables and thus simplify its logical foundation. But there is absolutely no reason for economics to treat as fiction the very springs of economic action—wants, beliefs, expectations, institutional attitudes, etc. For these elements are known to us by immediate acquaintance, that is, more intimately than any of the economic "observables"—prices, sales, production, and so forth.

No doubt, many mathematical economists must have been aware of the fact that in an arithmomorphic model there is no room for human propensities. Jevons started them searching for a cardinal measure of utility. More recently, others tried to establish such a measure for uncertainty. All these painstaking endeavors should be viewed with pride because science should leave no stone unturned. However, through these very endeavors we gradually came to realize that measurability, whether ordinal or cardinal, requires very stringent conditions. Some of these conditions were brought to light for the first time in my 1936 article "The Pure Theory of Consumer's Behavior," reprinted in *Analytical Economics*. By pursuing this line of thought in several other papers, included in Part II of that volume, I was able to show—convincingly, I hope—that neither wants nor expectations fulfill the conditions of measurability. The apparent solidity of all demonstrations of how to establish a measure for wants or expectations derives from "the ordinalist fallacy" —as I proposed to call the idea that a structure where we find "more" and "less" is necessarily a *linear* continuum.

But our thirst for measure is so great that some have tried to dispose of all evidence and logical arguments against the measurability of human propensities by arguing that if mental attitudes are "inaccessible to science and measurement, the game is lost before the first move is made."[73] Clearly, the game to which the statement applies cannot be other than the game of "science is measurement." But why should this be the only game a scientist can play? It is precisely because of this question that I have tried to present in these pages all the evidence I could muster—however technical or tedious this evidence may seem at first—in order to prove that no science can completely avoid dialectical concepts. The reason, as I have explained, is that no science can ignore Change forever. The idea that human propensities, which are the main vehicle of economic

[73] S. S. Stevens, "Measurement and Man," *Science*, February 21, 1958, p. 386. This is Bentham's old refrain. But Bentham at least confessed that it runs against the grain of elemental facts. See Chapter IV, above, note 3.

Change, are not arithmomorphic concepts, therefore, is not a fancy of some unscientific school of thought.

The obvious conclusion is that if economics is to be a science not only of "observable" quantities but also of man, it must rely extensively on dialectical reasoning.[74] Perhaps this is what Marshall meant by "delicacy and sensitiveness of touch." But in the same breath he added that the economic science "should not be invertebrate . . . [but] have a firm backbone of careful reasoning and analysis."[75] It is highly significant that Marshall did not say "exact reasoning." For dialectical reasoning cannot be exact. But as I argued earlier (in Chapter II, Section 6), dialectical reasoning can be correct and ought to be so. There are two known methods for testing the correctness of dialectical reasoning: Socratic analysis and analytical simile. Surprisingly enough, we owe them to Plato, who used them freely throughout the *Dialogues*.[76] Two thousand years later, in 1690, William Petty surprised political scientists by proposing to apply one of Plato's methods to economic reasoning: "The Method I take to do this, is not yet very usual; for instead of using only comparative and superlative Words, and intellectual Arguments, I have taken the course . . . to express myself in Terms of *Number*, *Weight*, or *Measure*, [which] at worst are sufficient as Suppositions to shew the way to that Knowledge I aim at."[77]

Perhaps the most obvious merit of an arithmomorphic model is that which is acknowledged by almost every criticism of mathematical economics: the merit of bringing to light important errors in the works of literary economists who reasoned dialectically. In this respect, the role of a mathematical model in economics as well as in many other sciences is analogous to that of the rule of casting out nines in arithmetic. Both are expedient ways of detecting errors in some mental operations. Both work negatively: if they reveal no error, it does not mean that the dialectical argument or the arithmetical calculation is wholly correct. Important though this last point is, only F. H. Knight, it seems, saw that economic theory shows "what is 'wrong' rather than what is 'right.'"[78]

The second role of an arithmomorphic model is that of illustrating certain points of a dialectical argument in order to make them more understandable. One may use, for instance, an ophelimity function containing a

[74] Let me remind the reader that my meaning of dialectical reasoning differs from that of Hegel and, hence, of Marx. Cf. Chapter II, above, note 27; also below, note 80.

[75] Marshall, *Principles*, p. 769.

[76] "The higher ideas . . . can hardly be set forth except through the medium of examples" (*Statesman*, 277), suffices as an illustrative quotation.

[77] *The Economic Writings of Sir William Petty*, ed. C. H. Hull (2 vols., Cambridge, Eng., 1899), I, 244 f.

[78] Knight, *On the History* (note 24, above), p. 177.

special parameter in order to discuss didactically the problem of change in tastes or a probability distribution to illustrate the situation of an individual confronted with uncertainty.[79] Or, like Walras or Leontief, we may construct a system of indefinite dimensions in order to illustrate some important aspects of a whole economy.[80]

These two roles of the mathematical model circumscribe the *raison d'être* of what currently passes for "economic theory," which is to supply our dialectical reasoning with a "firm backbone." An analytical simile, therefore, must be formulated with the utmost rigor without any regard for its factual applications. That is why there is no room in "pure theory" even for pseudo-arithmomorphic concepts, such as price index, cost of living, aggregate production, and the like. They have been denounced by almost every theoretical authority[81]—and rightly as far as pure theory is concerned.

In spite of all the denunciations these pseudo-arithmomorphic concepts fared increasingly well. Macroeconomics by now has smothered microeconomics almost completely. The phenomenon, far from being perplexing, has a very simple reason. Coordinates such as standard of living, national real income, aggregate production, etc., are far more significant for the analysis of the economic process than Mr. X's tastes or the pricing rule of entrepreneur Y. Like all other vital coordinates of the same process, they are dialectical notions. They differ from others only because if abstractly reduced to an individual and to an instant they can be represented by a number. From this number we can then construct a pseudo measure, which is always some sort of average. The fact that we can never tell which formula we should choose in order to compute this average, nor why a greater or a smaller number than the one obtained by some formula would also do, shows that a pseudo measure is in essence a dialectical concept.

As is often the case, the same reason why pseudo measures are poison to "theory" accounts for their success in the description and analysis

[79] I used precisely this Platonic method in analyzing the hysteresis and novelty effects in consumer's choice. Cf. "The Theory of Choice and the Constancy of Economic Laws" (1950), reprinted in AE. The conclusion at which I arrived—symmetrical to Marshall's observation concerning long-run supply schedules—is that demand curves too are irreversible. The same analytical simile also enabled me to pinpoint the delusion that experiments with an individual leave him as he was at the outset and, hence, enable us to predict his complete behavior.

[80] Let me add that an analytical simile would not work in case the epistemological approach to the economic process follows Hegelian Dialectics exactly, as was Marx's case. Cf. Chapter IX, Sections 13 and 14.

[81] E.g., N. G. Pierson, "Further Considerations on Index-Numbers," *Economic Journal*, VI (1896), 127 ff; Lionel Robbins, *An Essay*, p. 66; W. W. Leontief, "Implicit Theorizing: A Methodological Criticism of the Neo-Cambridge School," *Quarterly Journal of Economics*, LI (1937), 350.

of concrete facts. In proper use, an index or an aggregate is not a fine bullet, but a piece of putty which covers a dialectical target, such as "the standard of living" or "the national product," better than a bullet. That is why an increasing number of economists share the view that macroanalysis, though it is only vaguely clear, is far more productive than the traditional microeconomics with its Ockham's razor. But, perhaps, the real reason is that they have ultimately come to realize that the more significant variables pertain to society, not to the individual.

The preceding observations should not be interpreted as a motion to place the mathematical macromodel on a high pedestal in the gallery of blueprints. Actually, as a blueprint a macromodel is vulnerable from more sides than a micromodel.

To begin with, a macromodel, in contrast with that of Walras-Pareto, is admittedly incomplete because, we are told, the significant macro-coordinates are too numerous for our power of calculation. The excuse is familiar. The truth, however, is that their number exceeds our analytical power and, hence, we are unable even to say which are the significant coordinates. To recall the earlier discussion of objective accuracy, we understand why it is not very clarifying to explain *ex post* that a model is not a blueprint because some significant variables were left out. Yet that is what we are compelled to explain most of the time.

Secondly, macroeconomic models generally consist of a system of equations which has a quite special structure: they involve only analytical functions. Now, the peculiar property of an analytical function, $f(x)$, is that its value for *any* x is completely determined by the values $f(x)$ has in *any interval, however small*.[82] The reason why we use only such functions is obvious. Without analytical functions we would not be able to extrapolate the model beyond the range of past observations.[83] But why should economic laws, or any other laws for that matter, be expressed by analytical functions? Undoubtedly, we are inclined to attribute to reality a far greater degree of orderliness than the facts justify. That is particularly true for the linear macromodels—save perhaps the case of models such as Leontief's which deal only with material flows. Yet even linear macromodels are usually hailed for having run successfully the most terrific gantlet of statistical analysis. But we often forget to ask whether the gantlet was not a mere farce. The validity of statistical tests, even the nonparametric ones, requires conditions which a rapidly changing structure such as the economic process may fulfill only by sheer accident.

[82] Cf. Chapter VIII, Section 5.

[83] Let me add a thought that seems important: without analytical functions we would be unable also to argue that a law changes with the scale or with the phases of the business cycle, for example.

Besides, if one formula does not pass the test, we can always add another variable, deflate by another, and so on. By cleverly choosing one's chisels, one can always prove that inside any log there is a beautiful Madonna.[84]

Thirdly, the very idea of a mathematical (read arithmomorphic) relation between pseudo measures, like those used in economics, is a manifest contradiction in terms. For, in contrast with the conditions prevailing in other domains, in economics there is no basis for the average income, for instance, to be represented by the same average formula at all times or in all places. Though a statement such as "average real income increases with the proportion of industrial production in the gross national product" is not sharply clear, it raises far fewer questions than if it were replaced by some complex mathematical formula. Of course, there should be no restriction on the ingredients that one may use in his analytical kitchen, whether he is an economist or a natural scientist. If one thinks that "lower semi-continuity," "bicompactness," "the Lipschitz condition," or any other such sophisticated ingredient could enhance his cooking, he should by all means be free to use them. But one should not let himself become so infatuated with these exotic ingredients as to forget why he went into the kitchen in the first place. The quality of the final product alone counts. As to this quality, a consummate econometrician lays bare the conclusion of his long experience for us to ponder on it: "We must face the fact that models using elaborate theoretical and statistical tools and concepts have not done decisively better, in the majority of available tests, than the most simple-minded and mechanical extrapolation formulae."[85]

In my opinion, this is not all. The more complicated the model and the greater the number of the variables involved, the further it moves beyond our mental control, which in social sciences is the only possible control. There are no laboratories where social phenomena may be subject to experimental control. A "simple-minded" model may after all be the more enlightening representation of the economic process provided that the economist has developed his skill to the point of being able to pick up a few but significant elements from the multitude of cluttering facts. The choice of relevant facts is the main problem of any science, as Poincaré

[84] See my "Comments" on G. H. Orcutt, "Toward Partial Redirection of Econometrics," *Review of Economics and Statistics*, XXXIV (1952), 206–211, and "Further Thoughts on Corrado Gini's *Delusioni dell' econometria*," *Metron*, XXV (1966), 265–279.

[85] T. C. Koopmans, *Three Essays on the State of Economic Science* (New York, 1957), p. 212. Naturally, the statement refers to the success of the models in *predicting future events*, not in *fitting the past observations* used in estimating the parameters. As noted above, there is no shortage of econometric tools by which an economist can carve as good a fit as he may please.

and Bridgman insisted;[86] it is the vital problem in economics, as another consummate econometrician, James Tobin, now cautions us.[87] A "simple-minded" model comprising only a few but well-chosen factors is also a less deceptive guide for action. That is why some economists interested in the problems of economic development have shifted from mathematical macromodels to a less exact but more valuable analysis of the sort professed, especially, by S. Kuznets. Such analysis may not seem sophisticated enough. But sophistication is not an end in itself. For, as more than one physicist or economist has observed, "if you cannot—in the long run—tell everyone what you have been doing, your doing has been worthless."[88]

From whatever angle we may look at arithmomorphic models, we see that their role is "to facilitate the argument, clarify the results, and so guard against possible faults of reasoning—*that is all.*"[89] This role is not only useful, as everyone admits, but also indispensable—a point some tend or want to ignore. Unfortunately, we are apt, it seems, to be fascinated by the merits of arithmomorphic models to the point of thinking only of the scalpel and forgetting the patient. That is why we should keep reminding ourselves that *an arithmomorphic model has no value unless there is a dialectical reasoning to be tested.* To return to an earlier analogy, the rule of casting out nines is of no use if we have no arithmetic calculation to check. If we forget this point we run the great risk of becoming not "mathematicians first and economists afterwards"—as Knight once said[90]—but formula spinners and nothing else.

4. Economics and Man. Arithmomorphic models, to repeat, are indispensable in economics, no less than in other scientific domains. That does not mean also that they can do all there is to be done in economics. For, as Schrödinger argued in the case of biological life, the difficulty of the subject of economics does not lie in the mathematics it needs, but in the fact that the subject itself is "much too involved to be fully accessible to

[86] H. Poincaré, *Foundations of Science*, p. 363, and note 71, above.

[87] Cited in Koopmans, *Three Essays*, p. 209.

[88] E. Schrödinger, *Science and Humanism* (Cambridge, Eng., 1951), pp. 8 f. The same opinion is held by Werner Heisenberg, *Physics and Philosophy: The Revolution in Modern Science* (New York, 1958), p. 168; J. K. Galbraith, *Economics and the Art of Controversy* (New Brunswick, N.J., 1955), p. 43.

[89] Knut Wicksell, *Value, Capital and Rent* (London, 1954), p. 53. Italics mine. The point, of course, goes back to Marshall who, moreover, practiced it by relegating his mathematical similes to the back of his *Principles*. J. M. Keynes, *The General Theory of Employment, Interest, and Money* (New York, 1936), p. 297, is another economist of great repute to insist that "the object of [economic] analysis is, not to provide a machine, or method of blind manipulation, . . . but to provide ourselves with an organized and orderly method of thinking our particular problems." And it is highly pertinent to note that all these economists were sufficiently keen mathematicians.

[90] Knight, *Ethics of Competition*, p. 49. See also Keynes, *General Theory*, p. 298.

mathematics."[91] And what makes this subject not fully amenable to mathematics is the role that cultural propensities play in the economic process. Indeed, if man's economic actions were independent of his cultural propensities, there would be no way to account for the immense variability of the economic pattern with time and locality.

The well-known conflict between standard economics and all other schools of economic thought is a striking illustration in point. The conflict stemmed from the cultural differences between the economic process known to one school and that known to another. Nothing is more natural than the inability of the standard economists to understand their German colleagues who insisted on bringing such "obscurantist" ideas as *Geist* or *Weltanschauung* into the economic science. On the other hand, it was equally normal for the German school to reject an idea which reduces the economic process to a mechanical analogue.

The much better faring of standard economics notwithstanding, it is the position of the historical school that is fundamentally the correct one. The point seems to be winning the consent, however tacit, of an increasing number of economists. And perhaps it is not too involved after all.

From time indefinite, the natural sciences have cherished a positivist epistemology according to which scientific knowledge covers only those phenomena that go on irrespective of whether they are observed or not. Objectivity, as this criterion is often called, requires then that a proper scientific description should not include man in any capacity whatsoever, that the world of science "must be purged progressively of all anthropomorphic elements."[92] This is how some came to hold that even man's thinking is not a phenomenon.[93] But with the discovery of the quantum of action and of the Principle of Indeterminacy, the ideal of a man-less science began losing ground rapidly among physicists—curiously, more so among physicists than among philosophers of science and social scientists.[94] The natural scientist came to realize that, as Louis de Broglie put it, he is in a continuous hand-to-hand battle with nature.[95] And being a man, he cannot possibly describe nature otherwise than in terms "adapted to our mentality."[96] True, we no longer think of an atom as a

[91] E. Schrödinger, *What Is Life?* (Cambridge, Eng., 1944), p. 1.

[92] Planck, *The New Science*, p. 188.

[93] As A. J. Ayer, for instance, seems to imply in his *Language, Truth and Logic* (2nd edn., New York, 1946), pp. 46 f, 57 f and *passim*. But see E. Schrödinger, *Nature and the Greeks* (Cambridge, Eng., 1954), pp. 90 ff.

[94] Cf. Niels Bohr, *Atomic Physics and Human Knowledge* (New York, 1958), p. 98; Heisenberg, *Physics and Philosophy*, pp. 52 f.

[95] Louis de Broglie, *Physics and Microphysics* (London, 1955), p. 131.

[96] P. W. Bridgman, "Permanent Elements in the Flux of Present-Day Physics," *Science*, January 10, 1930, p. 20. Also Broglie, *Physics and Microphysics*, p. 114; Heisenberg, *Physics and Philosophy*, p. 81.

billiard ball in miniature; instead, we think of it as a system of equations. Also, in pure mathematics we no longer think of numbers as an abstract representation of the intuited actuality but as symbols subject to operations by symbols. But that does not prove in the least that the scaffold of science is no longer anthropomorphic or disprove Poincaré's intuitionist position that "there is no logic and epistemology independent of psychology,"[97] independent of how man's mind functions. Yes, even equations and symbolic operations are man-made. By the very nature of its actor, every intellectual endeavor of man is and will never cease to be human. The claims to the contrary are either vitiated by logical circularity (if they address themselves to a human mind) or empty (if they do not).

Nothing more need be said to see that for a science of man to exclude altogether man from the picture is a patent incongruity. Nevertheless, standard economics takes special pride in operating with a man-less picture. As Pareto overtly claimed, once we have determined the means at the disposal of the individual and obtained "a photograph of his tastes . . . the individual may disappear."[98] The individual is thus reduced to a mere subscript of the ophelimity function $\phi_i(X)$. The logic is perfect: man is not an economic agent simply because there is no economic process. There is only a jigsaw puzzle of fitting given means to given ends, which requires a computer not an agent.

If standard economics has not completely banished the individual from its discourse it is because a weakening assumption has been added to those outlined above. This assumption is that although every individual knows his own means and ends, no one knows the means and ends of others. "A farmer can easily calculate whether at the market prices it is more advantageous for him to use a horse or a tractor . . .; but neither he nor anyone in the world can determine the effect [of the farmer's decision] on the prices of horses and tractors."[99] The puzzle can then be solved only by groping—*tâtonnement*. This is how the individual came to be endowed with some economic activity, that and only that of shifting resources by trial and error between various employments, contemporaneous or not. And since the founders of standard economics—like most economists—aspired to provide an analysis of the economic reality in which they actually lived, the rules of the *tâtonnement* as well as the nature of the ends were molded upon attitudes and practices prevalent in a capitalist society. One may therefore understand why Rosa Luxemburg regarded economics as the study of how an uncoordinated, chaotic system

[97] H. Poincaré, *Mathematics and Science: Last Essays* (New York, 1963), p. 64.

[98] Pareto, *Manuel*, p. 170; V. Pareto, "Mathematical Economics," *International Economic Papers*, no. 5, 1955, p. 61.

[99] Pareto, *Manuel*, p. 335. My translation.

such as capitalism can nevertheless function. Natural also is her conclusion that the economic science will die of starvation with the advent of the socialist society where scientific planning will replace the *tâtonnement*.[100]

That in all societies man's economic actions consist of choosing is beyond question. It is equally indisputable that the ultimate outcome of *economic* choice is expressible as a vector $X(x_1, x_2, \dots, x_n)$, the coordinates of which are quantities of some commodities. Now, some economic choices are *free choices*, that is, the individual is as free to choose one of the alternatives as if he had to choose a card out of a deck or a point on a line. But the most important choices usually are not free in this sense. They imply a certain action by the agent. In its general form the economic choice is not between two commodity vectors, Y and Z, but between two complexes (Y, B) and (Z, C), where B and C stand for the actions by which Y or Z is attainable. Ordinarily, there exist several actions, $B_1, B_2, \dots, B_k$ by which, say, Y may be attained. One may beg for a dollar, or pinch the cash register, or ask his employer to give him one for keeps. What one will do on the average depends on the cultural matrix of the society to which he belongs. The point is that whether the outcome of choice is Y or Z depends also upon the *value* the actions B and C have according to the cultural matrix of the economic agent. To leave an employer with whom one has been for some long years only because another employer pays better is, certainly, not an action compatible with every cultural tradition. The same can be said about the action of an employer who lets his workers go as soon as business becomes slack.

Cultures differ also in another important respect. In some societies, most actions have either a great positive or a great negative value according to the prevailing cultural matrix. These values then count heavily in the choice of the individual. At the other extreme, there is the Civil Society, where, except for actions specifically barred by the *written* laws, the choice is determined only by the commodity vectors Y and Z. We can now see clearly why standard economics has fared so well in spite of its *homo oeconomicus*. For this *homo oeconomicus* chooses freely, that is, according to a choice-function involving only the commodity vector.

It is customary to refer to the societies where choice is determined also by the action factor as "traditional societies." But the term is, obviously, a pleonasm: every society has its own tradition. That of the Civil Society is that only the written law, sometimes only the opinion of the court, tells one whether an action is allowed or forbidden. Think of the frequent cases in which the Federal Trade Commission asks the courts to decide whether or not the action meets the socially accepted standards.

The opinion that the choice-function of the *homo oeconomicus*, that is,

[100] Rosa Luxemburg, "What is Economics?" (mimeo., New York, 1954), pp. 46, 49.

the utility index, adequately represents the economic behavior in any society is still going strong. I can foresee the argument that after all one can include the actions in the commodity vector by distinguishing, say, between x_k obtainable through action B and the same x_k obtainable through action C. That this suggestion only covers a difficulty by a paper-and-pencil artifact needs no elaboration. More familiar, however, is the position epitomized by Schumpeter's argument that "the peasant sells his calf just as cunningly and egotistically as the stock exchange member his portfolio of shares."[101] The intended implication is that the standard utility function suffices to describe economic behavior even in a peasant community. But Schumpeter, obviously, referred to a peasant selling his calf in an urban market to buyers whom he scarcely knows. In his own community, however, a peasant can hardly behave as a stock exchange broker. As an increasing number of students of peasant societies tell us, for the peasant it does matter whether he can buy cheap only because a widow, for example, must sell under the pressure of necessity. The stock broker does not care why the seller sold cheap: he has no means of knowing from whom he buys.

In recent years, a great number of economists have been engaged in the study of the peasant economies in various underdeveloped countries. Their attachment to the utility and the profit functions as "rational choice-functions" has led many to proclaim that the peasant—or in general, any member of a "traditional" society—behaves *irrationally*. In fact, a substantial amount of work has been done on how to make the peasant behave *rationally*. But most of these writers do not seem to realize that what they propose to do is to make the peasant communities choose as the Civil Society does, according to a utility and a profit function. Whether this or any other pattern of economic behavior is the rational one is actually a pseudo problem.

5. Rational Behavior and Rational Society. From the deterministic viewpoint, the notion of "rational behavior" is completely idle. Given his tastes, his inclinations, and his temperament, the person who smokes in spite of the warning that "smoking may be hazardous to your health" acts from a definite ground and, hence, cannot be taxed as irrational. And if we accept the conclusions biologists have derived from the study of identical twins, that every man's behavior is largely determined by his genotype, then criminals and warmongers are just as "rational" as the loving and peaceful people.[102] But for a determinist even nurture (whether

[101] Joseph A. Schumpeter, *The Theory of Economic Development* (Cambridge, Mass., 1949), p. 80.

[102] For evidence in support of this thesis, see C. D. Darlington, *Genetics and Man* (New York, 1969), pp. 232–244, especially the list of genetically transmitted characters on pp. 240 f.

ecological, biotic, or cultural) cannot be otherwise than what it is: together with nature, nurture holds the individual in a predetermined and unrelenting grip. This is probably why, when a social scientist speaks of irrational behavior, he generally refers to a normative criterion. Take the villagers in some parts of the world who for the annual festival kill practically all the pigs in the village. They are irrational—we say—not only because they kill more pigs than they could eat at one feast but also because they have to starve for twelve months thereafter. My contention is that it is well-nigh impossible to name a behavior (of man or any other living creature) that would not be irrational according to some normative criterion. This is precisely why to an American farmer the behavior of a Filipino peasant seems irrational. But so does the behavior of the former appear to the latter. The two live in different ecological niches and each has a different *Weltanschauung*. The student of man should know better than to side with one behavior or another. The most he can do is to admit that the two behaviors are different, search for the reasons that may account for the differences, and assess the consequences.

Let us also note that to ascertain that a behavior is free from normative irrationality we would have to know all its possible consequences—which, of course, is beyond our intellectual reach. For example, let us admit that to please one's senses without harming one's body is a rational behavior. One hundred years ago, however, we would not have said that a smoker behaves irrationally: at that time smoking was not indicted for harming one's health. On the other hand, for a demiurgic mind perhaps every behavior has its ultimate "reason," however irrational that behavior may appear to us in the light of our incomplete knowledge. The existence of a *Göttliche Ordnung*—of a Divine Order—has neither been proved nor disproved.

"Irrational" may also denote the case in which the individual proclaims a certain commandment as desirable and for nonapparent reasons behaves differently. "Inconsistent" seems a far more appropriate term for this situation even if we admit that the individual is free to follow his avowed precept. Finally, a behavior may be termed "irrational" if it eludes any predictive rule.

In the light of these remarks, one ought to be puzzled by Bridgman's pronouncement that "we will not have a *true* social science until eventually mankind has educated itself to be more rational."[103] The puzzle is both instructive and complex. The pronouncement catches one of the greatest physicists of this century in the act of expressing his faith in the freedom of the will. Obviously, one must be free to will in order to change himself

[103] Bridgman, *Reflections* (note 32, above), p. 451. My italics.

from behaving "irrationally" to behaving "rationally." But Bridgman left us guessing what he understood by "rational." In all probability, the eminent scholar who laid the foundations of operationalism and who visualized science as a store of calculating devices enabling us to predict the future behavior of nature felt that mankind is irrational because in the most important situations man's behavior does not lend itself to such calculations. In other words, Bridgman thought of man's behavior as being irrational because it is dominated by that category of phenomena to which I have referred as rational of the third order (Chapter V, Section 1). Therefore, what he said in essence is that, unless mankind educates itself so that the behavior of man shall be *predictable* in the same sense in which the behavior of matter is, there can be no true social science. Of course, he did not say which of the possible behaviors should be repeated by every human being over and over again. But we may be certain that a mind such as his did not ignore the intellectual immensity of the issue of an eternal and rigidly obeyed categorical imperative.

More intriguing, however, is the fact that a variant of Bridgman's position, concerning what is to be done in order to have a social science of the same order of operationality as physics or chemistry, has been for some time now near the hearts of the worshipers of a thoroughly planned society, of Marxists in particular. From a different direction, the theme has been expounded with scholarly skill by Adolph Lowe in a recent volume: since man behaves irrationally (viz. unpredictably), the task of social sciences (of economics, in particular) must be to make man behave rationally (viz. predictably). The invitation is to kill two birds with one stone: to achieve both "the stability and growth of industrial systems [and] that degree of orderliness which is a precondition for theoretical generalization."[104] While sharing Lowe's dissatisfaction with standard economics, I take a basic exception to his writ. And my reason is that his proposed remedy of replacing the present science of economics by Political Economics—not to be confused with the old Political Economy—implies the same sin as that of standard economics, only in a more severe form.

As Lowe presents it, Political Economics is "the theory of controlled economic systems."[105] It presupposes a "controlling authority" capable of selecting the optimal "macrogoal" of the economy. After this selection, the same authority turns to the following tasks: (1) to determine the material course that will move the system toward the chosen macrogoal; (2) to find out the behavioral patterns required by this course and the

[104] Adolph Lowe, *On Economic Knowledge: Toward a Science of Political Economics* (New York, 1965), pp. xviii and *passim*.

[105] *Ibid.*, p. 156.

motivations capable of fostering these patterns; and (3) to discover the central regulations that will induce these "goal-adequate" motivations.[106] Even if we beg the question of whether there is such a thing as an objective optimal goal and, if there is, whether the control authority can always discover it, and even if we grant the possibility of planning on paper the course to the macrogoal, the problems raised by the other two tasks are formidable. Since Lowe certainly does not advocate the use of outright individual coercion for solving these problems, he must count on the existence of some calculating devices that may enable us to control motivations by ordinary regulations as efficiently as matter in bulk can be controlled by engineering contraptions. Such a supposition implies that even features not included in *homo oeconomicus* are subject to a strong degree of mechanistic orderliness, which is a supposition more unwarranted than the basic position of standard economics. In any case, the supposition constitutes the creed on which the belief in the feasibility of social engineering rests. And, interestingly enough, we find in Lowe's argument an echo of the basic thought that runs throughout Lenin's *The State and the Revolution*. Says Lowe, "One can imagine the limiting case of a monolithic collectivism in which the prescriptions of the central plan are carried out by functionaries who fully identify with the imposed macrogoals. In such a system the economically relevant processes reduce almost completely to technical manipulations."[107] But those who have tried hard and by all available means to instill this feeling of full identification with the imposed macrogoal let us know occasionally—as Nikita Khrushchev did a few years ago—that they have not met with success.

The full identification of the functionaries—rather, of absolutely every member of the controlled monolith—with the macrogoal naturally reminds us of other social living creatures, including the bees, the ants, and the termites. And this reminder leads us directly to the core of the issue overlooked by Political Economics or any other doctrine implying social engineering. In the case of the social insects, social production developed by division of labor within the endosomatic evolution of each species. An ant doorkeeper, for example, fulfills his task with its endosomatic instruments—its flat head in particular. Moreover, its biological structure is such that all it wants to do is to block the entrance to the galleries with its head. In the case of the human species social production is, on the contrary, the result of man's exosomatic evolution. On the average, man is born with a biological constitution such that it may fit the role of a ricksha man as

[106] *Ibid.*, pp. 133, 143, especially.
[107] *Ibid.*, p. 142.

well as that of a king. Also, there is absolutely nothing in the constitution of the average man that could make him not to wish to be the king. And the question is why he should be a ricksha man and not the king.

The contrast between the apparent harmony in which the social insects live and the permanent conflict among the members of the human societies has long since attracted the attention of social philosophers to the division between man's biological nature and his economic aspirations. Thus, Plato, the earliest advocate of a "rational" society controlled by an oligarchic elite, recommended that "each individual should be put to the use for which nature intended him,"[108] obviously thinking that nature intends some people to be slaves, others to be philosopher–dictators. And Plato's perception of the biosocial complex in any society of animals led him even to set out some rules for protecting the caste of guardians (as he called his supermen) against deterioration by genetic mixing with the hoi polloi.[109] But at the time Plato could not possibly think of the converse manipulation: to have people born so that each should fit a necessary task of the planned society. Until recently, this idea decorated only *The Dream of d'Alembert*, a satire written by the French Encyclopedist Denis Diderot, and the utopian, satiric vision of Aldous Huxley in *The Brave New World*. But the recent discoveries in molecular biology have inflamed the imagination of many popularizers and journalistic humbugs as well as that of a few biological authorities. The forebodings of these discoveries, we are advised, are that man shall soon be capable of "directly altering or *producing* the human gene string."[110] And since some insisted that "these are *not* long-term problems [but] are upon us now," during the last decade some of the greatest names in biology gathered in several symposia to consider the coming of the biological millennium and to formulate recommendations on how to prepare ourselves for it.[111] A biologist of J. B. S. Haldane's stature shortly before his death told his peers at one of these symposia that the future man will see not only such practices as the use of some thalidomide for producing astronauts (whose occupation would be facilitated by very short legs)

[108] Plato, *Republic*, II. 374 and V. 423.

[109] *Ibid.*, V. 459–460. Interestingly, these rules contained also some tricky devices by which to fool the masses and which were to remain the secret of the ruling oligarchy. In Plato the modern mind-shrinkers have an illustrious precursor indeed.

[110] Joshua Lederberg, "A Crisis in Evolution," *The New Scientist*, January 23, 1964, p. 213. My italics.

[111] Not fewer than five Nobelites attended the Ciba Foundation Symposium on *Man and His Future*, ed. G. Wolstenholme (Boston, 1963). Three Nobelites participated in the symposium held at Ohio Wesleyan University on *The Control of Human Heredity and Evolution*, ed. T. M. Sonneborn (New York, 1965). The quotation in the text is from *Man and His Future*, p. 363.

but also the production of chimeras combining the best biological features of man and other animals.[112]

But among the consecrated authorities no one has excelled the enthusiasm and assurance with which in a series of articles Joshua Lederberg has preached the imminence of a long list of biological wonders.[113] Thus, he asserts that "it would be incredible if we did not soon have the basis of developmental engineering technique to regulate, for example, the size of the human brain"[114] by some I.Q. pills, as the newspaper headlines have it. Most important for our present topic, however, is the great stress Lederberg lays on the *vegetative reproduction* of people, on the cloning of people (Lederberg's preferred term), on "Einsteins from Cuttings" (the expression by which journalists have dramatized the idea). This miracle, he insists, is the "evolutionary perturbation" that man is indeed on the brink of achieving.[115] By an incidental remark Lederberg lets us know that by cloning he understands the extension to man of a cellular surgery originated some twenty years ago by R. Briggs and T. J. King. The experiment, pursued also by others, consisted of transplanting somatic nuclei into enucleated eggs of the same (or even related) species of amphibians. Some of these hybrid eggs developed to various stages, occasionally to the adult stage.[116] Naturally, if the same trick worked in the case of man and with a practically complete success, then there would be no limit to the number of "Einsteins" we could produce. The vision thus opened reminds us of Diderot's in *The Dream of d'Alembert*: "in a warm room with the floor covered with little pots, and on each of these pots a label: warriors, magistrates, philosophers, poets, potted courtesans, potted harlots, potted kings."[117]

But, as the layman may convince himself by perusing the expository literature on this issue as well as on some equally surprising claims on behalf of euphenics, practically every one of Lederberg's peers dissents

[112] J. B. S. Haldane, "Biological Possibilities for the Human Species in the Next Ten Thousand Years," in *Man and His Future*, ed. Wolstenholme, pp. 354 f. The idea was echoed by K. Atwood who at a subsequent symposium envisioned the production of organisms "with a large brain so that it can indulge in philosophy and also a photosynthetic area on its back so that it would not have to eat." See "Discussion—Part I" in *Control of Human Heredity*, p. 37.

[113] In addition to the article cited in note 110 above, see Joshua Lederberg, "Biological Future of Man," in *Man and His Future*, ed. Wolstenholme, pp. 263–273, and "Experimental Genetics and Human Evolution," *American Naturalist*, C (1966), 519–531 (reproduced also in *Bulletin of the Atomic Scientists*, October 1966, pp. 4–11).

[114] Lederberg, "Biological Future of Man," p. 266, and "A Crisis," p. 213. For these techniques, which in fact are what medicine has been doing of old, Lederberg coined a new term: "euphenics." See his "Biological Future of Man," pp. 265 f, and "Experimental Genetics," p. 524.

[115] Lederberg, "Experimental Genetics," p. 526; also his "A Crisis," p. 213.

[116] For a convenient summary of the most significant results in this direction, see Morgan Harris, *Cell Culture and Somatic Variation* (New York, 1965), pp. 10–20.

[117] Denis Diderot, *Le rêve de d'Alembert* (Paris, 1951), p. 54. My translation.

from his overenthusiastic prognostication.[118] To cite only a few opinions: G. Pontecorvo finds that "a knowledge of human genetics far greater than the impressive knowledge we already have of, say, the genetics of bacteriophage T4 is required for rational human engineering, be it of the eugenic or of the euphenic type," while P. B. Medawar counsels us "to forbear from grandiose prophetic or retrospective utterances about the genetic welfare of mankind." Bentley Glass even laments over the ill repute into which genetics has fallen because of the extreme views of a few who have chosen to ignore the vast area of our ignorance.[119] I think, however, that there are some elementary yet decisive reasons which do not require great familiarity with all the details of biological knowledge and which, *from where we stand now*, plead against most of the wonders heralded by Lederberg's evaluation of the potentialities of biology.

A few of the obstacles against the extrapolation of the Briggs-King nucleic surgery to man may be briefly mentioned at this juncture.[120] First, there is man's inherent interdiction of having a nanotweeze. Without a nanotweeze the nucleic surgery must leave on the hybrid egg some scars which at the submolecular level have immense proportions. Such scars, naturally, will interfere with the normal development of that egg. Second, there is the tenet shared by most molecular biologists that after a certain developmental stage a somatic nucleus loses completely its capacity for inducing new development.[121] Since no known fact has yet called into question this tenet, we must hold as a flight of fancy the project of developing a new organism from a somatic nucleus of a *mature* individual. Finally, the argument that the Briggs–King surgery must work in man, too—as K. Atwood in unison with Lederberg maintains[122]—ignores the

[118] Lederberg, himself, recognizes that the consensus of others disagrees with his position, but claims that the difference is only about the time—a few years against a few decades—when the "potted" mankind will become a reality ("Experimental Genetics," p. 531). A symptom of Lederberg's evaluation of what is in store for mankind is his earnest invitation to start preparing for an intelligent conversation with other worlds than our own ("A Crisis," p. 212, and "Biological Future of Man," pp. 270 f).

[119] G. Pontecorvo, "Prospects for Genetic Analysis in Man," in *Control of Human Heredity*, ed. Sonneborn, p. 89. (In the same volume, see the opinions expressed by Sonneborn, pp. viii, 125, and by S. E. Luria, pp. 15 f). P. B. Medawar, *The Future of Man* (New York, 1960), p. 62. B. Glass, "Summary and Concluding Remarks," *Cold Spring Harbor Symposia on Quantitative Biology*, XXIX (1964), 480.

[120] For more details see Appendix G, Section 4.

[121] E.g., James D. Watson, *Molecular Biology of the Gene* (New York, 1965), pp. 416 f; Harris, *Cell Culture*, pp. 149 f; G. Klein, "Discussion—Part II," in *Control of Human Heredity*, ed. Sonneborn, p. 94.

[122] See "Discussion—Part I" in *Control of Human Heredity*, p. 36. In the same place, Atwood alludes to the story of an "immaculate conception" of a female rabbit to conclude that the same "surely *could* be done" in man. Whether there are any solid facts behind the story is a moot question, as is manifest from a discussion among some experienced biologists in *Man and His Future*, ed. Wolstenholme, p. 115.

elementary fact that for the normal development of an amphibian egg a marshy pond or a jar of water suffices and that, by contrast, the human egg requires fantastically complex and immensely delicate conditions. Medical science, as everyone knows, has difficulty in saving even a baby that happens to leave the maternal womb only a few days prematurely.

Medawar once coined the word "geneticism" for labeling the complex associated with the claim that our genetic knowledge and understanding are far greater than they are in fact.[123] Similarly, we may use "biologism" to denote the scientific temper that now extols the imminent wonders of a new euphenics (read "medical science") and the possibility of a potted mankind. The emergence of biologism, like that of economism, sociologism, and all other such isms, has its own explanation.

Man has been so successful in controlling to his advantage one physical process after another that he could not stop short of believing that he can achieve the same feat in the other domains. Every epoch has had its share of formulae for constructing a rational society. In this century, "economism" culminated in the advocacy of a completely controlled economy as the economist's philosophical stone. But as it became progressively evident that, without the use of extreme coercion, people are generally unwilling to identify themselves with the plan imposed from above, we began paying increasing attention to the means of controlling the mind. And, just as had happened with the idea of a completely controlled economy, some of us suggested that the control of the mind is a normal event in the evolution of civilization.[124] Lederberg—as we have seen a while ago—assures us that the same is true of the genetic control of the human species through cloning.

Through all this we may see that what inflames the hearts of those who seek and advocate the control of man by one means or another is the vision of a "rational" man and, especially, of a "rational" society. And since the economic process is, as I have endeavored to argue in this volume, an extension of man's biological essence, the emergence of biologism is within the expected order of things. The dogmatic acclaim of Michurin's and Lysenko's ideas in the U.S.S.R. speaks loudly on this point. Biologism, I believe, represents also the last form in which the belief that science can help man to create himself anew may manifest itself.[125] Indeed, if we can grow men so that a ricksha man will have the special strength to pull his vehicle all day long and also wish for no other fate, then there is no

[123] Medawar, *Future of Man*, pp. 61 f.

[124] E.g., *Man and Civilization: Control of the Mind*, a symposium held at the San Francisco Medical Center of the University of California, ed. S. M. Farber and R. H. L. Wilson (New York, 1961).

[125] This is the main reason why I have thought it instructive to discuss in Appendix G the predicament of biology as I see it in the light of unadorned facts.

longer any need for controlling the mind. One may then expect that biologism will eventually attract the attention of economists who by now seem only too eager to answer any extemporary call for planning. Indeed, what could be the use of knowing how to clone people if we did not know what kinds of people to clone and in what proportions?

Paradoxically, the assumption that "we" have the power of the mythological Parcae to bestow any quality whatsoever on a child to be born uncovers the irreducible difficulty of man's planning a "rational" society. As Pigou, one of the most subtle economists of this century, argued long before anyone earnestly thought of a potted mankind, "any social reformer suddenly endowed with [such] omnipotence would find himself in sorry plight."[126] Obviously, such a reformer would have to know what qualities are best for every new human, which means that he would have to possess omniscience as well. And it is highly interesting indeed to see how quickly the advocates of any kind of genetic planning try to gloss over the question of what qualities are desirable. All we are offered are platitudes such as "high genetic quality," or "attested ability," or "good health, high intelligence, general benevolence."[127] Diderot could think only of kings, aristocrats, and courtesans, probably because at the time these were considered as the vital occupations for society. More significant is the case of H. J. Muller, who in trying to sell his famous proposal of artificial insemination from sperm banks mentioned only genial men, like himself—"Lenin, Newton, Leonardo, Pasteur, Beethoven, Omar Khayyàm, Pushkin, Sun Yat Sen, Marx"—or supergeniuses combining the qualities of such individuals.[128]

Very likely, we shall soon see the unpleasant consequences of what we, the learned, have tried to implant in the minds of people, for the sobering truth must sooner or later strike us in the face. And the sobering truth—whose emphasis is quite timely—is that a world made only of geniuses, nay, only of men of science, nay, only of Ph.D's, could not survive for one single minute (any more than one made only of the Versailles crowd could). Equally obvious is the fact that millions of "Einsteins" or "Debussys" from cuttings are not likely to cause another revolution in physics or in music. Any new revolution requires a different kind of mind from

[126] A. C. Pigou, *Essays in Applied Economics*, p. 82.

[127] Julian Huxley, "The Future of Man—Evolutionary Aspects," p. 17; Haldane, "Biological Possibilities," p. 352; and F. H. C. Crick, "Discussion," p. 294, all in *Man and His Future*, ed. Wolstenholme.

[128] H. J. Muller, *Out of the Night: A Biologist's View of the Future* (New York, 1935), p. 113. Muller did recognize later the difficulty of deciding what qualities are desirable, but fell nevertheless back on the perennial platitude, "outstanding gifts of mind, merits of disposition and character, or physical fitness." See his "Means and Aims in Human Genetic Betterment," in *Control of Human Heredity*, ed. Sonneborn, pp. 110, 115.

that which fostered the previous turning point: in science as well as in arts, progress comes from novelty, not from the mere growth in numbers of what already exists. The mere growth in numbers of the learned may be even deleterious beyond a certain relative level. A landslide of mediocre, irrelevant works, in addition to being a waste of society's resources, would increase the difficulties of communication and, ipso facto, place an unnecessary burden on those truly capable of expanding our knowledge.

A living world needs in the first place "productive" people: farmers, miners, carpenters, garbage collectors, bootblacks, ricksha men, and the like. Consequently, the Master Mind should plan for "potting" people in the right proportions for these occupations. Now, there can be little doubt that a clonant of "Debussy" will wish only to compose impressionist music. But what I refuse to accept is that all a clonant of a ricksha man will like to do is to pull a ricksha—even if one grants that in a "rational" society a ricksha man may be paid more than a bowl of rice per day. The observation should put an end to the dreams of a "rational" society free from any social conflict. The worshippers of a planned society will have to admit—as many do—that some outright controls must be a part of any such scheme.

It is so humanly normal that when one—be it you or I—insists that "we" need to control society, one should have in mind a picture in which he is one of "we," the controllers, not one of the controlled. But in the case of a scholar there is mainly his characteristic conviction that he knows better than others what their minds should think, feel, and desire. Plato's caste of philosopher–guardians looms large in the visions and aspirations of many servants of science who prejudge not only the issue of who shall control society but also that of the purpose of the control. For not even science has the right to dismiss dictatorially the most fundamental question: what are people for? To enjoy one's life for its own sake or to be a pawn in a society controlled by the oligarchy of Master Minds?

Ethical questions are not likely to be welcomed by the advocates of a planned society. Neither Muller nor any other supporter of his idea of a sperm bank stopped to ask if a woman, generally, would not prefer to bear a son like her farmer husband rather than one resembling someone she does not know, nor cares about. The attitude of many scientists toward such a question is well illustrated by Crick and Pirie, who call into question people's right to have children and argue that the desire to have children is the result of "the kind of stories you read, the kind of pictures you see."[129] The situation has never been more impressively

[129] See "Discussion" in *Man and His Future*, ed. Wolstenholme, pp. 275, 283. Apparently, these authors have never heard of the highest desire of peasant families from time immemorial!

epitomized than by Adeimantus in his apostrophe to Plato: "you are making these people miserable for their own good."[130] And indeed, one can but shudder at some of Haldane's thoughts—that the premature death of a few million people every year is a fair price for keeping biologists busy in the laboratory, or that in his vision of society parents would not mind risking their children's life in some biological experiments.[131] Medawar had other reasons in mind when he said that "human beings are simply not to be trusted to formulate long-term eugenic objective,"[132] but, as we can now see, the most important reason is that our enthusiasm for experimenting may induce us to advocate, unwittingly, procedures not much different from those in force at Auschwitz. The Eugenic Society may be unable, as Marett said, to take us "nearer to the angels or to the apes."[133] But it may, certainly, take us nearer to the devils. As an attentive anthropologist rightly protested before an audience of eminent biologists, "scientists who know nothing but science can imperil the safety of the world."[134]

The purely genetical problems are not less formidable. As I have insisted in Chapter VIII, evolution is not a mystical idea. It is the result of the qualitative Change continuously brought about by the emergence of novelty by combination and the unidirectional work of the Entropy Law. This Change, we remember, is the reason why man is unable to predict the evolution of any species or of the environment with the same accuracy of detail as he can, in principle at least, calculate the past and the future of a mechanical system. Frequently, we hear biologists proclaiming that genetical evolution is "a story of waste, makeshift, compromise, and blunder." This, in my opinion, is an arrogant judgment. Medawar and other biologists to the contrary, the statement "nature does *not* know best" is not a profound truth.[135] Instead, it is the illusion created by our own ignorance of all the laws of evolution. Almost every biologist decries the fact that evolution perpetuates species that will not fit the conditions of the future. But, curiously, none of those who deplored this fact noted that if natural selection had perfect foresight, species would have to be immortal, in outright contradiction with the Entropy Law. In judging

[130] Plato, *Republic*, IV. 419.

[131] See pp. 234 and 358 in *Man and His Future*.

[132] *Ibid.*, p. 295.

[133] R. R. Marett, *Head, Heart, and Hands in Human Evolution* (New York, 1935), p. 72.

[134] Carleton S. Coon, "Growth and Development of Social Groups," in *Man and His Future*, ed. Wolstenholme, p. 126.

[135] Medawar, *Future of Man*, p. 100. See also Theodosius Dobzhansky, *Genetics and the Origin of Species* (2nd edn., New York, 1941), p. 160, and again in "Human Genetics: An Outsider's View," *Cold Spring Harbor Symposia on Quantitative Biology*, XXIX (1964), 5.

natural selection we may be prone to be wise after the event, but this wisdom is spurious. Before pointing the finger at nature, we need to prove that a world in which, say, the human species had emerged directly from the primeval warm mud—thus by-passing countless species now defunct—would have been viable.

If in artificial selection it is a fact that, even though "we are trying to be wise before the event, . . . the event proves that we are all too often ignorant,"[136] how can we hope to succeed with the grand plan of taking our evolution in our own hands? The crucial difficulty becomes crystal-clear if we remember the superb epigram of Thoday: "The fit are those who fit their existing environments *and* whose descendants will fit future environments."[137] This algorithm—we should note—is not a definition of the fit (as Thoday intended), but one of the ideal, immortal species. The more ambitious the scheme for the genetic "betterment" of mankind, the stronger is its implicit belief in the possibility of an ideal species. The truth—which deserves to be repeatedly emphasized—is that any such scheme is more apt to drive mankind into a cul-de-sac than to change it progressively into an ideal species. And the human species may indeed be driven into a cul-de-sac if its powers of adaptation to unforeseen circumstances are progressively clipped by the continuous selection of the carriers of great talent, high intelligence, or athletic fitness alone, as Julian Huxley, J. B. S. Haldane, and, especially, Joshua Lederberg propose to do. These proposals actually tend to make the artificial selection of man even more disastrously opportunistic than natural selection is accused of being. We simply are unable to know in advance the kind of demands the biogeographical environment will make on the human species by the end of this century, let alone a hundred or thousand years from now. What is more, even if we knew these demands, we would still be unable to draw *now* a genetic plan for meeting them successfully.[138] Only a very long, perhaps even nonfeasible, experiment would enable us to discover who, among us, are carrying the right gene for each demand. Socrates, who certainly could have only a superficial understanding of a eugenic plan, exclaimed nevertheless in relation to Plato's system: "Good heavens! . . . what consummate skill will our rulers need if the same principle [as for animals] holds of the human species!"[139]

[136] Medawar, *Future of Man*, p. 49.

[137] J. M. Thoday, "Natural Selection and Biological Progress," in *A Century of Darwin*, ed. S. A. Barnett (Cambridge, Mass., 1958), p. 317. My italics.

[138] Very curiously, Haldane, who does not otherwise seem bothered by our ignorance, never abandoned his old opposition to racial intermixing on the ground that no one can be certain about its consequences and that no one could unshuffle the genes, should integration prove deleterious. Haldane, *Heredity and Politics*, p. 185, and "Biological Possibilities," p. 356.

[139] Plato, *Republic*, V. 459.

The capital sin of biologism (as well as of any social scientism) is the refusal to see that in a domain where prediction is impossible it is foolhardy to believe that there are means by which man can achieve *some chosen ends and only these.* In this respect, just as in ordinary life, man cannot get something for nothing. The only difference is that the price to be paid for achieving the biosocial ends through some man-made scheme cannot be known in advance. And the danger of any ism is that we may discover only too late that, in spite of the sales talk of the overeager scientist, there is a price to be paid and that the price is much greater than what we have been induced to buy. Molecular biologists and biochemists are more prone to biologism. Consummate biologists are, on the contrary, more likely to warn us that our present knowledge is "palpably insufficient to devise [genetic] remedies, about which we could be confident that the remedy will not be worse than the disease."[140] The whole truth, however, is that this is a lasting, not a temporary predicament, for, as I have argued in this volume, man will be forever denied divine knowledge.

Because in biological phenomena causes are traced more easily than in economics or politics, monuments to the heresy of biologism are quite abundant. That of thalidomide is perhaps the best known of all. Think also of many other "wonder drugs" whose unforeseen side effects led to the interdiction of their use. According to one of its discoverers, probably the same fate awaits the "pill."[141] But think, above all, of the probable ultimate outcome of the mass use of antibiotics, which is already contoured on the horizon. The emergence of drug-resistant strains is a well-known signal. We also are becoming increasingly aware of the fact that the problem of the ecological balance, even if limited to that between man and microorganisms, is so complex that no human mind can comprehend it. Any cure of an infectious disease vacates an ecological niche for other microorganisms, which may turn out to be much more dangerous than the dislocated ones.[142] Incredible though it may seem to the uninitated, a famous microbiologist gave this counsel to his equally distinguished colleagues of a symposium: "If a universal antibiotic is found, immediately organize societies to prevent its use."[143]

The well-known economist Colin Clark was, I think, only unnecessarily blunt but not wrong in denouncing, before a selected array of biologists, the new wave of eugenic and euphenic doctrines "supported by some

140 Dobzhansky, "Human Genetics," p. 3.

141 G. Pincus, "Discussion" in *Man and His Future*, ed. Wolstenholme, p. 109.

142 Even the immunization against poliomyelitis is now suspected of making room for new virus infections. See Hilary Koprowski, "Future of Infectious and Malignant Diseases," pp. 201 f, and Lord Brain, "Discussion," p. 367, both in *Man and His Future*.

143 Koprowski, p. 216.

brilliant and misguided scientists, and which . . . will attract its quota of humbugs."[144] Clark, like myself, is a layman in biology. But as long as the validity of the objections, such as those formulated in this section, is elementarily obvious, they cannot be set aside with the remark that a layman does not know all the technical details. No technical details can do away with fundamental obstacles of a lasting nature.

There is, of course, another weapon that may be hurled at an argument of the type I have presented here: the familiar accusation of being anti-scientific. Much though it may displease the would-be accusers, I wish to remark again that most of the scientific authorities are on my side. Witness the fact that Medawar immediately came to Clark's support with the confession that what frightens him is the extreme self-confidence of the authors of large-scale eugenic plans and their complete conviction that they know not only what ends are desirable but also how to achieve them.[145] And this is only one fraction of the sin of scientism. Still more telling is the statement made by James Shapiro, of the Harvard group which in November 1969 succeeded in isolating a pure gene. Anti-scientific—protested Shapiro recently—are those who "dump pesticides on Vietnam . . . perform heart transplants without first learning about rejection, and give masses of antibiotics to people who don't need them,"[146] briefly, those who interfere with the life processes without caring an iota about the unforeseen and incalculable consequences of their actions. In shocking contrast, only a few weeks thereafter Christiaan Barnard—as reported by the press—declared that "At Cape Town, what I am aiming at is the brain transplant." Had he thought about this project beyond the purely surgical dexterity, he would have certainly said "body transplant," not "brain transplant." My point is that in the operation Barnard is hoping to achieve it is the brain's donor, not the brain's recipient, whose life is saved. Barnard will certainly not be able to save the life of a genial scholar struck by a fatal cerebral tumor, for example, by transplanting the brain of a moron donor.

The mystery of life, of human life in particular, will always stir the imagination of specialists and laymen alike. A minority of scientists, therefore, will always come up with some fantastic euphenic or eugenic schemes as biological knowledge, like all knowledge, advances from one spectacular breakthrough to another. And just as now, the danger will never be that such wonder schemes will be knocking at the door and man will not be prepared to use them wisely—as some biologists and countless

[144] "Discussion" in *Man and His Future*, p. 294.

[145] *Ibid.*, p. 296.

[146] Quoted in James K. Glassman, "Harvard Genetics Researcher Quits Science for Politics," *Science*, February 13, 1970, p. 964.

journalists are clamoring about the present situation. The danger will always be the converse: the schemes will not be wisely probed and we will be only too eager to apply them. The "aspiration of the Fascist for a human state based on the model of the ant"—as Wiener described it[147]—will very likely attract increasing attention, energy, and salesmanship as it will become progressively evident that no social science can supply a formula for bringing about the "rational" society. But we may rest assured that although many miracles are still awaiting man's discovery, the coalescence of the endosomatic and exosomatic evolution of mankind will not be one of them. The reason is not the incompatibility between the mental nature of man and that of ant—as Wiener claims. It is simply the fact that to remake himself man needs both a knowledge and a power well beyond his reach. That is why, in the future as in the past, the human society will pass from the control of one elite to another and why each elite will have to influence not the genotypes of people, but their beliefs with the aid of a seemingly different, yet basically homologous, mythology.

6. Man and His Tradition. Like the social insects, man lives in society, produces socially and distributes the social product among his fellows. But, unlike the social insects, man is not born with an endosomatic code capable of regulating both his biological life and his social activity. And since he needs a code for guiding his complex social activity in a tolerable manner, man has had to produce it himself. This product is what we call tradition. By tradition man compensates for his "birth defect," for his deficiency of innate social instincts. So, man is born *with* an endosomatic (biological) code but *within* an exosomatic (social) one. It is because of the endosomatic code that a Chinese, for example, has slanted eyes and straight hair. It is because of the exosomatic code that a Filipino peasant cultivates his fields in the manner all Filipino peasants do, participates in the extravagant festivals held by his village at definite calendar dates, and so on. A biological process sees to it that the pool of genes is transmitted from one generation to another. Tradition does the same for what we call "values" or, more appropriately, "institutions," i.e., the modes by which every man acts inside his own community. The parallel calls for some punctuating.

First, a fundamental difference: the biological evolution is Darwinian; it does not transmit acquired characters. Tradition, on the contrary, is definitely Lamarckian, that is, it transmits only acquired characters, especially those that have proved to be useful to the community. Needless to say, tradition, just like biological heredity, often transmits institutions that are indifferent or deleterious. For thousands of years on end, one

[147] Norbert Wiener, *The* Human *Use of Human Beings: Cybernetics and Society* (Cambridge, Mass., 1950), p. 60.

Chinese after another was born with straight hair, a phenotypic character of no significance whatsoever. Similarly, the institution of hand shaking, although of no particular value, has survived among certain communities for centuries. In spite of cases such as these, it is unquestionable that every tradition has its superb *internal* logic. And it is because of this logic that we cannot set up a viable cultural matrix by choosing each one of its elements arbitrarily, any more than we can combine elements chosen at random into one chemical substance, or breed a chimera of plant and man (as that foreseen by K. Atwood). Provided that the metaphor is properly understood, the internal logic of tradition may be compared with the chemical bonds of a gene or of a whole nucleus. These bonds account for biological continuity of the human as well as of all other species. The internal logic accounts for the continuity of man's existence as a social animal. True, the articulations between the elements of a cultural matrix are not as inflexible as the chemical bonds. This is rather a merit, for the flexibility helps man to adapt himself more easily and more quickly to evolutionary changes brought about by his own inventions, by his growth in numbers, and by the evolution of the biogeographical environment. However, the same articulations are sufficiently strong to account for the inertia, sometimes quite remarkable, that most traditions have displayed under historical stress.

In connection with this inertia, I may note the piquant fact that the biologists who recently met in special symposia to discuss various schemes for the genetic control of mankind found themselves forced in the end to talk more about how to control tradition. Lederberg admitted openly that we are here because most of us present "believe that the present population is not intelligent enough to keep itself from being blown up."[148] That is not the only case in which tradition has been held responsible for man's evils. The usual judgment is that tradition is an obstacle to progress, and to some extent the judgment is right. On the other hand, had it not been for the inertia of tradition, every power-hungry dictator and every overconfident and overambitious scientist would have had no difficulty in subjecting mankind to their vast plans for a "rational" society, with the probable result that the human species would be defunct by now. The role of tradition in the life of mankind is, however, more comprehensive than that.

The economic process, to recall, does not go on by itself. Like any non-automatic process, it consists of sorting. Sorting, in turn, requires an agent of the kind illustrated by Maxwell's fable. Moreover, it is the sorting agent that constitutes the most important factor in any such

[148] "Discussion" in *Man and His Future*, p. 288.

process. For low entropy will turn into high entropy in any case. But it depends upon the type of sorting activity whether a greater or a smaller amount of environmental low entropy is absorbed into or retained by the process. In other words, it depends upon what sort of Maxwellian demon keeps the process going. It suffices to compare two different varieties of the same species living within the same environment in order to convince ourselves that not all Maxwellian demons are identical. Not even two specimens of the same race are always identical Maxwell demons.

In the case of a single cell, the corresponding Maxwellian activity seems to be determined only by the physico-chemical structure inherited by the cell; in the case of a higher organism, it is a function of its innate instincts as well. An eagle can fly because it is born both with wings and with the instinct to fly. But man too can fly nowadays even though he has neither a biological constitution fit for flying nor an innate instinct to do so. The upshot is obvious: the Maxwellian activity of man depends also on what goes on in his mind, perhaps more on this than on anything else. And it is the role of tradition to transmit knowledge as well as propensities from one generation to another.

The intense interest in the problem of the economic development of the "underdeveloped" countries has brought an increasing number of scholars and students in direct contact with numerous "traditional societies." At first, most argued that the people of such societies behave "irrationally" since their behavior differs from ours, from that of the Civil Society. But, gradually, numerous students have come to realize the importance that cultural propensities have in the economic process and also for the strategy of inducing economic development. Unfortunately, however, most policies of economic development still rest on the old fallacy bred by the mechanistic philosophy, the fallacy that *it is the machines that develop man, not man that develops machines*. Highly surprising though it may seem, the most frank and pinpointed recognition of the fallacy has come from a Soviet author: "It is not the machine created by man, but man himself who is the highest manifestation of culture, for the thoughts and dreams, the loves and aspirations of man, *the creator*, are both complex and great."[149]

Anthropologists and historians have long since thought that the introduction of any economic innovation in a community is successful only if the community can adapt itself culturally to it, i.e., only if the innovation becomes socially approved and understood.[150] Among the Anglo-

[149] S. T. Konenkov, "Communism and Culture," *Kommunist*, no. 7, 1959. English translation in *Soviet Highlights*, no. 3, I (1959), 3–5. Italics mine.

[150] G. Sorel in the "Introduction" to G. Gatti, *Le socialisme et l'agriculture* (Paris, 1902), p. 8; Richard Thurnwald, *Economics in Primitive Communities* (London, 1932), p. 34; V. Gordon Childe, *Social Evolution* (New York, 1951), p. 33.

American economists at one time only a rebel such as Veblen argued that it is dangerous to place modern machines in the hands of people still having a feudal economic *Anschauung*.[151] No doubt, "dangerous" is hardly the proper term here, but probably Veblen wanted to emphasize the immense economic loss as well as the great social evils resulting from a forced introduction of modern industries into a community deprived of the corresponding propensities.[152] But let us be frank about it: who can deny that the danger created by the discovery of atomic power derives from the cultural backwardness of mankind in relation to the new technology? All cultures have always lagged behind the technological progress of the time—some more, some less. But the lag, whether for mankind as a whole or for sections of it, has never been so great as at present.

The point has obvious implications for any policy aimed at speeding up the growth rate of an economy. They have been recognized sporadically and mainly by "unorthodox" economists. Leonard Doob, for instance, insisted that no planning can succeed unless it is based on a knowledge of the social environment, that is, of the tradition of the people who will be affected by it. An even stronger thesis is put forward by J. J. Spengler, who argues that the rate of economic growth depends upon the degree of compatibility between the economic and noneconomic components of the respective culture.[153] These observations should not be dismissed easily, for all analyses of why the results of our economic foreign aid often have not been proportional to its substance converge on one explanation: local mores.

Actually, there are a few facts which suggest that the influence of the economic *Anschauung* upon the economic process is far more profound than the authors quoted above suspected. I shall mention only the most convincing ones. Soviet Russia, at a time when she had hardly introduced any innovation besides central planning, felt the need to act upon the economic *Anschauung* of the masses: "The purpose of politically educative work [in the forced-labor camps] is to eradicate from convicted workers the old habits and traditions born of the conditions prevailing in the

[151] Thorstein Veblen, *Imperial Germany and the Industrial Revolution* (New York, 1964), pp. 64–66, and *Essays in Our Changing Order*, ed. L. Ardzrooni (New York, 1934), pp. 251 f.

[152] Or as P. N. Rosenstein-Rodan was to put it in "Problems of Industrialization of Eastern and South-Eastern Europe," *Economic Journal*, LIII (1943), 204, "An institutional framework different from the present one is clearly necessary for the successful carrying out of industrialization in international depressed areas."

[153] Leonard Doob, *The Plans of Men* (New Haven, 1940), pp. 6 f; J. J. Spengler, "Theories of Socio-Economic Growth," in *Problems in the Study of Economic Growth*, National Bureau of Economic Research (New York, 1949), p. 93. See also K. Mannheim, "Present Trends in the Building of Society," in *Human Affairs*, ed. R. B. Cattell *et al.* (London, 1937), pp. 278–300.

pattern of life of former times."[154] Strong though the pressure exercised through numerous similar educative works has been on the people of the USSR, the result was such that, at the Twenty-First Congress of the CPSU, Nikita Khrushchev still had to announce: "To reach communism . . . we must rear the man of the future right now."[155]

A far more familiar case in point is the great economic miracle of Japan. There is no doubt in my mind that only the peculiar economic *Anschauung* of the average Japanese can explain that miracle. For, I am sure, no expert on planning could draw up an economic plan for bringing an economy from the conditions of Japan in 1880 to those existing today. And if he could, he must have known beforehand that the people were the Japanese and also realized that the complete data in any economic problem must include the cultural propensities as well.

Nothing is further from my thought than to deny the difficulties of how to study the economic *Anschauung* of a society in which one has not been culturally reared. Nor am I prepared to write down a set of instructions on how to go about it mechanically. But if we deny man's faculty of empathy, then there really is no game we can play at all, whether in philosophy, literature, science, or family. Actually, we must come to recognize that the game is not the same in physical sciences as in sciences of man; that, contrary to what Pareto and numberless others preached, there is not only one method by which to know the truth.[156]

In physics we can trust only the pointer-reading instrument because we are not inside matter. And yet there must be a man at the other end of the instrument to read it, to compare readings and to analyze them. The idea that man cannot be trusted as an instrument in the process of knowing is, therefore, all the more incomprehensible. Curiously, physicists are aware of their handicap, that is, of the fact that they cannot interrogate nature: all they can do is to observe the *behavior* of matter. As one great physicist after another has pointed out, the student of man has additional means at his disposal. He can feel into facts, or resort to introspection, or, above all, find out the motives of his subject by interrogating him.[157] If *per absurdum* a physicist could converse with the electrons, would he refuse to ask them: why do you jump? Certainly not. Yet the physico-parallelism has been exaggerated by some social scientists to the venue that, since we cannot converse with inert matter, we should

[154] Resolution of the 1931 All-Russian Congress of Workers of the Judiciary in *Report of the Ad Hoc Committee on Forced Labor*, United Nations, ILO, Geneva, 1953, pp. 475 f.

[155] Quoted in Konenkov, "Communism and Culture."

[156] Pareto, *Manuel*, p. 27.

[157] E.g., Planck, *ibid.*, p. 105; Bohr, *ibid.*, p. 78; H. Margenau, *Open Vistas: Philosophical Perspectives of Modern Science* (New Haven, 1961), p. 198.

not converse with people either. There is a fundamental reason for physicists to embrace pure behaviorism, but pure behaviorism has no place in the sciences of man. As F. A. Hayek observed in his splendid denunciation of the behavioral dogma in social sciences, "when we speak of man we necessarily imply the presence of certain familiar mental categories,"[158] that is, the same mental categories as those possessed by the speaker. Even physicists felt it necessary to remind the social scientists who have decided to ignore the essence of their object of study that "the principal problem in understanding the actions of men is to understand how they think—how their minds work."[159] And as I have argued in many places of this book, no electrode, no microscope, indeed no physical contraption can reveal to us how men's minds work. Only one man's mind can find out how another man's mind works by using the bridge provided by the familiar mental categories and propensities that are common to both. Man may not be as accurate an instrument as a microscope, but he is the only one who can observe what all the physical instruments together cannot. For if it were not so, we should send some politoscopes to reveal what other people think, feel, and might do next—not ambassadors, counselors, journalists, and other kinds of observers; and as we have yet no politoscopes, we should then send nothing.

But perhaps one day we will all come to realize that man too is an instrument, the only one to study man's propensities. That day there will be no more forgotten men, forgotten because today we allegedly do not know how to study them and report on what they think, feel, and want. A "peace army," not only a "peace corps," is what we need. This, I admit, may be an utopian thought, reminiscent of the *Narodniki's* slogan, "To the people." But I prefer to be utopian on this point than on the New Jerusalem that uncritical scientism of one kind or another holds out as a promise to man.

[158] F. A. Hayek, *The Counter-Revolution of Science* (Glencoe, Ill., 1952), p. 79.
[159] Bridgman, *Reflections*, p. 450.

Appendices

APPENDIX A *On the Texture of the Arithmetical Continuum*

1. I trust that if, in Chapter III, Section 2, my metaphor of distinct beads without a string had been confined to natural numbers, it could not cause any kind of frowning. It remains then to see whether anything in the progressive construction of the arithmetical continuum from the basis of ordered but unstrung integers may possibly render the metaphor illicit. In this appendix I shall examine this problem from as broad an angle as its intimate connection with the theme developed in Chapter III seems to warrant.

Nowadays, the fact that there is a large gap between one integral number and its successor appears as the acme of self-evidence. Yet we can very well visualize integral beads strung together so that each bead would touch its neighbor(s). Should we then say that the set of integral numbers is a continuous aggregate without any holes between its elements? Perhaps people may have once thought of it in this way, although not in these sophisticated terms.

Be this as it may, for the sake of making my argument clearer, let us visualize the integral beads so arranged. When the need for fractional (rational) numbers arose, nothing stood in the way of pushing the integral beads apart to make room for the others. The insertion of the new beads changes the structure of the old set in only one respect of immediate interest. In the new set, between any two beads there is an infinite number of other beads; moreover, it is impossible to say which bead precedes or which bead succeeds a given bead. However, this difference does not mean that *now* the beads are held tightly together by a string. Ever since Pythagoras it has been known that the new set, too, is full of holes; that is, there is still no string. But if we were ignorant of the existence of these holes—as the Pythagoreans were in fact before their discovery of incommensurability—we all would be baffled by the suggestion that the set of rational "beads" is not held tightly by a string. What has happened thereafter is a familiar story.

With every new job invented for numbers, a new series of holes was discovered in what had been thought earlier to be a "continuous" entity. And no sooner were new holes discovered than they were filled up with new numbers. The process repeated itself several times until the aggregate of numbers reached its present extension known as the arithmetical continuum. Needless to add, during each of the preceding phases it was possible to proclaim that no continuity could be conceived beyond that represented by the number system known at that particular epoch. Yet *ex post* we know that any such proclamation, whenever issued, was wrong. Why should then the same proclamation by Bertrand Russell about the arithmetical continuum be free from the same kind of self-delusion? As Poincaré observed, "the creative power of the mind [has not been] exhausted by the creation of the mathematical continuum."[1] It would be foolish, therefore, to expect that no new jobs will be found for numbers and hence that no new holes will be discovered in the continuum of Dedekind and Cantor.

Actually, we need not wait, nor look far, for such jobs. To take an example familiar to the mathematical economist, the calculus of probability teaches that the probability that an absolutely continuous variable, X, shall assume a given value, x, is zero. Yet this "zero" covers an infinite gamut of distinct and relevant cases. There is the case in which it is *absolutely impossible* for X to assume the value x: the sample variance, for instance, cannot have a negative value. But "zero" also covers the case in which X *must* necessarily assume the value x now and then: in the abstract scheme, there are infinitely many men whose exact height is six feet. Also, men of six feet are, in some definite sense, more frequent than those of seven feet. Yet these differences cannot be expressed *directly* with the aid of the elements of the arithmetical continuum. True, we do shift the problem from a difference of zero probabilities to one of probability densities. But even this procedure does not enable us to distinguish between the case in which $X = x$ is impossible and that in which $X = x$, though possible, has a zero probability density.

And if, like most of us, an instructor in statistics has battled with immense difficulties in making his students fully understand why Prob

[1] Henri Poincaré, *The Foundations of Science* (Lancaster, Pa., 1946), p. 50.

$[x_1 \leq X \leq x_2]$ may be positive although Prob $[X = x] = 0$ for every x, $x_1 \leq x \leq x_2$, he should blame neither himself nor the students. These difficulties are exogenous to statistics. Their roots lie deep inside mathematical analysis itself. It may be that mathematicians, after having convinced themselves that the real number system is "perfect and connected," are now somewhat reluctant to entertain the thought that it might be "imperfect" by still having holes that could be filled up with new numbers.[2]

Be this as it may, the actual difficulties—as I hope to show in this appendix—stem from two sources. Surprising though this may seem, the first source is the confusion created by the underhand introduction of the idea of measure at a premature stage in arithmetical analysis. The second source (an objective one) is the impossibility of constructing a satisfactory scale for the infinitely large or for the infinitely small with the aid of real numbers alone.

2. Two points should be made perfectly clear from the outset. First, in order to mark differences such as that between the probability that a man shall be shorter than six feet and the probability that a man's height shall be six feet exactly, we do not necessarily have to attribute a real number to each element. Ordering all probabilities in one aggregate of unspecified elements would completely suffice. Second, as the considerations of the preceding section indicate, there is nothing to prevent us (i.e., no inconsistency in relation to order arises) from intercalating new elements in the "hole" obtained by cutting an ordered aggregate into two.

That the taproot of the concept of number is the operation of ordering elements and not that of measuring quanta is a mathematical commonplace nowadays. A more telling way of saying the same thing is that the basic role of the concept of number is to enable us to talk about the elements of an ordered aggregate. When stripped of technical garb, numbers are nothing but names that can be given to the elements of an ordered aggregate in a consistent fashion with the structure of that aggregate. For instance, the real numbers are the names that can be so given to an ordered aggregate having the characteristic properties of the aggregate known as the arithmetical continuum. It is the aggregate in

[2] The definition of a continuous set as a perfect and connected set belongs to G. Cantor, *Contributions to the Founding of the Theory of Transfinite Numbers* (New York, n.d.), p. 72. For a Dedekindian definition of the linear continuum, see R. L. Wilder, *The Foundations of Mathematics* (New York, 1956), pp. 140 f.

To recall, in mathematical jargon an ordered aggregate is "perfect" if every sequence of the aggregate has a limiting element within the aggregate and every element is a limiting element of such a sequence. An ordered aggregate is "connected" if between any two elements there is another element.

question—we should emphasize—that determines the way its elements must be named, not conversely.

The point that given any ordered aggregate of discretely distinct elements we can build another aggregate by intercalating some ordered aggregates between the elements of the first is no novelty. We may even use the capitalized form, Number, to denote a member of an ordered aggregate derived in this manner from the arithmetical continuum. But the point I wish to bring home is that, no matter how often we repeat this operation, the elements of the new aggregate cannot lose their character of being discretely distinct. In other words, we cannot reach dialectical continuity from a base of discretely distinct aggregates, however "dense" these may be.

At this juncture, it is worth mentioning also that "Zero" in the sense of Nothing is totally alien to the concept of order. In an ordered aggregate any element may be given the name of Zero just as any other name we may invent. But if by doing so we establish some connection between that element and Nothing, then we have implicitly adulterated the concept of order by an impurity of measure. Whatever we may say thereafter about that structure no longer pertains to pure order. The same is true of any use of "infinite" in connection with an ordered aggregate if the term implies actual infinity. I can hardly overemphasize my contention that all the axiomatic bases proposed for the system of natural numbers, such as the famous one by G. Peano,[3] are impure in this sense because they assume a first element before which there is Nothing.

3. To proceed systematically, let us denote as usual the arithmetical continuum by R and cut it into two subsets, the nonpositive and the positive numbers. In the hole thus created let us place a set $[\alpha]$ consisting of the ordered set of all positive numbers. This operation naturally leads to the ordering $Zero \prec \alpha \prec r$ for any α and any positive r. Symmetrically, we can intercalate the set $[-\alpha]$ between the subset of negative numbers of R and its *Zero* with the ordering $-r \prec -\alpha \prec Zero$. More generally, let us define an aggregate $[p]$ of Numbers spelled in the complex manner $p = (r, \gamma)$, where r and γ are any members of R, and order the aggregate by the following rules:

$$(1) \qquad \begin{aligned} (r_1, \gamma_1) &\prec (r_2, \gamma_2) \quad \text{if } r_1 < r_2; \\ (r, \gamma_1) &\prec (r, \gamma_2) \quad \text{if } \gamma_1 < \gamma_2. \end{aligned}$$

It is immediate that this ordering, which represents the familiar ordering of the points of the Euclidean plane (r, γ) by the lexicographic rule, is

[3] Cf. Wilder, *Foundations*, p. 66.

transitive. An extended operation of addition in $[p]$ suggests itself immediately:

$$(r_1, \gamma_1) + (r_2, \gamma_2) = (r_1 + r_2, \gamma_1 + \gamma_2). \tag{2}$$

With respect to this operation, $[p]$ is an Abelian group, its modulo being $(0, 0)$.

Let us now try to introduce a measure in $[p]$ that will preserve the ordering and the addition (2). We need consider only the subset of all p's such that $(0, 0) \prec p$ or $p = (0, 0)$. The order-preserving condition requires that

$$\text{Meas } (0, 0) < \text{Meas } (0, \gamma) < \text{Meas } (r, 0) \tag{3}$$

for any $\gamma > 0$. From (2), we obtain $n \times (1, 0) = (n, 0)$ for any integer n, and from this, by a well-known procedure, we can derive $r \times (1, 0) = (r, 0)$. This relation induces us to define

$$\text{Meas } (r, 0) = r, \quad r \geq 0, \tag{4}$$

and replace (3) by

$$0 < \text{Meas } (0, \gamma) < r \tag{5}$$

for any $r > 0$. A fundamental principle of the theory of measure, which says that a measure smaller than any positive number is zero, is invoked at this stage.[4] On its basis, from (5) it is concluded that for any $\gamma > 0$

$$\text{Meas } (0, \gamma) = 0, \tag{6}$$

a conclusion to which I shall return later on (Section 10).

Let us also note that from (1) and (2) it follows that for $\gamma > 0$, $r > 0$,

$$\mathbf{S}_i(0, \gamma) = (0, n\gamma) = n(0, \gamma) \prec (r, 0), \quad i \in [n] \tag{7}$$

where the sum involves as many terms as the power of the set $[n] = (1, 2, \ldots, n)$. Consequently, the set $[p]$, in contrast with R, does not satisfy the Archimedean axiom; in other words, no Number $(r, 0)$ can be reached by repeated addition of a $(0, \gamma)$. We can express this fact by saying that with respect to $(0, \gamma)$ any $(r, 0)$ is an *infinitely great Number*.

[4] This principle had been implicitly used in arithmetical analysis long before the modern theory of measure was inaugurated by Émile Borel. Borel himself used it implicitly in his analysis of sets of measure zero (Émile Borel, *Leçons sur la théorie des fonctions*, Paris, 1898, p. 47), but was more explicit in his *Les nombres inaccessibles* (Paris, 1952), p. 128. Generally, however, the principle is only imperfectly expressed: for example, "the linear measure of the set of points in a linear interval (a, b) is taken to be $b - a$" regardless of whether the end points are included or not, or "a linear interval has the plane measure zero." E. W. Hobson, *The Theory of Functions of a Real Variable and the Theory of Fourier's Series* (2 vols., New York, 1957), I, 165.

To see the full implication of this conclusion, let us note that we can establish for the subset $[(0, \gamma)]$ a scale by the same procedure as that used for $(r, 0)$. On this *second* scale,

$$\text{meas}\,(0, \gamma) = \gamma, \qquad \text{meas}\,(r, 0) = \infty, \tag{8}$$

the last relation being obtained from meas $(0, \gamma) <$ meas $(r, 0)$ by invoking another principle of measure, namely, that a number greater than any positive number is infinite, ∞.

Relations (8) are the obvious correlatives of (4) and (6). And since we want the measure to be compatible with the operation of addition, we have in general

$$\text{Meas}\,(r, \gamma) = r \tag{9}$$

and

$$\text{meas}\,(r, \gamma) = \infty \quad \text{or} \quad \gamma, \tag{10}$$

according to whether $r > 0$ or $r = 0$.

4. Nothing need be added for us to connect the set $[(0, \gamma)]$ with the concept of infinitesimal that sprang from Newton's fluxions and, over the years, has been the target of controversies in which some of the greatest mathematicians participated.[5] Not later than ten years after Newton's death, a famous philosopher, Bishop Berkeley, denounced the infinitesimals as "the ghosts of departed quantities"; in our century another famous philosopher protested that "the philosophy of the infinitesimal . . . is mainly negative."[6] Often, we also read that G. Cantor and G. Peano proved "the non-existence of actually infinitely small magnitudes."[7]

The truth is that they proved only that these infrafinite numbers—as is preferable to call them in order to avoid any confusion with the infinitesimals of the calculus—do not satisfy all the axioms of the arithmetical continuum. In view of this difference it may be legitimate for one to judge that "*in Arithmetical Analysis the conception of the actually infinitesimal has no place.*" It certainly has no place, but only because (as we shall see in Section 10 below) it was banished from mathematical analysis by a hidden postulate. To argue then—as Hobson did in continuation—that the infrafinite number is a "variable in a state of flux, never a number, . . . a form of expression, appealing as it does to a mode of

[5] Cf. Cantor, *Contributions*, p. 81; Hobson, *Theory of Functions*, I, 57 f.

[6] George Berkeley, "The Analyst or, A Discourse Addressed to an Infidel Mathematician," *The Works of George Berkeley* (4 vols., Oxford, 1901), III, 44; Bertrand Russell, *Mysticism and Logic* (New York, 1929), p. 84.

[7] Philip E. B. Jourdain in the Preface to Cantor, *Contributions*, p. 81.

thinking which is essentially non-arithmetical" is to treat a serious issue in the Berkeley-Russell vein.[8]

True, the substitution of an infrafinite domain in place of every real number, as in the construction of $[p]$, abolishes the Archimedean property of the real number system.[9] But so does the completion of R by the transfinite cardinal numbers, Cantor's Aleph-numbers. These satisfy

$$(11) \qquad \mathrm{S}_i \aleph_k < \aleph_{k+1}, \quad i \in I,$$

even if I has the power of $\aleph_k$. As a matter of fact, in support of the infrafinite we can invoke Cantor's own defense of the transfinite: "All so-called proofs of the impossibility of actually infinite numbers are . . . false in that they begin by attributing to the numbers in question all the properties of finite numbers, whereas the infinite numbers . . . must constitute quite *a new kind of number*."[10]

We should expect, therefore, some propositions about the infrafinite to irritate our ordinary common sense just as others, about the transfinite, did at first. For example, the relations established in the foregoing section lead to

$$(12) \qquad \text{Meas}\,(r, 0) + \text{Meas}\,(0, \gamma) = \text{Meas}\,(r, 0).$$

Undoubtedly, this is what Johann Bernoulli, S. D. Poisson, and many other classical mathematicians meant by saying that "a quantity which is increased or decreased by an infinitely small quantity is neither increased nor decreased." This way of expressing their thoughts may not be quite fortunate. But to denounce the idea itself as bordering "on the mystical and the absurd,"[11] is a symptom of an unfortunate partialism. For nowadays we no longer find anything mystical or absurd in the correlative relation of (12),

$$(13) \qquad \text{meas}\,(r, 0) + \text{meas}\,(0, \gamma) = \text{meas}\,(r, 0).$$

On this very idea—that with respect to an infrafinite scale all finite numbers have an infinite measure just as all transfinite numbers have an infinite measure on a finite scale—G. Veronese erected his geometry of the infrafinite and transfinite.[12] To put it more generally, there are—as we

[8] Hobson, *Theory of Functions*, I, 43.

[9] For completeness we may add that another property of R—separability—also goes overboard. Cf. Wilder, *Foundations*, pp. 140 f.

[10] Cantor, *Contributions*, p. 74. My italics.

[11] H. Eves and C. V. Newsom, *An Introduction to the Foundations and Fundamental Concepts of Mathematics* (New York, 1958), p. 186. Also E. T. Bell, *The Development of Mathematics* (New York, 1940), p. 263.

[12] Giuseppe Veronese, *Fondamenti di Geometria* (Padova, 1891), of which there is also a German translation, *Grundzüge der Geometrie* (Leipzig, 1894).

shall argue presently—an infinite (in both directions) succession of classes of Numbers, each class having its own scale; every class is *finite* with respect to its scale, *transfinite* with respect to that of the immediately preceding class, and *infrafinite* with respect to that of its successor. Which scale we may choose as the finite scale is a matter as arbitrary as the choice of the origin of the coordinates on a homogeneous straight line.[13]

5. It is beyond question that there are countless instances in which without the infrafinite it would be impossible to express the differences that the analytical hairsplitting has created and continuously creates in its apparently irreversible march. The set $[p]$, a first and much imperfect step though it is into the world of the infrafinite, furnishes some simple illustrations. Thus, if the range of possible values of an absolutely continuous stochastic variable, X, is (A, B), the probability of $X = x, A \leq x \leq B$, is represented by some $(0, \gamma)$, whereas the probabilities of $A < x_1 < X \leq x_2 < B, A < x_1 \leq X < x_2 < B$ are represented by two different p's, (r, γ_1) and (r, γ_2). For a still more instructive illustration, let us consider the simple case in which Prob $[X = x] = (0, \gamma)$ for any $A \leq x \leq B$. Then, by a fruitful analogy with Lebesgue integral, we can put

$$\mathbf{S}_i(0, \gamma) = [\gamma(B - A), 0], \quad i \in (A, B), \tag{14}$$

where $\mathbf{S}_i$ is a sum in which there is one term for every element of the interval (A, B). We have only to replace each side of the relation (14) by the corresponding measures in order to transform it into the simplest case of a Lebesgue integral. What (14) says, in addition, is that, although the Archimedean Axiom does not work for the case of a *countable* sum of infrafinite numbers, it might work if the power of the sum is that of the arithmetical continuum. That this is not always true is shown by the fact that according to the self-same idea of Lebesgue,

$$\text{Meas}\,[\mathbf{S}_i(0, \gamma)] = 0, \quad i \in \Gamma, \tag{15}$$

where Γ denotes the famous ternary set of Cantor.[14]

[13] On this very basis (the homogeneity of the straight line), Veronese (*Fondamenti*, pp. 101–103) argued that, in contrast with Cantor's transfinite ordinal sequence $\omega, \omega + 1, \omega + 2, \omega + 3, \ldots$, we should conceive the infinite ∞_1 not only followed by $\infty_1 + 1, \infty_1 + 2, \infty_1 + 3, \ldots$, but also preceded by $\ldots, \infty_1 - 3, \infty_1 - 2, \infty_1 - 1$. "There is no first infinite number" because in the homogeneous infinity there are "many numbers $\infty_1 - n$, distinct from ∞_1, between the finite numbers and the number ∞_1." It is instructive to relate this position to the fact that in the Cantorian system the proposition that $\aleph_0$ is the *first* transfinite number has been proved only with the aid of Zermelo's controversial Axiom of Choice (mentioned in note 24 of Chapter III). See Hobson, *Theory of Functions*, I, 208.

[14] For which see, for instance, B. R. Gelbaum and J. M. H. Olmsted, *Counterexamples in Analysis* (San Francisco, 1964), pp. 85–87.

6. There is no denial that the veil of numerical measure screens the infinite spectrum of differences that do exist. Such is, for instance, the difference between $\mathbf{S}_i(0, \gamma)$, $i \in \Gamma$, and $\mathbf{S}_i(0, \gamma)$, $i \in N$, where N is the set of all positive integers. The problem is whether there is some scale on which all these differences can be systematically portrayed.

The first step toward the construction of such a scale implies the solution of a relatively simpler problem: what kind of infrafinite number should replace $(0, \gamma)$ in (15) or in $\mathbf{S}_i(0, \gamma)$, $i \in N$, in order that these sums shall have a finite measure. In other words, is there an infrafinite number π such that Meas $(\mathbf{S}_i\pi) = 1$ for $i \in N$? Even though this last question may seem to be the simplest of its class, it is particularly suited to show how immensely complex are the issues stirred by the concept of the infrafinite.

The question is related to the problem of choosing a positive integer *completely at random*, that is, by a procedure such that the probability of choosing any integer shall be the same. Within the arithmetical analysis, the answer is that this probability, π, is zero. It is a paradoxical answer because, if $\pi = 0$, the probability of choosing a number not greater than n is $\mathbf{S}_i\pi = 0$, $i \in [n]$, for any n. We must then conclude, as Borel observes, that the number chosen will certainly be an "inaccessible number," that is, a number that surpasses the limit, not of our imaginative power, but of our capacity of dealing with it in actuality. On the basis of this paradox Borel argues that the uniform distribution over a denumerable set is a mathematical absurdity. So, one must necessarily assign smaller probabilities to the inaccessible numbers—the more inaccessible they are, the smaller their probabilities.[15] Borel does admit that this conclusion is based to a large extent on *practical* considerations. Let us note, however, that if practical considerations were made the criterion of separation between sense and nonsense much of what passes for high mathematics would represent nonsense.

Borel's argument, however, can be applied even if the existence of an infrafinite number, π, such that Meas $(\mathbf{S}_i\pi) = 1$, $i \in N$, is assumed: for then necessarily Meas $(\mathbf{S}_i\pi) = 0$, $i \in [n]$. And this does not constitute a different paradox from that arising from the contrast between (14) and (15). The case of denumerable probabilities cannot, therefore, be singled out on this basis. In my opinion, what singles out a denumerable sum from a continuous sum, such as (14) or (15), is the fact that there is yet no concept of measure within a denumerable set. This is the explanation for the fact,

[15] Borel, *Les nombres inaccessibles*, pp. 37–42. Incidentally, Borel's concept of an inaccessible number supplies a specially interesting illustration of the omnipresent dialectical concepts in my own sense. There is only an imperfectly defined boundary (i.e., a penumbra limited by other penumbras) that separates the accessible from the inaccessible numbers. Yet this does not prevent us from being sure that 11 is an accessible and 1000^{1000} an inaccessible number (Borel, *ibid.*, p. 4).

noticed by Borel, that although the mathematician's notion of an unlimited series of integers is "apparently so clear and precise," the problem of probability in the denumerable is more intricate than in the complex structure of the continuum.[16]

Following an idea of Borel we may admit that the set of points of abscissae $(10, 10^2, 10^3, \ldots)$ is more rarefied than those of abscissae $(1, 2, 3, \ldots)$.[17] In case the order of the elements is given by the objective data of the problem, we can easily use Borel's observation for constructing a measure. The measure of the subset $[mn]$ of N for a given m is $1/m$, that of $[10^n]$ zero. However, the concepts of a denumerable set implies only that there is *some*, not a *given*, way of ordering the elements exhaustively in a sequence. Almost all denumerable sets of points in a space of higher dimension than the first are not associated with some "natural" order. This point finds an excellent illustration in a paradox which is used by Borel against the uniform distribution over a denumerable set and which is independent of whether we admit the existence of the infrafinite π or not.

F. Hausdorff has shown that on a sphere we can construct three disjoint denumerable sets A, B, C, such that one rotation brings A in coincidence with $C + B$, and another rotation brings A in coincidence with B, B with C, and C with A. From the apparently inevitable idea that the probabilities of choosing a point from sets of points that are congruent in the Euclidean geometry are equal, it follows that Prob $[x \in A] =$ Prob $[x \in B]$ + Prob $[x \in C]$ by the first rotation, and Prob $[x \in A] =$ Prob $[x \in B] =$ Prob $[x \in C]$ by the second. All these probabilities then must be zero. But if A, B, C are the only sets from which a point is chosen, the same probabilites must add up to unity. Hence, the paradox.[18]

7. It is elementary that the infrafinite number τ that would satisfy

[16] Émile Borel, *Probability and Certainty* (New York, 1963), p. 79; Borel, *Les nombres inaccessibles*, p. 100. The issue is more perplexing than Borel's remark suggests. As pointed out by Paul Lévy, *Théorie de l'addition des variables aléatoires* (Paris, 1937), p. 25, for the probability distributions over a set with a higher power than the continuum "there is a complete chaos." This situation is undoubtedly due to the fact that the concept of measure—let us admit it—is based on our intuitive notions of length, area, and volume. Perhaps the refusal to have anything to do with the infrafinite prevents us from approaching the problem from the direction suggested above: what kind of infrafinite σ corresponds to Meas $(\mathbf{S}_i\sigma) = 1$, $i \in F$, if F is a set with a given power higher than that of the continuum?

[17] Borel, *Les nombres inaccessibles*, pp. 85 f. Naturally, if we allow the sets to be "reshuffled" in the way used often in the analysis of the denumerable power, then rarefaction loses all meaning. But let us not forget that reshuffling would destroy measure even in the continuum: by reshuffling Γ we can attribute to it any measure we may please. These are mathematical issues still not elucidated.

[18] Borel, *Les nombres inaccessibles*, pp. 95–100, 124–126; Borel, *Les paradoxes de*

Meas $(\mathbf{S}_i\tau) = 1$, $i \in \Gamma$, must belong to a different class than $(0, \gamma)$ in (14) or π in Meas $(\mathbf{S}_i\pi) = 1$, $i \in N$. The fact that the product of two infrafinite numbers $(0, \gamma_1) \times (0, \gamma_2)$ cannot belong to $[p]$ without contradicting its non-Archimedean structure, also leads us to the same conclusion. We are thus induced to define $(0, \gamma_1) \times (0, \gamma_2) = (0, 0, \gamma_1\gamma_2)$, where $(0, 0, \gamma_1\gamma_2)$ is an infrafinite number of the second order. This obviously draws us into the same infinite regress as that so keenly described by Cantor: "in the successive formation of number-classes, we can always go farther, and never reach a limit that cannot be surpassed—so that we never reach an even approximate [grasping] of the Absolute. . . . The Absolute can only be [conceived], but never [realized], even approximately."[19] The difference is that the infrafinite moves in the opposite direction, from finite to the Absolute Nothing which, if one stops to think about it, is a philosophical notion as baffling as the Absolute Infinity. Borel even thought that "the infinitely small, although apparently closer to us and more familiar than the infinitely large, is relatively speaking more difficult to measure and to understand."[20]

One very simple idea to deal with the infinite regress of infrafinite classes is to define a Number

$$\rho = (r_1, r_2, r_3, \dots) \tag{17}$$

such that r_i is an infrafinite number of the first order with respect to r_{i-1}, of the second order with respect to $r_{i-2}, \dots$ and a transfinite number of the first order (not in Cantor's sense) with respect to r_{i+1}, and so on. The idea of ordering this new aggregate by the lexicographic rule fits in naturally. The operations of addition and multiplication as well as the definitions of measure on various scales are easily extended to $[\rho]$ in the

l'infini (2nd edn., Paris, 1946), pp. 198–210. Borel even complicates the paradox by using Zermelo's Axiom of Choice to divide the whole sphere into three sets having the same properties as A, B, C. In this case, however, one may object that since these sets are not measurable, one should not speak of probability in connection with them. In mathematics "probability" and "measure" are interchangeable concepts. Consequently, it is far from certain that the Hausdorff paradox proves—as Borel, *Les paradoxes*, p. 210, argues—that Zermelo's axiom must be discarded.

[19] Cantor, *Contributions*, p. 62*n*. Cantor, manifestly, had in mind the Absolute Infinite of Hegel, that is, that infinite, Ω, for which it is not permissible to write $\Omega + 1$, because there is nothing that is not already covered by Ω. Consequently, neither the Burali-Forti nor the Bertrand Russell antinomies can work in the case of Ω. In point of fact, the solution proposed by Bertrand Russell to his antinomy—namely, to eliminate the concept of "the class of all classes" from Logic—is tantamount to saying that Logic should recognize that Ω is the only class that cannot be a member of any class that contains other members. (For the antinomies mentioned, see Wilder, pp. 55 f, 124.)

[20] Émile Borel, *Probability and Certainty*, p. 84.

same fashion as used for $[p]$. The operation of division is then straightforward, as can be illustrated by the simple case of

$$\rho : (1, \gamma) = \rho \times (1, -\gamma, \; \gamma^2, -\gamma^3, \ldots). \tag{18}$$

The novelty of Veronese's idea in relation to Dedekind's is that the geometrical line consists of all points that have a $[\rho]$ as abscissae, not only of those whose abscissae are real numbers. But so thoroughly are we conditioned to think of the line only according to Dedekind's postulate,[21] that we are apt to treat the representation of the line by a Veronese continuum as a mathematical absurdity. Yet none other than David Hilbert, the founder of the purely axiomatic geometry, showed that a non-Archimedean geometry, such as that of Veronese, works just as well as that of Dedekind.[22]

8. In view of the numerous dogmatic proclamations that the *actual* infinitesimal simply does not exist, we should not fail to mention a very elementary application of $[\rho]$ in which the infrafinite is so actual that even a mid-grader might be able to draw it on a piece of paper. Ordinary straight angles, such as B_1OX in Figure 3, are measured by a scale of finite real numbers. But one may also consider—as even the Greek geometers did—horn-shaped angles, formed by a straight line and a curve or by two curves. A_1OB_1 and A_2OA_3 are examples of such angles. It stands to reason that the horn-shaped angle A_1OX should be considered greater than A_2OX: the corresponding straight angles formed by the tangents to OA_1 and OA_2 satisfy the inequality $B_1OX > B_2OX$. It also stands to reason that the horn-shaped angle A_1OX should be considered greater than the straight angle B_1OX. The problem reveals its sharper aspects if we consider the horn-shaped angles A_3OX and A_4OX (OA_3 and OA_4 being tangent to OX at O). Since for both of these angles the corresponding straight angles are zero, their difference can be shown only on another scale, an infrafinite scale between the finite and zero. And on this scale, in turn, it is not possible to represent all the second-order differences, i.e., the differences between the differences displayed by angles such as A_3OX and A_4OX.

The manner in which this perplexing issue can be, in part, resolved

[21] R. Dedekind, *Essays on the Theory of Numbers* (Chicago, 1924), pp. 6–8; see also note 22, Chapter III.

[22] David Hilbert, *The Foundations of Geometry* (2nd edn., Chicago, 1921), pp. 34–36. In relation to my thesis about the granular texture of the arithmetical continuum it is highly significant to mention that Dedekind, *Essays*, pp. 37 f, correctly speculated that the validity of Euclid's *Elements* is not affected if, on the contrary, we "thin" the line by excluding from it all points whose abscissae are transcendental numbers.

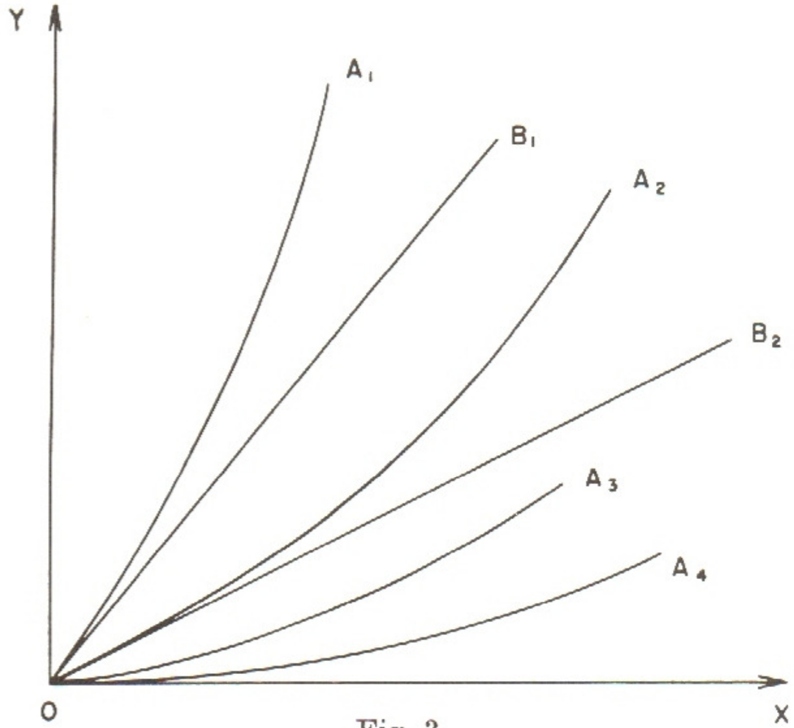

Fig. 3

with the aid of $[\rho]$ becomes perfectly clear if we consider only the curves, (OA), that in the neighborhood of the origin are convex and represented by an analytical function

$$y = r_1 x + r_2 x^2 + r_3 x^3 + \cdots, \qquad r_1 \geq 0, r_2 \geq 0. \tag{19}$$

The aggregate of the horn-shaped angles formed by these curves with OX at O constitutes a glaring example of an ordered aggregate of quantum elements that cannot be represented by the arithmetical continuum because this continuum is not rich enough in "beads." To turn to some simple geometry, if for two curves, C' and C'', we have $r_1' > r_1''$, the greater horn-shaped angle formed with OX is that of C'. If, as is the case for OA_3 and OA_4, $r_1' = r_1''$, but $r_2' > r_2''$, the greater angle is that of C', although the difference between the two angles measured on the same scale as that of the preceding case is zero. Any horn-shaped angle, such as A_3OX, represents some infrafinite number. The infrafinite, therefore, far from being a ghost of departed quantities, is there "in flesh and blood" directly in front of us for anyone who wishes to see it.

The class of functions (19) may be extended, first, to include even nonanalytical functions provided they have derivatives of all order for $x = 0$. In this connection, Felix Klein makes the highly interesting observation that the horn-shaped angle made by the function $y = Ae^{-1/x^2}$, $A > 0$, whose derivatives of all orders are zero for $x = 0$, is smaller than any angle formed by a curve (19).[23] However, it would be a

[23] Felix Klein, *Elementary Mathematics from an Advanced Standpoint: Geometry* (New York, 1939), p. 206.

gross mistake on our part to conclude from this observation that Klein's functions fill all the "space" between the finite and zero, and a colossal mistake to think that we can reach zero by such functions. Zero corresponds only to $y = 0$; and $Ae^{-1/x^2} > 0$ for any $A > 0, x > 0$. A still finer point is that there are functions—$y = Ae^{-1/x^4}$, for example—forming a horn-shaped angle smaller than those of Klein's functions, and still others forming an angle smaller than those formed by the "super-Klein" functions, and so on *ad infinitum*. We thus see that $\rho = [0, 0, 0, \ldots]$, too, covers an endless variety of horn-shaped angles, not only the angle of absolutely no content. Besides, the horn-shaped angles of many curves, although different, are represented by the same ρ, the reason being now of a different nature. For example, the angles formed with OX by the curves of $y = x^{3/2}$ and $y = x^{4/3}$ correspond to the same $\rho = (0, +\infty, -\infty, +\infty, \ldots)$. We are thus compelled to see how we can distinguish between one ∞ and another ∞. All this proves that even $[\rho]$ is not rich enough to describe all possible differences between horn-shaped angles.

9. The correlative point for the march from finite to the Absolute Infinite has been known for some time now. I refer to the discovery by Paul du Bois-Reymond concerning an infinite sequence of increasingly growing functions

$$\varphi_1(x) \prec \varphi_2(x) \prec \varphi_3(x) \prec \cdots \prec \varphi_n(x) \prec \cdots, \tag{20}$$

"increasingly growing" meaning that, given any $K > 0$, there is an $X_n > 0$ such that for any $x > X_n$ we have $\varphi_{n+1}(x) > K\varphi_n(x)$. The famous theorem proved by Bois-Reymond says that for any sequence (20) we can find a still faster growing function, φ, i.e., such that $\varphi_n \prec \varphi$ for any n. A second theorem states that for any φ satisfying this condition, we can find a function ψ_1 such that $\varphi_n \prec \psi_1 \prec \varphi$ for any n. By reiteration, we obtain the ordinal pattern

$$\varphi_1 \prec \varphi_2 \prec \varphi_3 \prec \cdots \prec \psi_3 \prec \psi_2 \prec \psi_1 \prec \varphi. \tag{21}$$

This pattern proves that there is no Archimedean scale for all classes of the infinitely large.[24]

[24] G. H. Hardy, *Orders of Infinity: The 'Infinitärcalcül' of Paul du Bois-Reymond* (Cambridge, Eng., 1924), pp. 11 f. Since the ordering of (21) recalls the ascending and descending sequences by which irrational numbers are defined in one familiar approach, it is well to point out—for subsequent use—that (21) does not necessarily define a cut function, χ, i.e., such that $\varphi_n \prec \chi \prec \psi_m$ for any n and m. To wit, let (ψ) be the class of functions such that $\int^\infty \psi^{-1}\, dx$ exists and (φ) the class of those for which the same integral does not exist. Obviously, no function corresponds to this cut. For this and other highly interesting remarks on the problem of scale, see Borel, *Leçons sur la théorie des fonctions*, pp. 111–119.

Undoubtedly, the roads from the finite either to the Absolute Zero or to the Absolute Infinite through the dominion of Analysis are equally long and never reach their ultimate destinations. It may be difficult for us to realize this fact if we insist on looking only at the sparse stakes placed on these roads by the arithmetical continuum.

10. To round up the technical argument of this appendix we need to return to the point, mentioned earlier, concerning the hidden postulate by which the infrafinite is barred *ab initio* from arithmetical analysis. A fable will enable us to silence some of the preconceptions that our arithmetical habit of thought may cause us to hold unawares.

Let us imagine a fabulous lamp that switches itself "on" and "off" indefinitely according to the following time schedule. The lamp is on at $t = 0$; thereafter, at every instant

$$(22) \qquad t_n = 1 + \frac{1}{2} + \frac{1}{2^2} + \cdots + \frac{1}{2^{n-1}}, \qquad (n = 1, 2, 3, \cdots),$$

the lamp switches itself "off" or "on" according to whether n is odd or even, time meaning clock-time.[25] Several points should now be clear, although not all may be immediately obvious.

First, since any t_n is a rational number *smaller* than $t^* = 2$, by no kind of logic can we regard t^* as a member of the aggregate $(t_1, t_2, t_3, \ldots)$. Second, the state of the lamp—there are four states in all—is completely determined at any instant t if t is a real number such that $0 < t < t^*$. Third, *without additional information* it is impossible to ascertain the state of the lamp at $t = 100$ or even at t^*: the fable does not say a word about these states.[26] The lamp may, for instance, be "vanishing" at t^* without contradicting the fable. Fourth—and the most crucial point—the lamp may very well vanish even at an instant t' earlier than t^*.

We must expect the fourth statement to be denounced on the spot as utterly absurd by almost everybody. If t' is an instant earlier than t^*, then—it will be countered—there is a k such that t_k is a later instant

[25] The apparently obvious point that no actual lamp could actually flicker in the manner described, which in its essence goes back to Aristotle, *Physics*, 263[b] 15–35, has not been taken for granted by all philosophers. It was Bertrand Russell, "The Limits of Empiricism," *Proceedings of the Aristotelian Society*, XXXVI (1935/6), 143 f, who launched the notion that to perform an infinite number of distinct tasks in a finite time interval is not a logical absurdity. The fabulous lamp enabled, however, J. F. Thomson, in "Tasks and Super-Tasks," *Analysis*, XV (1954), 1–13, to refute the idea. Recently, however, there seems to be a growing enthusiasm for blueprints of machines that, allegedly, can perform such super-tasks as to print all the decimals of π or to recite all integers within a finite time interval. Cf. A. Grünbaum, "Are 'Infinity Machines' Paradoxical?" *Science*, January 26, 1968, pp. 396–406.

[26] Cf. Thomson, just cited, pp. 5 f.

than t'. And since according to the fable the lamp must continue to flicker after t_k, it is absurd to suppose that it may vanish earlier.

Countering in this manner, however, ignores an essential point: from the fact that t' is earlier than t^*, i.e., $t' \prec t^*$, it does not follow that $t' \prec t_k$ for some k. The analytical pattern mentioned in the foregoing section in relation to the theorems of Bois-Reymond proves that the ordering $t_1 \prec t_2 \prec t_3 \prec \cdots \prec t^*$ is wholly compatible with the existence of a t' such that $t_k \prec t' \prec t^*$ for any k. To circumvent the possibility that the relevance of this observation be doubted for the particular case under discussion, we may note, first, that the fable says nothing about the texture of the time continuum, and, second, that if this continuum is similar to that of [p] of Section 2, above, there are instants $t' = (t^*, \gamma)$, $\gamma < 0$. For any such instant, $t_k \prec t' \prec t^*$ for any k.

What eliminates this last alternative is a series of postulates. The first proclaims the geometrization of time:

The continuum of time and that of line are identical.

The logic of measuring time by the position of the tip of a clock's hand is based on this postulate. The second postulate is Dedekind's:

The line continuum and the continuum of Numbers are identical.

There remains to show why Numbers such as (r, γ) are not included in the category recognized by arithmetical analysis. The exclusion is the result of a postulate which, although implicitly used over and over in arithmetical analysis, is not found, search as one may, explicitly stated in the literature. We may call it the Postulate of the Negation of the Infrafinite:

If a Number (considered without its sign) is smaller than any positive real number, that Number is zero.

In the light of Section 3, above, we can see that this postulate has its roots in the concept of measure, not in that of pure order. What it says in essence is that since Zero is the only Number (note the capital N) with a zero measure, relation (6) means that $(0, \gamma)$ can be but zero. Hence, the infrafinite does not exist. The general claim that measure is a process foreign to ordering is certainly valid. But because of the infiltration of the last postulate into the preliminary proceedings of arithmetical analysis, the equally general claim—that arithmetical analysis is completely independent of the concept of measure—calls for some revision.

11. The point that should now be stressed is that the postulates just spelled out still do not enable us to determine the state of our fabulous lamp at t^*. The recognition of this fact has its definite place in mathematical analysis, namely, in the concept of ordinary discontinuity. And as J. F. Thomson showed, it also represents the death blow for the assertion that an infinity of tasks can be performed within a finite time interval.

On this basis—and on noting the parallelism between the manner in which the switching of the lamp is arranged and that in which Zeno describes Achilles' race after the tortoise—we may be tempted to argue that even mathematical analysis bears out Zeno. There is, however, a fundamental difference between the two fables which was clearly pointed out by Aristotle. Locomotion is the continuous Change par excellence; in the jargon of kinematics, motion is a continuous function between two continuous variables, time and distance. Hence, when $t^* - t = 0$ the distance between Achilles and the tortoise also must be zero, not some indeterminate number. All other forms of Change, like that of switching a lamp on or printing another decimal of π, consist of definite "tasks" (in Thomson's jargon) or of "actual" units (in Aristotle's).[27] Zeno's artfulness, as Aristotle noted, was to describe Achilles' locomotion as if it consisted of an infinite number of tasks or units, of a super-task, and then cry "paradox."[28] However, to walk from A to B is not a different task than to walk from B further on to C, unless one *actually stops* in B—a fact that would introduce the needed discontinuity.

12. The popular refutation of Zeno's paradox is based on the idea that the *sum* of the infinite series (obtained from t_n for $n \to \infty$)

$$1 + \frac{1}{2} + \frac{1}{4} + \cdots + \frac{1}{2^n} + \cdots$$

is $t^* = 2$, *sum* being understood in the ordinary sense in which it applies to a finite number of terms.[29] Some even argue that logically there is absolutely nothing wrong with the super-task of infinitely many distinct operations of addition.[30] Such ideas, I believe, grow out of some loose expressions which blur the essential character of the concept of limit and which we often let slip into mathematical texts. Even though these expressions cause hardly any harm to the arithmetical analysis *per se,* they ought to be avoided because they are apt to breed some muddled thinking on some related issues, in particular on that of the existence and the nature of the infrafinite numbers.

The concept of limit is a legitimate (and also fruitful) *association* between an infinite sequence and a number. Anything that might suggest a more intimate relation between the two terms—especially, a relation of identity—fosters analytical confusion. For a concrete and salient

[27] Aristotle, *Physics*, 260^b–261^a and 263^b 4–5.

[28] *Ibid.*, 263^a 22–23.

[29] We find this position taken even by Alfred North Whitehead, *Process and Reality: An Essay in Cosmology* (New York, 1929), p. 107.

[30] See, in particular, J. Watling, "The Sum of an Infinite Series," *Analysis*, XIII (1952), 39–46.

example, let us consider the association of the number zero with a sequence of *positive* numbers

$$(a_1, a_2, a_3, \ldots, a_n, \ldots), \tag{23}$$

such that for any $\epsilon > 0$ we have $a_n < \epsilon$ for any $n > N(\epsilon)$. It is perfectly legitimate to express this association by some symbolism, such as "$\lim a_n = 0$ for $n \to \infty$," or by some other diction, such as "a_n is an approximation of zero." But some confusion is allowed to creep in if it is not clearly emphasized that 0.999 . . ., for instance, is only a convenient notation instead of "$\lim (b_n = 9 \sum_1^n 10^{-i})$ for $n \to \infty$." Unfortunately, even the writings of many a mathematical authority are silent on the essential difference between the decimal representations of 1/4 by 0.25 and by 0.24999. . . .[31] But the greatest source of confusion is the widely used expression "in the limit" or, as Bertrand Russell would say, "after an infinite number of operations a_n becomes zero." The truth is that a_n *never* becomes zero, for no matter how far we travel along the sequence (23) we will find only positive numbers. This being so, it should be obvious to everyone that it is the limit of that sequence, i.e., zero, which is the ghost of the departed positive numbers, a_n. Bishop Berkeley's quip is used here not as a jibe of the concept of limit but to accentuate that this concept involves a transfinite jump. Cantor, to whom we may turn once more for light, does not identify his first transfinite number ω with the endless sequence of integers; nor does he say that the finite integer n becomes ω in the limit. Instead, he places ω at the transfinite end of the sequence of integers, i.e., after these integers are all departed.

The confusion, in a latent state, between the infrafinite and a sequence with the limit zero pervades many thoughts in infinitesimal analysis. To give a salient example: occasionally we come across the argument that, because $1 - b_n$ may be smaller than any arbitrary positive ϵ, $1 - 0.999 \cdots = 0$ is valid in the purely arithmetical sense on the authority of the Postulate of the Negation of the Infrafinite. But this does not do. The postulate concerns a number, not a variable in "a state of flux," such as $1 - b_n$ is. Hobson, we can now see, connected this state with the wrong object: if properly conceived, an infrafinite number is as fixed and distinct an entity as any finite or transfinite number.

13. It is precisely because of this last property that the infrafinite cannot enable us (any more than the ordinary number can) to arrive at an arithmomorphic representation of Change. It does, however, not only

[31] For instance, in Borel, *Les paradoxes de l'infini*, p. 118, we find the strange argument that the limit of the sequence 0.19, 0.199, 0.1999, . . . must be written 0.1999 . . ., not 0.2, if "continuity is not to be suppressed."

illuminate some of the unsuspected imperfections inherent in the "perfect" aggregate of ordinary numbers but also aid us in removing some of these imperfections. For an illustration, let us return to the problem of events which though possible have a zero probability measure. With the aid of the infrafinite we can give a precise definition to quasi impossibility or quasi certainty so as to distinguish clearly between these situations, on the one hand, and impossibility and certainty, on the other. As I have argued elsewhere,[32] such a distinction is indispensable for a complete analysis of expectations even though it does not have a continuous bearing on our behavior in face of uncertainty. It is only by the infrafinite that we can avoid the analytical imbroglio involved in Borel's advice "not [to] be afraid to use the word *certainty* to describe a probability that falls short of unity by a sufficiently small quantity."[33] That such a probability, being finite, is smaller than even quasi certainty is a minor point; but equating it with certainty leads straight into the doctrine of Azaïs and Marbe.[34]

14. We should be now in position to see that, over and above the technical intricacies of the topic discussed in this appendix, there shine the crystal-clear teachings of Aristotle on the general concept of continuum. For when all is said and done, we cannot fail to recognize that a continuum, however defined or apprehended, is indissolubly connected with one fundamental idea on which Aristotle insisted painstakingly. What is infinitely divisible remains so at all times. And since a point is indivisible, there is no bridge between a *proper part* of a line, i.e., a part that has the *specific* quality of line, and a *point*. The line, like everything that is continuous, "is divisible [only] into divisibles that are infinitely divisible."[35] It would be hard even for a detractor of Aristotle—not a rare phenomenon nowadays—to close his eyes at the vindication of this thought by the arithmetical analysis itself. Indeed, if in (23) the a_n's represent the successive parts of an infinite division of a line segment, the

[32] See my article "The Nature of Expectation and Uncertainty" (1958), reprinted in AE, pp. 251–253. On this point see also Richard von Mises, *Probability, Statistics and Truth* (2nd edn., London, 1957), pp. 33 f.

[33] Borel, *Probability and Certainty*, p. vii.

[34] For which see Appendix C in this volume. In this connection, let me observe another imbroglio. All statistical tests are based on the assumption that the probability coefficient is a *positive* number; there is no test for the case of quasi-certain events. Consequently, the arguments invoking the failure of statistical tests to support the existence of Extra Sensory Perception are unavailing: Extra Sensory Perception may be possible although with a zero probability. (Needless to add, my point does not imply that this is the actual case.)

[35] Aristotle, *Physics*, 231^b 15–16.

very concept of limit, if not abused, proclaims that the process will never produce a point, i.e., a "segment" without length.

But Aristotle also held that "nothing continuous can be composed of indivisibles: e.g., a line cannot be composed of points, the line being continuous and the point indivisible."[36] On the other hand, the arithmetical continuum has been created solely out of indivisibles—the individually distinct beads of my metaphor. The contradiction has been exposed by many a mathematical authority: "the *generic* distinction between a continuous geometrical object, and a point . . . situated in that object, is not capable of direct arithmetic representation."[37] The arithmetical continuum conceived only as an aggregate of indivisibles offers no room for the metrical properties of space or of any other continuous structure for that matter. Metrical geometry may be a "little corner of Geometry"—as Bertrand Russell once said[38]—but its role in the issue is paramount: it provides the only acid test of the relevance of the arithmetical continuum outside arithmetical analysis itself. And since for the identification of a point in a metric space we must use metric coordinates, i.e., lengths, the notion of measure had to be woven subsequently into the original warp of the arithmetical continuum. In this manner, the arithmetical continuum was endowed with the infinitely divisible parts that —as Aristotle said—any continuum must possess. Divorced from the concept of measure, the place of the arithmetical continuum, even as regards the other branches of mathematics, would probably have to be in a glass case to be only admired as the most sublime yet perfectly idle creation of the human mind.

Another development, which has recently sprung from some ideas latently imbedded in arithmetical analysis, also vindicates Aristotle's position. It is the analysis of dimensionality, which overtly recognizes the unbridgeable gap between point, line, area, etc. Undoubtedly, Aristotle would have said not only that the point is no part of the line, but also that the line is the end of an area, not a part of it. Or to put it differently, at the level of area the line emerges as an indivisible. If this teaching were wrong, then we should ponder long over the question of why arithmetical analysis has not yet been able to transform dimension itself into a continuous concept so that dimension $\sqrt{2}$, for instance, should also exist as a meaningful conception. Perhaps, the very absurdity of an image having a dimension between that of the point and that of the line explains why the analysis of dimensionality has not even ventured in that direction.

[36] *Ibid.*, 231^a 24–25. See also Immanuel Kant, *Critique of Pure Reason* (Everyman's Library edn., New York, 1934), p. 136.

[37] Hobson, *Theory of Functions*, I, 89.

[38] Russell, *Mysticism and Logic*, p. 91.

By and large, it appears that the ultimate difficulty besetting all our attempts to penetrate the mystery of the general concept of continuum with the aid of some number structure is the impossibility of avoiding discontinuity completely. Discontinuity unavoidably appears, as we have just seen, in the analysis of dimensionality; it also cannot be avoided in any arithmomorphic model aimed at placing the transfinite or the infrafinite on a basis acceptable to Logic. Among the elements on which Cantor erected his concept of the transfinite as well as among those on which the system [ρ] rests, the most pellucid kind of discontinuity—that of the integer system—occupies a prominent place. Perhaps, this fact is the inevitable consequence of the original sin of all attempts to reduce the continuum to its opposite, the discretely distinct Number. He who insists on sowing only discontinuity seeds should not marvel at the discontinuity inherent in his crop. But neither should he try to deny its existence.

APPENDIX B *Ignorance, Information, and Entropy*

1. As we have seen in Chapter V, Section 4, the entropy of a system was first defined by Clausius as a function of some other macrocoordinates that can be *measured* directly. That definition is still the only one that enables us to determine the entropy of an actual system. With the advent of statistical thermodynamics, entropy was defined anew, as a function of the positions and velocities of all particles included in the system (Chapter VI, Section 1). According to this new definition, entropy can be *calculated* from the knowledge of these microcoordinates. Naturally, the reverse does not work: being given the value of the entropy of a system, we cannot derive the individual positions and velocities. Yet our ignorance about the actual microstate is not total, nor is the degree of this ignorance the same for every value of entropy.

Let us take a very simple example, of four particles labeled U, X, Y, Z and two states A and B. Let us consider the microstate U, X, Y in A and Z in B, and denote by S the entropy of this microstate. Since macrocoordinates do not depend on which particular particles are in each state, every microstate in which any three particles are in A and the other in B must have the same entropy S. From the knowledge of S we know therefore the macrostate; i.e., we know that there are three particles in A and one in B, but not which particular particle is in each state. However, we know that there are four microstates that are compatible with S. And if we would happen to deal with the microstate in which U and X are in A and Y and Z in B, then from the knowledge of the corresponding entropy S' we would know that there are six microstates compatible with S'.

The momentous idea of Boltzmann is that $S = k \ln 4$ and $S' = k \ln 6$.[1]

On this basis, G. N. Lewis argued in a 1930 paper that the entropy of a system constitutes an index of our degree of ignorance about the microstructure of the system. The idea is perfectly reasonable. Knowing S, we wonder which of the *four* compatible microstates is actually the case; knowing S', the spectrum of possibilities increases to *six* microstates.[2] Clearly then, as the entropy of our system increases from S to S', our degree of ignorance—or our degree of incertitude—about the actual microstate increases, too. As Lewis put it: "The increase in entropy comes when a *known* distribution goes over into an *unknown* distribution. The loss, which is characteristic of an irreversible process, is *loss of information*."[3]

Several points, which are crucial for the argument to follow, should now be well marked out.

The first is that the preceding analysis implies in no way whatsoever that there is an index of the degree of ignorance in every other situation, say, as one wonders whether there is life on Mars or whether a chemical just synthesized may cure hay fever. The most we can infer from it is that such an index may be constructed also for the cases in which we can establish some sort of measure for each possible alternative.

Second, we must not ignore the fact that, even if this last condition is fulfilled, degree of ignorance is not a measurable variable. Degree of ignorance shares the same analytical difficulties with the notions of order (or disorder) in statistical thermodynamics or with those of price level and national product in economics. All these variables are not measurable even in the ordinal sense. They accept the relations "more" or "less," but only if these relations are taken dialectically. As a result, the most we can do is to establish pseudo measures for each one of them. Moreover, the gamut of these pseudo measures is as unlimited as that of averages, for the choice of a pseudo measure, too, is restricted only by a few conditions. And precisely because of the dialectical nature of the pseudo measures, there is no way of eliminating the cases in which two pseudo measures of the same variable yield entirely different rankings.

An instructive illustration of the last remarks is the suggestion of O.

[1] See Chapter VI, formula (2).

[2] At this moment, it is important to recall that only those microstates which have the same total energy must be taken into account (cf. note 5, Chapter VI). The point is that although Boltzmann's formula gives the same S, $S = k \ln 4$, also for any of the four microstates in which one particle is in A and three particles in B, these microstates must be ignored if the other macrostate (consisting of three particles in A and one in B) represents the given total energy.

[3] G. N. Lewis, "The Symmetry of Time in Physics," *Science*, June 6, 1930, p. 573.

Onicescu[4] to measure order (or information) by what he terms "informational energy":

$$(1)\qquad \mathscr{E} = \sum_{1}^{s} (N_i/N)^2 = \sum_{1}^{s} f_i^2,$$

where $f_i = N_i/N$. This certainly is as good a pseudo measure of order as what we now call the *negentropy* per particle:[5]

$$(2)\qquad H = \sum_{1}^{s} (N_i/N) \ln (N_i/N) = \sum_{1}^{s} f_i \ln f_i.$$

Like H, $\mathscr{E}$ reaches its minimum for the microstate of lowest order, $f_1 = f_2 = \cdots = f_s$ (and only for this), and its maximum for any macrostate of highest order, $f_k = 1$ (and only for this). But as said, it does not rank order in the same way as H does.[6] However, as Onicescu showed, $\mathscr{E}$ has some analytical properties as interesting as those of H.[7] For instance, if $f_{ik} = f_i'f_k''$ is a compound structure, then $\mathscr{E}(f) = \mathscr{E}(f')\mathscr{E}(f'')$. Hence, log $\mathscr{E}$ has the same additivity property as H.

An interesting suggestion grows out of the simple relation between informational energy and the standard deviation of $(f_1, f_2, \ldots, f_s)$:

$$(3)\qquad \mathscr{E} = \sum_{1}^{s} \left(f_i - \frac{1}{s}\right)^2 + \frac{1}{s}.$$

This relation induces us to remember that the process by which thermodynamic equilbrium is reached consists of a progressive diffusion of available heat, hence, of a progressive reduction of the differences between the energy levels. Almost any pseudo measure of dispersion may therefore serve as a pseudo measure of order.[8] In fact, Boltzmann's own H-function is a pseudo measure of dispersion. A relatively simple algebra will prove these points.

Let $g(x)$ be a strictly convex function over the closed interval $[a, b]$,

[4] Octav Onicescu, "Énergie informationnelle," *Comptes Rendus de l'Académie des Sciences*, Series A, CCLXIII (1966), 841 f. See formula (8), Chapter VI.

[5] See formula (6), Chapter VI.

[6] Since the point is important for understanding the peculiarities of pseudo measures, it may be well to give a proof of it. Since $\sum p_i = 1$, we have:

$$d\mathscr{E} = \sum (p_i - p_s)\, dp_i, \qquad dH = \sum (\ln p_i - \ln p_s)\, dp_i.$$

For $d\mathscr{E}$ and dH to have the same sign for any dp_i, it is necessary that the coefficients of dp_i in these sums be always proportional. Obviously, save in the case of $s = 2$, this cannot be true for all values of the p_i's.

[7] For the analytical properties of H, see C. E. Shannon and W. Weaver, *The Mathematical Theory of Communication* (Urbana, Ill., 1949), pp. 19–22.

[8] I say "almost" because for $s > 3$ and the highest order the interquartile range is zero.

$a < b$, i.e., a function defined for every $x \in [a, b]$ and such that for any $x, y \in [a, b]$ and $\alpha \in [0, 1]$ we have

$$(4) \qquad g[\alpha x + (1 - \alpha)y] \leq \alpha g(x) + (1 - \alpha)g(y),$$

where the equality prevails if and only if $x = y$ or $\alpha = 0, 1$. Let $a \leq x_1 \leq x_2 \leq \cdots \leq x_s \leq b$, and let us put $G = \sum_1^s g(x_i)$, $X_k = \sum_1^k x_i$, and $M_k = X_k/k$. From (4) it follows that

$$(5) \qquad g(x_i) \leq \frac{b - x_i}{b - a} g(a) + \frac{x_i - a}{b - a} g(b),$$

and by addition,

$$(6) \qquad \frac{G}{s} \leq \frac{b - M_s}{b - a} g(a) + \frac{M_s - a}{b - a} g(b),$$

where the equality is valid if and only if it is valid in (5) for every i. This last condition, in turn, is equivalent to

$$(7) \qquad a = x_1 = x_2 = \cdots = x_j, \qquad x_{j+1} = x_{j+2} = \cdots = x_s = b,$$

for some j. Also from (4) and from

$$(8) \qquad M_{k-1} \leq M_k = \frac{k - 1}{k} M_{k-1} + \frac{1}{k} x_k \leq x_k, \quad 1 < k \leq s,$$

we obtain

$$(9) \qquad g(M_k) \leq \frac{k - 1}{k} g(M_{k-1}) + \frac{1}{k} g(x_k),$$

where the equality prevails if and only if $M_{k-1} = x_k$, consequently, if and only if

$$(10) \qquad x_1 = x_2 = \cdots = x_k.$$

By simple induction (9) yields

$$(11) \qquad g(M_s) \leq G/s,$$

where the equality is valid if and only if

$$(12) \qquad x_1 = x_2 = \cdots = x_s.$$

From (6) and (11) we obtain immediately the following result.

LEMMA A: *If the x_i's are subject to the constraint $X_s = ta + (s - t)b$ for given positive integers s and t, $t < s$, then G reaches its maximum for* (7) *and its minimum for* (12).

Let us consider the case of $0 \leq x \leq 1$ and $X_s = 1$. For $g(x) = x \log x$ (with $g(0) = 0$), we obtain the property of the extrema of H, for $g(x) = x^2$, that of $\mathscr{E}$. A large class of functions having the same property corresponds to $g(x) = x^\alpha$, $\alpha > 1$. The case of $g(x) = |x - (1/s)|^\alpha$, for $\alpha \geq 1$, yields the familiar class of moments about the mean.[9]

Let $(x^0) = (x_1^0, x_2^0, \ldots, x_s^0)$ be a set such that $\sum_1^s x_i^0 = 1$ and $0 \leq x_i^0 \leq 1$. Given k, $0 \leq k \leq s$, let us denote by (x^k) the set such that $x_i^k = x_i^0$ for $i \leq k$, and $x_i^k = (\sum_{k+1}^s x_j^0)/(s - k)$ for $i > k$. Clearly, $\sum_k^s x_j^k = \sum_k^s x_j^{k-1}$. Consequently by Lemma A, we have:

$$\sum_k^s g(x_j^{k-1}) \leq \sum_k^s g(x_j^k). \tag{13}$$

If we put $G_k = \sum_1^s g(x_j^k)$, (13) yields

$$G_0 \leq G_1 \leq \cdots \leq G_{s-1} = G_s, \tag{14}$$

a relation which shall prove useful later on.

If we accept the statistical theory of heat and, above all, if we accept the tenet that Boltzmann's formula gives in each case the same value as the value of entropy determined experimentally by Clausius' formula, then we must obviously prefer Boltzmann's H-function over all other pseudo measures of order in thermodynamics. But the H-function has a marked edge over the others also in information theory, where, as we shall presently see, it is directly connected with the relative frequency of a particular event.[10]

2. Between entropy and information there exists, however, a relation of a different nature than that analyzed above. It comes from the fact that we cannot obtain, transmit, or even keep in store information of any kind without an increase in the total entropy of the isolated system in which we act. To determine the speed of a particle we must cast a beam of light on it; this will necessarily produce a dissipation of available energy and, hence, increase the total entropy. The same result is produced by the barking of a dog which wants to inform its master that it wants to be let in the house. Also a typesetter increases the total entropy when he sets a sentence in type, even if he sets a completely garbled sequence of letters. In general, if between two time instants $t_0 < t_1$, some information of *any kind* has been obtained or transmitted, then the increase in entropy $S_1 - S_0$ can be decomposed into two parts: $S - S_0$, the increase that would have come about if the operations necessary for obtaining

[9] The case of $\alpha = 1$ can be proved directly by inspecting the sign of the total differential of G (as in note 6, above).

[10] Relation (42), below.

or transmitting the information had not been performed, and $S_1 - S$, which by definition represents the increase in entropy caused by these operations. The relation

$$(15) \qquad S_1 - S_0 = (S - S_0) + (S_1 - S)$$

is a tautological consequence of the Entropy Law. The point was used in a 1929 paper by L. Szilard to explode the paradox of the Maxwell demon. No such demon could perform its assumed task, Szilard argued, without first obtaining some information about the approaching particles, i.e., without first increasing the total entropy of the system.[11]

One elementary point needs nevertheless to be emphasized now: relation (15) is no longer a tautology if we turn it around and say that $S_1 - S$ is a measure of "the amount of information" obtained by that additional increase in the total entropy. Of course, we could turn it into a tautology by defining implicitly "the amount of information" to be equal to $S_1 - S$. But such an implicit definition raises numerous thorny issues.

First, it would practically compel us to say further that all terms in (15) represent amounts of information. To wit, it led Lewis to conclude that "gain in entropy always means loss of information, and nothing more."[12] But strongly anthropomorphic though its ties are, the Entropy Law is a law of nature expressible in purely physical terms. According to Lewis' twisting, however, we should give up testing this law on the physicist's workbench by measuring the physical variables involved in its Classical definition—a recommendation which is hard to accept. I cannot imagine that a physico-chemist discussing the structure of a molecule of some chemical compound or an engineer analyzing a thermal engine would find it right to say that the entropy of the corresponding system means nothing but his own degree of ignorance.

Second, the implicit definition would alter the basic notion of information beyond recognition, nay, beyond any practical utility. To wit, the transmission of a completely nonsensical message may very well cause an increase in total entropy greater than that of a highly important discovery.

It should be instructive, therefore, to examine in some detail the course of ideas which has gradually led to the position that entropy and information are equivalent entities.

3. A specific definition of "the amount of information" in relation to a probability distribution was introduced in 1948 by Norbert Wiener by

[11] L. Szilard, "Über die Entropieverminderung in einem thermodynamischen System bei Eingriffen intelligenter Wesen," *Zeitschrift für Physik*, LIII (1929), 840–856. For the Maxwell demon, see Chapter VII, Section 7, above.

[12] Lewis, "Symmetry," p. 573.

looking at the problem, not *ex ante* (as Laplace did), but *ex post*.[13] As he explained, "if we know *a priori* that a variable lies between 0 and 1, and *a posteriori* that it lies on the interval (a, b) inside $(0, 1)$," it is quite reasonable to regard any positive and monotonically decreasing function of [measure of (a, b)/measure of $(0, 1)$] as an ordinal measure of the amount of the *a posteriori* information. Briefly,

$$\text{(16)} \qquad \text{Amount of information} = F\left[\frac{\text{measure of } (a, b)}{\text{measure of } (0, 1)}\right],$$

where $F(x)$ is strictly decreasing with x. But since it is reasonable to expect that (16) should yield the same value for all intervals equal to (a, b), it is necessary to assume that the variable related to (16) is uniformly distributed over $(0, 1)$, in which case [measure of (a, b)]/[measure of $(0, 1)$] is the probability that the variable lies within (a, b).

Another way of looking at the problem is this. A card is going to be drawn by chance from a deck. At that time, there are fifty-two question marks in your mind. If you are thereafter told that the card drawn is an honor card, thirty-two of those marks vanish; there remain only twenty of them. Should you be told that the card is a spade honor card, you would be left with only five question marks. So, the smaller the proportion of the initial question marks left after some information is made available to a person, the greater is the importance (or the amount) of that information. The general principle is thus obvious: the amount of information $I(E)$ that the event E of probability p has occurred is ordinally measured by the formula

$$\text{(17)} \qquad I(E) = F(p),$$

where F is a strictly decreasing function which, for obvious reasons, may be assumed to satisfy the condition $F = 0$ for $p = 1$. We may take, for example, $F = 1 - p^{\alpha}$. Wiener has chosen the negative logarithm

$$\text{(18)} \qquad I(E) = -\log p.$$

The choice has obvious merits. If in (16), we assume $a = b$, then the information is extremely valuable because it completely determines the variable. With (18), the value of (16) is infinite. If, on the other hand, $(a, b) = (0, 1)$, then the information tells us nothing that we did not

[13] Norbert Wiener, *Cybernetics* (2nd edn., New York, 1961), pp. 61 f. The basic idea was advanced much earlier, at a 1927 meeting. See R. V. L. Hartley, "Transmission of Information," *Bell System Technical Journal*, VII (1928), 535–544.

know already. The value of (18) is in this case zero, and all is in order.[14] But the salient advantage of introducing the logarithm derives from the transformation of the classical formula for compounding events

$$p(A \cap B) = p(A) \times p(B \mid A) \tag{19}$$

into a sum

$$\log p(A \cap B) = \log p(A) + \log p(B \mid A). \tag{20}$$

From (18), it follows then that the amounts of information that come in succession are additive

$$I(A \cap B) = I(A) + I(B \mid A). \tag{21}$$

4. All this is in order. But Wiener, by a highly obscure argument (in which he acknowledged a suggestion from J. von Neumann), concluded that "a reasonable measure of the amount of information" associated with the probability density $f(x)$ is

$$\int_{-\infty}^{+\infty} [\log f(x)]\, f(x)\, dx, \tag{22}$$

and further affirmed that this expression is "the negative of the quantity usually defined as entropy in similar situations."[15] There is in Wiener's argument a spurious analogy as well as an elementary error of mathematical analysis. There is no wonder that the problem of the relationship between Boltzmann's H-function and the amount of information is far from being elucidated even after so many years.

That the logarithmic function appears both in (18) and (22) is not a sufficient reason for regarding (22), too, as representing a measure of an amount of information. Curiously, Wiener did not see that in (18) we have the logarithm of a *probability*, whereas in (22) the logarithm is applied to the *probability density*. And as I shall presently show, (22) can by no means be regarded as the continuous form of the H-function. What is more, the concept of entropy as defined by Boltzmann—i.e., by the H-function (2)—cannot be extended to a continuous distribution.

We may begin by noting that according to Wiener's definition (18),

[14] Let me point out that as long as we speak of the degree of ex ante belief in the occurrence of an event E of probability p, any strictly increasing function of p provides an ordinal measure of that belief. Moreover, as G. L. S. Shackle argued in *Expectations in Economics* (Cambridge, Eng., 1949), the greater the degree of the ex ante belief, the smaller the degree of surprise *after* E has occurred. The close kinship between the degree of ex post surprise and the amount of information is obvious. Hence, any formula for the amount of information is also a measure of the degree of surprise, and conversely.

[15] Wiener, p. 62.

which now is the generally accepted one, we have no right to speak of an amount of information if we do not refer to *the occurrence of some stochastic event*: the drawing of an honor card from a deck, the shooting within the second circle around a target and so on. So, we must ask: What is the *occurred* event that may be associated with a probability distribution? The answer is that there is none. Yet there are several ways in which a relationship may be established between information (understood in some very restricted sense, as is the case in (18) too) and the H-function. Actually, there is a large class of functions for which this is true. I shall even pursue the argument for the general case so as to make it abundantly clear that the problem does not necessarily call for Boltzmann's concept of entropy.

Let $E_1, E_2, \ldots, E_s$ be a set of mutually exclusive and completely exhaustive events of probabilities $p_1, p_2, \ldots, p_s$, $\sum p_i = 1$. By (17), when (and if) event E_i will occur, the amount of information that it has occurred will be $F(p_i)$. But since we do not know yet which event will occur, we can only resort to a rational guess of the *future* amount of information. A long-trodden path leads us to the *expected* amount of information:

$$\Phi_F(p) = \sum_1^s p_i F(p_i). \tag{23}$$

Alternatively, we can interpret Φ_F as the expected degree of surprise caused by one of the future events of the same probability distribution.[16] In both cases, Φ_F is an *ex ante* estimation of an *ex post* coordinate. Analytically, $\Phi_F(p)$ is in fact a peculiar *statistic* of a distribution and, as said, not a characteristic of a single event. And I say "peculiar," because Φ_F is a statistic involving *only* the probabilities of a distribution.

Given a field of stochastic events, we may partition it in a great number of ways. And it is clear that the value of Φ_F depends on the manner in which we have partitioned the field: Φ_F, therefore, *is not an invariant characteristic of a stochastic field*. For simplicity let us take $F(p) = 1 - p$ and consider the field of cards drawn from a standard deck by an *unbiased random* mechanism.[17] If each card is considered as a separate event,

$$\Phi_F(p) = 1 - \left(\frac{1}{52}\right) = \frac{51}{52}, \tag{24}$$

and if the field is partitioned in "honor cards" and "spot cards,"

$$\Phi_F(p) = \frac{80}{13^2}. \tag{25}$$

[16] Cf. note 14, above.

[17] Because $F = 1 - p$, we have $\Phi = 1 - \mathscr{E}$.

The fact that Φ_F is larger for the first, finer partition is a property common to a large class of functions $F(p)$. We need only assume that $h(p) = pF(p)$ is strictly concave[18] and that $h(0) = 0$. Let us transform the partition (p) into (p') in such a way that every p_k is divided into a sum

$$p_k = p'_j + p'_{j+1} + \cdots + p'_{j+i}, \qquad 0 \leq i. \tag{26}$$

Let x and y be such that $0 < x < y < x + y < 1$. From this ordering and from the condition of strict concavity, we obtain

$$\begin{aligned} h(x) &\geq \frac{y - x}{y} h(0) + \frac{x}{y} h(y), \\ h(y) &\geq \frac{x}{y} h(x) + \frac{y - x}{y} h(x + y). \end{aligned} \tag{27}$$

These inequalities yield

$$h(x) + h(y) \geq h(x + y), \tag{28}$$

which step by step leads to

$$\Phi_F(p) \leq \Phi_F(p'). \tag{29}$$

This property—which is shared in particular by $-H$ and $1 - \mathscr{E}$—expresses the fact that a finer classification is always apt to yield more information.[19] But we should not be mistaken by our paper-and-pencil constructions, here or elsewhere: the fact is not proved by (29); (29) only confirms the appropriateness of our formalizations.

Let us now consider the probability density $f(x)$ of an absolutely continuous distribution. Since

$$\int_{-\infty}^{+\infty} f(x)\, dx = 1, \tag{30}$$

we can find n intervals $-\infty < x_1 < x_2 < \cdots < x_{n-1} < +\infty$ such that the probability over each one of them is $1/n$. For this partition, (23) becomes

$$\Phi_F(n) = F\left(\frac{1}{n}\right). \tag{31}$$

From (27) it follows that

$$\frac{h(x)}{x} > \frac{h(y)}{y} \tag{32}$$

[18] A function $h(x)$ is strictly concave if $g(x) = -h(x)$ is strictly convex.

[19] By a finer classification we mean here not only a greater number of classes (which may overlap with the initial ones) but a further partition of the initial classes, as shown by (26).

for any $x, 0 < x < y$. Hence, $F(1/n)$ has a limit, finite or infinite, for $n \to \infty$. If we denote it by F_0, (31) yields

$$\lim_{n\to\infty} \Phi_F(n) = F_0. \tag{33}$$

This is a highly interesting result, for it shows that *the expected amount of information for an absolutely continuous distribution depends only on the ordinal measure adopted—more exactly, on the* lim $F(p)$ *for* $p \to 0$*—and not on the distribution itself.*

For example, if $F(p) = 1 - p$ (the modified Onicescu formula), then

$$\lim_{n\to\infty} \Phi_F(n) = 1. \tag{34}$$

For entropy, i.e., for $F(p) = -\ln p$, we have

$$\lim_{n\to\infty} \Phi_F(n) = +\infty, \tag{35}$$

which proves my earlier contention that Boltzmann's H-function cannot be extended to a continuous distribution.

5. There is a second (and, I believe, novel) way of relating the H-function or its generalization to information. Let $A_1, A_2, \ldots, A_s$ be s individuals, each owning some amount of land, x_i. Let us assume that at first we know only the total amount of land, $X = \sum x_i$. The only picture that we can have of the land distribution is that at which we arrive by the Principle of Insufficient Reason, namely, that everyone owns the same amount of land, X/s. If subsequently we are told the amount owned by A_1, the picture for the others will be that each owns $(X - x_1)/(s - 1)$. If x_2 also becomes known, then our rational guess is that everyone of the others owns $(X - x_1 - x_2)/(s - 2)$. When finally x_{s-1} becomes known, the entire distribution is known.

What we need now is a function of the distribution of X such that it will increase (or will not decrease) as the actual distribution becomes progressively known. By (14), the function $G = \sum x_i g(x_i)$ satisfies this condition. Hence, it may be taken as a measure of the information we have about the distribution of X. If the actual distribution is *uniform*, then $G_0 = G_s$, which is within reason: the complete information does not modify in any way the picture we had in our mind before any information became available.[20]

The preceding results obviously hold good if each x_i is replaced by the

[20] Needless to add, H and $\mathscr{E}$ possess the property (14) and, hence, the above considerations apply to the available information about a *discrete* distribution of probabilities.

relative share $\xi_i = x_i/X$, in which case $\sum \xi_i = 1$. And if we recall the propositions proved in Section 1, above, concerning the extrema of $G = \sum \xi_i g(\xi_i)$, we see that this generalized form of the H-function may serve also as a measure of *concentration*.

6. Still another way of connecting entropy with information is due to C. E. Shannon who, almost at the same time with Wiener, presented it in a classic memoir on *communication* theory. Unlike Wiener, Shannon wanted to arrive at a measure of the capacity (or the power) of a code system to transmit or store *messages*. Also unlike Wiener, Shannon was not concerned with whether a message contains any valuable information.[21] For a specialist in communication this is perfectly understandable: the cost of transmitting a message is, for all practical purposes, independent of whether the message has a vital importance for the whole world or is wholly nonsensical. The basic problem in communication is which code has the largest capacity "to transmit information."[22] The shift in the meaning of "information" is accentuated by Shannon from the outset: "the number of messages . . . or any monotonic function of this number can be regarded as a measure of the information produced when one message is chosen from the set" of all messages of the same length.[23]

The number of different messages consisting of N signals of the binary code—N dots or dashes—is 2^N. In general, if the code consists of s different signals, the number of messages is s^N. Following the suggestion of R. V. L. Hartley (cited in note 13), Shannon took the logarithm of this number as a measure of the information capacity. Moreover, in view of the important role played by the binary system in electronic systems of transmission and storage, it seemed natural to choose the logarithm to the base 2. Thus, the Shannon-information for a message of N binary signals is simply

$$\log_2 2^N = N \tag{36}$$

in units called "bits," short for "binary unit."[24] For $s > 2$, the same information is measured by $N \log_2 s > N$. So, the Shannon-information per signal is one bit for the binary code and $\log_2 s$ bits for the general case. Its important role in communication theory derives from the fact that it is independent of the length of the message.

The case of messages transmitted in some ordinary language is more complicated, since not all sequences of signs constitute messages. A long sequence of the same letter, for example, has no meaning in any language;

[21] Shannon and Weaver, *Mathematical Theory of Communication*, p. 3.
[22] *Ibid.*, especially pp. 7, 106.
[23] *Ibid.*, p. 3.
[24] *Ibid.*, pp. 4, 100.

hence it must not be counted in measuring the information capacity of a language. To arrive at the formula for this case, Shannon sought a function that would fulfill some reasonable analytical conditions.[25] However, the same formula can be reached by a direct way that has the merit of pinpointing the reason why that formula is identical to Boltzmann's H-function.

We take it as a simple fact that the relative frequency with which every written sign—a letter, a punctuation mark, or the blank space—appears in any language has a pseudo *ergodic* limit. If $p_1, p_2, \ldots, p_s$ denote these frequency-limits,[26] a *typical* message of N signs must contain $N_1 = p_1N, N_2 = p_2N, \ldots, N_s = p_sN$ signs of each type. The total number of typical messages is given by the well-known combinatorial formula

$$W = \frac{N!}{N_1!\, N_2! \ldots N_s!}. \tag{37}$$

The mystery is now divulged: (37) is the same formula from which Boltzmann derived his H-function for N very large:

$$\ln W = -N \sum p_i \ln p_i \tag{38}$$

which is relation (4) of Chapter VI, above. The Shannon-information per signal thus is

$$(\ln W)/N = -H, \tag{39}$$

which again is found to be independent of N.

We may also note that the relative frequency of the typical messages among all messages of length N is

$$P = Wp_1^{N_1}p_2^{N_2}\cdots p_s^{N_s} = Wp, \tag{40}$$

where p is the frequency of any given typical message. This yields

$$\ln P = \ln W + \ln p. \tag{41}$$

And since according to a well-known proposition of calculus of probabilities $P \to 1$ for $N \to 1$, we have $\ln W + \ln p = 0$, or[27]

$$p = e^{NH}, \tag{42}$$

which reveals an interesting link between the H-function and some relative frequency (or some probability, if you wish).

[25] *Ibid.*, pp. 18–20, 82 f.

[26] The reason why I refuse to refer to these coefficients as "probabilities" should be clear from what I have said in Chapter VI, Section 3. True, in a language the letters do not follow each other according to a fixed rule. But neither do they occur at random, like the spots in a die tossing.

[27] Shannon arrives at an equivalent formula by a different route. Shannon and Weaver, p. 23.

Like Wiener, Shannon noted the identity between (39) and Boltzmann's formula and proposed to refer to it as "the entropy of the set of probabilities $p_1, p_2, \ldots, p_n$."[28] But we should not fail to note also a fundamental difference between the two approaches. The connection between the Shannon-information and the H-function is inevitable, since the number of typical messages (the basic element in Shannon's theory) is given by (37) or, for a large N, by e^{-NH}. Thus, no matter what function of W we choose for measuring the information capacity of a language, we cannot get rid of H. In the case of typical messages, Wiener's formula (18) yields $-\log(1/W) = \log W = -NH$, which is the same formula as Shannon's (38). But for Shannon this represents a strictly technical coordinate, the number of bits in typical messages of length N, while for Wiener the same formula represents the amount of information. Moreover, as I explained in Section 4, above, Wiener's approach may be extended to expected information (or expected surprise). It is only then that H comes in as a formula valid for *any* distribution. But that formula is not unique; there are countless others and these have no relation whatsoever with H.

In spite of the appearance of the H-function in both Shannon's and Wiener's generalized approach, these approaches are not identical—which naturally does not mean that they have no points of contact either.

7. Shortly after Wiener and Shannon presented their results, Weaver noted that "when one meets the concept of entropy in communication theory, he has a right to be rather excited—a right to suspect that one has hold of something that may turn out to be basic and important."[29] And, indeed, the emergence of the entropy formula in communication theory reactivated the thoughts expressed before by Szilard and Lewis and led some writers to maintain, not only that obtaining or transmitting information produces an increase in entropy, but also that "information *is* negentropy"—as one champion of this thesis, L. Brillouin, puts it.[30] The full implication of this position is instructively revealed by R. C. Raymond as he explains that "the entropy of [an] organism may be taken as the equilibrium entropy of the constituents of the organism less the information entropy necessary to the synthesis of the organism from equilibrium components of known entropy."[31] We must assume that he

[28] *Ibid.*, p. 20.

[29] *Ibid.*, p. 103.

[30] L. Brillouin, *Science and Information Theory* (2nd edn., New York, 1962), p. xii (my italics). Wiener's statement that "just as the amount of information in a system is a measure of its degree of organization, so the entropy of a system is a measure of its degree of disorganization" (*Cybernetics*, p. 11), seems to be different from that of Brillouin.

[31] R. C. Raymond, "Communication, Entropy, and Life," *American Scientist*, XXXVIII (1950), 277.

would have also explained that the entropy of the universe is equal to that of Chaos less the information necessary to rebuild the universe from Chaos. Raymond's illustration, more so than any I happen to know, brings to the surface the basic issues of the thesis under discussion: the definition of information and the equivalence of this definition with physical entropy.

The gist of Raymond's illustration is formalized in what Brillouin termed the Negentropy Principle of Information.[32] It states that

$$S^1 = S^0 - I \tag{43}$$

where S^0 is the entropy of a *partial* (nonisolated) system before "an external agent" poured into the system the amount of information I, S^1 is the final entropy of the system, and

$$I = -kNH, \tag{44}$$

k being Boltzmann's constant.[33] It is elementary that if (43) is to have any physical sense I must be measured in the same units as entropy, i.e., as Boltzmann's constant. But why should the amount of information be defined by (44)? The valuable role played by Shannon's formula (38) in communication theory and its coincidence with Boltzmann's H-function may at most support the choice of (44) independently of (43). But then we need to prove that (43) is factually true. On the other hand, if we define the amount of information by the difference $S^0 - S^1$, then, as I have argued in Section 2, we transform (43) into a tautology and strip the Negentropy Principle of Information of any factual significance.

An elaborate proof of this principle offered by Brillouin boils down to our identity (41). Only he argues that $k \log P$ is the physical entropy of the system and $k \log p$ the entropy of the given message, and defines their difference, $-k \log W$, as the amount of information contained in the message, which for a large N reduces to (44).[34] But, clearly, the relation thus established does not coincide with (43). A simpler proof, also offered by Brillouin, is similarly based on an algebraic identity and is not much more enlightening.[35] Translating the terms of a formal identity into some concrete terms is hardly the proper way of establishing a factual truth. The danger is even more clearly seen in the case of the Negentropy Principle of Information since H, as we have seen, is susceptible of several concrete interpretations. But the reason why the thought that this principle may after all be true and that (43) and (44) merely confirm

[32] Brillouin, *Science and Information*, ch. 12; "Physical Entropy and Information," *Journal of Applied Physics*, XXII (1951), 338–343; "The Negentropy Principle of Information," *ibid.*, XXIV (1953), 1152–1163.

[33] For this constant see Chapter VI, Section 1.

[34] Brillouin, "Physical Entropy," pp. 340–342.

[35] Brillouin, *Science and Information*, pp. 152 f.

it must be abandoned has been stressed by more than one specialist on communication. Shannon's formula (39) gives only the number of bits per signal in an optimum coding; otherwise, "it is at an appreciable remove from the physical entropy."[36]

To be able to distinguish the white from the black, and even from the grey, in the Negentropy Principle of Information as well as in the various claims based on it, let us put in one single picture all the elements that are explicitly or implicitly involved in it. Let an isolated system be divided into two subsystems U and U_1, and let the respective entropies at t_0 be $S^0 > S_1^0$. Let $S_1^1 - S_1^0$ be the increase in entropy of U_1 caused by the operations necessary to obtain some information I which is subsequently transmitted onto U. U_1 is "the external agent" of Brillouin and U is the nonisolated system to which (43) refers. For example, U may initially be a blank magnetic tape on which a certain message is subsequently recorded with the aid of the negentropy lost by U_1. For the occasion, we may also ignore, as Brillouin invites us, all increases in entropy not connected with the operations for obtaining and transmitting the information.[37] The final outcome is obvious. The entropy of both subsystems is altered. The Negentropy Principle of Information asserts that the entropy of subsystem U is $S^0 - I$ where I is the amount of information given by (44).

Even though we are told that "only information connected with certain specific physical problems . . . will be thought of as related to entropy"[38] —and, I am sure, that the recorded tape mentioned above belongs to this category—one point is still not clear. In case the tape was *not* blank initially, would it not be possible that the message recorded subsequently may increase its initial entropy and, hence, that $S^0 < S^1$? Since this possibility is not at all excluded, should we change the sign of I in (43) and say that, in this case, we have recorded *neginformation*? Perhaps, we actually should say so. For if "information can be changed into negentropy *and vice versa*"—as Brillouin claims[39]—then, naturally, neginformation should change into entropy. The equivalence must work both ways. The point, I think, indicates that the Negentropy Principle of Information is only a mere play on words: negentropy replaces a decrease of entropy in a subsystem and information replaces negentropy. And I am afraid that we are mistaken then to believe that in that principle we have gotten hold of something basically important and novel. The risk we run is that of setting outstretched claims.

[36] D. Gabor, "Communication Theory and Physics," *Philosophical Magazine*, XLI (1950), 1169.

[37] Brillouin, *Science and Information*, p. 231.

[38] *Ibid.*, p. 152.

[39] *Ibid.*, p. 184. My italics.

Once we have started thinking that information and negentropy are two equivalent but not identical concepts, nothing seems more natural than to go on and assert that "the amount of negentropy used in the discovery of a scientific law is proportional to the 'absolute information' contained in this law."[40] Passing over the fact that in this statement "information" no longer has the same meaning as in (44), we can read it as

$$S_1^1 - S_1^0 = \alpha I, \tag{45}$$

where α must be a universal constant. This relation establishes the increase in entropy of the subsystem U_1 considered above. Though (45) is a far stronger proposition than (43), there exists not even an attempt at proving it. In fact, I think that it cannot be proved. But let us nonetheless accept it as valid. The entropy of the entire system, $U + U_1$, has then increased by $(\alpha - 1)I$, which must be strictly positive in view of the fact that the system has meanwhile produced some work. Consequently, the thought that $\alpha = 1$ should have never occurred to us for the simple reason that it would be tantamount to entropy bootlegging. Yet we read that "an information must always be paid for in negentropy, the price paid being larger than (or *equal* to) the amount of information received."[41] Entropy bootlegging is even more strongly implied in Brillouin's schematic analysis of Maxwell's demon. With the aid of the information (negentropy) poured into the subsystem U by an electric torch (our subsystem U_1), the demon "operates the trap door, and rebuilds negative entropy, thus completing the cycle

negentropy → information → negentropy."[42]

A first difficulty with this cycle should be obvious. Negentropy is spent not only in getting information, but also in using it—in fact, even in keeping it intact in storage. So, to operate the door the demon must use some additional negentropy. And unless we accept Maxwell's paradox as a disproof of the Entropy Law, this additional negentropy could not possibly be recuperated by the demon's operations. All the less can we then say that these manipulations will recuperate in part the negentropy spent on getting the information.

A second difficulty pertains to the earlier issue of what is measured by (44) and, hence, of what is the exact operational meaning of (43). As explicitly admitted by Brillouin and most writers on information theory,

[40] L. Brillouin, "Thermodynamics and Information Theory," *American Scientist*, XXXVIII (1950), 597.

[41] Brillouin, "The Negentropy Principle," p. 1153. My italics.

[42] Brillouin, *Science and Information*, p. 164. The idea that information can be used to decrease the entropy of the system in which we act and thus "recuperate part of the negentropy previously used in getting the information" (Brillouin, "The Negentropy Principle," p. 1153) seems to be a steadfast claim of information theory.

the amount of information defined by (44) abstracts completely from the thought element.[43] To put it differently, the notes of a Beethoven symphony scrambled as one may please would still be a "symphony." Let us assume that we record on tape the information needed by the demon but we scramble the sounds so as to render the message totally unintelligible. Since the scrambling does not decrease the amount of information *as defined by* (44) should we maintain that the demon can still use that information to operate the trap door efficiently?

Another glory claimed by information theory is an alleged generalization of the Entropy Law.[44] This is obtained by changing the sign of the entropy terms in (43), so that $\bar{S} = -S$ is the negative entropy, the negentropy. The new relation, $\bar{S}^1 = \bar{S}^0 + I$, is then taken to mean that the total negentropy of a system is composed of negentropy and information. The fact that the total entropy cannot decrease is thus expressed by the inequality

$$\Delta(\bar{S} + I) \leq 0 \tag{46}$$

which is presented as a generalization of $\Delta S \geq 0$. But, again, we find no comments on the factual meaning of this new law established by a mere manipulation of words and symbols. And I myself wonder what thing in information theory could stop us from replacing (46) by

$$\Delta I \leq 0, \tag{47}$$

as G. N. Lewis, we remember, proposed to express the Entropy Law. The last relation at least does not raise the issue of the conversion of $\bar{S}$ into I and vice versa.

8. That there are some connections and similarities between negentropy and information understood as a piece of useful knowledge is beyond question. First, there is the fact, expressed by our relation (15), that no information can be obtained, transmitted, or received without the expenditure of some free energy. Second, like free energy (negentropy), information is subject to degradation. If transmitted, it may become garbled in part; if received, it may be marred by errors of recording; if stored, it is gradually eroded by the inevitable entropic degradation of ordered structures.[45]

[43] Brillouin, *Science and Information*, pp. x–xi, 155; "Negentropy and Information in Telecommunications, Writing, and Reading," *Journal of Applied Physics*, XXV (1954), 599. Also Shannon, cited in note 23 above.

[44] Brillouin, *Science and Information*, pp. 153–156.

[45] Brillouin ("Thermodynamics and Information," p. 595) speaks of "a law of *degradation of absolute information*, very similar to the famous law of degradation of energy stated by Lord Kelvin." Curiously, this time he refers to the degradation of the *value* of information: Newton's laws, for instance, no longer have today the value they once had.

It is facts such as these that in all probability fostered the notion of an equivalence between negentropy and information as outlined in the preceding section. But once the term "information" was adopted as a new label for what Shannon originally called the negentropy carried on the cable on which a telegram is transmitted—a notion only vaguely related to information in the usual sense—confusing the two meanings of "information" became an inevitable risk. Perhaps, without the adoption of this term, the new endeavor would not have had the external luster that, in my opinion, is responsible for the unusual stir caused by information theory. Think, for example, of a physicist of Broglie's stature, who at first maintained that even an analogy between negentropy and information, "instructive and attractive though it is, is full of pitfalls," but who seemed later inclined to accept the claimed generalization of the Entropy Law, mentioned above.[46]

As a matter of fact, in the literature of information theory the caveats that "information" I must be understood not as knowledge but strictly in the special sense in which it is defined by (44), if made at all, are outnumbered by hope-raising claims. We read, for instance, that the Negentropy Principle of Information "applies in different branches of physics, technology, and *even in some very general problems of human knowledge*."[47] Such remarks remind us of the similar claims that machines can think, which grew out of the fact that "thought" is given a different meaning than thought.[48] Here, too, the claims ignore that "information" is not information. And even the founder of cybernetics protested that "information is information, not matter or energy," which implies that it is not entropy either.[49] For some, it seems, all this has been in vain.

Time and again, we can see the danger of calling a shovel by the name of "spade" on the ground that there is some similarity between the two and that the scientific terminology should not be confused with that of the common vernacular. But our minds simply cannot be schizophrenic to the extent of keeping the two terminologies totally apart. What Bentham said about "utility" and what I have said elsewhere in this essay about "continuum" and "thought" applies to "information" in information theory: it was an unfortunately chosen word for what it purports to denote.

[46] Louis de Broglie, *New Perspectives in Physics* (New York, 1962), pp. 66, 72 f.

[47] Brillouin, "The Negentropy Principle," p. 1153 (my italics); also *Science and Information*, p. xi.

[48] See Chapter III, Section 10, above.

[49] Wiener, *Cybernetics*, p. 132.

APPENDIX C *A Simple Model for Boltzmann's H-Theorem*

An extremely simple model was imagined by P. and T. Ehrenfest in order to illustrate how collisions in a system of particles obeying only the laws of locomotion bring about a chaotic state provided that the system satisfies the statistical postulate (mentioned in Chapter VI, Section 2). The model is used with the same intent by some manuals of statistical mechanics.[1] It has the great advantage that it does not require any special familiarity with the science of thermodynamics itself and, hence, its analysis is accessible also to the uninitiated. Curiously, however, if an uninitiated (like myself) pushes this analysis further than it is currently pushed, he will discover that in fact the model brings into the open the cumulative flaws of the claim that the statistical approach constitutes a bridge between locomotion and thermodynamic phenomena. In particular, such an analysis bears out my earlier point that the existence of collisions renders most of the formal arguments of statistical mechanics idle in relation to actual systems.

Let us imagine a very large number of particles moving in a plane, each having initially only one of the four velocity directions shown in Figure 4. In the same plane there also are numerous obstacles consisting of equal squares distributed irregularly and oriented as shown in black on the same diagram. It is clear that the velocity direction of a particle after colliding with an obstacle may change into one of two other directions.

[1] P. and T. Ehrenfest, *The Conceptual Foundations of the Statistical Approach in Mechanics* (Ithaca, N.Y., 1959), pp. 10–13. Also D. ter Haar, *Elements of Statistical Mechanics* (New York, 1954), pp. 336–339.

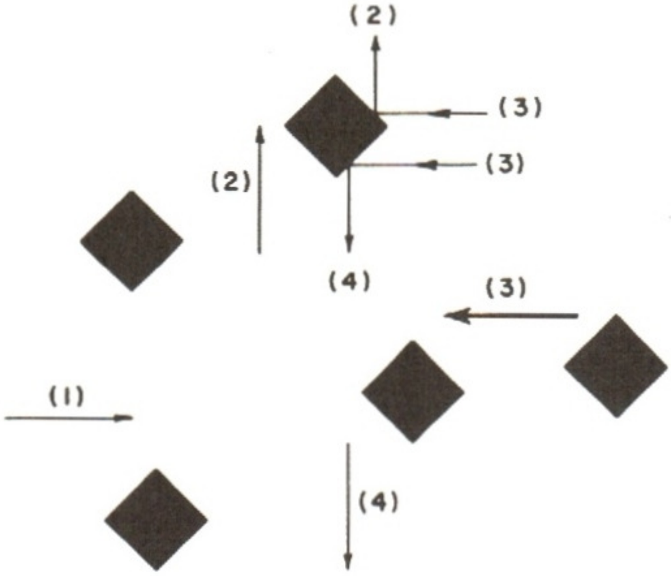

Fig. 4

For instance, direction 1 may change either into 2 or 4 but not into 3. There are then only four "states" (directions) in the system at all times.

Let N_1^n, N_2^n, N_3^n, N_4^n, $\sum N_i^n = N$, be the number of particles in each state at the time t_n. Let N_{ij}^n be the number of particles that, as a result of the collisions during the time interval $\Delta t_n = t_{n+1} - t_n$, are changed from state i into state j. Since we can choose Δt_n so small that no particle collides with more than one obstacle during such an interval, we have $N_{13}^n = N_{24}^n = N_{31}^n = N_{42}^n = 0$. The statistical postulate then says that the distribution of the particles at t_n is such that

$$
\begin{aligned}
&N_{12}^n = N_{14}^n = \kappa N_1^n, \qquad N_{23}^n = N_{21}^n = \kappa N_2^n,\\
&N_{34}^n = N_{32}^n = \kappa N_3^n, \qquad N_{41}^n = N_{43}^n = \kappa N_4^n.
\end{aligned}
\tag{1}
$$

Obviously, we must have $0 < 2\kappa \leq 1$. From (1) we obtain

$$
\begin{aligned}
N_1^{n+1} &= \kappa(N_2^n + N_4^n - 2N_1^n) + N_1^n,\\
N_2^{n+1} &= \kappa(N_1^n + N_3^n - 2N_2^n) + N_2^n,\\
N_3^{n+1} &= \kappa(N_2^n + N_4^n - 2N_3^n) + N_3^n,\\
N_4^{n+1} &= \kappa(N_1^n + N_3^n - 2N_4^n) + N_4^n,
\end{aligned}
\tag{2}
$$

and further,

$$
\begin{aligned}
N_1^{n+1} - N_3^{n+1} &= (1 - 2\kappa)(N_1^n - N_3^n),\\
N_2^{n+1} - N_4^{n+1} &= (1 - 2\kappa)(N_2^n - N_4^n),\\
N_1^{n+1} + N_3^{n+1} - N_2^{n+1} - N_4^{n+1} &= (1 - 4\kappa)(N_1^n + N_3^n - N_2^n - N_4^n).
\end{aligned}
\tag{3}
$$

This system yields

$$
\begin{aligned}
N_1^n - N_3^n &= (N_1^0 - N_3^0)(1 - 2\kappa)^n,\\
N_2^n - N_4^n &= (N_2^0 - N_4^0)(1 - 2\kappa)^n,\\
N_1^n + N_3^n - N_2^n - N_4^n &= (N_1^0 + N_3^0 - N_2^0 - N_4^0)(1 - 4\kappa)^n.
\end{aligned}
\tag{4}
$$

Let us first consider the case in which $0 < 1 - 2\kappa < 1$, when $|1 - 4\kappa| < 1$. In combination with $\sum N_i^n = N$, relations (4) yield for $n \to \infty$

$$\lim N_1^n = \lim N_2^n = \lim N_3^n = \lim N_4^n = N/4. \tag{5}$$

Now, if the initial state is chaotic, i.e., if $N_1^0 = N_2^0 = N_3^0 = N_4^0$, then (2) shows that the system will continue forever in the same state. If the initial state is not chaotic, then (5) shows that it will tend toward a chaotic state. We have thus reached the result obtained by the Ehrenfests.[2]

However, for $\kappa = 1/2$ relations (4) yield

$$\begin{aligned} N_1^n = N_3^n &= N/4 + (-1)^n(N_1^0 + N_3^0 - N_2^0 - N_4^0)/4, \\ N_2^n = N_4^n &= N/4 - (-1)^n(N_1^0 + N_3^0 - N_2^0 - N_4^0)/4. \end{aligned} \tag{6}$$

In this case, the mechanical system does not tend toward a chaotic state, unless the special initial condition $N_1^0 + N_3^0 = N_2^0 + N_4^0 = N/2$ also obtains.[3] I suppose that this exception would be set aside by the argument that we can always take Δt_n so small as to have $\kappa < \frac{1}{2}$.

Now, if we accept this last view, then we can take Δt_n so small as to make $\epsilon = (\Delta N_i^n)/N_i$ as small as we please. On this basis, we can prove in addition that Boltzmann's H-theorem, in its stricter form, is true for the model under consideration. From (2), we have

$$\begin{aligned} \Delta N_1^n &= \kappa(N_2^n + N_4^n - 2N_1^n), \\ \Delta N_2^n &= \kappa(N_1^n + N_3^n - 2N_2^n), \\ \Delta N_3^n &= \kappa(N_2^n + N_4^n - 2N_3^n), \\ \Delta N_4^n &= \kappa(N_1^n + N_3^n - 2N_4^n). \end{aligned} \tag{7}$$

Since we can neglect now the terms of second order of smallness with respect to ϵ, from Boltzmann's formula $H = \sum N_i \ln (N_i/N)/N$ and (7) we obtain

$$N\Delta H^n = \sum \Delta N_i^n \ln N_i^n = \kappa \sum (N_i^n - N_j^n) \ln (N_j^n/N_i^n) \leq 0, \tag{8}$$

where the subscripts in the last sum are taken circularly. This proves the theorem.

But let us reexamine critically the above proofs.

We may observe, first, that (as noted in Chapter VI, Section 2) nothing prevents us from assuming that relations (1) are true in actuality for *one* value of n. However, the proof of the results (5) and (8) require that the

[2] An interesting point is that the same result follows from far more general conditions. Relations (5) obtain even if κ is replaced in (1) by κ_n provided that we still have $0 < 1 - 2\kappa_n < 1$ for every $n \geq 0$.

[3] This strange case of a system which, though not chaotic initially, becomes chaotic after the first interval Δt_0 is an interesting illustration of how much is assumed away by the statistical interpretation of thermodynamics.

same relations are satisfied for *any* n. And it goes without saying that this requirement is not fulfilled but for some specially designed models.

Second, the proof of (8) requires further that we are free to choose Δt_n so small that ϵ should be sufficiently small. The explicit condition is that $\kappa = \kappa_0 \Delta t_n$, i.e., κ itself is of the first order of magnitude with respect to Δt_n. How strong this condition is in connection with (1) may be seen by imagining Δt_n so small that no particle collides with any obstacle during that interval. There is here a troublesome issue pertaining to the discreteness of the phases through which the system passes as time varies *continuously*. This issue bursts forth even if we grant that there exists an infinite sequence $[t_n]$ such that (1) is true for every t_n and also that κ is so small that ϵ is sufficiently small. For what we can prove then is only that (5) and (8) are true for the discrete sequence of instants $[t_n]$. Consequently, nothing is said about the state of the system at any $t \neq t_n$. That is, we do not know whether N_i^t, the number of particles in the state i at $t \neq t_n$, will tend toward $N/4$ for $t \to \infty$. Nor do we know whether ΔH^n will increase between t and t' if t and t' do not belong to the sequence $[t_n]$.

Third, let us ignore the snags mentioned above and assume that our system fulfills the statistical postulate expressed by (1). Let us then consider the system obtained by reversing all velocities at t_n, $n > 0$. This system does not fulfill the statistical postulate required for the proof of our theorems (5) and (8). For if it did, then relations (3) should remain valid after κ is replaced by some κ' and n interchanged with $n + 1$. This condition yields $(1 - 2\kappa)(1 - 2\kappa') = 1$, which, in view of the fact that $0 \leq 1 - 2\kappa < 1$ and $0 \leq 1 - 2\kappa' < 1$, cannot be true.[4]

[4] Of course, nothing can be said about the system obtained by reversing all velocities at t_0.

APPENDIX D *Boltzmann's Analogies of the H-Curve*

Boltzmann conceived some telling analogies to explain how the Entropy Law is reflected into his H-curve.[1] To reexamine one of these analogies, let us consider a series of tossings of a perfect coin and put $e_k = 1$ or 0 according to whether or not the k-th tossing shows "tails." Let n be a given integer and $a_i = e_i + e_{i+1} + \cdots + e_{i+2n-1}$ be a $2n$-moving sum of the series (e_i). By plotting the points

$$(1) \qquad x_i = \frac{i}{n}, \qquad y_i = \left|1 - \frac{a_i}{n}\right|,$$

we obtain what Boltzmann calls the H-curve of this "lottery." It is an elementary point of statistics that most of these points lie near the axis of the abscissae, whereas those for which y_i is close to unity are extremely rare events. Boltzmann is right in concluding that these last points are more likely to constitute "peaks" of the H-curve rather than be on an ascending or descending slope: $y_{i-1} < y_i$, $y_{i+1} < y_i$ is more probable than $y_{i-1} \gtrless y_i \gtrless y_{i+1}$. The conclusion can be verified by ordinary algebra, yet too complicated for inclusion here. But the same algebra shows a point not touched by Boltzmann: if y_i is very close to zero there is almost the same probability for $y_i < y_{i+1}$ as for $y_i \geq y_{i+1}$. That is, by the analogy intended, the chances that a chaotic state, once attained, should perpetuate itself for quite a long time are not as great as Boltzmann generally claims in defending his formulation of the Entropy Law.

[1] See note 24 of Chapter VI, above.

One difference between the actual microstates of a gas and the analytical example that is crucial for Boltzmann's point about the average "trend" of the H-curve concerns the special structure of the series (a_i). To wit, if $y_i = 1$ the probabilities that $y_{i+1} = 1$ and $y_{i+1} < 1$ are the same, 1/2.[2] Moreover, if $y_i = 1$ and $y_{i+1} = 1 - 1/n$, then obviously $y_{i+k} \leq y_{i+2}$ for any $k \leq 2n$. A similar proposition holds for the case in which $y_i = 1$, $y_{i+1} = 1 - 1/n$, $y_{i+2} \leq y_{i+1}$. By the intended analogy, this means that if the entropy starts to increase from its lowest level it cannot *possibly* return to it before the system undergoes N additional changes, N being the number of particles in the system. In the case of a gas this number is of the order of 10^{23}, an impressive magnitude. On the other hand, there is nothing in the supposition that microstates are equiprobable to prevent a molecule that caused the change from $y_i = 1$ to $y_{i+1} = 1 - 1/n$ from returning thereafter to its earlier state. On the contrary, the formula of thermodynamic probability—(5) or (6) of Chapter VI—is based on the assumption of complete independence of the successive microstates. That is, any macrostate may be succeeded immediately by a least probable one. There is then a discrepancy between Boltzmann's thermodynamic probability and his "lottery": in the lottery, a_i, instead of being independent of every a_k, is correlated stochastically with $a_{i+1}, a_{i+2}, \ldots, a_{i+2n-1}$. Perhaps Boltzmann unwittingly sought to reflect in the analogy the intuitive idea that there must be *some* "correlation" between successive macrostates. Indeed, it is hard to conceive that during a short time interval, Δt, the gas molecules in one corner of the recipient have the same "chance" to collide with those in the opposite corner as with the neighboring ones.[3] However, intuitive though this idea may be, Boltzmann made no allusion to it, probably because it would have confronted him again with the fundamental difference between a stochastic sequence and the phase sequence of a mechanical system.

[2] If, instead, each a_i is determined by an independent tossing of $2n$ coins *at a time*, then the probability of $y_{i+1} = 1$ is independent of whether or not $y_i = 1$ and is always equal to $1/2^{2n}$. In determining the average number of the cases for which $y_i = 1$ in a sequence of N successive a_i's, Boltzmann uses this formula (which is a mistake).

[3] Cf. P. W. Bridgman, *Reflections of a Physicist* (2nd. edn., New York, 1955), pp. 255–257.

APPENDIX E *The Birkhoff Theorems*

Let D be a bounded domain and T be a one-to-one transformation of D into itself. That is, T is such that to every point M of D there corresponds one and only one point $M_1 = T(M)$ of D, and conversely. In addition, let T be a measure preserving transformation, which means that if the subset S of D is transformed into the subset S', then S and S' have the same measure. Let us denote by $f_n(M;S)$ the relative frequency of the points

(1) $M, \qquad M_1 = T(M), \qquad M_2 = T(M_1) = T^2(M), \ldots, M_n = T^n(M)$

that belong to some given subset S of D.

Birkhoff's "ergodic" theorem[1] says that if T has the properties mentioned above, then

$$\lim_{n \to \infty} f_n(M;S) = f(M;S). \tag{2}$$

In other words, the relative frequency f_n has a limit which depends not only on S but also on M.

Let us assume that T has also the following property: the measure of any proper subset S of D which is transformed by T into itself is either zero or equal to that of D. In this case T is said to possess the property of *metrical transitivity*, or *indecomposability*, or *ergodicity*. Birkhoff's second theorem[2] says that under this stronger condition

$$\lim_{n \to \infty} f_n(M;S) = \frac{\text{Measure of } S}{\text{Measure of } D} = f(S). \tag{3}$$

[1] See note 40 of Chapter VI, above.
[2] See note 37 of Chapter VI, above.

In other words, the limit of f_n is the same regardless of the initial state. Consequently, all *gross* states will appear with the same frequency in any mechanical system that is metrically transitive.

For an example of a nontransitive continuous transformation, which is related to our simple system of a perfectly elastic billiard ball,[3] let D be the square $0 < x \leq 1$, $0 \leq y \leq 1$, and let $M_1(x + y, y)$ or $M_1(x + y - 1, y)$ correspond to $M(x, y)$ according to whether $x + y \leq 1$ or > 1. This is a one-to-one transformation which transforms D into itself and preserves the areas but it is not ergodic: the subset $0 < x \leq 1$, $0 < a \leq y \leq b < 1$, of area $b - a \neq 0$ is transformed into itself. For $M_0(x_0, y_0)$ and y_0 irrational the frequency of $f_n(M_0, S)$ tends toward σ, σ being the linear measure of the intersection of S with the line $y = y_0$. Clearly, σ may have any value between 0 and 1. For y_0 rational, the transformation of the intersection of D and the line $y = y_0$ is again nontransitive and, hence, (3) does not apply; the limit of $f_n(M_0, S)$ depends on M_0, too.

[3] Figs. 1 and 2 and note 42 of Chapter VI, above.

APPENDIX F *Probability and the Time Dimension*

1. Propositions that come to us through a long lineage of authority are apt to become surprisingly resilient. This seems to be the case with the proposition that I have denounced as false in Chapter VI, Section 4, namely:

A. *If an uncertain event has not occurred during a series of observations we have not waited long enough.*

Not only does this proposition come up regularly in our chats on some probability problems but it also appears in the formal arguments of many an authority on the subject. Even an authority on probability such as Henri Poincaré aimed at upholding the logical tightness of statistical thermodynamics by arguing that "the chances are that we should wait a long time for the concourse of circumstances which would permit a retrogradation [of entropy]; but sooner or later they will occur, after years whose number it would take millions of figures to write."[1] More recently, A. Wald invoked an equivalent idea in defending the Frequentist school of probability.[2] In fact, the idea is absolutely fundamental for that school of thought. To recall, Frequentists define the probability coefficient, p, of an event E as the limit toward which the observed frequency, f_n, tends in a limitless series of observations. That is, for any $\epsilon > 0$ there exists an $N(\epsilon)$ such that for any $n \geq N(\epsilon)$ we have

$$(1) \qquad |f_n - p| < \epsilon.$$

[1] H. Poincaré, *The Foundations of Science* (Lancaster, Pa., 1946), p. 304.

[2] A. Wald, "Die Widerspruchsfreiheit des Kollektivbegriffes," *Colloque consacré à la théorie des probabilités* (Paris, 1938), II, 92.

The only difference between this definition—which actually implies a postulate about physical facts—and that of mathematical limit is that, as Frequentists explicitly insist, $N(\epsilon)$ only exists but cannot be *named.*[3] That in spite of this qualification the Frequentist position emasculates the concept of probability can be shown without much difficulty.[4] But the hidden relation between this position and Proposition A calls for some additional spadework.

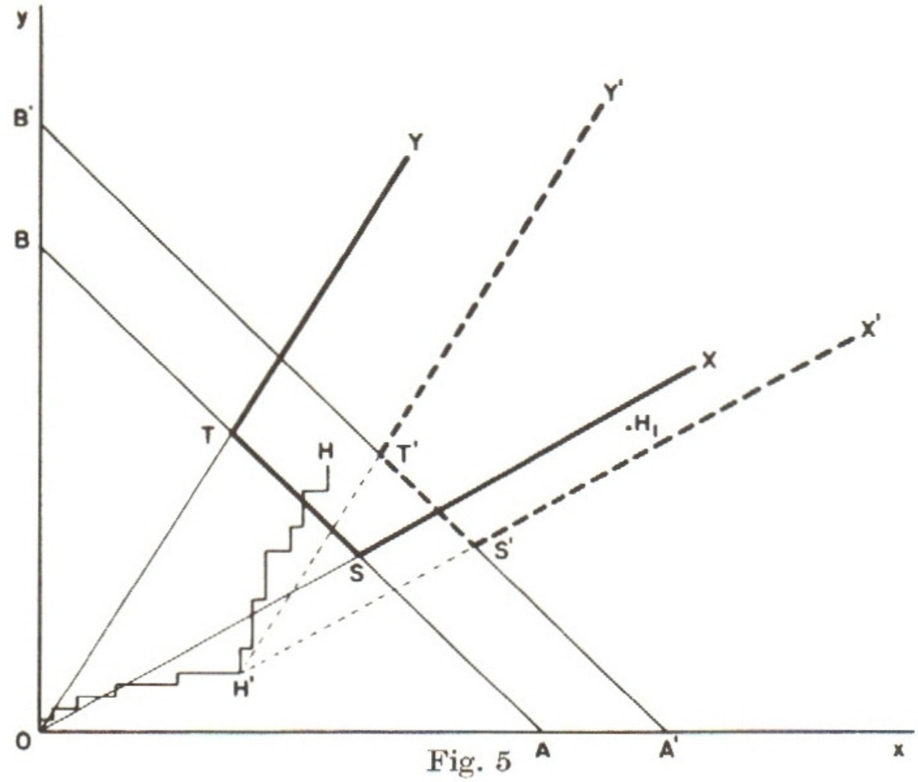

Fig. 5

Let us consider the simple case in which $p = 1/2$ and let us represent the successive absolute frequencies of E and non-E in a series of observations by the coordinates Ox and Oy respectively (see Figure 5). The history of any such series is represented by a staircase line such as OH. All possible results at the end of n observations are points on the line $x + y = n$. Let $0 < \epsilon < 1/2$ be given and let OX and OY correspond to the equations $y = (1 - 2\epsilon)x/(1 + 2\epsilon)$ and $y = (1 + 2\epsilon)x/(1 - 2\epsilon)$ respectively.

[3] On this point see Wald, *ibid.*, p. 92, and, especially, Ernest Nagel, "Principles of the Theory of Probability," *International Encyclopedia of Unified Science* (Chicago, 1955), vol. I, part 2, pp. 363, 369. Hans Reichenbach, in *The Theory of Probability* (Berkeley, 1949), pp. 347, believes that the impasse can be avoided by requiring only that the sequence f_n be *semiconvergent*, by which he means that only the finite sequence of f_n accessible to human observation shall "converge 'reasonably.'" He even adds that, if the infinite rest does not converge, "such divergence would not disturb us." The proposal either ignores the difficulty of defining convergence for a finite series or smuggles a dialectical "reasonably" into a manifestly positivist philosophy.

[4] Cf. Section II of my article "The Nature of Expectation and Uncertainty" (1958), reprinted in AE.

Let AB correspond to $x + y = N(\epsilon)$. Condition (1) can now be interpreted thus: above AB, no history line can get outside the domain $XSTY$; only we cannot know *ex ante* its exact form. It should be observed that *ex ante* we cannot exclude either the possibility that the history line shall get outside OST before reaching AB or that it should pass through some arbitrarily chosen point within $XSTY$.

Let us assume the case to be that in which $H'(x', y')$ is outside OST and put $x' + y' = n'$. Let us ignore the first n' observations and apply the above argument to the rest of the series. So, let $H'X'$ and $H'T'$ be parallel to OX and OY respectively and let $A'B'$ be $x + y = N'(\epsilon) + n'$, where $N'(\epsilon)$ applies to the truncated series and need not be equal to $N(\epsilon)$. We can then say that the history line from H' on must remain within the domain $X'S'T'Y'$ and that no special point within this domain is excluded. In other words, the history line may pass through H_1. But this contradicts the first "prediction." And we should not fail to note also that the proof of this contradiction invokes only that $N(\epsilon)$ and $N'(\epsilon)$ exist, not that they can be named.

The contradiction could be reduced if we would introduce an additional strange principle. An individual who comes as a kibitzer, so to speak, while another person is already engaged in observing a phenomenon should not make any stochastic prediction, the reason being that his prediction may contradict the other's. One can see how truly strange this proposal is by noting that any human is a kibitzer in relation to an imagined permanent observer of nature. The upshot is that only such an observer can make valid stochastic predictions and only if the origin O in our diagram represents the origin of the universe! Only in relation to this origin, therefore, can we say whether an observer has waited "long enough." Letting the origin of a series of observations slide arbitrarily on the scale of cosmic time leads to the contradiction unraveled above.

2. Among the various paradoxes thought up by Émile Borel in relation to probability the most amusing one is that of the monkey typists which by chance may type, say, the complete works of Shakespeare. The moral of the paradox is that, although the paper-and-pencil operations may attribute to an event a *positive* probability, the event need not be observed in actuality. And as we have seen, an equivalent form of this paradox besets the probabilistic interpretation of thermodynamics: although a reversal of entropy has a positive probability, it has never been observed. The purpose of Proposition A is precisely that of clearing away paradoxes of this type. Curiously, the proposition is invoked only in connection with events of extremely low probability. For the other cases, the accepted teaching is that if the event has not occurred, we had better revise the

a priori probabilites used in our calculations instead of waiting any longer.

Proposition A, I contend, is fallacious because it confuses a true proposition with its converse, slightly changed by a verbal legerdemain. The true proposition is that the probability of a random event to occur in a series of future observations tends monotonically to unity as the number of observations increases indefinitely.[5] Translated into the common vernacular, it says that "*if one is prepared to wait a sufficiently long time* the event is bound to occur." This proposition is turned next into "the event will occur only if *one waits a sufficiently long time.*"

We should note, first, that during this course "a sufficiently large number of observations" has been translated into "a sufficiently long time." We may grant—with some reservations, though—that the first expression has some objective meaning. But what could be the meaning of the second expression? A time interval is a dimensional entity, not a pure number. And like all dimensional entities, its measure may be fantastically small or fantastically large according to the unit chosen. But let us postpone for a while the discussion of this issue and pass to the second observation. Even without the questioned translation, nothing in the theory of probability bears out the converted proposition. On the contrary, the received doctrine teaches that it is definitely false. According to this doctrine, an all-spade hand, though a "rare event," may be the first to be dealt at a bridge game. For even if the hand has not come up at all during one million deals, its probability is not greater on the one million and first deal than on any other. Nor is it smaller in case the hand has occurred on the immediately preceding deal. It is thus seen that concealed deeply in the apparently innocuous reply "you have not waited long enough" there lies the heretical dogma of Hyacinthe Azaïs and Karl Marbe of an inherently compensatory hazard.[6] A simple way of exposing this heresy is to draw attention to the fact that if, after a long run of "tails" in the tossing of a fair coin, one million people were each to toss a fair coin, about half of them would still get "tails"—as the orthodox theory says. And if one would now counter that the Marbe-Azaïs dogma applies only to the tossings of the *same* coin—which is a gratuitous interpretation—he should be reminded of Joseph Bertrand's famous aphorism, "the coin has neither conscience nor memory."[7]

[5] See Chapter II, Section 7, above.

[6] For which see AE, p. 250. As I have subsequently discovered, Azaïs and Marbe had a predecessor in J. L. d'Alembert. See his *Mélanges de littérature, d'histoire et de philosophie* (5 vols., Amsterdam, 1767), V, 283. D'Alembert argues that the supposition that "heads" may never come up "is possible within mathematical rigorousness. It is only *physically* that the proposition is false." And, just like Azaïs and Marbe, he advises that after a long run of "heads" we should bet on "tails," "as many a gambler does" (p. 289). My translations.

[7] Joseph Bertrand, *Calcul des probabilités* (Paris, 1889), p. xxii. My translation.

Another point that should be clear is that Borel's paradox is about a physical fact, not about states of mind in relation to subjective belief, nor about the rational move in a game involving risk. To wit, the observation that the probability of entropy reversal even in a drop of water is so small that "we can forget about it" may apply to the rational decision of whether to drink water but is alien to the paradox. Yet we find even a Nobel laureate for molecular physics arguing that "*on the scale of magnitudes that are of practical interest to us, perpetual motion of the second kind is in general so insignificant that it would be foolish to take it into consideration.*"[8] Equally familiar is the statement that "we can bet [that a reversal of entropy will not occur] for a billion generations to come."[9] Such statements cannot dispose of the paradox any more than Blaise Pascal's famous calculation of the gambling advantage of believing in God proves the factual existence of God.

Only an argument considering the probability as a physical coordinate can be pertinent to the issue. The snag is that in this case a positive probability, however small, by definition means that the corresponding event must occur *sometimes*—only we do not know *when*.[10] No belief and no bet can change this truth.

3. For an elucidating example of the argument based on physical probability, let us cite an authority such as Bridgman: "These probabilities [of entropy reversals] are so fantastically small that even in all the history of the human race the 'chances' are very small that such a thing has happened, and of course they are still smaller that any individual will observe such a thing in his own lifetime."[11] A stricter enunciation of the same idea, originating with Boltzmann, has perpetuated itself from one book to the next: "A simple calculation, using the appropriate law of probability [proves] that a chance combination of motions that led all the [mixed] hydrogen and oxygen back into their original [positions] *would . . . not occur* for $10^{10^{10}}$ years."[12]

[8] Jean Perrin, *Atoms* (London, 1920), p. 87; see also K. Mendelssohn, "Probability Enters Physics," in *Turning Points in Physics*, ed. R. J. Blin-Stoyle (Amsterdam, 1959), p. 51.

[9] Philipp Frank, "Foundations of Physics," *International Encyclopedia of Unified Science* (Chicago, 1955), II, 451.

[10] The argument applies even to quasi-impossible events. But this category has no special significance for the present argument.

[11] P. W. Bridgman, *The Nature of Thermodynamics* (Cambridge, Mass., 1941) pp. 162 f. See also Mendelssohn, "Probability Enters Physics," p. 53. In another place, however, Bridgman argues that "purely logical statistical considerations never can justify us in predicting events so rare that they have never yet been observed" (*Reflections of a Physicist*, 2nd edn., New York, 1955, p. 261)—a position with which I am in perfect agreement.

[12] David Bohm, *Causality and Chance in Modern Physics* (London, 1957), p. 161

It is thus seen that the time-dimension is completely ignored in spite of its crucial role in problems of this sort. For let us assume, for example, that the probability of some event E in a stochastic structure of outcomes is 10^{-4}. Small though this probability is, there is a very high probability, $1 - 10^{-10}$, that E should occur at least once in 2.3×10^5 outcomes. Now, if one outcome occurs every second, we need be willing to wait only three days in order to be fairly certain to witness E. In this case, we can hardly say that E is a rare event in time. But let the velocity of outcomes be one outcome per century, and the same E would be an extraordinary event even in the life of our planet.

In general, let Δ be the interval of time during which a given mechanism produces one and only one outcome. If the time scale is now divided appropriately into intervals of size Δ, then during each such interval a particular event E may either occur only once or not at all. Let us assume, as is proper for the circumstance, that the mechanism is timeless (i.e., it remains throughout identical to itself). Let p be the probability that E shall occur during Δ and let $t = n\Delta$ be an interval during which we are prepared to observe the outcomes. The probability that E shall occur during t is $P(t) = 1 - (1 - p)^n = 1 - (1 - p)^{t/\Delta}$. This clearly shows that Bridgman's argument—that $P(t)$ is small because p is fantastically small—does not stand if Δ is so small that t/Δ is fantastically great. To say anything about the magnitude of $P(t)$ we must know the velocity $1/\Delta$ with which the mechanism in point produces outcomes. It is only in relation to Δ, taken as a unit, that we can say whether t is large or small.

On the other hand, the laws of thermodynamics tell us nothing about the velocity with which macrostates are changed in relation to clock-time. There is no basis, therefore, for speaking of an entropy flow in the strict sense.[13] This is the reason why thermodynamics cannot predict in the same fashion as mechanics.[14] Bridgman's argument thus hangs up in the air. Actually, it still would even if we granted that $P(t)$ is small.

4. To explain why no entropy reversal has been observed yet, some authors have introduced an additional proposition of an *atemporal* nature. Borel, in particular, is known for arguing that the axiom "*events whose probability is extremely small never occur* [are factually impossible]" ought to be part and parcel of the foundation of physical probability. By

(my italics); see also Perrin, *Atoms*, p. 87*n*. The "magic" figure $10^{10^{10}}$ goes back to L. Boltzmann's 1898 *Lectures on Gas Theory* (Berkeley, 1964), p. 444. The method on which he based his calculation was given in the paper cited in note 32 of Chapter VI, above, and is elaborated in D. ter Haar, *Elements of Statistical Mechanics* (New York, 1954), p. 342. For my criticism of this method, see Chapter VI, Section 2.

[13] Bridgman, *Nature of Thermodynamics*, pp. 140 f.

[14] See note 65 of Chapter V, above.

way of an example, Borel, like many others we have already cited, asserted that if the probability of an event is of the order of 10^{-200} the event "has never been observed and will never be observed by any human in the entire universe."[15] In this strict form, the proposition undermines the currently accepted speculation that life on earth has come about by the mere play of chance combinations.[16]

One particularly damaging difficulty with Borel's axiom should be obvious. Would the axiom hold for 10^{-199}, for 10^{-198}, and so on? Where must we stop? But for argument's sake, let us grant that the category of "extremely small probabilities" has a least upper bound π. In other words, let us assume that there may after all exist an elementary quantum for probability, too.[17] From the viewpoint of current probability theory, several serious obstacles stand in the way of this speculation. First, it is hard even to speculate on how the fundamental formulae of the calculus of probability should be modified in accordance with the new law. Second, and more important, the existence of a probability quantum will force us to admit on the basis of these formulae that for any event of probability $p > \pi$ there can be no run greater than r, r being determined by the inequalities $p^r \geq \pi > p^{r+1}$. The skeletons of d'Alembert, Azaïs, and Marbe will have not only to be let out from the cupboard but also resuscitated to a glorious life.

[15] Émile Borel, *Elements of the Theory of Probability* (rev. edn., Englewood Cliffs, N.J., 1965), pp. 57 ff. Boltzmann, in *Gas Theory*, p. 444, expresses the same idea in a dialectical form: an extremely small probability is "practically equivalent to *never*." But the idea can be traced back as far as d'Alembert, note 6, above.

[16] P. Lecomte du Noüy, *The Road to Reason* (New York, 1948), pp. 122–126.

[17] As suggested by R. B. Lindsay and H. Margenau, *Foundations of Physics* (New York, 1936), p. 167.

APPENDIX G *Limitations and Extrapolations in Biology*

1. One main tenet of the doctrine of Change developed in this volume is the essential difference in the way actuality appears to the inquisitive mind as this mind shifts its attention progressively from the inorganic to the superorganic domain. On two occasions, the discussion of some economic issues required that I insist in greater detail on the difference between the biological and physicochemical domains. I took then the position that biology cannot achieve, in general, results of the same practical import as the sciences of inert matter and cannot achieve, in particular, the extravagant feats that a few overenthusiastic biologists claim to be now in the offing.[1] To justify this position, it should certainly suffice for a layman like myself to produce proof that most authorities share it in some form or another.[2] This is the layman's special privilege. In exchange, the layman is saddled with a tremendous handicap: the general prejudice that he misinterprets or exaggerates. A very recent statement by Erwin Chargaff should, I believe, spare my position any hasty judgment. Chargaff, whose laborious analyses of various nucleic acids supplied the indispensable scaffold for the recent advances in the knowledge of the nucleus and who should thus be particularly qualified to evaluate the situation, obviously thought that the situation calls for some strong wording: "The asinine prognostications of instant happiness through mail-order eugenics (in every household an Einstein, possibly embellished with the nose of Cleo-

[1] See Chapter X, Section 3, and Chapter XI, Section 5.
[2] See Chapter XI, Section 5, especially note 119.

patra) may be counted among the symptoms of the onset of barbarism, as evidenced by the increasing brutalization of humanity."[3]

I nevertheless think that I owe the interested reader a brief and explicit writ of my own position that the obstacles which limit our powers (and to a certain extent our understanding) in the biological field are inherent in the very conditions of man's existence and, consequently, are as lasting as these conditions.

2. After a series of vacillations extending over one hundred years, practically all biochemists now agree that every component of a living cell has a definite molecular structure at least when it exists in a state (ordinarily the crystalline state) that can be scrutinized in vitro by physicochemical procedures. However, the most important and most numerous of the components of a living cell differ from ordinary molecules in several crucial respects.

First, there is the dimensional immensity of these biomolecules. As they are now depicted in many manuals, most biomolecules are macromolecules, i.e., complex giant assemblies of atomic nuclei "surrounded by electronic clouds of fantastic and changing shapes."[4] The DNA complex of the rudimentary chromosome of a small virus such as the bacteriophage T4 has in all about 200,000 nucleotide pairs (approximately 1.3×10^8 daltons) divided among some 100 genes. The chromosomal complex of some aquatic animals contains between 10^{11} and 10^{12} nucleotide pairs; that of man, like that of any mammal, has about 5×10^9 such pairs.[5] But even a biomolecule taken by itself may have a molecular weight as great as 10^8 (according to some, even 10^{11}). This means that a biomolecule may have as many atoms as there are people in a medium-sized country or stars in an average galaxy. The difference is that the structure of a molecule is so orderly and delicate that a mere change of a few atoms may drastically alter the qualitative function of the biomolecule.[6]

[3] Erwin Chargaff, "What Really Is DNA? Remarks on the Changing Aspects of a Scientific Concept," *Progress in Nucleic Acid Research and Molecular Biology*, VIII (1968), 329.

[4] Albert Szent-Györgyi, "The Promise of Medical Science," in *Man and His Future*, ed. G. Wolstenholme (Boston, 1963), p. 192; James D. Watson, *Molecular Biology of the Gene* (New York, 1965), pp. 111–115.

[5] On the structure of the cell and of the macromolecules, see Watson, *Molecular Biology*, pp. 2–10, 69, 80–93, and *passim*. For a technical description of the DNA complex see Watson, pp. 261–296, and especially Chargaff (cited above). Briefer, simplified presentations are found in C. H. Waddington, *The Nature of Life* (New York, 1962), pp. 36–52; S. E. Luria, "Directed Genetic Change: Perspectives from Molecular Genetics," in *The Control of Human Heredity and Evolution*, ed. T. M. Sonneborn (New York, 1965), pp. 4–9; C. D. Darlington, *Genetics and Man* (New York, 1969), pp. 119–123.

[6] V. M. Ingram's discovery that only one amino acid differentiates between normal

The second difference is that macromolecules are polymeres, i.e., they are made of some standard building blocks—twenty amino acids for the proteins and five organic bases for the nucleic acids. Now, if there existed compounds in which, say, sodium and chlorine entered in varying proportions, one could not infer from an analysis showing only that the numbers of sodium and chlorine atoms are equal that the analyzed "substance" was common salt. The point is that "pure substance" loses its operational meaning in connection with substances which may exist in numerous polymeric or isomeric forms. In many parts of this domain there are only shadows. Indeed, a chemist working with a substance consisting of giant molecules may never see it in the sense in which one can look at salt or aspirin. For all these reasons, some biochemical authorities doubt that we can speak of a DNA molecule, for example, in the classical sense of this term.[7]

The third difference is that, giant though the biomolecules are, the chains of which they are made are ordinarily held together by *weak* chemical bonds—loosely, as it were. The result is that it does not take very much energy to break up such a fragile biomolecule into smaller ones—as happens, for instance, with the double helix of DNA which separates into its two strands when only slightly heated. This fact helps us to understand in part how inside a living cell weak bonds are almost continuously broken and remade at ordinary physiological temperatures, even though the remaking phenomenon is still surrounded by great mysteries.[8]

The fourth difference becomes apparent as soon as we ask not merely "what these substances *are*, but what they *do*."[9] This question takes us definitely beyond chemistry into quantum mechanics. In all probability, what makes a cell tick is a particular current of *single* electrons "cascading down and giving up their energy piecemeal." So, the living state does not consist of regular closed molecules, but of charge transfer complexes forming an "accumulation of ions against a gradient, concentrations becoming equalized in death."[10] This idea, which apparently is gaining

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and sickled-cell hemoglobin was mentioned in Chapter X (see note 50). On the other hand, in the case of a very large protein a palpable difference may not appear before a substantial number of such acids are changed.

[7] N. W. Pirie, "Patterns of Assumption about Large Molecules," *Archives of Biochemistry and Biophysics*, Suppl. 1, 1962, pp. 21–29, and Chargaff, "What Really Is DNA?" pp. 320–323, 327.

[8] Watson, pp. 60 f, 102–139, 285; Chargaff, "What Really Is DNA?" p. 323.

[9] Albert Szent-Györgyi, *Introduction to a Submolecular Biology* (New York, 1960), p. 10.

[10] *Ibid.*, pp. 25, 64, 132–134. See especially Szent-Györgyi's illuminating explanation of photosynthesis, *ibid.*, chap. iii. On the gradient concentration and the corresponding entropic transformations see also Watson, pp. 80–83, 102–109, 138, 160.

increasing acceptance, will not clear up the mystery of life. But, together with the weak-bond structure, it lends some theoretical support to the generally accepted fact that many reactions occur all the time inside a living cell in a manner not reproducible in vitro.[11] The familiar examples are the transformation of glucose into work with extremely little heat dissipation and the fixation of nitrogen by leguminous plants at ordinary temperatures of the soil. Still more important is the fact that most proteins synthesized outside a cell system have no biological activity and that most of those having such activities cannot be so synthesized. It is because of the impossibility of antibody formation outside a living body that the struggle against cancer and transplant rejection is so frustrating.[12]

3. We need look no further to understand why a consecrated authority in the field of molecular biology such as James D. Watson had to come around and admit that there is a *special* chemistry of the living state: "the synthesis of a protein does not proceed according to rules governing the synthesis of small molecules."[13]

We obviously cannot think of synthesizing any substance unless we have a blueprint of its structure. And in the case of a macromolecule even this *analytical* task is formidable. The deciphering of the *linear* structure of insulin—a *nonspecific* and quite small protein consisting of only fifty-one amino acids—was a feat worthy of a Nobel Prize (F. Sanger, in 1958). Given the size of these molecules, even with the recently developed apparatuses several years of hard work are needed before one may solve the linear structure of a small size protein.[14] And not only are biomolecules giant complexes, but also the number of the possible molecules of any given kind staggers the imagination. Take the case of a protein of a moderate size, say, of 20,000 daltons or about 170 amino acids. Since there are only twenty different amino acids, the number of all possible proteins of that size is 20^{170}. The side of the cubic box that could contain one molecule of each type is 10^{50} light-years long! If we believe in the Big Bang origin of the universe, there has not been enough time for all these molecules to exist. Nor will they all be observed during the remaining life of the human species.[15] Dimensions such as these may be properly described as supercosmic.

[11] Albert Szent-Györgyi, *Nature of Life: A Study on Muscle* (New York, 1948), pp. 17, 69 f, 76 f.

[12] Watson, pp. 396, 437, 441.

[13] Watson, p. 160, which should be compared with Watson's earlier pronouncement, p. 68.

[14] *Ibid.*, p. 170.

[15] Harold C. Urey, "The Origin of Organic Molecules," in *The Nature of Biological Diversity*, ed. John M. Allen (New York, 1963), p. 2.

It goes without saying that this fantastic variety works against any substantial crystallization of the gained know-how into some general procedure. Most of the time, each successful attack works only in some specific conditions. So, nothing at this time encourages us to think that the blueprint—let alone the synthesis itself—may eventually be obtained by a general procedure for every macromolecule. Actually, even for the synthesis of small *inorganic* molecules there is not one general recipe. Molecular biologists who are not prone to intemperate evaluation of the reality do not overlook the fact that even a bacterium cell contains between 3,000 and 6,000 different biomolecules, approximately half of which are giant protein molecules. Their conclusion is that "*we shall not know in the near (or conceivably, even in the distant) future the exact 3-D structures of all the molecules in even the smallest cells.*"[16] Obviously, they do not expect to be able to know the entire structure of a cell in all its details, and much less to be able to construct a cell from its elementary parts—atoms and electrons. They are content with the hope of understanding more and more of what goes inside the cell, which is a magnificent prospect in itself.

The issue obviously concerns man's power of manipulation in the microcosmic domain. And as I have argued in Chapter X, Section 3, it is the Principle of Indeterminacy that denies man this power: the only form in which man can handle matter is in bulk. To synthesize a substance, of small or large molecules, we must resort to chemical reactions in which atoms are freed from their initial bonds and rearranged into new formations by various submolecular forces. But even for very simple structures with no weak bonds the problem of determining in every new case what chemical compounds to use in the reaction and what free energy may trigger it is far from simple. When it comes to the synthesis of a macromolecular compound, the obstacles are formidable, and it is easy to see why. There simply is no sure way of compelling millions of atoms to get into their precise positions of the corresponding 3-D structure. Besides, the numerous weak bonds further complicate matters: the chemical structure is liable to break into pieces before it is completely erected.

One may, it is true, go about the synthesis of a polymere of known structure by building it block by block in successive steps. The feat of Vincent du Vigneaud, who in 1953 synthesized the first protein, will certainly be duplicated for other biomolecules (as it has been actually done). But if we leave aside the fact that this expansion will not come about automatically—each synthesis has its specific difficulties—we should note that the oxytocin synthesized by Vigneaud consists of only eight building

[16] Watson, p. 100. See also Chargaff, "What Really Is DNA?" p. 329. We may again note that Max Perutz and J. C. Kendrew shared a Nobel Prize in 1962 for solving the 3-D structure of hemoglobin and myoglobin.

blocks! This number marks perhaps the lower limit of protein simplicity. But equally certain is that there must be an upper threshold for the number of building blocks that man can put together in the right way by an *ordinary* chemical reaction.

We may understand now why it took nature on our planet and it takes nature elsewhere billions of years to put together the simplest cell. Yet many biochemists now believe that man is on the verge of achieving the same feat in much less time and in a far more spectacular way: by mixing some inert ingredients in a test tube and exclaiming "Fiat," in the manner Genesis began according to the Scripture. Hardly any glorifying survey of the powers of biology fails to mention the experiment of S. L. Miller, who obtained a mixture of some typically organic compounds (including some amino acids) by subjecting a mixture of simple compounds to a high electrical discharge.[17] Nevertheless, given the complications signaled in connection with the systematic synthesis of macromolecules as well as the fathomless complexity of the charge transfer complexes of a living cell, we may rest assured that man cannot become a giver of life. Think only of the fact that in spite of all the journalistic din, we still do not know how many proteins—let alone their kinds—are in the smallest cell. And this number, as I pointed out earlier, is not to be counted on the fingers. Even some of those biologists who cannot resist proclaiming that the recent breakthroughs "*will soon enable us to understand all the basic features of the living state*" end by admitting that "the structure of a cell will never be understood in the same way as that of water or glucose molecules."[18]

4. Undoubtedly, what man can do has not always been preceded by what he understands. Throwing a stone, starting a fire, or smelting ore are not the only examples. We still do not have the slightest idea of how most drugs (those produced by the organism itself not excepted) achieve their effects. So accustomed are biologists to this situation that even those who hail the recent theoretical achievements turn to the possibility of semiblind empirical findings in defending an overenthusiastic view of what is in store for biology.[19] On what sort of empirical successes do such views feed?

Since we cannot put together a living cell in the same way as we put

[17] S. L. Miller, "Production of Some Organic Compounds Under Possible Primitive Earth Conditions," *Journal of the American Chemical Society*, LXXVII (1955), 2351–2361; S. L. Miller and H. C. Urey, "Organic Compound Synthesis on the Primitive Earth," *Science*, July 31, 1959, pp. 245–251.

[18] Watson, pp. 69, 85. The reader may find it highly instructive to look over Watson's long list of biological mysteries, beginning with "the primary function of histones" and ending with what causes a normal cell to "cease to grow and divide at the correct time." *Ibid.*, pp. 185, 442, and *passim*.

[19] E.g., E. L. Tatum, "Perspectives from Physiological Genetics," p. 28, and especially Sonneborn, "Discussion—Part III," pp. 126, both in *Control of Human Heredity*.

together a house (for example), we had to come around to the idea of taking ready-made cells and trying to "remodel" them according to our desire. The method, which is a close substitute for a chemical reaction, is tantamount to shooting clouds of billiard balls into billions of billions of configurations of similar balls, with the hope of obtaining a few desired patterns through the ensuing knock-outs. The analogy applies not only to the use of any radiation or mutagenic agents but also to the more recent techniques of algeny—another of the terms coined by Lederberg, short for genetic *alchemy*.[20] These new techniques consist of transformation, transduction, and conjugation.[21] My metaphor makes it perfectly clear why the probability of a right hit—even if, as in transduction, some balls are carried by a virus—is extremely low, while that of a lethal or an undesirable mutation is very high. With such a low efficiency, algeny has only a limited field of application.[22] And in view of the difficulties inherent in the nature of the macromolecules, it does not seem likely that this efficiency could be improved substantially in the near or the distant future. All the less can we count on bringing this efficiency to perfection so that a single cell, not only some cells among immensely many, may be remodeled exactly according to our intentions. The point is important because without such a technique biological engineering can hardly have any practical value. In addition, the point leads us to another solid obstacle to which the heralds of the genetical millennium seem to pay no attention.

For the argument's sake, let us assume that we knew how to remodel a single cell into a preselected pattern. Now, it is obvious that even if we want only to remodel a structure, a cell or a building, we still need a complete blueprint of that structure. Also for the argument's sake, let us pass over the difficulties of this requirement that were mentioned a while ago. There is one additional difficulty which is not likely to arrest the attention of a molecular biologist. Molecular biologists, because they work almost exclusively with masses of phages and bacteria, tend to identify their

[20] Joshua Lederberg, "Experimental Genetics and Human Evolution," *American Naturalist*, C (1966), 521.

[21] Transformation is the process in which a selected DNA chain is used for knocking out and replacing an equivalent chain of a cell nucleus. In transduction, a virus is used as a carrier of the chain inside the nucleus. Conjugation corresponds to a process analogous to cell mating. See Morgan Harris, *Cell Culture and Somatic Variation* (New York, 1964), pp. 84–95; Watson, pp. 215–228; Darlington, *Genetics and Man*, pp. 174–176.

[22] In connection with my evaluation of the practical applications of biological knowledge to husbandry (Chapter X, Section 3), I should add that occasional desirable mutations in plants have been obtained by radiation or chemical mutagens. Yet most advances in husbandry have so far been the result of "accidents" in crossbreeding—the most famous case, the hybrid corn, not excepted. An interesting review is offered by Paul C. Mangelsdorf in "Genetics, Agriculture, and the World Food Problem," *Proceedings of the American Philosophical Society*, CIX (1965), 242–248.

own position with the chemist's. Indeed, if a molecular biologist determines the structure of a single bacterium from a colony grown from one bacterium, he is practically certain that any other cell from the colony will have the same structure.[23] The probability of a mutation, we know, is extremely low.

The problem changes fundamentally in case we wish to remodel an egg of a higher animal, especially, of man. Apart from the irrelevant case of genetically identical and totally homozygotous parents (save for sex), a fertilized egg of any sexually reproduced species is a *unique* entity, in the sense that we cannot possibly obtain a faithful copy of it.[24] The same applies to a gamete, i.e., to an ovum or a spermatozoon. The impasse is irreducible: if we use the unique cell for analysis, there is nothing left for remodeling.

5. Insurmountable obstacles to eugenic engineering emerge almost from every direction. Let us, for example, observe that if we want to change the chemical structure of a cell or of any compound, it is not for the sake of that structure but for that of its qualitative functions. Consequently, if genetical engineering is to become a useful reality, we must know not only how to change cell C_1 into C_2 but also what qualitative manifestations are associated with every cell structure. In other words, we must know the complete relation between genotypes and phenotypes for every species of animal, plant, or bacterium we may wish to remodel.

Now, the point that characters are only exceptionally related to well-defined chemical reactions is freely accepted by almost every molecular biologist. Some note also that most characters are "hopelessly complex,"[25] so that just to describe a phenotype completely is a hopeless task. Moreover, the chemist's predicament (on which I dwelt in Chapter V, Section 1) is even more burdensome in the case of a molecular geneticist. Since in most cases one must observe first a chemical structure in order to know its qualities, the molecular geneticist has to observe and describe the phenotype of every possible genotype. Time and again, simple arithmetic shows the impossibility of this prerequisite.

We should first recall a few properties of DNA. As we know since the Crick–Watson discovery, each nucleotide may be filled by one of the four organic bases—adenine (A), cytosine (C), guanine (G), and thymine (T)—

[23] Needless to add, the same applies to the eggs of animals reproduced asexually. I may also note that to determine the structure of a colony we may in fact need to sacrifice quite a large number of individuals.

[24] Since this is an argument by *reductio ad absurdum*, it may ignore the fact that a higher animal, if totally homozygotous, is not viable: there always are some genes that are lethal in the homozygotous state.

[25] Watson, p. 420.

in such a way that A is always paired with T and C is always paired with G.[26] In addition, according to an older and highly important finding of Chargaff, the proportion of the pairs (A, T)—and, perforce, of the pairs (C, G)—in the DNA complex of every individual of a given species is the same.[27] Finally, we must take into account the fact that any switching around of a pair produces a different DNA, because the two strands of the double helix are not interchangeable.[28]

Let us then denote by N the total number of the nucleotide pairs in the DNA complex of a given species, and by $f_1, f_2, f_1 + f_2 = 1$, the specific proportions of the pairs (A, T), (C, G). The total number of all possible genotypes viable or not of that species is

$$(1) \qquad \Gamma = \frac{N!}{N_1!\,N_2!}\,2^N,$$

where $N_i = f_i N$.

Here comes Boltzmann's H-formula, the pseudo entropy, again! For large values of N, (1) yields[29]

$$(2) \qquad \Gamma \approx 10^{N(\log 2 - H_{10})},$$

if we use the logarithm to the base 10, and

$$(3) \qquad \Gamma \approx 2^{N(1 - H_2)},$$

if we use the logarithm to the base 2.

For the human species, we may recall that N is estimated to be 5×10^9. And, according to Chargaff's results,[30] $f_1 = 0.605$. Hence, by (3), the genetic code of man has an information capacity of 10^{10} bits. This number represents also the amount of information (in Norbert Wiener's sense[31]) of the DNA complex of any human individual, yours or mine.

The dimension of the relation between human genotypes and phenotypes is more directly grasped through (2), which yields $\Gamma \approx 10^{3 \times 10^9}$, a

[26] Watson, p. 261.

[27] Erwin Chargaff, "Chemical Specificity of Nucleic Acids and Mechanism of their Enzymatic Degradation," *Experientia*, VI (1950), 201–209; Chargaff, "Isolation and Composition of the Deoxypentose Nucleic Acids and of the Corresponding Nucleoproteins," in *The Nucleic Acids*, eds. E. Chargaff and J. N. Davidson (3 vols., New York, 1955–1960), I, 350–360. Also Watson, pp. 265 f.

[28] Chargaff, "What Really Is DNA?" p. 319.

[29] One detail should be well marked here. In the logarithmic form of Stirling's formula, $\log(n!) \approx n \log(n/e) + (1/2)\log(2\pi n)$, we may neglect the last term for very large values of n. This we did in Chapter V, relation (4), and Appendix B, relation (38). Because we are interested only in the order of magnitude, we can continue to do so here, although we are primarily concerned with Γ, not with $\log \Gamma$.

[30] Chargaff, "Isolation and Composition," p. 353.

[31] See formula (18) in Appendix B, above.

number of the same order of magnitude as the magic number $10^{10^{10}}$ by which Boltzmann thought that practical infinity may be represented.[32] True, we do not know (and very likely we shall never know) how many DNA complexes counted in Γ are viable.[33] But given the fantastic size of Γ, we need not doubt that the viable complexes are more numerous than all the protons in the universe (whose number, according to Eddington's speculation, would be 10^{79}). Let us also note that, since the molecular weight of a nucleotide pair is 660 daltons, the molecular weight of a human DNA complex is 33×10^{11}, i.e., 165×10^{6} times greater than that of the average proteins placed in the supercosmic box mentioned in Section 3, above. This molecular weight ratio is so great that we can rest assured that if the viable complexes alone were placed in a cubic box, that box, too, would be of supercosmic dimensions.

On the other hand—and surprisingly enough—all the DNA complexes of the present world population could be easily stored in a small thimble! Should a biologist achieve the impossible feat of analyzing genetically and describing phenotypically every person alive now, his sample of the entire parent population would be proportionately much smaller than that of a drop of water from all the earth's oceans. Such a sample would be utterly insufficient, in spite of its large absolute size, for inferring anything substantial for a relation that involves a qualitative variable (the phenotype).

But the idea of analyzing genetically a large number of human beings runs against a more elementary obstacle which, inexplicably, is ignored by the eugenic schemes requiring clinics where everyone could be so analyzed. The point is that just to print the initials A, T, G, C of the sequence of nucleotides on only one strand of a DNA complex, we would need about 6,000 volumes of the same size as the one you are now reading. Hard to believe though it may seem, one's complete identification card is a small library, which, moreover, must not contain even one typographical error! Before airing the idea of genetic clinics, therefore, one should stop to consider how many printing establishments and perfect proofreaders would be needed to sustain the project and whether the world will still be able to carry on any other publishing activity. No doubt, as Bentley Glass judged,

[32] Appendix F, note 12, above.

[33] The paucity of our knowledge of the human DNA complex is another factor that sharply contrasts with the confidence some manifest in the imminent feasibility of eugenic or even euphenic engineering. Not until a few years ago was the correct number of human chromosomes shown to be 46, instead of 48 as had been believed for long. See J. H. Tjio and A. Levan, "The Chromosome Number of Man," *Hereditas*, XLII (1956), 1–6. As to the number of the human genes, there are only arbitrary speculations, according to which it may be as small as 50,000 or as great as 1,000,000. Of these only about one hundred have been identified and only a very few have been superficially located. See G. Pontecorvo, "Prospects for Genetic Analysis in Man," in *Control of Human Heredity*, ed. Sonneborn, p. 89.

"nightmare" is the proper word for describing any vision of genetic clinics.[34]

Nothing, I believe, could show more dramatically the predicament of biological engineering as a sound, analytical science. At one end, there is the astronomical number of DNA complexes with as many individually distinct phenotypic maps; at the other end, the inframicroscopic dimensions of the components of an immense complex that have to be removed, replaced, or shifted around.

6. To all that I have said so far, one may oppose the fact that in biology, more so than in the other natural sciences, most of the discoveries that have an operational value are the product of some inspired yet semiblind search. This position—to which I have already alluded—claims that the euphenic and eugenic millennium for mankind may be brought about only by a series of lucky strikes on the workbench: knowledge of complete relations of the kind considered in the preceding section is not a strict prerequisite for operational successes. The argument does not, however, always rely only on the facile point that no one can say that the right breakthrough will not come up, with time.[35] Ordinarily, it extrapolates from some empirically established facts and may also invoke some of the workable principles of molecular biology. But what should arrest our attention first of all is the fact that these extrapolations (at least, all that are highly significant) are not of the same nature as those encountered in the other natural sciences. The latter usually are quantitative extrapolations; the former sweep over a qualitative spectrum which may be as wide as that which includes everything from phages to mammals. For this reason alone, we should normally expect a biological extrapolation to collapse even under a very elementary kind of probing. And most of them do so.

We can easily understand why microorganisms, especially phages, have provided the preferred field of research for the molecular and chemical biologists. Bacteria and viruses are cheap and, above all, produce one additional generation in only a few minutes. This facility, however, has had its price: it has produced a professional deformation to which only a few molecular biologists have remained immune. It is the bacteria-complex, as we may call it. It is the dogma that "any successes in manipulating or

[34] Bentley Glass, "Summary and Concluding Remarks," *Cold Spring Harbor Symposia on Quantitative Biology*, XXIX (1964), 478. I need not insist on special difficulties —that at present we have not the slightest idea of how to isolate intact chromosomes, of how to solve the sequence of even a small DNA molecule, and so on. Cf. Chargaff, "What Really Is DNA?" pp. 327–329; H. J. Muller, "Means and Aims in Human Genetic Betterment," in *Control of Human Heredity*, p. 107.

[35] The point is used, for example, by Tatum in "Perspectives," p. 34, and Robert DeMars in "Investigations in Human Genetics with Cultivated Human Cells: A Summary of Present Knowledge," in *Control of Human Heredity*, p. 77.

controlling the genetical material of microorganisms should eventually be applicable to higher multicellular organisms, including man."[36] To justify this prediction one needs much more, extremely much more, than the mere observation that the biochemistry of the cell must be subject to the same laws in both cases. If the structures involved are as markedly different as a bacterium and a man, identity of principles does not necessarily ensure equality of success. The cells of higher organisms are more complicated than bacteria: they have a double, instead of a simple, set of chromosomes. Among many other things, we may cite the difference between the cellular and nuclear membranes of the two kinds of cells.

But the most important differences have their roots in the fact that a colony of some bacterium or of some phage, unlike all somatic cells of an organism, represents a homogeneous mass. Bacteria and phages only reproduce themselves endlessly and, most of the time, identically. By contrast, the fertilized egg of a higher organism gives rise to an immense number of new cells that display categorical qualitative differences. None of these cells is identical, not even almost identical, to the egg itself.

Somatic cells do divide and form colonies if detached from the organism and provided with adequate conditions for growth. But even in this case, the process is not the same as for bacteria. As many specialists emphasize, any cell culture ends by being a colony of *degenerated* cells, akin to cancerous rather than to normal cells.[37]

If a bacterium comes in contact with a bacteriophage, for example, we know what will happen: the phage will penetrate the cell of the bacterium and, according to circumstances, will either destroy it or be incorporated in it. But if viruses enter the human body, no one can predict precisely which cells will be affected or what will happen to the organism itself. The point is important for the idea aired by many biologists of curing diabetes or other similar inherited defects by the algenic replacement of the guilty gene. Presumably, the idea implies that the guilty gene will be replaced in every cell of the organism. Yet nobody apparently has even tried to suggest how such a fantastic operation could be achieved. A human organism has some 5×10^{12} somatic cells!

For these and other, more technical, reasons, many a biologist insists

[36] Tatum, "Perspectives," p. 22. But, like most of his equally distinguished colleagues, Tatum soon comes around to doubt that any of "the techniques of microbial genetic engineering has sufficient efficiency or specificity to warrant much hope of its eugenic applicability to more than an exceptional situation [in] higher organisms such as man." *Ibid.*, p. 28.

[37] Harris, *Cell Culture*, pp, 162–169, 176 f; Alex Comfort, "Longevity of Man and his Tissues," in *Man and His Future*, p. 225; G. Klein, "Discussion—Part II," in *Control of Human Heredity*, p. 93. This fact adds a great question mark to another extrapolation on which Joshua Lederberg builds part of his characteristic high hopes. See his "Biological Future of Man," in *Man and His Future*, ed. Wolstenholme, p. 265.

that a system of microorganisms or a cell culture should be regarded only as a useful model, "a tool for learning tricks and developing techniques" for ulterior purposes.[38] And the same authors are unanimous in cautioning us against the false hope created by extrapolating the algenic operations valid for unicellular structures to multicellular organisms. "We must not," warns Watson, "be mesmerized by our past successes into asserting uncritically that our achievements at the molecular level with bacteria can automatically be extended to the cells of [exceedingly complex objects such as] higher plants and animals."[39] But perhaps the simplest and most direct way of exposing the central difficulty is Klein's concluding protest: "when a human cell behaves...like a microbe, it is no longer a human cell, much less a man."[40]

7. As mentioned in Chapter XI, Section 5, above, the experiments initiated by R. Briggs and T. J. King with amphibians provided the basis for the claim that cloning people is an imminent biological tour de force. Even though in this case the extrapolation is much more modest than that from microbe to man, it rests on as great a disarray of principles and facts as is found behind every other extravagant euphenic or eugenic vision.

It is important that we should bear in mind from the outset the two principles that constitute the indispensable theoretical scaffold for the possibility of cloning not only people but any sexually reproduced species. They are: (1) *The Chromosomal Sufficiency*, which proclaims that all the necessary information for the development and functioning of an organism is contained in the chromosomal DNA of the fertilized egg; and (2) *The Chromosomal Identity*, which states that the chromosomal complex of every somatic cell is identical to that of the egg from which the organism developed.[41]

The facts hinting at the possibility of artificial cloning actually go back to the famous experiments of Hans Driesch (1891), which proved that an organism may develop not only from an egg but also from a somatic cell if this comes from a very early embryonic phase.[42] The findings of Briggs

[38] G. Pontecorvo, "Discussion—Part II," in *Control of Human Heredity*, p. 96.

[39] Watson, p. 414. Similar, even stronger, warnings come from many other authors. E.g., Luria, "Directed Genetic Change," pp. 14–16; R. D. Hotchkiss, G. Klein, "Discussion—Part I," in *Control of Human Heredity*, pp. 41–44; Pontecorvo, "Prospects," p. 89.

[40] Klein, "Discussion—Part II," in *Control of Human Heredity*, p. 92.

[41] For these principles, see Watson, pp. 10 f, 255, 418. (The Principle of Chromosomal Identity should not be confused with the DNA specificity established by Chargaff.)

[42] See Chapter V, Section 1, above, especially notes 16–18. Also Jacques Loeb, *The Organism as a Whole from a Physicochemical Viewpoint* (New York, 1916), chap. vi; Harris, pp. 3–5.

and King represent a new step in the same direction: they show that a somatic nucleus even from a later developmental phase is capable of inducing development when transplanted into an enucleated egg. But they also show something equally important, namely, that the more advanced the phase from which the somatic nucleus comes, the smaller is the probability that the engineered egg will develop beyond a given stage. In other words, with every new phase of development the somatic nuclei progressively lose their power of inducing development. If the phase is too advanced, the nucleus can no longer produce any development at all.[43]

Even if we beg the question of where the development of an engineered human egg may be safely completed (the issue of rejection should not be overlooked), the Briggs–King experiments may justify only the cloning of people from embryo cells. However, at the embryonic level there are no recognized Einsteins or Beethovens. In the ultimate analysis, the results obtained by Briggs and King (and, later, by others) point in exactly the opposite direction to that considered by the advocates of the feasibility of cloning. Far from supporting this vision, these results have revealed some substantial obstacles to it. In the first place, they call into question at least the validity of the Principle of Chromosomal Identity. What is more, they strengthen, not weaken, the position that the development of an egg into an organism is an irreversible (rather, irrevocable) phenomenon. According to this position, a completely differentiated somatic cell—a nerve, a liver, or a marrow cell of a fully grown animal, for example—cannot return either by itself or by man's intervention to its initial state of an egg capable of developing into a new organism.[44]

Curiously, the fact of this irreversibility is accepted even by those biologists who do not move an iota from the Principle of Chromosomal Identity, with which it is in clear-cut contradiction. So, we should not be surprised if some of the same biologists admit in the end that the process by which a whole organism develops from one cell constitutes a permanent source of bewilderment for biologists.[45] In this, they are one with the "traditional" biologists who have continuously insisted that development at any level of organization is still "a largely inaccessible and unintelligible process" from the molecular viewpoint.[46]

It is especially in connection with the Principles of Chromosomal Suffi-

[43] R. Briggs and T. J. King, "Changes in the Nuclei of Differentiating Gastrula Cells, as Demonstrated by Nuclear Transplantation," *Proceedings of the National Academy of Sciences*, XLI (1955), 322, and "Nuclear Transplantation Studies on the Early Gastrula, *Rana pipiens*," *Developmental Biology*, II (1960), 252, 266. See also note 116 of Chapter XI, above.

[44] For references, see note 121 in Chapter XI, above.

[45] Watson, p. 416.

[46] Darlington, p. 162.

ciency and Identity that most of the serious difficulties of development spring up. For a quick review, let C_i^k be one of the cells in existence after the first k divisions of a fertilized egg C_1^0. According to Driesch's findings, C_1^1 and C_2^1 can each develop into a whole organism if they are *separated* at that stage. Hence, they should be identical *in toto* to C_1^0. But, in this case, why should the same cells left *unseparated* develop into one single individual? Also, if $C_1^0 \equiv C_1^1 \equiv C_2^1$, by induction we should have $C_1^0 \equiv C_i^k$ for any i and k; i.e., there should be no development, but only growth—as in the case of bacteria. On the other hand, if we accept the idea that development begins only after a certain division, we introduce a qualitative jump hard to justify from the physicochemical viewpoint. Perhaps we should assume that inside any fertilized egg there is some sort of time-mechanism. But if we do so, how can we reconcile development with the Principle of Chromosomal Identity?

To save this last principle, it has been suggested that not all genes are alive at all times. The idea, in turn, led to a highly complicated system of "repressors" and "inducers." But, time and again, all evidence for this repressing–derepressing system comes from phages or bacteria.[47] Besides, no one seems disposed to argue that this system suffices even in the case of microorganisms to explain differences of behavior. Most important of all, no indication exists that a repressor–derepressor is responsible for the fact that a liver cell synthesizes proteins different from those synthesized by a nerve cell. Maybe, as some biologists contend, the repressed genes "are not there at all." And they insist, rightly, that we have no proof whatever for the Principle of Chromosomal Identity.[48]

The indirect defense of this principle (amended by some repressor–derepressor system) is that "*no one* will ever be able to work out *all* the chemical details" of somatic development and that we are still unable "*to study differentiation outside an intact organism*."[49] In this manner, we succeed only in veiling the truth, which is that the development of an organism cannot be reduced to a biology of the individual cell. Development is a process which involves all parts (not only the chromosomal DNA) of the egg and, later, all somatic cells. The point, in my opinion, is a synthetic

[47] On this system and the problem of somatic differentiation, see Watson, chaps. 14 and 15. In recent years, the evidence (for phages and bacteria) has been enriched by the successful isolation of some "repressors." See, for example, W. Gilbert and B. Müller-Hill, "Isolation of the *Lac* Repressor," *Proceedings of the National Academy of Sciences*, LVI (1966), 1891–1898, and by the same authors, "The *Lac* Operator Is DNA," *ibid.*, LVIII (1967), 2415–2421.

[48] Klein (cited in note 37), p. 94. In connection with this doubt we may note the recently established fact that the cells of a female organism are not genetically identical: there is a marked difference between the two X-chromosomes of some cells and others. Watson, p. 419.

[49] Watson, pp. 418, 438.

judgment and, as such, needs no proof. Only an ultramechanistic philosophy was able to make us reject this judgment. So, we are now considering every laboratory proof of its validity as even more valuable than its specific content would otherwise warrant.[50]

The Principle of Chromosomal Sufficiency also raises a troublesome question in relation to the Briggs–King results. If cytoplasm plays no distinct role in development, why should a somatic nucleus induce development *only if transplanted within the cytoplasm of an egg*? To my knowledge, no biologist has even entertained this question. The explanation, I think, is rather simple, yet quite telling.

Fascinated as modern biologists are by the combinatorial algebra of the simple Mendelian model and, more recently, by codes and codons, no one seems to realize that it is much more important to know what "causes" a pea plant *to have flowers* than to know what "causes" its flowers *to be pink*.[51] It is curious to see one biologist after another disclaiming any sympathy with idealism while talking about *form* without *substance*, i.e., only about the determinants of characters. Even those biologists who are not reducing biology to molecular phenomena instruct us that what we inherit is only the potentiality of developing this or that character.[52] In this way, one does not come to ask the very simple question: why does not a fertilized egg of a *white* mouse develop into a *white* rabbit or, even, into a *white* bear? But to speak about the material body, not only about characters, one must admit that the cytoplasm, too, plays a definite role both in heredity and somatic development. The point is that this role, although no longer susceptible to doubt, does not fit into the Mendelian model.[53] Darlington put his finger on the sore spot of the DNA-biology as he ob-

[50] This does not imply that some of the proofs of cell interaction are not highly interesting by themselves. For such proofs, see, for example, W. R. Loewenstein, "Communication through Cell Junctions: Implications in Growth Control and Differentiation," in *Developmental Biology*, 1968, Suppl. 2, pp. 151–183.

[51] The simple Mendelian model assumes that each character has only two forms—say, pink and white—which are controlled by one pair of alleles independently of other characters. Most of the time, however, one gene controls several characters (pleiotropism) and one character is controlled by several genes (polygeny). Moreover, how the polygenes act is still a great mystery, a fact which sets a drastic limitation to practical eugenics. Cf. P. B. Medawar, *The Future of Man* (New York, 1960), pp. 54 f; Theodosius Dobzhansky, "Human Genetics—An Outsider's View," *Cold Spring Harbor Symposia on Quantitative Biology*, XXIX (1964), 3. The point bears upon the euphenic vision, mentioned earlier, which is based on the idea that only one gene is responsible for the innate defect.

[52] E.g., C. H. Waddington, *Nature of Life*, p. 29.

[53] For the role of the plasmagenes—the active elements of the cytoplasm—in heredity, one must usually go to some specialized literature, e.g., Harris, pp. 2, 95–113. For a less technical presentation, see Darlington, pp. 146–149, 157–163. I take it that the role of cytoplasm in development is demonstrated by the Briggs–King results.

served that the plasmagenes (those little-known cytoplasmic determinants) elude us simply because they are "the very life of the cells."[54]

Everything tends to suggest that we must discard the Principle of Chromosomal Sufficiency, too. But, in this case, one may legitimately doubt whether the individuals developed by the Briggs–King transplanting are true clonants, i.e., identical twin brothers, of the donor of the nucleus. Some of their characters may come from the donor of the egg. This is an important issue which no biologist seems to have raised. But no one is to blame. Because of the infinitely many qualities and forms displayed by the living state, in biology there is always an unusually great number of questions to assail us. It is inevitable that at any one time we should overlook many of these questions, perhaps even many more than those with which we are actively struggling. The picture, as I attempted to draw it in this appendix, is one that reaches into supercosmic dimensions but is greatly deficient in definite contours. The two characteristics go together.

[54] Darlington, p. 153.

Index

(Note: In the text some words are used in two meanings distinguished by capitalization—for example, "Time" and "time." In the index, because every entry begins with a capital, it is necessary to preserve the distinction by using all capitals—for example, "Time" becomes "TIME.")

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Nicholas Georgescu-Roegen

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