A First Look at the Second Metamorphosis of Science

E. Atlee Jackson

SFI WORKING PAPER: 1995-01-001

A First Look at the Second Metamorphosis of Science

by

E. Atlee Jackson
Santa Fe Institute
Santa Fe, New Mexico
and
Department of Physics, and
Center for Complex Systems Research
Beckman Institute
University of Illinois at Urbana-Champaign

PREFACE

What follows is a preliminary collection of thoughts concerning the fundamental changes that are occurring in the foundations, methods and objectives of science. This organization was begun while I was on sabbatical leave from the University of Illinois, and had very enjoyable and stimulating interactions as a visiting faculty member of the Santa Fe Institute, from January until August, 1992. This organization was continued during 1992-1993, including another visit to the Santa Fe Institute in July-August 1993. Some additional relevant quotations from scientists have be appended during my 1994 visit to SFI.

Abstract: During the period of approximately 1570-1790 the first metamorphosis of science transformed the operational foundations of science, that were largely the heritage from the time of Aristotle, into its modern form. These new foundations consisted of the use of (1) Physical Experiments and the use of (2) Mathematical Models, involving differential equations. This metamorphosis was largely due to Brahe, Kepler, Galileo, Newton, Leibniz, Euler, and Lagrange. These operational methods were accompanied by the development of several philosophical attitudes and beliefs. One attitude (an implicit but operative frame of mind) arose from the loss of concern about the limitations of the encoding and decoding of information between experiments and mathematics; that is, an increased identification of the physical world with the mathematical models used to make very limited predictions of that world. The more overt philosophical beliefs related to the fundamental character of reductionistic methods in science. Among these beliefs was the idea that it is possible to synthesize this reductionistic knowledge, thereby obtaining a theory of the universe, capable of predicting all phenomena in nature. The idea that there is indeed any such thing as one set of laws which govern the behavior of the universe has its origins in antiquity, where the laws often referred to the desires of a god. This belief was immeasurably reinforced within Science by the success of Newton's 'universal law' of gravitation. The blending of these persuasions was perhaps captured best, intended or not, in Einstein's famous remark, "I shall never believe that God plays dice with the world."

Over the past century the character and structure of science has been going through a second process of fundamental change which has been brought about by two classes of discoveries. The first group of discoveries concern mathematical results which are directly related to the limitations in what we can learn about the dynamic behaviors in nature from these mathematical models. The discoveries of these limitations should have produced a 'loss-of-innocence' era, but they have largely gone unnoticed by scientists, even though they have profound implications concerning the future character of science. Among these discoveries, which apply to essentially all mathematical models of physical systems, are our inherent limitations to make: (1) analytic mathematical deductions, (2) deterministic physical predictions, and (3) structurally stable models of closed systems. In addition, other mathematical discoveries were made which struck at the basic idea that mathematical systems are consistent, and can establish any result which is true. Each of these discoveries have, or will have, a direct impact on the philosophical preconceptions of many scientists, since they shatter widely held beliefs about our ability to extract (deduce) information about nature only through the use of mathematical modeling and analysis, together with physical experiments. Physical experiments also stimulated other mathematical studies that further confirmed this lack of physical predictability. These results establish that our knowledge about the behavior of complex systems is limited to 'comprehensible' sets of observables (windows of comprehension), that involved 'bounded reducibility', and also establish that we are only capable of bounded predictability of physical events (proportional to our bounded information about the state of the system).

In the second half of this century a totally different class of opportunities and discoveries have been made possible by a new operational basis for scientific investigations, due to the digital computer. This expanded the operational bases of (1) Physical Experiments, and (2) Mathematical Models, established during the first metamorphosis, to include the third operational basis for obtaining knowledge about nature, that will be referred to as (3) Computer Experiments. Operations carried out in each of these three areas can yield independent knowledge relevant to our understanding

of nature, and can interface directly with either of the other two areas. In particular, the lost deductive dreams concerning mathematical models, the information encoding-decoding limitations that exist between physical experiments and mathematical solutions, and the bifurcation processes of dynamic models, have already been clarified and extended by Computer Experiments. Some of the numerous potentials for this interplay between the three operational methods is discussed below. For example, information about the dynamics of physical systems can be achieved using the interface between Computer Experiments and Physical Experiments, which does not rely on any mathematical modeling (or algorithms) related to the observables of the physical system. Moreover, using only this experimental information, limited predictions can be made without any dynamic models. Also certain types of local and global mathematical models can be sought, which is a simple form of computer/human induction. Computer Experiments also allow for the search of dynamics (algorithms) which scientists have not been capable of imagining in their inductive dreams; a more profound form of the computer inductive process is related to ongoing developments of computer 'genetic' dynamics. Computer Experiments can graphically represent incomprehensible data sets, and can search for 'emergent' properties of complex systems. Also there is a branch of research that is exploring fundamental issues related to the possible finite-informational character of all dynamic phenomena, and its natural association with both reconstruction methods and Computer Experiments.

With the recognition of deductive limitations and the expanded operational foundation, the general character of science is going through a process of metamorphosis which is both exciting in its richness, and impossible to presently define with any precision. Some aspects of this metamorphosis, and their bearing on widely held philosophical dreams of scientists, will be discussed.

1. INTRODUCTION

In this study, we will be concerned with the changes that have occurred in both the operational and philosophic foundations of Science, particularly over the past four hundred years. Until noted otherwise, 'Science' will refer here to that activity which attempts to understand that part of nature that can be based on quantifiable experiments (ideally, reproducible), and the use of logical formal descriptions. As will be discussed later, the second metamorphosis of Science will require generalizations of this definition.

Until the end of the nineteenth century the basic operational methods of Science, as established over the period 1570-1790, primarily by Brahe, Kepler, Galileo, Newton, Leibniz, Euler, and Lagrange, were generally considered to be fixed and well-established. These classic operational methods involved (1) Physical Experiments and (2) Mathematical Models, which were connected through the use of various inductive and deductive processes, associated with a "scientific method" originally outlined by Francis Bacon [51]. The stunning successes of these methods in reducing a wide variety of physical phenomena to a limited number of physical concepts and mathematical equations quite reasonably engendered confidence in scientists that they had found the appropriate 'scientific approach' which could unlock the secrets of all quantifiable observations. The discovery that it was necessary to introduce nonobservable concepts such as fields and wave functions in no way altered this confidence, even though many basic questions remain in the physical interpretation of quantum measurements. Thus there existed, and continues to exist, a confidence that this operational basis of physical experiments coupled with mathematical deductions should be capable of explaining (in a predictive sense) all of our quantitative observations of nature.

What should have caused some reflection on the generality of this scientific approach were several discoveries, beginning around 1890, that had a very modest and classic origin; namely the gravitational three-body problem, to be discussed in the next section. What these and other discoveries in the first half century signified was that an era involving a ‘loss of innocence’ had occurred in science (‘innocence’ being used in the sense of scientists lacking maturity, being naive, and credulous when it comes to their inductive/deductive powers). However these discoveries during the first half century, paled in comparison to the sensational discoveries related to relativistic and quantum effects, and accordingly went largely unnoticed. It wasn’t until the 1950’s, with the advent of the digital computer, that the opportunity arose to explore these earlier concepts, and uncover many other new phenomena. One of the earliest examples (1954) of a fundamental dynamic phenomenon to be uncovered by Computer Experiments was the surprising discovery by Fermi, Pasta, and Ulam, using MANIAC I at Los Alamos National Laboratories, which showed that some long-cherished assumptions about the irreversible behavior of coupled nonlinear oscillators in fact need not occur. Around 1950 there were also new theoretical understandings of both the analytic limitations and dynamic complexities uncovered around the turn of the century. Moreover, in 1954, Kolmogorov was establishing the analytic foundations for the famous KAM theorem(s) of the 1960’s, which were closely related to the Fermi-Pasta-Ulam phenomenon. However, during that period there was very little communication between those doing Computer Experiments, and the mathematicians concerned with existence and convergence questions.

Fortunately this isolation between methods began to change around 1960, when Physical Experiments, notably the Belousov-Zhabotinski chemical dynamics, also began to contribute to the general awareness of the variety of complex dynamic behaviors in nature (a brief history of these developments, and references, can be found in [1]). The decade of the 1960s began the active stage of the second metamorphosis of Science, with its wealth of new experimental, theoretical, and philosophical ideas, much of which has been stimulated by the new experimental approach made possible by digital computers. Computer Experiments need to be recognized to be a co-equal operational method, together with Physical Experiments and Mathematical Modeling, in the process of understanding some of nature’s infinite varieties of behavior. In what follows this point will be explored both with specific examples and, more importantly, exploring the practical philosophical impact which this operational triad may have on the future development of this metamorphosis of Science.

To understand the content and impact of the second metamorphosis of Science, it is necessary to review in some detail both the operational bases established by the first metamorphosis, and the philosophical heritage which it produced among many scientists.

2. THE FIRST METAMORPHOSIS OF SCIENCE

The western scientific heritage over a period of approximately 2000 years was due largely to the influence of Aristotle. Among the lasting contributions which he introduced was the use of logical rules of reasoning to arrive at conclusions, and the division of natural phenomena into categories (e.g., physics and biology) in which a degree of systematic organization could be attempted. This division of the study of natural phenomena into categories produced the divergence from the holistic perspective of eastern cultures, and afforded the opportunity for the very productive insights into nature. It also opened the door for the reductionistic concepts to arise, which have had an important philosophical impact on scientists.

Between approximately 1570 and 1790 a number of scientists transformed this heritage into what became the standard scientific method, until recent times. Since the concept of science before and after this period had a drastic change in its basic structure, it is appropriate to refer to this change as a metamorphosis, as will be done here. The scientists primarily responsible for this metamorphosis were Brahe, Kepler, Galileo, Newton, Leibniz, Euler, and Lagrange. Collectively they establish the two operational procedures used in the modern natural sciences for over one hundred years.

First, Brahe, Kepler, and Galileo showed the importance of using experimental observations to determine how nature actually behaves, rather than how we might think that it should behave. These observations involved no new technology, such as the telescope, and therefore could have been achieved much earlier, given the appropriate philosophical beliefs. Among the ancient misconceptions that were corrected by these observations was the idea that all planets move in circular orbits about the sun, and Aristotle's belief that it is necessary to act on a body with a force in order to sustain its motion. These, and other examples, firmly established the importance of experimental observations as one of the operational foundations of science.

The second operational basis of science was the introduction of mathematical equations to describe the dynamics of mechanical systems. Newton was the first to appreciate the possible generality of such descriptions when he formulated the concept of universal gravitational interactions, thus bringing both planetary and terrestrial projectile motion all within one framework. Newton's analyses were highly geometrical in character, but contained limiting concepts which latter became formalized into the differential equations that have become the standard mathematical equations of science. This clarification and formalization process took over sixty years following the publication of his famous Principia in 1687. This mathematical structure was due to Leibniz, and more specifically to Euler (the Mozart of mathematics) and Lagrange. An essential point to note is that Newton derived all of his dynamic results without the use of the calculus formulation used today - a point to which we will return.

The metamorphosis brought about by these discoveries was the establishment of a standard method of scientific investigations within the areas of physics and chemistry, at least prior to 1950. Beginning with experimental observations, generalizations are postulated and expressed in the form of differential equations (the process of induction). The great advantage of mathematical equations is that Aristotle's rules of logic can be applied, enabling scientists to make logical inferences (deductions) from assumed initial conditions. Thus, assuming that these mathematical deductions could be achieved, and that the proper 'encoding-decoding' is used between physical and mathematical information, many predictions of events not previously observed could be made from such mathematical 'models'. These could be checked against new experiments, and a refinement of the mathematical model (requiring a new inductive process) could be made if it was required. This led to a cyclic (iterative) process, the so-called Scientific Method, which is widely believed to actually be used in science [46]. It is illustrated in the following figure:

Physical Experiments	—(induction)→	Mathematical Models
		(differential equations)
(physical information)	—(encoding)→	(initial/boundary conditions)
↑		|
(comparisons)		↓
physical predictions	←(decoding)—	(deductions)

In the case of sufficiently simple mathematical models, this process has often been used with great success, however not at the 'fundamental' level often proclaimed to be Science's ultimate goal of understanding. Before considering this philosophical heritage of the above method, we will review more carefully the contents of these operational methods.

3. THE TWO CLASSIC OPERATIONAL METHODS

In this section we examine more carefully some of the essential aspects of the two classic operational methods.

3.1 PHYSICAL EXPERIMENTS

A physical experiment uses some set of instruments, subjectively selected, to measure 'observables' that have a relatively correlated relationship. These measurements yield a finite amount of data, taken over a finite time interval. This selection is not subject to any logical criteria, but the experiments must either be reproducible (within accepted accuracies) or comparable to similar situations (e.g., in astronomy, geology, biology, etc.) by any experimenter.

We emphasize that this finite data can be represented by a finite set of ordered finite bit strings. In dynamical situations the ordering is normally done using the concept of time. Time in this context involves the comparison of the physical system with another physical system called a 'clock', which is generally accepted by scientists. Clocks are physical systems which simultaneously (in the relativistic sense) produce the same increasing numbers ('time', a finite bit string), to some prescribed accuracy for a prescribed interval of time. While it is common to think that clocks are selected to yield "uniform time intervals", there is no well-defined meaning to this concept. There is no method for comparing one minute with another minute. Moreover there is no empirical basis for this feeling. Indeed, clocks, not being closed systems, and subject to instabilities, can only define scientific time to accuracies which are dependent on the intervals in question (as exemplified by the datings of geological strata). Nonetheless, there is a nearly visceral inclination to think of time as somehow 'flowing equably', to paraphrase Newton, and numerous discussions of different types of 'time' are common (e.g., [8], p. 147 ff.). However, scientific time is well-defined to be given by accepted 'clocks'. As such it has limited accuracy and duration. By way of contrast, the formal time that occurs in mathematical equations has neither of these limitations.

This focus on finite numbers is very basic and important in understanding the relationship between the three operational methods of science. Indeed, the concept of finite numbers does not do justice to our limited abilities to understand nature, for it is not the set of finite numbers that we can comprehend. Rather, we comprehend some set of 'small' finite numbers, which might be usefully referred to as 'comprehensible numbers'. Comprehensible numbers are those numbers that we can write down, look at, compare, peruse, contemplate, discuss, and make judgments about. This hardly gives 'comprehensible numbers' a mathematical precision - nor should it, since it is a psychological concept - but larger numbers do not enter into the human judgments within Science, since they are never a required aspect of our associations (encoding-decoding) between the three operational bases of Science. It might be noted that Kolmogorov [2] has suggested that numbers might be usefully divided into small, medium, large, and extra-large categories. The present perusable numbers would presumably fall within his small, medium, of "medium-large" categories. In any case, the essential point for what follows, is that we need to be sensitive to our limited abilities to observe, record, compare, and understand sets of numbers.

Physical measurements can easily produce finite but incomprehensible data sets. To be used as a physical experiment this data set must be reduced to some comprehensible set. This is typically done by some type of averaging over the incomprehensible set. The essential requirement is that the selected averaged quantities must yield a "deterministic" set of observables, to be discussed below.

3.2 MATHEMATICAL MODELING

At its more primitive historical level, mathematical modeling involved relating several spatiotemporal physical measurements with the help of simple mathematical equations or constructs. As emphasized by Galileo [3],

"Philosophy is written in that great book which ever lies before our yes, I mean the universe, but we cannot understand it if we do not first learn the language and grasp the symbols in which it is written. This book is written in the mathematical language, and the symbols are triangles, circles, and other geometrical figures, without whose help it is humanly impossible to comprehend a single word of it."

Clearly Galileo's mathematical language was geometrical and algebraic, as was the mathematics of those preceding him. Indeed it is rarely appreciated that the mathematical language of Newton was also geometrical and algebraic, and only involved calculus in a verbal context [4,5]. The long development of differential equations as we know them today, culminating in Euler's "Mechanica" sixty-three years after Newton's "Principia" and Lagrange's "Mecanique analytique" in 1788 which contained no geometrical references (or diagrams!), is nicely chronicled in Park's book [4]. This introduction of differential equations has had far reaching consequences on the beliefs of scientists, not the least of which are those associated with the 'inductive' process that leads from physical observations to these formal mathematical models. This issue will be discussed further on in this section.

The reason for the introduction of mathematics is simply that some formal system, subject to rules of logic, must be introduced for the purpose of deductions or predictions. Advancing from that objective, one looks for classes of physical experiments that can be described by the same equations, and thereby develops 'laws' involving a range of physical phenomena. Having made some progress along these lines, notably since Newton established the gravitational principle that join Galileo's terrestrial and the Brahe-Kepler celestial conclusions, attempts were made to collect other laws under 'more fundamental' laws which are again given some mathematical formulation. By this stage, a considerable body of philosophical considerations have come into play, particularly as it concerns the concepts of theories, the introduction of numerous nonempirical (metaphysical) concepts, and the increasing application of reductionism.

The issue of reductionism will be discussed in some detail in the following sections. Here we note briefly a rather rare discussion by a physicist on the introduction of nonobservable concepts into mathematical theories. Einstein wrote:

"We shall call "primary concepts" such concepts as are directly and intuitively connected with typical complexes of sense experiences."[7a]

"The aim of science is, on the one hand, a comprehension, as complete as possible, of the connection between the sense experiences in their totality, and, on the other hand, the accomplishment of this aim by the use of a minimum of primary concepts and relations. (Seeking, as far as possible, logical unity in the world picture, i.e., paucity in logical elements.)"[7b]

"Science uses the totality of the primary concepts, i.e., concepts directly connected with sense experiences, and propositions connecting them. In the first stage of development, science does not contain anything else. Our everyday thinking is satisfied on the whole with this level. Such a state of affairs cannot, however satisfy a spirit which is really scientifically minded; because the totality of concepts and relations obtained in this manner is utterly lacking in logical unity. In order to supplement this deficiency, one invents a system poorer in concepts and relations, a system retaining the primary concepts and relations of the "first layer" as logically derived concepts and relations. The new "secondary system" pays for its higher logical unity by having elementary concepts (concepts of the second layer), which are no longer directly connected with complexes of sense experiences. Further striving for logical unity brings us to a tertiary system .... Thus the story goes on until we have arrived at a system of the greatest conceivable unity, and of the greatest poverty of concepts of the logical foundations, which is still compatible with the observations made by our senses. We do not know whether or not this ambition will ever result in a definitive system. If one is asked for his opinion, he is inclined to answer no. While wrestling with the problems, however, one will never give up the hope that this greatest of all aims can be attained to a very high degree.... The layers are not clearly separated. It is not even absolutely clear which concepts belong to the primary layer."[7c]

It needs to be emphasized that it is not just the primary concepts, but also their interrelations which constitute the "first layer". Thus Newton's differential equations, which contain the secondary concept of instantaneous velocity, and the tertiary concept of instantaneous acceleration, can always be represent by the primary concept of positions, but taken at three different times [1a].

It is worth noting that H. Margenau, the professor of natural philosophy and physics at Yale University, argued that dx/dt is not a secondary concept because it can be determined with the use of speedometer, and similarly one could use an accelerometer to measure the acceleration [29]. This is a remarkably limited vision of the use of instantaneous velocity and acceleration as it occurs in science; it has no bearing on its fundamental use, concerning the motion of particles, planets, or the like. On the other hand Margenau clearly distinguished that use of time in mathematical equations ('constitutive definition') and the operational definition associated with clocks ('epistemic definition'). Such refinements of definitions, as they apply to different operational methods used in Science, are of great importance, and will be returned to below.

Mathematical Modeling has given rise to a number of metaphysical concepts, some based on the presumed power that it yields in deducing future events, and others of a more subtle character, arising from its continuous nature. Before discussing the philosophical concepts that arose from these models, we first focus on the fact that mathematics is the science of the infinite, as once described by Leibniz. Since the time of Cantor, around 1870, mathematics has become a good deal more infinite. For while Leibniz, Galileo, Gauss, and Aristotle all recognized the possibility of extending some sets indefinitely (so they are potentially infinite), Cantor introduced the concept of a set which is itself infinite (an idea supported by Russell and Hilbert, and rejected by Poincaré). Whichever infinite set one considers in mathematics (potential or actual), such sets are quite distinct from the perusable finite sets involved in Physical Experiments, as already discussed above.

The means by which the finite experimental data is abstracted into the formal world of continuous mathematical equations is a truly mysterious process called 'scientific induction'. Logical 'induction' means that one draws conclusions about all members of a class from the examination of only a few members of that class. Since there is no assurance that physical observations constitute 'a few members' of the class of solutions of any differential equations, the process of scientific induction, which leads from the former to the latter, involves a significant extrapolation . A philosophical attitude must first be acquired ('education'), which allows one to accept the premise that physical observations are indeed special examples of solutions of differential equations. Once that attitude is acquired, the inductive process proceeds by various mysterious routes, a number of which have been recently explored [6]. It is clear, however, that to every physical observation, being defined only within a range of real numbers, there corresponds a continuum of mathematical states, and hence a continuum of solutions of any system of differential equations. This is hardly the 'inductive' process we presumably started with! The physical state is not a special example of a mathematical solution but rather a representation of an infinite number of mathematical solutions.

We are now so accustomed to the use of the differential equations in modeling the behavior of nature, that we take such concepts as continuity, limits (infinite sets), irrational numbers, etc. as necessary elements in the description of nature. But occasionally, somebody (like Richard Feynman) notices that there must be some over-prescription in this equating of the solutions of differential equations and physical observations, as illustrated by the following quotation [8]:

"It always bothered me that, according to the laws as we understand them today, it takes a computing machine an infinite number of logical operations to figure out what goes on in no matter how tiny a region of space, and no matter how tiny a region of time. How can all that be going on in that tiny space? Why should it take an infinite amount of logic to figure out what one tiny piece of space/time is going to do? So I have often made the hypothesis that ultimately physics will not require a mathematical statement, that in the end the machinery will be revealed, and the laws will turn out to be simple, like the chequer board with all its apparent complexities."

This quotation is a testimonial to the shallowness of our scientific educational methods. When a genius such as Feynman, with his unique imaginative abilities and deep insights into the limitations of science, was nonetheless "always bothered" by such an issue, it is clear that our educational methods must be very successful at indoctrinating students into an uncritical acceptance of the prevailing theoretical 'wisdom'. A serious discussion of the relationship between experimental observations and the formal mathematical structure which is used to describe and 'predict' observations, is essentially never addressed in scientific courses. Even mathematical descriptions which do not involve calculus, such as difference equations (maps), and cellular automata all have variables which are defined on the field of real (or complex) numbers. These fields are always infinite sets. Thus such infinite sets are an inherent aspect of mathematical descriptions. An interesting exception to this statement is a recent doctoral thesis by van Bendegem [49].

Another important way that infinity has crept into our thinking about nature is in the infinite extent of the mathematical time variable. As noted under Physical Experiments, there is no physical (scientific) reason to introduce concepts which are based on this range of time. It may be mathematically expedient to consider such limits, but they are physically irrelevant. More importantly, such concepts induce illusions about the infinite physically-predictable character of our models. These issues will be taken up below.

4. SOME PHYSICISTS' PHILOSOPHICAL HERITAGE FOLLOWING THE FIRST METAMORPHOSIS

With rare exceptions (e.g., Popper, Kuhn) philosophers of science are rarely read by scientists. The most significant philosophy of science for the future development of science is the philosophy adapted by scientists themselves. The traditional philosophers of science do not generally view science through the same lens that are used by scientists. As a result one gets reactions such as those of Freeman Dyson, "There's a whole culture of philosophy out there somewhere with which we have no contacts at all... there's really little contact between what we call science and what these philosophers of science are doing - whatever that is." Therefore, in this section we will only consider those philosophical opinions of some of the influential scientists of this century.

The many successes of the above scientific method, when applied to various areas within the field of physics, have been widely proclaimed and do not need any elaboration. These successes, limited though they are, have generated a number of beliefs about how the universe (nonetheless!) must behave (e.g., descriptions of the universe as 'not arbitrary', and 'governed by laws', are considered both meaningful and correct). Moreover, our ability to understand everything is taken as axiomatic by some physicists. To highlight some of these ideas, which will be illustrated in the following quotations, we list some of them very briefly:

- The universe is govern by some 'laws' (an axiomatic set, subject to logical rules).
- We can ultimately determine these "fundamental" laws by a process of reduction, which involve determining only pair-wise interactions.
- The process of synthesis, by which these laws can be used to predict macroscopic process, is known.
- Once these laws are obtained, it is possible ('in principle') to deduce all macroscopic phenomena; i.e., the synthesis can be performed.
- The physical states of Nature correspond one-to-one with the mathematical states of the differential equations of the theories.
- We can obtain information about the physical state of a system (that is, ALL of the needed variables in the theory) to any desired degree of accuracy; hence no modification in the theory is involved with increased precision.
- (hence) Mathematical determinism is the same as physical determinism.

These beliefs will be evident in the quotations that follow, but one should note that they are sustained with great difficulty even by those that advocate these viewpoints. Qualifications abound in their reasoning.

A closer examination of the 'many successes' noted above shows that they only occurred because the physical phenomena that were considered could be modeled by sufficiently simple equations (to be discussed below). When issues arose which addressed the fundamental relationship between physical experiments and mathematical models, namely the meaning of encoding and decoding information between these two operations, there was typically confusion and disagreement. For example, the long history of controversy in the area of irreversibility, and the 'arrow of time', is due in large part to a lack of distinguishing physical and mathematical information - but this is another long story of its own. Despite these facts, and the fact that successes were limited to a very restricted range of physical phenomena, many scientists developed an enormous confidence in their ability to uncover 'fundamental' aspects of nature, suggesting to them that they are indeed on the road to obtaining 'theories of everything'. This is not an isolated dream of some fringe element of the scientific community, but rather the claim of some very influential and vocal scientists.

4.1 "THEORY OF THE UNIVERSE (OF EVERYTHING!)"

An early quotation of Einstein (1918) illustrates the mixture of awareness and faith that lies behind this philosophical belief [7]:

"In regard to his subject matter...the physicist has to limit himself very severely: he must content himself with describing the most simple events which can be brought within the domain of our experience; all events of a more complex order are beyond the power of the human intellect to reconstruct with the subtle accuracy and logical perfection which the theoretical physicist demands. Supreme purity, clarity, and certainty at the cost of completeness. But what can be the attraction of getting to know such a tiny section of nature thoroughly, while one leaves everything subtler and more complex shyly and timidly alone? Does the product of such a modest effort deserve to be called by the proud name of a theory of the universe?

In my belief the name is justified; for the general laws on which the structure of theoretical physics is based claim to be valid for any natural phenomenon whatsoever. With them, it ought to be possible to arrive at the description, that is to say, the theory, of every natural process, including life, by means of pure deduction, if that process of deduction were not far beyond the capacity of the human intellect."

Alas, the last phrase contains two crucial requisites with very different characters. The first is the equating of 'the description' of a natural process with 'the theory' of that process. This illustrates the lack of distinction between the informational content of physical experiments and mathematical models, which became a common attitude among many scientists. It is an example of the treachery of formalisms. The second requisite, which is more technical in character, namely 'if that process of deduction were not far beyond the capacity of the human intellect'. Without even addressing the very fundamental issues raised by the first requisite, the second requisite certainly raises questions as to why these efforts warrant to be called by the proud name of the 'theory of the universe'. Fifteen years later, in his preface to Max Planck's book "Where is Science Going?" [30], Einstein expressed very similar ideas, although somewhat less sweeping. He reduced the scope from 'a theory of the universe' to a 'World-Picture', and elaborated upon the limitation expressed in the last phrase above. Nonetheless, it is quite clear that this 'World-Picture' continued to hold the philosophical high-ground in his thoughts, despite the fact that it was logically unfounded and scientifically lacking in content. This can best be appreciated by reading the following excerpt from Einstein's preface:

"Among the various pictures of the world which are formed by the artist and the philosopher and the poet, what place does the world-picture of the theoretical physicist occupy? Its chief quality must be a scrupulous correctness and internal logical coherence, which only the language of mathematics can express. On the other hand, the physicist has to be severe and self-denying in regard to the material he uses. He has to be content with reproducing the most simple processes that are open to our sensory experience, because the more complex processes cannot be represented by the human mind with the subtle exactness and logical sequence which are indispensable for the theoretical physicist.

Even at the expense of completeness, we have to secure purity, clarity and accurate correspondence between the representation and the thing represented. When one realizes how small a part of nature can thus be comprehended and expressed in an exact formulation, while all that is subtle and complex has to be excluded, it is only natural to ask what sort of attraction this work can have? Does the result of such self-denying selection deserve the high-sounding name of World-Picture?

I think it does; because the most general laws on which the thought-structure of theoretical physics is built have to be taken into consideration in studying even the simplest events in nature. If they were fully known one ought to be able to deduce from them by means of purely abstract reasoning the theory of every natural process of nature, including that of life itself. I mean theoretically, because in practice such a process of deduction is entirely beyond the capacity of human reasoning. Therefore the fact that in science we have to be content with an incomplete picture of the physical universe is not due to the nature of the universe itself but rather to us.

Thus the supreme task of the physicist is to the discovery of the most general elementary laws from which the world-picture can be deduced logically."

Presumably, in light of his previous remarks, the phrase ‘can be deduced logically’ in the last sentence means in a ‘theoretical’ sense; but then, of course, it has no scientific operational meaning, and therefore sheds no light on the vast majority of natural phenomena. Einstein appeared to take comfort in the idea (not fact) that this presumed limitation “is not due to the nature of the universe itself but rather to us”. Obviously that is an element of faith.

More recently, the influential physicist Stephen Hawking made the following remarks [9]:

“Now, if you believe that the universe is not arbitrary , but is governed by definite laws, you ultimately have to combine the partial theories into a complete unified theory that will describe everything in the universe. But there is a fundamental paradox in the search for such a complete unified theory. The ideas about scientific theories outlined above assume we are rational beings who are free to observe the universe as we want and to draw logical deductions from what we see. In such a scheme it is reasonable to suppose that we might progress ever closer toward the laws that govern our universe. Yet if there really is a complete unified theory, it would also presumably determine our actions. And so the theory itself would determine the outcome of our search for it! And why should it determine that we come to the right conclusions from the evidence? Might it not equally well determine that we draw the wrong conclusions? Or no conclusion at all?

The only answer that I can give to this problem is based on Darwin’s principle of natural selection. The idea is that in any population of self-reproducing organisms, there will be variations in the genetic material and upbringing that different individuals have. These differences will mean that some individuals are better able than others to draw the right conclusions about the world around them and to act accordingly....your scientific discoveries may well destroy us all, and even if they don’t, a complete unified theory many not make much difference to our chances of survival. However, provided the universe has evolved in a regular way, we might expect that the reasoning abilities that natural selection has given us would be valid also in our search for a complete unified theory, and so would not lead us to the wrong conclusions.

Because the partial theories that we already have are sufficient to make predictions in all but the most extreme situations, the search for the ultimate theory of the universe seems difficult to justify on practical grounds....The discovery of a complete unified theory, therefore, may not aid the survival of our species. It may not even affect our life-style. But ever since the dawn of civilization, people have not been content to see events as unconnected and inexplicable. They have craved an understanding of the underlying order in the world. Today we still yearn to know why we are here and where we came from. Humanities deepest desire for knowledge is justification enough for our continuing quest. And our goal is nothing less than a complete description of the universe we live in.”

This was followed by the observations [9a]:

“What would it mean if we actually did discover the ultimate theory of the universe? ...we could never be quite sure that we had indeed found the correct theory, since theories can’t be proved. But if the theory was mathematically consistent and always gave predictions that agreed with observations, we could be reasonably confident that it was the right one.... Even if we do discover a complete unified theory, it would not mean that we would be able to predict events in general, for two reasons. The first is the limitation that the uncertainty principle of quantum mechanics sets on our powers of prediction ....In practice, however, this first limitation is less restrictive that the second one. It arises from the fact that we could not solve the equations of the theory exactly, except in very simple situations. (We cannot even solve exactly for the motion of three bodies in Newton’s theory of gravity, and the difficulty increases with the number of bodies and the complexity of the theory.) We already know the laws that govern the behavior of matter under all but the most extreme conditions. In particular, we know the basic laws that underlie all of chemistry and biology. Yet we have certainly not reduced these subjects to the status of solved problems .... So even if we do find a complete set of basic laws, there will still be in the years ahead the intellectually challenging task of developing better approximation methods, so that we can make useful predictions of the probable outcomes in complicated and realistic situations. A complete, consistent, unified theory is only the first step: our goal is a complete understanding of the events around us, and of our own existence.”

The reference here to both the problem of three gravitating bodies, and to ‘intellectually challenging task of developing better approximation methods’ are both fundamental issues that will be discussed in the next section. However, it is important to emphasize that any ‘approximation’ is not a deductive process, but constitutes a new mathematical theory that is logically quite separate from whatever theory preceded it within any theoretician’s mind.

The Nobel Laurete Steven Weinberg has also written widely in defense of obtaining theories ‘capable of accounting for everything’. It is not altogether accurate to take extracts from these pieces, but nonetheless that will be done in an attempt to give some flavor of his philosophy. He wrote in 1988 [12]:

“Newton’s dream, as I see it, is to understand all of nature, in the way that he was able to understand the solar system, through principles of physics that could be expressed mathematically. That would lead through the operation of mathematical reasoning to predictions which should in principle be capable of accounting for everything.” “The goal is the formulation of a few simple principles that explain why everything is the way it is. This is Newton’s dream and it is our dream.”

The undefined phrase ‘in principle’ appears widely in such discussions, as will be seen below. Continuing, one encounters what he views as ‘final answers’, ‘final principles’, and ‘what is important’:

“... the importance of phenomena in everyday life is, for us, a very bad guide to their importance in the final answer.” “We are interested in the final principles that we hope we will learn about by studying these (elementary) particles. So the first lesson is that the ordinary world is not a very good guide to what is important.”

4.2 REDUCTIONISM:

In another extensive article [13] Weinberg wrote, in defense of attacks on reductionism:

“... in their attacks on reductionism ...(they) miss the point. In fact, we all do have a sense that there are different levels of fundamentalness....We do have the feeling that DNA is fundamental to biology. It’s not that it’s needed to explain transmission genetics, and it’s certainly not needed to explain human behavior, but DNA is fundamental nonetheless. What is it then about the discovery of DNA that was fundamental to biology? And what is it about particle physics that is fundamental to everything?”

Once again, there is that assured assessment that particle physics is ‘fundamental to everything’! As to his question concerning DNA, he might begin by referring to Mayr [45]: “The discovery of the double helix of DNA and its code was a breakthrough of the first order. It clarified once and for all some of the most confused areas of biology and led to the posing of clear-cut new questions, some of which are now among the current frontiers of biology. It showed why organisms are fundamentally different from any kind of nonliving material.” What from our knowledge of particle physics is as fundamental to “everything” as this knowledge obtained from the discovery of DNA? Moving on with assurance, Weinberg continues:

“In all branches of science we try to discover generalizations about nature, and having discovered them we always ask why they are true. I don’t mean why we believe they are true, but why they are true. Why is nature that way? When we answer this question the answer is always found partly in contingencies, that is, partly in just the nature of the problem we pose, but partly in other generalizations. And so there is a sense of direction in science, that some generalizations are 'explained' by others."

"Another complication in trying to pin down the elusive concept of 'explanation' is that very often the 'explanations' are only in principle. ... we also would say that a chemical behavior, the way molecules behave chemically, is explained by quantum mechanics and Coulomb's law, but we don't really deduce chemical behavior for very complex molecules that way.... In this case we can at least fall back on the remark that ...we could if we wanted to. We have an algorithm, the variational principle, which is capable of allowing us to calculate anything in chemistry as long as we had a big enough computer and were willing to wait long enough."

"The meaning of 'explanation' is even less clear in the case of nuclear behavior. No one knows how to calculate the spectrum of the iron nucleus, or the way the uranium nucleus behaves when fissioning, from quantum chromodynamics. We don't even have an algorithm.... Nevertheless, most of us are convinced that quantum chromodynamics does explain the way nuclei behave. We say it explains 'in principle', but I am not really sure of what we mean by that."

"Still, relying on this intuitive idea that different scientific generalizations explain others, we have a sense of direction in science. There are arrows of scientific explanation, that thread through the space of all scientific generalizations.... These arrows seem to converge to a common source! Start anywhere in science and, like an unpleasant child, keep asking "Why?". You will eventually get down to the level of the very small." "...sometimes it isn't so clear which way the arrows of explanation point."

Again note the dominate role of 'in principle' statements. Moreover the above question 'why?' is always a reductionistic question. As Weinberg noted, it does not involve such questions as "why does a fluid form vortices behind an obstacle?" or "why do evolutionary processes develop along certain lines?", or "why do we see images, or hear sounds?" These and endless other questions do not yield an arrow to the level of the very small. They do, sometimes, lead to a smaller level than that used for the gross description of the phenomena (to "see" or "hear sound" requires some reduction, but the fluid equations are the most reduced theory that gives any hope of 'explaining' vortex formation). There is a level of reductionism beyond which a theory fails to predict or reproduce the original phenomenon, and hence to 'explain' it in any acceptable sense. Weinberg, for all of his apparently strongly reductionistic beliefs, describes himself as 'a compromising reductionist'; to be contrasted with the characterization of him by Ernst Mayr [15] as 'a horrible example of the way physicists think' and 'an uncompromising reductionist'. This perhaps raises as many questions about his final stance as it may answer. To illustrate his meaning of this position, Weinberg wrote:

"...the notion that the other sciences will eventually lose their identity and all be absorbed into elementary particle physics; they will all be seen as just branches of elementary particle physics ... I certainly don't believe that. Even within physics itself, leaving aside biology, we certainly don't look forward to the extinction of thermodynamics and hydrodynamics as separate sciences; we don't even imagine that they are going to be reduced to molecular physics, much less to elementary particle physics."

Nonetheless, he went on to remark:

"...we understand perfectly well that hydrodynamics and thermodynamics are what they are because of the principles of microscopic physics. No one thinks that the phenomena of phase transitions and chaos...could have been understood on the basis of atomic physics without creative new scientific ideas, but does anyone doubt that real materials exhibit these phenomena because of the properties of the particles of which the materials are composed?"

One may marvel at why ‘we understand perfectly well’ based on some unsubstantiated ‘principles’ and ‘properties’, and ‘without’ should be replaced with, ‘for they required’; the rest is, of course, an act of blind faith.

He also makes the following passing reference:

“There is in the philosophical literature a term, emergence, that is used to describe how, as one goes to higher and higher levels of organization, new concepts emerge that are needed to understand the behavior at that l level.”

“Now reductionism... is not a fact about scientific programs, but is a fact about nature....I would call it objective reductionism ... I wish to emphasize that what I am talking about here is not the future organization of the human scientific enterprise, but an order inherent in nature itself.”

Despite this, Weinberg felt that it was possible to argue for the construction of the 4.4 billion dollar superconducting supercollider on the grounds that particle physics “... is in some sense more fundamental than other areas of physics”, even though “the future organization of the human scientific enterprise” is not related to this ‘fundamentalism’. I personally find this reasoning obscure, at best.

4.3 AESTHETICS:

The importance of the aesthetical aspects of theories is illustrated by Weinberg’s remarks:

“This brings me to the second lesson. It is that if we are talking about very fundamental phenomena, then ideas of beauty are important in a way that they wouldn’t be if we were talking about mere accidents. ... when we formulate the equations of quantum field theories or string theories we demand a great deal of mathematical elegance because we believe that the mathematical elegance that must exist at the root of things in nature has to be mirrored at the level we are working.”

“The kind of beauty we are looking for is more like the beauty of a piano sonata than that of a grand opera, in the specific sense that the theories we find beautiful are theories which give us a sense that nothing could be changed.... we are looking for a sense of uniqueness, for a sense that when we understand the final answer, we will see that it could not have been any other way. My colleague, John Wheeler, has formulated this as the prediction that when we finally learn the ultimate laws of nature we will wonder why they were not obvious from the beginning.”

John Wheeler, apparently at a later date (1983), also remarked that “the only law is that there is no law”. He has also proposed a very different and radical suggestion, falling in an area of “recognition physics” to which we will return in a later section, in an article “On Recognizing ‘Law Without Law’ ” [14]. He subsequently revised this view, to allow for a “guiding principle”[14a]; also see [14b].

Another recent discussion about the importance of aesthetic criteria has been given by Penrose [40], (pg. 421), referring also to the importance of aesthetics in the considerations of Dirac, Chandresekhar, Hadamard, and Poincaré . While mathematical formalations can undoubtedly have aesthetic appeal, the deification of aesthetic qualities of mathematical equations is but another example of the treachery of formalisms. There is not necessarily any connection between the aesthetics of equations and the aesthetics of the predictions of those equations.

This has been made particularly clear during the past century. We have come to realize that there are new, more physically significant forms of aesthetics contained in the dynamics generated by some systems of equations. It is this dynamic aesthetics that we see about us every day, both temporally and spatiotemporally. Moreover, the existence of this dynamic aesthetics is not associated with any obvious aesthetic property in the structure of the equations. On the contrary, some very ordinary looking systems of equations can be responsible for dramatically beautiful dynamic effects. Thus the criteria for physically significant aesthetics within mathematical or algorithmic formalisms, has been shifted from the equations and focused on the spatiotemporal structures generated by those formalisms. This is an important new perception of these formalisms.

4.4 TWO-FRONTIER VIEW

In 1977, Victor Weisskopf wrote an extensive article [44], giving his views on the frontiers and limits of science. At this time he distinguished between two kinds of frontiers: the external and the internal.

"The external frontier delimits the exploration of those realms of nature that lie beyond currently understood principles. Typical external frontiers are represented by the fields of subnuclear research and astronomy. The subnuclear studies penetrate one step beyond nuclear physics.... The external frontier therefore is the pace where we find new ways of natural behavior beyond the terrestrial limits of space and energy. Probably nature is subject to new laws or to still unknown extensions of our present laws when those limits are trespassed. In these areas the romance of discovery is especially manifest: we find unexpected and mysterious objects and processes that appear to be unexplainable and beyond any known laws of nature as we penetrate into the deeper and darker realms of the universe."

There is no doubt, from this recounting of the "romance of discovery ... into the deeper and darker realms of the universe", where Weisskopf spent much of his research life.

"The internal frontier is a much broader area where the basic principles are believed to be known but where the apparent complexity of the phenomena prevents us from understanding and explaining them. The internal frontier mostly concerns the first rung on the quantum ladder- the world of atoms and molecules ... However, while the principles on which the atom operates are understood, the complexities of that operation remain very great.

Let me illustrate this in the following way. Assume that a group of intelligent theoretical physicists have lived in a closed building from birth and have never had any occasion to see structures in nature. All that these physicists are supposed to know are the fundamental principles upon which atomic structures is based - i.e., the existence of atomic nuclei and electrons, quantum mechanics, and the nature of electric forces. What would be the result if these physicists were asked to predict how the atom manifests itself in nature and to what structures it gives rise?

They would most likely be able to predict that atoms exist; they could probably predict that atoms join to form molecules; and they might be able to describe what kinds of simple molecules actually exist in nature. They might even be able to forecast the formation of macromolecules or chains of molecules, the fact that molecules can join to produce solids, and the existence of many different solids such as metals, crystals, and salts. But I am most certain that these theorists would never predict the existence of liquids. A liquid is a highly complex phenomenon in which the molecules stay together yet move along each other; it is by no means obvious why such a strange substance should exist. In the same manner, a great deal of chemistry, and certainly the existence of life, would be impossible to predict. This is to illustrate that an understanding of the principles by no means implies and understanding of the world of phenomena."

He proceeded to make the following important observation:

"When we face the realities of our environment, we deal with these structures and superstructures rather than with the atoms that make them up. This is why objects, concepts and ideas that the scientists uses when he tries to understand what goes on do not deal directly with atoms but rather with the structures that are immediately involved in the phenomena under study. This is the characteristic situation along the internal frontier of science."

He then gave a few examples of these "structures and superstructures", such as temperature, pressure, entropy, and phases, from thermodynamics, and chemical bonds and kinetics of reactions from chemistry.

"Each of these concepts and principles are either known to be consequences of fundamental laws of atomic structure, or it is made plausible that they do not contradict them or require a change of, or addition to, the fundamental laws."

"This raises and interesting question: Since we do not understand many, perhaps most, of the phenomena of complex and organized matter, how can we be sure that all the basic principles of the atomic world are known? How can we be so bold as to assume that we fully understand the fundamental laws that govern the behavior of atoms if so many structures, processes, and phenomena in our environment are not completely understood ...? In spite of this, very few scientists today would maintain that there are new fundamental principles to be discovered with regard to life or the other phenomena I have mentioned."

Here, again, we find the attribution of "fundamental" to "laws" which have no relevance to our understanding of "perhaps most of the phenomena of complex and organized matter", and that there are no "new fundamental principles to be discovered with regard to life or the other phenomena" he mentioned. This is a totally unjustified use of the word "fundamental", and it commonly haunts all of the traditional science discussions, particularly involving nuclear or high-energy physicists.

It is from this distortion of the fundamental character of efforts to understand complex phenomena, and the corresponding irrelevance of the "laws" at the atomic level to our understanding of most of these macroscopic phenomena, that produces the following misfocused observation:

"...there is today a general belief that the basic principles of the atomic world are known and no additional law or principle is necessary in order to explain the phenomena of the atomic realm, including the existence and development of life. This assurance stems in large part from the fact that, while we cannot explain many complex phenomena, the complexity in itself is not surprising but plausible and expected."

Whatever ones belief is concerning the completeness of "the basic principles of the atomic world", these "basic principles" play no role in our understanding of macroscopic, complex phenomena. That is the reason we need to search for such concepts as Weisskopf's "structures and superstructures". And to say that "the complexity in itself is not surprising but plausible and expected" hardly fits his own parable concerning the isolated physicists who would never have predicted the existence of a liquid, nor "a great deal of chemistry, and certainly the existence of life". It is also clear that he was unaware of the startling dynamic discoveries that had occurred over the three decades previous to his article, in such fields as electrical engineering, meteorology, biology, and astronomy.

Weisskopf also focused on the hierarchies in nature, starting with the nucleons, and electrons then forming nuclei and atoms, then atoms forming molecules. But at this point he suggested that one consider two paths: one involving only inanimate objects while the other leads to living systems. Hence the first proceeds up the line of molecules to liquids and crystals, which form minerals and rocks, forming planets and stars, forming galaxies, forming the universe. The second path leads from molecules to macromolecules, forming cells, forming multicellular species, some with brains. Individuals of a species may form groups, tribes, and societies.

"There is an obvious tendency of nature [to go] from disorder to order and organization.... Matter is never quite isolated from its surroundings and always loses its heat, which escapes in some highly unordered form of relatively large entropy. Hence the second law [of thermodynamics] requires an increase of order in a warm material when it is in contact with its surroundings. ... I have called this conclusion the fourth law of thermodynamics."

"There is a distinction between order in living and dead nature. At the very end of everything, when the sun is extinguished, matter will be even more ordered than it is now, because all random heat motion will be frozen. But everything will be cold, dead, and unchanging. It is the temperature gradient between the hot sun and the colder Earth that produces the living order, ever changing and developing, through reproduction and evolution."

While one can certainly pick at the use of such descriptions as "entropy", and particularly "even more order" (what is the "order" of a living system relative to a crystal?), nonetheless, the emphasis of the role of energy fluxes in maintaining life is an important one.

The important issue of "understanding" is touched upon only briefly:

"In this sequence of hierarchies the line between the external and internal frontiers must be drawn roughly at the atomic nucleus. " All that is more macroscopic can "be thought, in principle, to be based upon atomic and molecular structure.... However, a knowledge of the basic laws is insufficient for a real understanding how the "parts" are related to the "whole" at each step of the hierarchies. ... Real understanding implies the distinction between the essential and the peripheral."

Thus the atoms are generally peripheral, whereas identifying the "structures and superstructures" is the essential task in finding the "parts" needed to "understand" the "whole". His defining remark on this issue was:

"The term understand should mean a general recognition that the phenomenon fits into the framework of science, that it is "demystified"."

The issue of "understanding", which historically was often connected with our ability to predict a systems behavior, has become a more subtle issue, with the advent of computer "reconstruction" methods, artificial neural networks, and the like. This area needs a lot of reflection.

4.5 LIMITATIONS AND NEW HORIZONS

One of the early spokesmen for a realistic assessment of the scientific knowledge contained in our modern theories of science was Anderson [16]. Specifically, he wrote to oppose a point of view that had been expressed by Weisskopf [46] a decade before the above writing [44], and illustrated in the passage:

"Looking at the development of science in the Twentieth Century one can distinguish two trends, which I will call "intensive" and "extensive" research, lacking a better terminology. In short: intensive research goes for the fundamental laws, extensive research goes for the explanation of phenomena in terms of known fundamental laws. As always, distinctions of this kind are not unambiguous, but they are clear in most cases. Solid state physics, plasma physics, and perhaps biology are extensive. High energy physics and a good part of nuclear physics are intensive. There is always much less intensive research going on than extensive. Once new fundamental laws are discovered, a large and ever increasing activity begins in order to apply the discoveries to hitherto unexplained phenomena. Thus, there are two dimensions to basic research. The frontier of science extends all along a long line from the newest and most modern intensive research, over the extensive research recently spawned by the intensive research of yesterday, to the broad and well developed web of extensive research activities based on intensive research of past decades".

Such a statement is clearly fertile ground for a rebuttal, since it totally misconstrues the impact of this "intensive" research on our knowledge of most phenomena in nature. He would have us believe that intensive discoveries caused “a large and ever increasing activity” to explain “hitherto unexplained phenomena”, and that “extensive research” is “spawned by the intensive research of yesterday”, or “based on intensive research of past decades”. Nothing could be much further from the historical facts.

Anderson was one of the first modern “established” physicists (being a Nobel Laureate helps) to openly oppose such a viewpoint. He began with the observations:

“The reductionist hypothesis may still be a topic for controversy among philosophers, but among the great majority of active scientists I think it is accepted without question. The working of our minds and bodies, and of all the animate and inanimate matter of which we have any detailed knowledge, are assumed to be controlled by the same set of fundamental laws, which except under certain extreme conditions we feel we know pretty well.

It seems inevitable to go on uncritically to what appears at first sight to be an obvious corollary of reductionism: that if everything obeys the same fundamental laws, then the only scientists who are studying anything really fundamental are those who are working on those laws. In practice, that amounts to some astrophysicists, some elementary particle physicists, some logicians, and a few mathematicians, and a few others. This point of view, which it is the main purpose of this article to oppose, is expressed in a rather well-known passage by Weisskopf”, which is given above.

Anderson then remarked:

“The effectiveness of this message may be indicated by the fact that I heard it quoted recently by a leader in the field of materials science, who urged the participants at a meeting dedicated to “fundamental problems in condensed matter physics” to accept that there were few or no such problems and that nothing was left but extensive science, which he seemed to equate with device engineering.

The main fallacy in this kind of thinking is that the reductionist hypothesis does not by any means imply a “constructionist” one: The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. In fact, the more the elementary particle physicists tell us about the nature of fundamental laws, the less relevance they seem to have to the very real problems of the rest of science, much less to those of society.”

While Anderson is certainly on the right track here, I think that it is unfortunate that we have allowed the elementary particle physicists to usurp the term “fundamental laws” for what they are investigating. It is much more accurate to describe these laws as the “elemental laws”, because they refer only to the most elementary activities in nature. Hence, while some may take comfort in the assumption that all physical processes are controlled by these elementary laws, they have shed no light on our understanding of most natural phenomena. There is no empirical bases to the assumption that the forces in the aggregate are simply linear combinations of these forces. As pointed out by Penrose [43], we know that this fails for gravity, if Einstein’s general theory of relativity is accepted. In effect, this was also Anderson’s point, as he continued: “The constructionist hypothesis breaks down when confronted with the twin difficulties of scale and complexity. The behavior of large and complex aggregates of elementary particles, it turns out, is not to be understood in terms of a simple extrapolation of the properties of a few particles. Instead, at each level of complexity entirely new properties appear, and the understanding of the new behaviors requires research which I think is as fundamental in its nature as any other. That is, it seems to me that one may array the sciences roughly linearly in a hierarchy, according to the idea: The elementary entities of science X obey the laws of science Y.

X	Y
solid state or many-body physics	elementary particle physics
chemistry	many-body physics
molecular biology	chemistry
cell biology	molecular biology
*	*
*	*
psychology	physiology
social sciences	psychology

But this hierarchy does not imply that science X is “just applied Y”. At each stage entirely new laws, concepts, and generalizations are necessary, requiring inspiration and creativity to just as great a degree as in the previous one. Psychology is not applied biology, nor is biology applied chemistry.”

I think that the above simple table has the seed of a very important insight into the future structure of the objectives of science. This will be discussed in section 12.

A.J. Leggett , in “The Problems of Physics” (Oxford Univ. Press, 1987), is one of the very few physicists to discuss the limitation of our knowledge based on our reductionistic knowledge. In connection with the reductionistic approach, Leggett has noted that [17c]:

“For a physicist to claim that he “knows” with certainty exactly how a particular physical system will behave under conditions very far from those under which it has been tested ... seems to me arrogant”

However, even a person as enlightened as Leggett can not refrain from a form of defense [17c]:

“Let me now return to the claim that ‘the four basic interactions... together with cosmology, account for all known natural phenomena’. This is, of course, not a statement of fact, but an act of faith. It is not an unreasonable one. What anyone who makes it is saying, in effect, is that while there are many natural phenomena which currently have not actually been explained in detail in terms of the four basic interactions, there is no clear example at present of a single phenomenon which we can prove cannot be so explained; so that the principle of Occam’s razor suggests that we should try to get along with the interactions we know about.”

This, of course, is erroneous. Three-body problems generally ‘cannot be so explained’ as he effectively noted himself in the previous quotations, so one wonders what it means to ‘get along with the interactions we know about’. We can’t ‘get along’ very far, as he notes [17d]:

“Is the behavior of complex systems indeed simply a consequence of the ‘complex interplay among many atoms, about which Heisenberg and his friends taught us all we need to know long ago’.... Or does the mere presence of complexity or organization or some related quality introduce new physical laws? To put it another way, would the complete solution of the basic equations of quantum mechanics - Schrödinger’s equation - for, say, the 10^{16} - odd nuclei and electrons composing a small biological organism actually give us, were it achievable, a complete description of the physical behavior of such an organism? The convention answer to the question is undoubtedly yes. But what few people realize is the flimsiness - or rather, the complete absence - of positive experimental evidence for this conclusion.”

One might add, that it is not only the absence of experimental evidence, but any rigorous mathematical evidence.

"It is indeed true that application of the formalism of quantum mechanics to complex systems yields predictions for currently measurable quantities which are often in good agreement, quantitatively as well as qualitatively, with the experimental results; and that in cases where there is substantial disagreement, there are usually enough unknown factors in the experimental system or approximations in the theory that the discrepancy can be plausibly blamed on one or the other or a combination thereof."

A very important point is raised here, on which we will only comment briefly, and that is this idea of "approximations in the theory". What this, in fact, frequently amounts to is the use of an entirely new theory (a new inductive process is involved). This is partially reflected in Leggett's further important remarks in connection with quantum mechanics [17d]:

"Certainly there is at present no clear evidence that such quantum-mechanical calculations give the wrong answers. What is rarely appreciated, however, is that, in the context of meaningful tests of quantum mechanics, in almost all cases up till now one has been dealing with very 'crude' features, which are in some sense the sum of one-particle (or 'one-quasi-particle') properties, or at best properties of pairs of particles. Where the specifically quantum-mechanical aspects of the behavior of a complex system can indeed be regarded as effectively the sum of the contributions of such small groups of microscopic entities, quantum mechanics seems to work well. Beyond this,..., it has barely begun to be genuinely tested."

"Even the phenomena of superfluidity and superconductivity, spectacular as they are, are still in the relevant sense the sum of a large number of one- or two-particle effects [17b]."

For a further brief review, read chapter 6 in his book. It should be emphasized that this above limitation is endemic to all of dynamics, whether classical, quantum mechanical, or of nonclassical forms.

"I am personally convinced that the problem of making a consistent and philosophically acceptable 'join' between the quantum formalism which has been so spectacularly successful at the atomic and subatomic level and the 'realistic' classical concepts we employ in everyday life can have no solution within our current conceptual framework [17e]."

5. THE THEORETICAL HERITAGE OF THE FIRST METAMORPHOSIS

In 1933 Einstein remarked [7d]: "If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds." One might reasonably express a similar sentiment: "If you want to find out whether the philosophical ideas advocated by theoretical physicists have support in Science, fix your attention on their accomplishments." While the many successes of Science are widely known, rarely is it pointed out how disjoint are these successes; that is, the nonuniversal character of the 'Laws', 'Theories', 'Principles', and 'Approximate Explanations' that have been achieved over the past three hundred years.

The explanation of Kepler's laws by Newton's 'universal' theory of gravitation was one of the few, and hence outstanding, examples of the process of obtaining more 'fundamental' theories of nature. It undoubtedly was a major factor in establishing the philosophical attitudes discussed in the last section. What followed Newton, however, was an entirely different story. A listing of some other 'laws', 'theories', 'principles', and 'approximate explanations', all from the 'hard' (i.e., more easily tractable) sciences, gives some sense of the disjoint (nonuniversal) character of our present understanding of Nature. It will be argued below that this is not a temporary feature of our understanding, but a fundamental character of Science as it has and will exist in the future. The second metamorphosis of science is directly related to this fundamental change in the character and objectives of Science, to be discussed below.

Without getting into an extended comparison of existing theories, etc., consider the following list: (1) Newton's 'universal' theory of gravitation, (2) the laws of thermodynamics, (3) Boltzmann's kinetic theory of gases (and irreversibility), (4) Maxwell's electromagnetic theory, (4) the Boltzmann-Gibbs theory of equilibrium statistical mechanics, (5) various theories of hydrodynamics (Euler, Navier-Stokes, etc.), (6) Einstein's theory of Brownian motion, (7) Einstein's special theory of relativity, (8) Einstein's general theory of relativity, (8) Schrödinger's wave-theory of quantum mechanics, (9) Pauli's exclusion principle, (10) Heisenberg's uncertainty principle, (10) the theories of quantum electrodynamics, (10) the Bardeen-Cooper-Schrieffer 'theory' of superconductivity, not to mention numerous theories associated with the area of high-energy physics and elementary particles.

It is widely known that various connections relate, or are assumed to form relations between some of these ideas (e.g., the Lorentz transformation; limits where Planck's constant tends to zero, or the velocity of light tends to infinity, etc.). It is also commonly assumed that many represent 'approximations' of more general theories. The concept of 'band theories' in solids, the 'theory' of superconductivity, the Boltzmann equation, or the various forms of hydrodynamic models are ready examples. All of these 'approximations' involve the introduction of logically independent concepts, none of which can be deduced from more 'fundamental' theories. They form another inductive step from experiments to mathematical models which are quite independent of any deductive process. Likewise it is clear that most of the above concepts are logically and empirically disjoint. An extraordinary example is Pauli's exclusion principle, which (according to quantum mechanics) lies at the heart of all of the order we observe in physical systems, since it is basic to the periodic tables. This principle stands in singular isolation from all of the other laws, with no known suggestion of a 'fundamental' relationship. Margenau [30] has suggested that this type of unique law may be required in other areas of science, particularly as it relates to life. This opinion is not shared by many.

In any case the above theories, laws, principles and approximations represent our best understanding of various realms of natural phenomena; they are our 'windows' on Nature. It will be argued below that such 'windows' will be the basis of Science in the future, and that the challenge will be to find the new conceptual structures that will unite these 'windows'. To understand the need for this new conceptual basis, we need to review the changes in our knowledge and operational abilities that have taken place over the past century.

6. THE BEGINNINGS OF THE SECOND METAMORPHOSIS OF SCIENCE: (1890-)

"You have learnt something. That always feels at first as if you had lost something."

— G.B. Shaw, Major Barbara, Act 3

"The great obstacle of man is the illusion of knowledge.... The great significance of the voyages of discovery was their discovery of ignorance ..."

—D. Boorstin [48]

The second metamorphosis of science began around 1890 with mathematical discoveries that indicated some fundamental limitations concerning our ability to carry out the deductive portion of the above methodological cycle. This was followed by another mathematical result in 1913 that proved that the informational encoding and decoding, leading to physical predictions, likewise can not be accomplished in most cases, unless a much more limited meaning of ‘predictions’ is used. Yet other mathematical discoveries extending from 1935 to 1988, showed that the inductive portion of this cycle is much more fragile than scientists might hope; the idealization that systems can be treated as being isolated from the universe, so frequently used in theories, cannot be used to establish the physically-desired property of ‘structural stability’ (to small changes) of these theories. Finally, in 1931, Kurt Gödel made the devastating discovery that neither the consistency nor the completeness of any sufficiently general mathematical system can not be proved within that system by generally accepted logical principles. Since all of these discoveries involved what scientists always believed was their most secure basis, mathematics, these discoveries have a profound impact on the philosophical basis for their beliefs concerning what they can predict in nature (recall the proviso at the end of Einstein’s 1918 quotation).

Before discussing these discoveries, it might be noted that there are several types of possible discoveries :

1. those that give answers to formulated questions.
2. results which yield new insights/concepts, never previously formulated.
3. results which contradict preconceptions or theories.

The discoveries that will be discussed in this section, and which might be referred to as ‘loss-of-innocence’ discoveries, fall within the preconceptions category (3). The great beauty of several of these discoveries is that they also fall in category (2), and nicely captured in the above quotations.

6.1 THE LOSS OF DEDUCTIVE INNOCENCE

The gravitational three-body problem, which had been studied by astronomers for many years, had defied all attempts to obtain a general analytical solution. The mathematical equations of Newton, for point particles attracting each other with an central-force law, contained eighteen position and velocity variables. On very general grounds one knows that such equations have eighteen constants of the motion; that is, functions of these positions, velocities, and time, which do not change with time (these functions can be associated with the required eighteen initial conditions). Despite the best efforts of many talented and dedicated theoreticians, only the ten “classic” constants of the motion could be found (total energy, total linear momentum, total angular momentum, and the equations describing the motion of the center of mass, which is explicitly time-dependent).

In 1887, Bruns proved that the only independent algebraic integral of the motion of the three-body problem are the ten classic integrals noted above. In such statements, and those which follow, the discriminate reader should focus on adjectives (here, algebraic; later, analytic, Hamiltonian, etc.), which are important both in terms of their limited mathematical applicability and, more interestingly, in their implicit statement concerning how scientists approach the analysis of nature (their philosophical predilection, which gets institutionalized in methods and perspectives). Since algebraic functions were far more general than scientists generally considered, Bruns’ theorem was a major negative statement about their efforts.

The second major blow to theoretical efforts to uncover these integrals of the motion came with a famous (in future considerations) theorem due to Poincaré in 1890. This theorem went far beyond the three-body problem, and struck at the heart of the only systematic method which theoreticians knew how to approach such a problem, namely by perturbation methods. This theorem concerned any Hamiltonian system which, when expressed in terms of action-angle variables (J, Θ), is an analytic function of a "perturbation parameter", ϵ,

$$H(J, \Theta, \epsilon) = H_0(J) + \epsilon H(J, \Theta)$$

where all $H(J, \Theta)$ are periodic in every Θ_i $(i = 1, \ldots, N)$, and where the Hessian $|\partial^2 H_0/\partial J_i \partial J_k|$ does not vanish identically. Under these conditions the theorem states that there exists no single-valued integral of the motion that is analytic (a power series) in ϵ, and is periodic in all Θ, other than the above Hamiltonian. A final, nonperturbative theorem was added by Painlevé in 1898, which stated that the only independent integrals of motion for the N-body problem which involves the velocities algebraically are the ten classic integrals. More details can be found in Whittaker's classic book [20], but the reading is not always easy. While Poincaré's theorem is not difficult to prove, attempts to generalize it have caused some errors (see, e.g., [21]).

Those who are now accustomed to the modern varieties of dynamic systems, will recognize that these adjective-limitations represent a significant restriction on the relevance of these results to many systems of present interest. On the other hand, no such detailed information (global integrals of the motion) is generally attempted for such systems, and totally different questions are explored, and these theorems at least suggest that is an intelligent approach. Thanks to Kolmogorov, Arnold, and Moser, we now know that much more sophisticated perturbation methods must be employed to even uncover the existence of families of regular solutions for Poincaré's Hamiltonian system (when ϵ is not zero). (For a simplified outline and references, see [1], Appendix L).

While Poincaré's theorem had very little impact on the scientific community as a whole, it did stimulate the imagination of such people as Fermi (see the discussion in [1]), and drew great praise from Brillouin [23]. Brillouin noted, in referring to Born's book on the mechanics of the atom, that:

"M. Born considers the great Poincaré theorem, which apparently contradicts the "general" perturbation methods of M. Born and restricts their validity to exceptional cases where the theorem does not apply. .... Born mentions these grave difficulties and does not elaborate further. He nonetheless adds some interesting comments : "It thus was never possible to prove the stability of the solar system ... The perturbation methods used in celestial mechanics are not convergent.... We thus see that it is impossible, for purely theoretical reasons, to prove the absolute stability of atomic systems." Let us add that these semiclassical methods completely failed in the case of the helium atom."

As far as I have encountered, these appear to be the most notable reactions to the above theorems from the leading physicists of the first half of this century. Considering that these results represented a major loss in the deductive arsenal of theoretical scientists, this is rather remarkable.

6.2 THE LOSS OF DETERMINISTIC INNOCENCE: THE POINCARÉ-TANGLE, BOREL-UNCERTAINTY, MACRO-UNCERTAINTY

To any practicing engineer, it is hardly a revelation to point out that we can only determine the state of a system to some finite accuracy, and that it is not realistic to assume that 'in principle' this uncertainty can be made arbitrarily small. This basic insight is by no means as readily accepted by many theoretical (as contrasted with experimental) scientists, who typically ignore this in physical circumstances of vastly greater complexity than the engineering realm. They often have a much more innocent (naive, and credulous) viewpoint concerning their powers to assign conditions to physical systems. Moreover, it is not as widely appreciated that this loss of accuracy generally has a profound impact on what can be predicted in the future. Fortunately this loss of determinism does not apply to all systems on the same time scales, which is why engineering methods work on certain systems, and why many astronomical predictions can be made well into the future. However, when it comes to the 'explanation' (in a predictive sense) of most of the dynamic features observed in nature, this loss of determinism is of paramount importance, both practically and in terms of the impact it has on our philosophical viewpoints.

A second insight of Poincaré's , formulated in Poincaré's geometric theorem, and proved by Birkhoff in 1913 [26], apparently drew no notice from the scientific community. Fortunately it drew Birkhoff attention, and his great power of deduction, which confirmed and extended Poincaré's insights [25]. It is a masterful piece of reasoning, (for an elementary presentation, see [1b] pg 44ff ; for the real analysis, see [24,26]). What is particularly beautiful about this result is that it not only clearly proves the innocence of any preconceptions of our ability to physically determine the future, but it also replaces it with a discovery of category (2) noted above, and captured in George Bennard Shaw's observation.

The theorem proposed by Poincaré was inspired by his study of the of the restricted three-body problem (involving a particle of negligible mass moving in the plane of two masses circling their center of gravity). The dynamics of the light mass is in a four-dimensional phase space, and restricted to a three-dimensional manifold, defined by the total energy. He then introduced the concept of a first return map, by considering an orbit in the vicinity of a periodic orbit, and a two-dimensional manifold transverse to this orbit at some point. Since this orbit must return near to this point, by assumption, this defines a "map" ,i.e., a one-to-one association of points on this manifold element and other points on this element, which moreover is measure preserving. This wonderful invention of the "Poincaré map" allowed Birkhoff to prove a conjecture of Poincaré's , made in 1912.

In brief, the idea is that a periodic orbit is an invariant point of such a map, and for most systems (including the three-body problem) most points which are nearby the invariant point will map about it in a circular fashion (so-called nonintegrable systems). The rate of rotation differs for different circles, from which it can be shown that between these circles there are at least two other invariant points (periodic orbits). One of these points also has invariant circles about it (an elliptic fixed point), whereas the other is a hyperbolic fixed point (with a one-dimensional manifold of points approaching it, and another leaving it). Note that this occurs between any two circles, and the discovered elliptic point likewise has its circles, each with pairs of fixed points (etc.,etc.). Moreover the "inset" and "outset" manifolds of the hyperbolic points generically intersect each other, producing an incomprehensible dynamic tangle (the "Poincaré Tangle"). These geometric ideas led Poincaré to note that dynamic systems are much more sensitive to initial conditions than had ever before been appreciated. He wrote in Vol.3 of Les Méthodes Nouvelles de la Mécanique Céleste, concerning the intersection of these inset and outset manifolds:

"The intersections form a kind of lattice, web, or network with infinitely tight loops; neither of the two curves ... must ever intersect itself, but must bend in such a complex fashion that it intersects all the loops of the network infinitely many times.

One is struck by the complexity of this figure which I am not even attempting to draw. Nothing can give us a better idea of the complexity of the three-body problem and of all problems in dynamics where there is no holomorphic integrals and the perturbation series diverge."

Many less cautious individuals have attempted to give their rendition of this Poincaré tangle (e.g., [1a], Fig. 6.70).

This type of discovery not only produced a loss-of-innocence, but also opens up an awesome panorama of dynamic activity which had never before been contemplated. The theorem which proved Poincaré's vision was given later by Birkhoff [24], in which he showed that, in any nonintegrable Hamiltonian system, any neighborhood (note: any!) of a periodic orbit of the elliptic type includes infinitely many periodic orbits both with elliptic and hyperbolic types; moreover, at most a finite set of these orbits have periods less than a given constant. Later, Birkhoff extended Poincaré's theorem to higher dimensions [25].

Poincaré's insight was not shared by many scientists, but Borel [28] was one who drew explicit attention to the meaningless nature of 'determinism' when applied to many-body systems . He computed, for example, that a displacement of 1 cm, on a mass of 1 gram, located in a nearby star (such as Sirus) would change the gravitational field on the earth by a fraction of about $10^{(-100)}$. Moreover an error of $10^{(-100)}$ in the initial conditions of a molecule in a gas would make it impossible to predict the molecular collisions for more than a fraction of a second. Thus 'determinism' of this many body system is realistically impossible beyond a fraction of a second. It should be noted that this Poincaré -Borel classical uncertainty preceded the Heisenberg uncertainty principle by many years, and is much more relevant to our predictive limitations at the macroscopic level of everyday experiences.

What these results illustrate is that system are subjected to limited predictability, not only from limited initial information, but also because of the combined non-isolation and dynamic instabilities. The uncertainty noted by Poincaré , Borel and Brillouin related to our inability to give precise numerical values to the initial states of the dynamic variables in the mathematical models, and the non-isolation of real systems. What is much more rarely noted, if ever, is that our physical uncertainty of the situation if much greater than simply the small inaccuracies which we can assign to each of the dynamic variables. There is a 'macro-uncertainty' which occurs any time we deal with a large number of dynamic units in a limited region, simply because they can not all be observed simultaneously, since many 'live in the informational-shadow' of others. In other words it is generally unavoidable, in any n-body system, for most of the bodies to be obscured by others; hence no values can be assigned to many of these dynamic variables (e.g.,[1a], Fig. 6.111).

Frequently, since many variables in the mathematical models are secondary or tertiary concepts (at best), and hence not observable, no initial values based on observations can be assigned to them. However it is interesting to note that even though a mathematical model, such as Newton's equations for a particle acted on by a time-dependent force, contains the unobservable quantities dx/dt and its derivative, the general solution can be written explicitly in terms of the observable positions at two different times [1a, pg. 526], provided that 'integrals' of the force are known; which is at least the case if the force is constant.

6.3 THE INSTABILITIES OF CLOSED-SYSTEM MATHEMATICAL MODELS, AND THE REALISTIC OPEN-SYSTEM CRITERION

The mathematical models which are constructed to represent the dynamics of physical systems typically contain parameters whose precise values are not known, or which we may not want to specify, except in some general range. In most cases it seems reasonable to assume that the precise value of these parameters should not be important in determining the general character of the resulting dynamics, since they are often only approximately known. In other words, in 'physically reasonable' models, $dx/dt = f(x)$ where $x \subset R_n$, we want the dynamics to be 'stable' to 'small' changes in the function $f(x)$. An early attempt to make this idea more precise, by specifying some reasonable meaning to the "stability" of the dynamics, and the smallness of the change of $f(x)$ (defining a vector field in the phase space), was made by Andronov and Pontriagin in 1935. They introduced the idea of structural stability of a vector field, defined by $f(x)$, in the following way:

The equations $dx/dt = f(x)$ are said to be structurally stable if, for any sufficiently small change, $df(x)$, in $f(x)$ (e.g., $df(x)$ is differentiable and its magnitude is less than some constant for all x), the phase portrait of $dx/dt = f(x)$ is topologically equivalent to the phase portrait of $dx/dt = f(x) + df(x)$. Put another way, if the vector field defined by $f(x)$ can be smoothly deformed in a 'rubber phase space' into the vector field of $f(x) + df(x)$, then the system $dx/dt = f(x)$ is structurally stable.

This would seem to be a minimal type of requirement, for it makes no demands on the temporal relationship between the two flows, but even so it turns out (at least mathematically) to be too demanding to be useful. Over the years (1952-1970) it was proved that most vector fields are structurally stable only if the phase space has dimension is less than or equal to two. However, the fact that most are not structurally stable (SS) does not imply that most physically significant equations are not SS.

There has been several alternative methods to come to grips with this concept of stability, which on a naive bases might seem to be obvious. One method involved the restriction of the allowed vector fields, $f(x)$, to what are called gradient systems, $f(x) = -dV(x)/dx$. The classification of the stability of these systems, initiated by R. Thom, led to the general field of catastrophe theory. A simple discussion of this can be found in [1], and a more thorough, and nicely written exposition is given in [41].

The next mathematical result has a long history, being only satisfactorily resolved by Zeeman in 1988 [27]. The suggestion was made by Andronov and Pontriagin in 1937 that all physically meaningful equations of motion should enjoy a certain 'structural stability', meaning that their solutions should not differ significantly from the solutions of another 'nearby' equations of motion. The relevancy to physical models is that many parameters in the differential equations (e.g., masses, concentrations, reaction rates, damping rates, etc.) are only known approximately, but presumably a small change in these values should not have drastic effects on the character of the solutions. Over a number of years it was proved that most equations with more than two variables are in fact very sensitive to such changes. These theorems, involving 'most' equations, did not necessary address those equations of physical interest, but no substitute definition of 'structural stability' was found which clarified this issue, until 1988. At that time Zeeman suggested that the stability concept should be based on the behavior of systems which are acted upon by weak stochastic actions. The very nice aspect of this suggestion is that real systems are indeed not isolated, but always acted upon by such weak disturbances; thus it is physically sensible. Moreover, this idea leads to the study of the distributions of solutions generated by such stochastic perturbations, and the comparison of these distributions for 'nearby' equations. If these distributions are equivalent, Zeeman called the systems epsilon-stable (because of the weak perturbations). The mathematical theorems for such comparisons are relatively easy to establish, and the epsilon-stability of most systems can then be proved. One of the lessons of all of this is that the inductive process, which is most commonly used to obtain models of isolated systems, requires a more realistic (non-isolated) context in order to establish a physically reasonable stability concept for mathematical models. Is this a whisper from the holistic eastern philosophies? For more details about this important idea, see [26] and [27].

6.4 THE INCOMPLETE CHARACTER OF FORMAL SYSTEMS

The final mathematical result struck at the heart of those scientists who had the belief that they can capture all of the truths about nature in a mathematical system of some sort (not necessarily systems of differential equations, or whatever). In 1931 Kurt Gödel published a paper "On Formally Undecidable Propositions of Principia Mathematica and Related Systems". What he proved was that, if a mathematical system is sufficiently general to included the arithmetic of whole numbers, and if one uses logical principles generally accepted by foundational schools, then: (a) there exists no proof within the system that its results will always be consistent; in other words, one can not prove that the system cannot yield a result A and also not-A; (b) if the above system is consistent, it is incomplete. This means that there are meaningful statements, say S, such that neither S or not-S is provable within this system. Since either S or not-S must be true, this means that there are true statements which cannot be proved within that system. Expressed another way, if the system is consistent, there are undecidable true statements. For a readable and authoritative account of the repercussions of these and other discoveries on the foundations of mathematics, and the resulting philosophical trauma they caused, see Kline [39]. Remarkably enough, this trauma has apparently been largely confined to mathematicians, having little discernible impact on most theoretical scientists, whose Mathematical Models and philosophical dreams depend precisely on such systems. For further aspects of these issues, see Penrose [40], and Casti [41].

The final philosophical concept which warrants a brief discussion, is the logical basis of all of modern science. Aristotle was also the fountainhead for the development of rules of reasoning, which originated in his Organon (c. 300 B.C). Many of these rules had previously been used by mathematicians, but he recognized that they could be abstracted and applied to all reasoning. An important example of such abstraction was his principle of the law of the excluded middle, namely every meaningful statement is either true or false (which may have been abstracted from the recognition of odd or even integers). It is remarkable that his logic formed the basis of all intellectual reasoning for over two thousand years.

This logic was put into a symbolic and algebraic context by Boole's analysis around 1850, at which time it was also generalized by De Morgan to deal with more general relationships. Russell and Whitehead used this symbolic method, culminating in their Principia Mathematica (1910-1913), in an attempt to axiomatize all of mathematics. However the rules of logic suffered setbacks around the turn of this century, bridged by the results of Cantor and Gödel . The genesis of these difficulties ("contradictions", "paradoxes", or "antimonies", depending on ones predilection) was the introduction of various infinite sets by Cantor . The history of the "loss of certainty" in mathematics has been nicely reviewed by Kline [32]. As an example, the law of the excluded middle (a proposition must be either true or false), which historically arose from its application to finite sets, is not generally accepted when applied to infinite sets (e.g., by Brouwer and other "intuitionists"). Gödel's famous incompleteness theorem asserts that if any formal theory is consistent, and adequate to embrace the theory of whole numbers, then it must be incomplete . This means that there must be some meaningful statement in number theory whose truth or falsity can not be proved in the theory. Such a result is seen by some as an argument for the denial of the law of the excluded middle. It will be argued in what follows that these beautiful results of mathematical logic have no bearing on the application of the excluded-middle reasoning which is always applied in the sciences, precisely because of the irrelevance of infinite sets in the physical sciences. This insight is one of the very important facts which has been emphasized by the results of the new element in the Operational Foundations of Science, to which we now turn.

7. THE THREE OBJECTIVE/SUBJECTIVE TIERS IN SCIENCE: OPERATIONAL BASES; VALIDATION METHODS; UNIFYING OBJECTIVES

It is generally taken as axiomatic that, when Science is to be explored in some generality, the only approach to this enterprise is to raise general philosophical questions at the outset. This is not only the viewpoint of the philosophers of Science, but of many scientist, usually in the later years of their life. The results of the such contemplations by some physicists have been outlined in section 4.

The thesis that will be outlined here is that this "top-down" approach, and the related philosophical questions need to be the last issues addressed in understanding Science. It is first necessary to look carefully at the information-generating processes that are accepted and used by scientists. It is then necessary to see how these processes are to be joined in some manner, to give "validations" of theories developed to explain observed sets of observations, and predictions of future observations. Finally, based on these two levels of understanding of how Science can be developed at any given time, we can explore questions concerning how we can unify different sets of observations within some broad set of principles. At this point the philosophical issues begin to come into full bloom, but now constrained by the prior clarification of what scientists can do (whether they recognize it or not) in their study of Nature. One of the great disadvantages of approaching the study of Science from the philosophical side is that rarely are the information-generating processes considered in any technical detail, and the technical aspects of validation methods are likewise usually ignored, with attention being focused on the subjective aspects that are present in the selection processes of experiments and theories.

To emphasize these three distinct levels of scientific activity, I will refer to them as the three tiers of science. They have the added benefit of ordering, to some degree at least, the injection process of subjective considerations into the relatively objective components of Science, as we proceed up these tiers toward the philosophical end. Within the present context, an "objective" process will simply mean that it is one which is generally accepted by research scientists - a process that will pass a refereeing process in a respected journal in any of the Natural Sciences. Note that we are speaking here of a process (a clearly defined activity), not a theory nor the interpretation of a process, both of which have significant subjective content. While this definition of an objective process may appear a little simplistic to philosophers, it would probably be quite acceptable to most scientists—perhaps illustrating again their different views of what Science consists of in its real-life operation.

In outline the three tiers are:

THE OPERATIONAL BASES. This consists of those methods which scientists believe that they can use to obtain information about natural phenomena. They are the accepted information-generating process that can be used in any field of science. They constitute the most objective elements of science. Since the first metamorphosis they have been limited to physical observations (PO) and the logical deductions obtained from mathematical theories (MT). Within the field of physics, which focuses on inanimate phenomena and usually reproducible experiments, the two operational methods have been physical experiments (PE) and, in reality, mathematical models (MM), which deductively fall far short of the general "theories" of reductionists' dreams. These issues have been discussed in previous sections.

To the operational bases of PE and MM has been added the new informational-generating process of the digital computer. This process is quite distinct and independent of the classic methods and many concepts which are fundamental to the deductions developed in MM. They have the commonality in the rules of logic, but otherwise differ fundamentally. To emphasize the flexibility of the digital computers' potential for generating new forms of information, this operational basis will be referred to as Computer Experiments (CE). These three operational bases need to be defined with some care, and that will be done in the next section.

VALIDATION METHODS. The three bases, (PE, MM, CE), need to be connection by some method which produces a "validation" of our proposed understanding of a set of observations. The validation must involve a PE, which produces the set of observations of some natural phenomena. Historically this was first outlined by Francis Bacon, who indicated the need to relate the observations in PEs with proposed MMs. This led to the ideal concept of a "Scientific Method", outline in section 2. The realities associated with the applications of this Scientific Method clearly point out some of the subjective contents of science [46].

Now with the three operational bases, the possible methods by which we can validate our understanding of natural phenomena has been vastly expanded. Any cyclic path joining PE with the other two processes, (MM, CE), potentially several times in the cycle, is a possible method of validation. This is a very large topic, and will be explored only briefly in this study - leaving a more complete consideration to the future. Indeed, this will undoubtedly be one of the long-term developments in this second metamorphosis.

UNIFYING OBJECTIVES. The demise of the simplistic micro-reductionistic program in Science, which, as seen above, is still defended (even if in waffling fashion!) by eminent physicists, leaves an important philosophical vacuum. If we can no longer scientifically defend the concept that all physical phenomena can be deductively established (thereby explained) from some "fundamental laws of Nature", we need to seek new realistic principles which we can use to unify (relate) sets of observations associated with different MMs. This unification can not be based on the reductionistic dream-connection to some all-inclusive "Law", but must be restricted to validation methods that are in fact available.

The process of "understanding" any set of observations comes from deducing features of these observation, based on equations or algorithms that are developed from concepts which are already familiar and accepted. Thus one of the unifying aspects can be (and in fact, has been) the commonality of the "familiar concepts" that are used to deduce new phenomena. In other words, for any process of "understanding", we must retain a "one-level reduction", which can logically produce a derivation of the new phenomena from established "familiar concepts". The classic reductionistic thesis is that we can continue this program downward any number of levels (Weinberg's "Why?" viewpoint; section 4.2). As we have seen, this has neither been the historical route to the many successes in physics, nor can it be, for a variety of technical reasons associated with the possible validation processes. We generally have no way of scientifically validating a "two-level reduction" - that is, deductively establishing that our proposed equations or algorithms can produce a range of observations involving phenomena which are believed to be the consequences of interactions of compound objects which are concurrently maintained by the interactions of their component parts. (This is difficult even to say!) Whether or not this might be accomplished in some particular case can not be excluded, of course, but as a general principle it presently is unchallenged.

What is potentially fascinating about this exploration of unifying concepts is that the family of "familiar concepts" has now grown remarkably, both in size and character. This family now includes a wealth of dynamic concepts, which have been uncovered during the last century. The idea that we might use such dynamic concepts for the "familiar bases" in our understanding process is one which has begun to develop in a limited community of scientists over the past twenty years. Certain "universal" dynamic properties, different forms of chaos, strange attractors, collective coherent structures, bifurcation scenarios, and interaction phenomena, are just a few of our presently known members of this family. These phenomena are likely (often known) to be signatures of underlying interaction processes “one-layer” down in our inventory of familiar concepts. The idea that we may learn to view these dynamic processes as the basis for helping us conceptually unify a variety of physical phenomena, gives a whole new perspective to what had previously been termed “laws”. They certainly are not like Newton’s Laws; they are about the dynamics of macroscopic systems. Since “law” carries so much historical connotation, we need a new term to replace “law” in this context. It will undoubtedly emerge together with these dynamic phenomena.

For the present, only a few of these ideas will only be roughly sketched in the following sections. More will have to wait some time.

8. THE OPERATIONAL METAMORPHOSIS OF SCIENCE

The operational bases of Science are those methods that scientists generally accept as being valid methods for obtaining information about physical phenomena in Nature. During the past century there has been a significant change in these bases, both in their number and in the recognized limitations of their capabilities to yield information. To a lesser extent, there is also a growing recognition of new opportunities which these generalized bases offer for generating information. This has to do with the validation processes, outlined in the last section, which will be explored in a following section. In this section we will focus on clarifying the present operational bases, particularly distinguishing the objective from the less objective contents. In this tier of Science, it is desirable to establish as much ”objectivity” as possible, but this needs to be tempered in order to retain flexibility, as will be seen.

The development of the electronic digital computer around 1950 forever changed not only the operational bases of science, but also the possible methods of validating theories, and ultimately the unifying objectives of Science. These far-ranging changes are not yet widely appreciated, nor indeed is it presently clear where these changes may take our understanding of Nature. Some of these changes are closely associated with the ancient bases of Science, namely Physical Observations, as distinct from the more limited method of Physical Experiments. Thus one new bases has been added, and both of the other two bases have (or will be) modified in the future, including our recognition of some limitations that have only become apparent in the last century.

To allow for these more general bases of Science, we will distinguish them as:

1. Physical Observations
2. Finite Digital Manipulations
3. Mathematical Models

and discuss these in some detail, to clearly distinguish the differences in the way that these three methods allow us to explore phenomena in nature.

Physical Observations involve the systematic observation of sets of spatial patterns, $\{P_i\}$. These sets can be generated from observations of one system at different times, or the patterns associated with different systems.

Physical Observations

a) comparisons - classifications

}both qualitative or quantitative

b) Physical Experiments

<u>Finite Digital Manipulations</u>	<u>Mathematical Models</u>
Computer Experiments	So far, principally the
Metaphorical Dynamics	"Science of the Infinite"
Symbolic Manipulations	limitations of perturbation methods,
quantitative → qualitative	logic and closure
(incomprehensible numbers)	expansion due to Cantor sets,
	topology, finite groups
	Qualitative components - sets??

Physical experiments involve taking data of selected observables, and looking for those sets of observables which have some degree of coherent behavior. Such sets of observables might be usefully referred to as a 'windows of comprehensibility' through which we attempt to understand some aspect of nature. This observed data is both finite in extent, and finite in accuracy.

Physical experiments involve a set of observables $\{O(i,t),t\}$, each of which can be measured (made quantitative) within some finite accuracy, $\{A(i),T\}$. Thus, the mathematical continuum of real numbers is only observed as cells of these sizes. Any proposed correspondence between a Mathematical Model, or a Computer Experiment, and the observations of a Physical Experiment represents and infinite-to-one (for $MM \to PE$) or a many-to-one (for $CE \to PE$) relationship associated with the infinite-to-one or many-to-one initial states that correspond to the one physical state. For the Computer Experiment to be used predictively in Physical Experiments, it is necessary that its accuracy be greater than the $\{A(i),T\}$, which will define the many-to-one relationship.

The **mathematical models** are obtained by some mysterious process we call induction, in which this finite data is postulated to be related to the solutions of some set of mathematical equations. These equations are formal systems that are subject to the rules of logic, and which operated in a number system of infinite extent and infinite accuracy. Because of these facts, these models inherently require more information to solve, and yield more information in their solutions than will every be observed in any experiment. Every physical experiment is associated with a family (continuum) of mathematical solutions. Hence, in the decoding process noted above, it is crucial to determine which deductions can be associated with physical experiments. Mathematical deductions which are 'too refined' are not physically relevant.

The **digital computer experiments** operates in a world of numbers and logical operations which overlap aspects of both physical experiments and mathematical models. Such experiments thereby offer a totally different method for exploring nature. These experiments always generate finite data (sometimes very large, to be sure !), of finite accuracy (similar but not identical to physical experiments). On the other hand, all operations of a computer, specified by some algorithm, are subject to the same logical rules as the solutions (deductions) of mathematical models. In particular, all dynamics obtained by computer experiments is precisely reproducible (in contrast to physical experiments), but can only be carried out to finite accuracy (in contrast to mathematical solutions).

Computer experiments have a vast potential to uncover the wonders of nature, which is only vaguely appreciated at present. Many scientists still believe that computers are only good for obtaining approximate solutions of the mathematical models, being unaware of the many discoveries which have already been made in completely different contexts.

Before discussing these results, we first emphasize the new operational bases of science with the help of the following new interaction diagram:

PHYSICAL EXPERIMENTS
(Windows of rationality:
sets of coherent observables;
finite data sets, finite accuracy)
encoding/decoding
different forms of
information;
inductive/reconstructive
associations

COMPUTER EXPERIMENTS
(difference equations (maps);
algorithms based on logical inferences;
finite data sets on the integers;
graphical representations of
incomprehensible data sets)

MATHEMATICAL MODELS
(differential equations; some
relationship to observable sets;
infinite ranges/accuracies;
logical deductive processes)

Such a figure can not do justice to the complex interplay which can occur between these three bases, some of which will now be described.

9. DEFINITIONS OF 'DETERMINISM', 'PREDICTABILITY', 'REPRODUCIBILITY', AND 'CHAOS' ASSOCIATED WITH THE THREE OPERATIONAL METHODS

It was emphasized in the last section that science now has three distinct operational bases for obtaining information about nature. With this awareness of the distinction between these methods comes the corresponding need to be more precise about terms which are frequently used in science. In this section we will focus on the frequently used terms 'Determinism', 'Predictability', 'Reproducibility', and 'Chaos'.

(1) MATHEMATICAL MODELS :

This discussion will be limited to systems of N first-order ordinary differential equations, $dx/dt = F(x)$.

DESCRIPTION.

DETERMINISM :. A system is deterministic if for every $x(0)$ there exists a unique solution, $x(t) = g(t; x(0))$, for all $t \Rightarrow 0$ (e.g., $F(x)$ satisfies the Lipschitz and Wintner conditions). Determinism is thereby limited to properties of logical operations.

A system of ODE which are deterministic will be called a (mathematical) Dynamic System. Modifications for systems that only have solutions for finite time will not be considered, for simplicity.

PREDICTABILITY :.

STRONG :. the system has a strong predictive capacity if, given $x(0)$, the value of $x(t)($ any $t > 0)$ can be determined by a finite number of algebraic operations involving t. e.g. $dx/dt = y : \ dy/dt = 1 \rightarrow (x, y) = (x(0) + y(0)t + .5t,\ y(0) + t)$

WEAK :. the system has a weak predictive capacity if, for any $x(0)$ and $e > 0$, there are a finite number of algebraic operations, $N(e)$, such that one can obtain a predicted value, $P(x(t))$ for any $t > 0$, satisfying $|P(x(t)) - x(t)| < e$. e.g., $dx/dt = x$

FINITE :. the system has a finite predictive capacity if it has the weak predictive capacity, but only for $t < T$, where $T(e)$ is a fixed finite time.

LIMITED :. the system has a limited predictive capacity, if it has a weak predictive capacity for only a limited set of $x(0)$. e.g., the results of the KAM theorem

REPRODUCIBILITY :. The reproducibility of Mathematical Models is equivalent to their being Deterministic.

CHAOS :. There are several characterizations. Sensitivity of the character of solutions to a arbitrarily small change in the initial state, $x(0)$. The Poincaré -Birkhoff theorem established the topological sensitivity of solutions for the restricted three-body problem. Levinson proved the correspondence between solutions of a forced relaxation oscillator and any arbitrary Bernoulli sequence. A quantitative characterization, which can only rarely be obtained in the strong or weak predictive sense, is the Lyapunov exponent. (e.g., $x(t+1) = 2x(t)\mathrm{mod}(1),\ 0 < x(0) < 1$)

(2) COMPUTER EXPERIMENTS :

(These are operations that can be carried out on a electronic computer in finite time; They differ from the Turing universal computer because of the finite memory).

A Dynamic System is now defined in terms of difference equations, $x(t+1) = F(x(t))$, where $F(x)$ can be represented by some finite algorithm, t is on the integers, and x are finite bit strings.

DESCRIPTION.

DETERMINISM :. Since the logical operations are the same as in mathematical models, Determinism is the same, except it is limited to any finite time.

PREDICTABILITY :.

STRONG :. A system is strongly predictive if for any $x(0)$ and $t < T$ (an arbitrary finite time) there is a finite algorithm that will yield $x(t)$. No system is strongly predictive, since it implies a solution of the halting problem for any $x(t)$, which Turing proved could not be accomplished by a universal computer, and hence not with any Computer Experiment.

WEAK :. A weak form of prediction is obtained by 'running' the dynamic equations, beginning with $x(0)$, and obtaining a data bank of $x(t)$ for all $t < T$. Using this data bank predictions can be made about dynamic systems within that time frame.

REPRODUCIBILITY :. This is equivalent to Determinism.

CHAOS :. The characterizations in Computer Experiments are of several types. One makes use of Poincaré maps, which can only be done in computer experiments, and then obtain approximations of fractal dimensions; either capacity or correlation dimensions, using reconstruction methods. Another characterization is to evaluate 'Lyapunov-like' exponents, and if some are positive and the dynamics is bounded, define the system to be chaotic. There is no way to compute the mathematical Lyapunov exponents- they are a formal theoretical concept.

(3) PHYSICAL EXPERIMENTS :

DESCRIPTION.

DETERMINISM :. Since there is no logical formalism, Determinism is not a defined concept.

PREDICTABILITY :. Physical Experiments are only (possibly) predictable through their association with either Mathematical Models or Computer Experiments.

REPRODUCIBILITY :.

$\{A(I), T|N\}$ − REPRODUCIBLE :. A Physical Experiment is $\{A(i), T|n\}$-reproducible if, given the initial state $\{O(i, 0), 0\}$, defined on $\{A(i), T\}$, the observed sets $\{O(i, t), t\}$ are the same any time the experiment is performed, for all $t \leq nT$ but not the same if $t > nT$, CHAOS :.

1. Physical Experiment exhibits chaos if the data from a Poincaré map has an approximate fractal dimension (either capacity, or correlation, or information), as analyzed with the help of a computer (not a Computer Experiment).
2. A Physical Experiment exhibits chaos if it is reproducible, and n becomes proportional to the sum over i of $|\log A(i)|$ as the $\{A(i)\}$ are decreased. In other words, the duration over which the system is reproducible only increases proportional to the initial information about the state of the system.
3. A characterization of the strength of the chaos is the magnitude of this proportionality constant. It is presumably proportional to the Lyapunov or Lyapunov-like constants in MM or CE respectively.
   [Note: to my knowledge, no experiments have established chaos in Physical Experiments based on this method]

10. OPPORTUNITIES GENERATED BY COMPUTER EXPERIMENTS

Referring to the diagram at the end of section 8, we note the following:

- Properties of math models: one of the earliest discoveries of computers was to uncover dynamic features in mathematical models which were not known to be present. The earliest discovery was the Fermi-Pasta-Ulam phenomena related to the dynamics of solids, which was completely unexpected. This was followed by the two examples by Edward Lorenz's discovery of a strange attractor in his simplified model of fluid dynamics, and Zabusky's discovery of the preservation of interacting solitons in the historic Korteweg-deVries fluid-dynamic equation. This result inspired Gardner, Greene, Kruskal, and Miura to develop a new mathematical method for analyzing classes of nonlinear partial-differential equations. Since then many other discoveries related to bifurcation effects, intermittent dynamics, etc., have been discovered only because computer experiments are available. However, there are fundamental encoding/decoding issues related to connection between MM and CE, such as the shadowing properties, which need clarification.
- Coherent searches: physical experimental data can be examined to determine what deterministic properties may be present in a low-dimensional representation; this 'reconstructional' method is in fact what Kepler used when given Brahe's astronomical data; it is a basic pre-calculus approach to science. It is possible to discover general 'laws' about a system, as Kepler did, without constructing dynamic models of the system. Also future predictions can be made without establishing any global model of the dynamics, or polynomial-model reconstructions can be attempted (a form of inductive searching). This process, which seems at first to be very 'applied', is in fact directly related to basic issues concerning our knowledge and understanding of nature.
- Graphical representations: transcribes incomprehensible amounts of data into spatiotemporal patterns that can be compared with one another, and compared with physical experiments. This makes possible a 'continuous' transition from quantitative to qualitative characterization of observations, and a controlled approach to the study of 'emergent' properties of systems.
- Quantitative analysis of physical data: quantitative measures of chaos; Lyapunov exponents, fractal dimensions, correlations, and power spectra.

- Dynamics with dynamic algorithms: several methods of changing the dynamic algorithms by methods similar to biological genetic mutations allows for searches of dynamic behaviors not imagined a priori. This method makes it possible to obtain some understanding of the origins of adaptive behaviors (in response to environmental actions on the system), and evolutionary mechanism. This is an area with a rich future, which has hardly been touched at present.

It is essential to recognize the fact that computer experiments can both be a two-way bridge between Physical Experiments and Mathematical Models, as well as an independent source of physical understanding. Such experiments have a mind-bending potential for future explorations of nature's secrets, which is only vaguely recognized today.

11. THE PHILOSOPHICAL REVITALIZATION OF SCIENCE

The philosophical heritage of many scientists, as a consequence of the first metamorphosis of Science, can be distilled (with limited accuracy) into a short list of beliefs:

- The universe is govern by some 'laws' (an axiomatic structure, subject to logical rules); God doesn't play with dice; the universe is not 'arbitrary', etc. There is a clear Judeo-Christian foundation to this primary belief.
- We can ultimately determine these laws by a process of reduction and synthesis. The process of synthesis is viewed as a trivial appendage to reduction processes.
- Once we know these elemental ("fundamental") laws, we can (at least 'in principle') explain (predict) all natural phenomena, using methods of deduction
- The observed physical states of Nature correspond one-to-one with the states in our Mathematical Models ('laws')
- We can obtain all of the theoretically required information about the physical state of a system to any desired degree of accuracy.
- (hence) Mathematical determinism insures that there is physical determinism

Certainly one of the most exciting aspects of this metamorphosis of science is that it will inject fresh air into this philosophical countryside, which will not only blow away the false 'theories-of-everything' gods, but in the process will bring all of science into a more holistic, realistic, and stimulating era of exploration. Rather than have the illusion that science is nearly at the end of the theoretical road, and all that remains is some process of synthesis to deduce the behavior of all of nature, we are faced with an entirely new challenge; constructing a science based on the reality and facts concerning our limitations and new operational potentials, rather than living in some scientific Camelot, where fantasies are simply proclaimed. The above 'reconstruction method', in so far as it is based on observables, is the basis for a fundamental re-examination of the importance of the empirical finite-informational character of natural phenomena; a basic issue that has so far only been approached from the perspective of Computer Experiments [42, 42a, 42b]. The transition from the comfort of the above clearly defined, albeit fallacious, list of beliefs and goals, to a much more ill-defined but exciting generalization of Science, will not be a rapid nor easy process.

12. SEEKING THE UNIFYING PRINCIPLES OF SCIENCE

Once it is accepted that the microreduction (inward-bound) knowledge of elementary particle physics cannot be followed by any synthesis that sheds light on most natural phenomena, the challenge is to find realistic principles that can aid us in unifying our understanding the more complex dynamic phenomena in Nature. While it may be comforting for some to cling to metaphysical beliefs in the existence of “universal laws”, the real beauty of Nature will (and has) only become revealed with the development of understandings that are based on the interactions of “components”, or “agents” associated with “one-level-down” analyses. This insight has slowly become appreciated by a growing number of scientists (see Appendix A), and was forcefully endorsed by P. Anderson in 1972 [16], as discussed in Section 4.5.

In some manner, which is presently not clearly established, we build our understanding of a class of phenomena, not from deductions based on some “microscopic theory” of a physical system, but from a hierarchical insight, which makes the phenomena at one level “understandable” on the basis of special correlations, or relations, between a limited number of one-level-down (OLD) agents. The character of this “reduction understanding” is not based on rigorous deductions from the components of the OLD system; the OLD agents that lead to an understanding at the next level are generally (always?) statistical composites of the phenomenal components of the OLD level—the “understanding” comes from both uncovering the restricted ensemble of components that is relevant, and the average aspects of their interactive dynamics which are responsible for the physical phenomenon one wants to understand. Thus, in the inanimate studies of physics, concepts of “collective variables” (normal modes (phonons), plasma waves (plasmons), Debye-Hueckel shielding, polarons, Cooper-pairs, localized modes, solitons, etc) have been widely used to develop interactive dynamic models which statistically account for various physical phenomena. Biological hierarchies are also quite obvious (e.g., atoms, molecules, cells, organism, etc.), but the process of uncovering the relevant dynamic models is much more difficult.

There appears to be two steps involved in this process of understanding; the selective ensemble conditions, which yields both the collective “agents”, and their interactive states, and the suitable average properties of the resulting dynamics, which accounts for the phenomenon of interest. Some aspects of this process have been stressed and nicely illustrated by Garfinkel [52]. Thus, in ecological models he emphasizes that we do not keep track of individual wolves and rabbits in modeling predator-prey behaviors. Similarly we do not keep track of each electron and atom in arriving at Ohm’s law for electrical conductivity. Indeed, to focus on these individual features would cause us to entirely loose the ability to describe the phenomenon of interest. What we do in each case is to constrain considerations to an ensemble of situations which are relevant to our interests (e.g., a homogeneous distribution of wolves and rabbits, or electrons and atoms), and then proceed to obtain equations for certain statistical properties of that ensemble (e.g., the equation of interacting populations).

The essential point is that, not only do we not need to use microscopic information, we SHOULD NOT use this information—it actually “looses” (does not contain) the phenomenon we want to understand. The phenomenon is not a property of any general microscopically-described situation—it is a property that is only captured in a suitable average characterization of a (very) select ensemble of these microscopic situations (e.g., an ensemble defined by constraints on initial conditions, environments, etc.—we do not get a solid from a collections of atoms until we constrain the energy, volume, and spatial distribution of the atoms within all members of the ensemble). The selection of the ensemble bypasses the impossible task of describing how the system (e.g., the solid) arrived at that state through some dynamic process involving other particles, radiation, etc., all hopelessly impossible, but happily also totally irrelevant.

Another interesting example of the successful use of nondeductive reduction reasoning is in statistical mechanics, which was also stressed by Garfinkel. In this case the Gibbs' canonical distribution can be understood ("derived") by starting with two coupled macroscopic systems, (A,B), and then separating them. All of equilibrium statistical mechanics begins with the wonderful assumption that is simply based on our dynamic ignorance—namely the assumption that the probability that a system is in some microstate only depends on the energy of that state. This, of course, is not a result deduced from any microscopic knowledge (point 1)—it is a shot in the dark, which works wondrously in many, but not all, situations. If we moreover approximate the energy relationship of the coupled and decoupled systems by

$$E(A+B) = E(A) + E(B)$$

we conclude that the probabilities of the coupled and decoupled systems must be related by

$$P_{(A+B)}(E(A+B)) = P_A(E(A))P_B(E(B))$$

where the right side comes from the independence of the decoupled systems. Now, differentiating with respect the independent variables $E_{(A)}$ and $E_{(B)}$, it is easy to show that these probabilities must be the Gibbs' canonical distribution (e.g., [53]). But it is essential in this "deductive process" that we approximate $E(A+B)$ by $E(A)+E(B)$—that is, ignore the interaction energy of the macroscopic systems (point 2). As Garfinkel stressed, it is necessary to use the approximate (not the exact) energy of these coupled macroscopic systems, (A,B), in order to derive the fundamental distribution of statistical mechanics! And, of course, this is in addition to the other non-deductive assumption (point 1)!

The general character and constraints of the such approximation processes, which can be used in this hierarchical formulation of models of natural phenomena, will require considerable clarification in the future. However, focusing on the need to uncover the relevant approximate ensembles and interacting average properties, in order to generate our "understanding models" (wherein comes the artistry of genuine scientific insight—see [1], Fig. 1.3), rather than maintaining the illusion of deductive advances up some hierarchy, is an essential step in formulating unifying principles for our future "understanding" of natural phenomena.

It is perhaps also worth pointing out that the functional hierarchy in Nature is necessarily related to the energetic stability of the agents at the respective levels. Indeed it is the energetic stability of these OLD agents, within the context of the dynamics of the phenomenon at the next higher level, which makes this hierarchical realization possible. Schematically,

Inward-Bound Rigidity	Outward-Bound Functionality
← interactively	interactively→
more stable agents	more metastable agents
(less responsive agents)	(more responsive agents)

"Responsive" agents are obviously necessary for adaptive agents of all sorts—agents which develop relational situations. Indeed, this hierarchical structure has elements of ancient Greek-causal perspectives of Nature on the Inward-Bound end, and ancient Chinese-correlational perspectives of Nature on the Outward-Bound end. The characterization of early Chinese interests as being "correlative thinking" is due to Joseph

Needham, and was examined by him with great sensitivity and scholarship in his monumental study "Science and Civilization in China" [54]. But he also noted that, for the Chinese the relational positions of things within the whole made them what they are. I think that this "relational" characterization is much more insightful and meaningful than the relatively sterile "correlational" view, and I will use it in the scientific sense of general dynamic relations.

The observation here is that science is beginning to search for those principles that will allow for some *scientific understanding* of relational dynamics in Nature, which have always been central to Asian cultures. Indeed we are being drawn to hierarchical concepts, which are scientific generalizations of hierarchies conceived independently in China and Greece around 400 BC [55]. It may well be that there will be some conceptual unification with the East as part of the evolutionary development of science.

APPENDIX: SCIENTISTS PHILOSOPHIZING ON FOUNDATIONS OF SCIENCE

This appendix contains a further collection of thoughts by some scientists, and a few of my reactions, concerning various fundamental issues of science:

1. On basic aspects of chaos, complexity and reductionism E. Wiechert, L. P. Kadanoff, E. N. Lorenz
2. On various forms of reductionism E. Mayer
3. Ideas about limitations of knowledge from scientific laws R. P. Feynman, F. Dyson
4. Some "unifiers" and "diversifiers" in science F. Dyson, with quotations from E. Wiechert and J. Wheeler
5. Complementary aspects of Nature; inanimate reductionism J. R. Oppenheimer

A.1 ON BASIC ASPECTS OF CHAOS, COMPLEXITY, AND REDUCTIONISM

I think that I should lead off any discussion that touches on reductionism with the following wonderful quotation, reproduced by Freeman Dyson in his book "Infinite in All Directions," from which he drew this title. The physicist Emil Wiechert, who was amazingly ahead of his times (1896), said:

"So far as modern science is concerned, we have to abandon completely the idea that by going into the realm of the small we shall reach the ultimate foundations of the universe. I believe we can abandon this idea without any regret. The universe is infinite in all directions, not only above us in the large but also below us in the small. If we start from our human scale of existence and explore the content of the universe further and further, we finally arrive, both in the large and in the small, at misty distances where first our senses and then even our concepts fail us."

Modern scientist, particularly physicists, are only beginning to appreciate this vision of Nature. This topic will be returned to in the section "On "unifiers" and "diversifiers" in Science."

What follows are three articles by Leo P. Kadanoff on these topics. The first is from: "Chaos: A View of Complexity in the Physical Sciences" in "The Great Ideas Today" (Encyclopedia Britannica, Inc., Chicago, 1986), pg. 86. Reprinted in "From Order to Chaos, Essays: Critical, Chaotic and Otherwise" (World Scientific, Singapore, 1993), pg. 399-429 On pg 403 of this article:

"In recent years there has been some change in the attitude of many physicists toward complexity. . . .Physicists have begun to realize that complex systems might have their own laws, and that these laws might be as simple, as fundamental, and as beautiful as any other laws of nature. Hence, more and more the attention of physicists has turned toward nature's more complex and "chaotic" manifestations, and to the attempt to construct laws for this chaos. . .The concentration upon chaos has been a part of a change in our understanding of what it means for a law to be "fundamental" or "basic." Physical scientists have sometimes been tempted to take a reductionist view of nature. In this view, there are fundamental laws and everything else follows directly and immediately from them. Following this line of thought, one would construct a hierarchy of scientific problems. The "deepest" problems would be those connected with the most fundamental things, perhaps the largest issues of cosmology, or the hardest problems of mathematical logic, or maybe the physics of the very smallest observable units in the universe. To the reductionist the important problem is to understand these deepest matters and to build from them, in a step-by-step way, explanations of all other observable phenomena."

Only one of these problems is "deep" in a reductionistic sense, and that's the elementary particle problem—cosmology and mathematical logic are not scientific problems, in any verifiable phenomenological sense. Moreover, the terms "fundamental" and "basic" should be eliminated from the discussion of scientific knowledge. Continuing:

"Here I wish to argue against the reductionist prejudice. It seems to me that considerable experience has been developed to show that there are levels of aggregation that represent the natural subject areas of different groups of scientists. Thus, one group may study quarks (a variety of subnuclear particle), another, atomic nuclei, another, atoms, another molecular biology, and another, genetics. In this list, each succeeding part is made up of objects from the preceding level. Each level might be considered to be less fundamental than the one preceding it in the list. But at each level there are new and exciting valid generalizations which could not in any very natural way have been deduced from any more "basic" sciences. Starting from the "least fundamental" and going backward on the list, we can enumerate, in succession, representative and important conclusions from each of these sciences, as Mendelian inheritance, the double helix, quantum mechanics, and nuclear fission. Which is the most fundamental, the most basic? Which was derived from which? from this example, it seems rather foolish to think about a hierarchy of scientific knowledge. Rather, it would appear that grand ideas appear at any level of generalization."

While one should not think that there is a hierarchy of scientific knowledge, in the sense of some superiority-ordering of knowledge, there are hierarchies of both the functional complexity of systems, and their dynamic processes.

The following is from "On Complexity," Physics Today, March 1987

"The astrophysicist, who must understand the distribution of matter in the the distribution of matter in the universe; the biophysicist, asking perhaps how life arose; the plasma physicist, working with the intertwined structure of flux lines in a swirling ionized gas; the solid-state scientist, looking at the crystallization of a piece of steel—all these scientists must deal with complexity as an everyday issue. Until recently, many physicists have dismissed examples such as these as "dirt physics" or "squalid-state physics"—perhaps intending to suggest that these examples somehow contain less intellectual content than, say, a simple and easily interpreted spectrum. Here, I wish to suggest the possibility of the opposite view: that the observed complexity in the world around us raises questions that are absolutely fundamental to our understanding of the nature of physical law. Three such questions are:

How do very simple laws give rise to richly intricate structures?

Way are such structures so ubiquitous in the observed world?

Why is that these structures often embody their own kind of simple physical laws?"

It should be noted that these questions all focus on structure, not on dynamic properties. What is generally much more important in Nature is the functionality of systems, which is governed by intricate dynamic relationships between systems and their environment.

Another idea expressed by Kadanoff comes from "Greats," in Physics Today, April 1994:

"In studying physics we learn that events in the natural world occur through the working out of laws of nature and that many aspects of these laws are accessible to human intellect."

This suggests that physics teaches us to accept the existence of "laws of nature", some of which are not accessible to the human intellect, and that events in the natural world occur through "the working out" of all of these laws. He reinforces this "subset-of-laws" view, by continuing:

"One sees that the development of the universe or the Earth or a piece of granite is the result of natural law. As a corollary, one begins to understand that humans are also part of the natural world and subject to nature's laws. The existence and ubiquity of law is the primary lesson, and the exact subset of laws learned less significant."

While this view may be philosophically appealing to some, it contains little of scientific significance. The "ubiquity of law" may be a comforting concept, but it certainly is not substantiated by science—it falls far outside of any scientific scope, and always will. Science involves the search for methods of discovering those limited portions of our experiences that can by rationally understood - and these are limited indeed. As Kadanoff has often demonstrated, the fun is in chasing down these limited windows of comprehension.

In his book "The Essence of Chaos" (Washington U.P., Seattle, 1993), E.N. Lorenz introduced what he calls the "liberal concept of chaos", which is directly related to a very important, and unexplored area of research. To first quote from him (pg. 157):

"Here I am adopting the more liberal concept of chaos, and am including processes with some true randomness, provided that the processes would exhibit similar behavior if the randomness could be removed."

The reason that such a "stable chaos" is of essential importance is that all physical systems are in fact subject to "true randomness" because of their coupling with the universe (e.g., by gravitation, radiation, vibrations, etc.). If the dynamic model we use to describe some deterministic-chaos phenomenon does not have stable chaos, then we have no right to view this phenomenon in isolation from its environment, and it will be replaced by randomness. An important extension of this perspective is that some phenomena (chaotic or not!) could exhibit "similar behavior" with or without a random component. This is also a central issue in the roundoff effects of computers—do these roundoffs effect a given phenomenon, so that it does not exhibit the "similar behavior" when run on another computer?

Continuing these "similar-behavior" considerations, let's turn to Lorenz's "butterfly effect" ideas. Since "the butterfly effect" has become a standard characterization for the influence of a small perturbation on a large-scale phenomenon, it is well worth while to review what Lorenz actually said in his talk on December 29, 1972 (see the appendix of his book "The Essence of Chaos" (Washington U.P., Seattle, 1993)). The talk was entitled "Predictability: Does the flap of a butterfly's wings in Brazil set off a tornado in Texas?", and should be read in its entirety. Consider first Lorenz's leading two propositions:

1. If a single flap of a butterfly's wings can be instrumental in generating a tornado, so also can all the previous and subsequent flaps of its wings, as can the flaps of the wings of millions of other butterflies, not to mention the activities of innumerable more powerful creatures, including our own species.
2. If the flap of a butterfly's wings can be instrumental in generating a tornado, it can equally well be instrumental in preventing a tornado.

More generally, I am proposing that over the years minuscule disturbances neither increase nor decrease the frequency of occurrence of various weather events such as tornados; the most that they can do is to modify the sequence in which the events occur. The question which really interests us is whether they can do even this—whether, for example, two particular weather situations differing by as little as the immediate influence of a single butterfly will generally after sufficient time evolve into two situations differing by as much as the presence of a tornado. In more technical language, is the behavior of the atmosphere unstable with respect to perturbations of small amplitude?

He then notes the lack of experimental possibilities with the atmosphere, and our reliance on computer simulations for conclusions in this area. Then he continues:

"Although we cannot claim to have proven that the atmosphere is unstable, the evidence that it is so is overwhelming. The most significant results are the following.

1. Small errors in the coarser structure of the weather pattern—those features which are readily resolved by conventional observing networks—tend to double in about three days. As the errors become larger the growth rate subsides. This limitation alone would allow us to extend the range of acceptable prediction by three days every time we cut the observational error in half, and would offer the hope of eventually making good forecasts several weeks in advance.
2. Small errors in the finer structure—e.g., the positions of individual clouds—tend to grow much more rapidly, doubling in hours or less. . . .
3. Errors in the finer structure, having attained appreciable size, tend to induce errors in the coarser structure. This result, which is less firmly established than the previous ones, implies that after a day or so there will be appreciable errors in the coarser structure, which will thereafter grow just as if they had been present initially. . . .The hopes for predicting two weeks or more in advance are thus greatly diminished.
4. Certain special quantities such as weekly average temperatures and weekly total rainfall may be predictable at a range at which entire weather patterns are not."

What is very important about these examples is that they address a question which exists in all scientific observations. Since all physical systems are coupled to the randomness in their environment (and ultimately in the universe), the question arises as to which physical phenomena are strongly influenced by this coupling. As Lorenz notes, many small disturbances in a sensitive system may cumulatively have a large effect or a very limited effect on a particular macroscopic phenomenon. Most importantly, such disturbances can have such macroscopic effects, but the time scales of these effects can vary widely, depending on the phenomenon one is considering. Presently, there is no known characterization of the type of empirical phenomena, or the time scales, relating to this micro-to-macro influence. It is a basic area that needs clarification.

A.2 ON VARIOUS FORMS OF REDUCTIONISM

An important voice in the demise of (micro)reductionism is E. Mayr, who has written several excellent books, which explain the great insights in this matter that one obtains from Biology.

E. Mayr, 'The Growth of Biological Thought' (Belknap Press of Harvard Univ. Press, Cambridge, 1982)

Here I paraphrase and quote his discussion of various forms of reductionism:

CONSTITUTIVE REDUCTIONISM. Asserts that the material composition of organisms is exactly the same as that found in the inorganic world. Furthermore, it posits that none of the events and processes encountered in the world of living organisms is in any conflict with the physio-chemical phenomena at the level of atoms and molecules.

"These claims are accepted by modern biologists. The difference between inorganic matter and living organisms does not consist in the substance of which they are composed but in the organization of biological systems"

"Virtually all biologists accept the assertations of constitutive reductionism, and have done so (except the vitalists) for the last two hundred years or more."

EXPLANATORY REDUCTIONISM. Asserts that one cannot understand a whole until one has dissected it into its components, and again these components into their components, down to the lowest hierarchical level of integration.

"lowest hierarchical level of integration" would mean, in biological phenomena, reducing the study of all phenomena to the molecular level, implying that "Molecular biology is all of biology".

Mayr remarks that "Explanatory reductionism is sometimes illuminating. The function of the genes was not understood until Watson and Crick had figured out the structure of DNA. In physiology, likewise, the functioning of an organism is usually not fully understood until the molecular processes at the cellular level are clarified."

However, processes at the higher hierarchical level are often largely independent of those at the lower levels. The units of the lower levels may be integrated so completely that they operate (ONLY in terms of) units the higher levels. The functioning of articulation (motion of joints) can be explained without a knowledge of the chemical composition of the cartilage. Replacing the articulating surface with a plastic can fully restore the normal functioning of an articulation.

A facile application of explanatory reductionism can do more harm than good. Early cell theory interpreted an organism as "an aggregate of cells". Early population genetics considered the genotype to be an aggregate of independent geners with constant fitness values. In this regard, Mayr quotes P.W. Anderson's 1972 article [16]:

"The more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science, much less of society"

THEORY REDUCTIONISM. Asserts that the theories and laws formulated in one field of science can be shown to be special cases of theories and laws formulated in some other branch of science. If this is done, one branch of science has been "reduced" to the other branch. As a special case, biology is considered to be reduced to physics when the terms of biology are defined in terms of physics and the laws of biology are deduced from the laws of physics.

To this idea, Mayr remarks: "I am not aware of any biological theory that has ever been reduced to a physio-chemical theory."

"Theory reductionism is a fallacy because it confuses process and concepts"

Biological processes, such as meiosis, gastrulation, and predation are also chemical and physical process, but they are only biological concepts and cannot be reduced to physio-chemical concepts.

Species, competition, territory, migration and hibernation are examples of organismic phenomena for which a purely physical description is at best incomplete and usually biologically irrelevant.

More concise quotations from E. Mayr on these forms of reductionism appear in ‘Toward A New Philosophy of Biology’ (Belknap Press of Harvard Univ. Press, 1988)

1. “The term ‘constitutive reduction’ has been applied to any dissection of phenomena, events, and processes into the constituents of which they are composed. Such analysis is not opposed by the modern biologist, since he does not question that all organic processes can ultimately be reduced to or explained by physico-chemical processes. None of the events and processes encountered in the world of living organisms is in any conflict with a physico-chemical explanation at the level of atoms and molecules. What is controversial art two other kinds of reduction, explanatory reduction and theory reduction.
2. Explanatory reduction claims that all phenomena and processes at higher hierarchical levels can be explained in terms of the actions and interactions of the components at the lowest hierarchical level. Organicists, by contrast, claim that new properties and capacities emerge at higher hierarchical levels and can be explained only in terms of the constituents at those levels. For instance, it would be futile to try to explain the flow of air over the wing of an airplane in terms of elementary particles. Almost any phenomenon studied by a biologist relates to highly complex systems, the components of which are usually several hierarchical levels above the level studied by physical scientists.
3. Finally, there is theory reduction, which postulates that the theories and laws formulated in biology are only special cases of theories and laws formulated in the physical sciences, and that such biological theories can thus be reduced to physical theories. All authors in recent years who have studied this claim, including even several former reductionists, have come to the conclusion that such theory reduction is virtually never successful. As a matter of fact, theory reduction has been only partially successful even within the physical sciences, and has been singularly unsuccessful within the biological sciences. Indeed, none of the more complex biological laws has ever been reduced to, and explained in terms of the composing single processes.”

A.3 IDEAS ABOUT LIMITATIONS OF KNOWLEDGE FROM SCIENTIFIC LAWS

Here are a few reflections of R. P. Feynman, involving his typically atypical insights. The first is from the Int. J. Theor. Phys. 21, 467 (1982)

Consider the proposition that “...everything that happens in a finite volume of space and time would have to be exactly analyzable with a finite number of logical operations. The present theory of physics is not that way, apparently. It allows space to go down into infinitesimal distances, wavelengths to get infinitely great, terms to be summed in infinite order, and so forth; and therefore, if this propositions is right, physical law is wrong.’

From his “The Character of Physical Law”, pg 122

“In fact, although we have been talking in these lectures about the fundaments of the physical laws, I must say immediately that one does not, by knowing all the fundamental laws as we know them today, immediately obtain an understanding of anything much. It takes a while, and even then it is only partial. Nature, as a matter of fact, seems to be so designed that the most important things in the real world appear to be a kind of complicated accidental result of a lot of laws.”

Feynman’s description of “a kind of complicated accidental result”, and “a lot of laws”, not only dismisses a reductionistic view of nature, but also emphasizes our inability to deduce many results - what some might now refer to as an “emergent” phenomenon. His example is particularly beautiful:

"To give an example, nuclei, which involve several nuclear particles, protons and neutrons, are very complicated. . . .But the remarkable thing about nature is that the whole universe in its character depends upon the position of one particular level in one particular nucleus. In the carbon 12 nucleus, it so happens, there is a level at 7.82 million volts. And that makes all the difference in the world."

He then goes on to explain how Hoyle and Salpeter pointed out that three helium nuclei could fuse to form carbon only if there happened to be an energy level at 7.82 million volts—for this would allow the helium atoms to stay together sufficiently long, on the average, for the fusion to take place. Thus this energy level was needed to explain how carbon was formed in stars, from which the production of the heavier elements could be understood.

"And so, by a back-handed, upside-down argument, it was predicted that there is in carbon a level at 7.82 million volts; and experiments in the laboratory showed that indeed there is. Therefore the existence in the world of all these other elements is very closely related to the fact that there is this particular level in carbon. But the position of this particular level in carbon seems to us, knowing the physical laws, to be a very complicated accident of 12 complicated particles interacting. This example is an excellent illustration of the fact that an understanding of the physical laws does not necessarily give you an understanding of things of significance in the world in any direct way. The details of real experience are often very far from the fundamental laws."

One might certainly add that it is not just the "details of real experience", but most of our experiences—say life for example!

From F. Dyson's "Infinite in all directions"

"It is my hope that we may be able to prove the world of physics as inexhaustible as the world of mathematics" (subsequent to Godel's results) ". . .I hope that the notion of a final statement of the laws of physics will prove as illusory as the notion of a formal decision process for all of mathematics. If it should turn out that the whole of physical reality can be described by a finite set of equations, I would be disappointed."

There is no possibility that Dyson will be disappointed.

A.4 SOME "DIVERSIFIERS" AND "UNIFIERS" IN SCIENCE

In chapter 3 of his book "Infinite in all directions", Freeman Dyson gives a very nice discussion of the distinction between two philosophical views of science that have been held by scientists. He refers to these groups as "unifiers" and "diversifiers", which should be carefully used only in the way he intended, and not by other connotations of these words. Thus "unifiers" does not mean that it brings all branches of science together, because it is based on the misconception of a unification by way of reductionism. But, with this understanding, his distinction is very important and enlightening. Thus he begins with a comparison of the science in Athens and Manchester with the views of Einstein and Rutherford. Dyson makes this comparison from his observation that

"The science of the academic world tends to be dominated by unifiers, while the science of the industrial world tends to be dominated by diversifiers."

The title of his book comes from the following wonderful quotation due to the physicist Emil Wiechert (1896):

"So far as modern science is concerned, we have to abandon completely the idea that by going into the realm of the small we shall reach the ultimate foundations of the universe. I believe we can abandon this idea without any regret. The universe is infinite in all directions, not only above us in the large but also below us in the small. If we start from our human scale of existence and explore the content of the universe further and further, we finally arrive, both in the large and in the small, at misty distances where first our senses and then even our concepts fail us."

Dyson noted further:

"Today we still find scientists divided into two camps: the unifiers who, like Einstein, believe that nature can be reduced to a finite set of equations; the diversifiers who, like Wiechert, believe that nature is inexhaustible."

"Einstein said:" The creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed.'

One is left to speculate as to what "in a certain sense" is suppose to mean.

"Now it is generally true that the greatest scientists in each discipline are unifiers. This is especially true in physics. Newton and Einstein were supreme unifiers. The great triumphs of physics have been triumphs of unification. We almost take if for granted that the road of progress in physics will be wider and wider unification bring more and more phenomena within the scope of a few fundamental principles. Einstein was so confident of the correctness of this road of unification that at the end of his life he took almost no interest in the experimental discoveries which were then beginning to make the world of physics more complicated. It is difficult to find among physicists any serious voices in opposition to unification. One such voice is that of Emil Wiechert: "If we start from our human scale of existence and explore the content of the universe further and further, we finally arrive, both in the large and in the small, at misty distances where first our senses and then our concepts fail us."

"The remark of Wiechert's shows him to have been extraordinarily far-sighted. At the time when he was speaking, the leading theoretical physicists of Germany were still engaged in bitter arguments over the question of the real existence of atoms... Wiechert's words were ignored. His vision was too large for the time he lived in."

"In biology the roles are reversed. A very few of the greatest biologists are unifiers. Darwin was a unifier, consciously seeing himself as achieving for biology the unification which Newton had achieved for physics. ... The working lives of ninety-nine out of hundred biologists are spent in exploring the details of life's diversity... Unifiers like Darwin are as rare in biology as diversifiers like Wiechert are rare in physics. ...Darwin has only one successor and his name is Francis Crick."

"Diversifiers in physics, such as Wiechert in the 1890's and John Wheeler in our time, have tended to be pushed out of the mainstream."

"Here is the theme song of Wheeler's recent book, "Frontiers of Time": "Individual events. Events beyond law. Events so numerous and so uncoordinated that, flaunting their freedom from formula, they yet fabricate firm form." ."

"Wheeler's colleagues love him more that they listen to him. The physics of the unifiers has no room for his subversive thoughts."

"According to Wheeler, the laws of physics evolve progressively in such a way as to make the universe observable." (comprehensible?)

"In biology there has been a healthier balance. The mainstream of biology is the domain of the diversifiers, the domain of events numerous and uncoordinated, flaunting their freedom from formula. But with a unifier like Darwin or Crick arrives on the scene, he is not ignored. He is even, after a while, honored and rewarded. And his ideas flow into the mainstream. I am suggesting that there may come at a time when physics will be willing to learn from biology as biology as been willing to learn from physics, a time when physics will accept the endless diversity of nature as one of its central themes, just as biology has accepted the unity of the genetic coding apparatus as one of its central dogmas."

"Sometimes unity and abstract structure are overemphasized."

"There was once a time when the ideals of unity and diversity in science were briefly held in balance. This was in the seventeenth century, when modern science was in its first flowering and both Descartes and Bacon were honored. There was no clear separation between the sciences of cosmology and biology.... The eighteenth century dawned bleakly under a heaven grown empty and dead. Cosmology, ever since that time, has concerned itself only with an empty and dead universe."

"Einstein and Rutherford gave us complementary views of science, and each was to single-mindedly attached to his own view to understand the other. Both of them, for opposite reasons, rejected the compromise which Bohr's notion of complementarity offered them. For Einstein, the electron must ultimately be understood as a clumping of waves in a non-linear field theory. For Rutherford, the electron remained a particle, a little beggar that he could see in front of him as plainly as a spoon."

A.5 COMPLEMENTARY ASPECTS OF NATURE; INANIMATE REDUCTIONISM

J.R. Oppenheimer Science and the Common Understanding (Simmon & Schuster, N.Y., 1954)

He describes the concept of "complementary views" we can have of systems in a number of situations, not only in quantum mechanics. He illustrates this by the "complementary" descriptions of classical dynamics and of statistical mechanics. This particularly "complementarity" he views as a matter of convenience—"it makes no sense to do" otherwise (than take these two views) "since each description is appropriate to a context quite different from the other."

In more detail (pp 77):

"there is nothing in the classical dynamics which underlies kinetic theory to suggest that the behavior of a gas would be any different if we had performed the immense job of locating and measuring what all the molecules were doing. We might then, it is true, not find it natural to talk about temperature, because we would need no average behavior; we would have an actual one; but we could still define the temperature in terms of the total kinetic energy of the molecules ....

We have therefore a situation in which there are two ways of describing a system, two sets of concepts, two centers of preoccupation. One is appropriate when we are dealing with a very few molecules and want to know what those molecules do; the other appropriate when we have a large mass of matter and only rough and large-scale observations about it.

There is, however, no logical or inherent difficulty within the framework of classical physics in combining both descriptions for a single system. . . It is not that we cannot do this without violating the laws of physics; it is that it makes no sense to do it, since each description is appropriate to a context quite different from the other."

What this type of description ignores is that "locating" and "measuring" are empirical, not mathematical concepts. That the determinism of a mathematical equation modeling some system in no way implies an empirical determinism of this physical system. Empirical and mathematical determinism are quite independent concepts, which can only be related on a case by case basis. Empirical determinism is a involves both finite accuracy (a partitioning of the real numbers) and finite-duration of observational knowledge, which is not so with mathematical determinism. So there is indeed a logical and inherent impossibility, much less a "difficulty", in combining these descriptions.

It is also necessary to point out that the "immense job of locating and measuring what all molecules were doing", is not simply an "immense job", but an impossible job, with no scientific meaning - even within some allowed experimental errors. There is no way that such a "locating and measuring" experiment could be accomplished for macroscopic systems. One part of a system would interfere with "seeing" another part, even instantaneously, much less their associated velocities. This "immense-job" description does not adequately dissociate from Laplace's view of our abilities to determine situations in Nature.

However, when it comes to biological systems, and particularly psychological matters, he not only accepts but relishes the existence of fundamentally different complementarities in Nature. Thus:

"Indeed, an understanding of the complementary nature of conscious life and its physical interpretation appears to me a lasting element in human understanding and a proper formulation of the historic views called psycho-physical parallelism."

"Whether a physico-chemical description of the material counterpart of consciousness will in fact ever be possible, whether physiological or psychological observation will ever permit with any relevant confidence the prediction of our behavior in moments of decision and in moments of challenge, we may be sure that these analyses and these understandings, even should they exist, will be irrelevant to the acts of decision and the castings of the will as are the trajectories of molecules to the entropy of a gas."

"The wealth and variety of physics itself, the greater wealth and variety of the natural sciences taken as a whole, the more familiar, yet still strange and far wider wealth of the life of the human spirit, enriched by complementary, not at once compatible ways, irreducible one to the other, have a greater harmony. They are the elements of man's sorrow and his splendor, his frailty and his power, his death, his passing, and his undying deeds."

REFERENCES

(1) Jackson, E. A., Perspectives of Nonlinear Dynamics, Vol.1 (Cambridge University Press, 1990).
(1a) ibid, vol. 2, pp. 526-527.
(2) Kolmogorov, A. N., Automata and Life, in Cybernetics Today, pp. 20-41 (I. M. Makarov, MIR Pub., Moscow, 1984).
(3) Galileo, G. (1623). On Motion and On Mechanics, ed. and trans. I.E. Drabkin and S. Drake (University of Wisconsin Press, 1960).
(4) Park, David, The How and the Why (Princeton University Press, 1990).
(5) Claggett, M., The Science of Mechanics in the Middle Ages (Univ. of Wisconsin Press, 1959).
(6) Holland, J. H., K. J. Holyoak, R. E. Nisbett, P. R. Thagard, Induction; Processes of Inference, Learning, and Discovery (MIT Press, 1986).
(7) Einstein, A., Ideas and Opinions (Bonanza Books, N.Y.), pp. 225-226.
(7a) ibid, p. 274.
(7b) ibid, p. 293.
(7c) ibid, pp. 293-294
(7d) ibid, p. 270.
(8) Feynman, R. P., The 1964 Messenger Lecture at Cornell University reprinted in The Character of Physical Law (M.I.T. Press, 1985).

(9) Hawking, Stephen W., A Brief History of Time (Bantam Books, 1988) pp. 12-13.
(9a) ibid, pp. 167-169.
(9b) ibid, pp. 147ff.
(11) Weinberg, S., Towards the Final Laws of Physics, pp. 61-110 , in “Elementary Particles and the Laws of Physics” (Cambridge University Press, 1987).
(12) Weinberg, S., Newton’s Dream, pp. 96-106, in “Newton’s Dream” (M.S.Stayer, Ed.; Queen’s Quarterly, McGill-Queen’s University Press, 1988).
(13) Weinberg, S., “Newtonianism, Reductionism and the Art of Congressional Testimony”, Nature 330, Dec. 3, 433-437 (1987).
(14) Wheeler, J. A., “On Recognizing “Law Without Law””, Oersted Lecture, American Association of Physics Teachers and the American Physical Society, Jan. 25, 1983 (Am. J. Phys.).
(14a) Deutsch, D., “On Wheeler’s Notion of “Law without Law” in Physics”, Foundation of Physics **16**, 583-590 (1986).
(14b) “Between Quantum and Chaos; Studies and Essays in Honor of John Archibald Wheeler”, (Eds., W. H. Zurek, H. van der Merwe, W. A. Miller; Princeton Univ. Press, Princeton, 1988).
(15) Mayr, E. in “Evolution at a Crossroads” (Depew, D.J. and Weber, B.H., Eds.; MIT Press, 1982).
(16) Anderson, P., More is Different, Science 177 (Aug), 393-396 (1972).
(17) Leggett, A.J., The Problems of Physics (Oxford Univ. Press, 1987).
(17a) p. 178.
(17b) p. 183.
(17c) p. 176.
(17d) p. 177.
(17e) pp. 178-179.
(20) Whittaker, E. T., Analytical dynamics of particles and rigid bodies (Dover, 1944).
(21) Jackson, E. A., “Nonlinear Coupled Oscillators. I. Perturbation Theory, Ergodic Problem”, J. Math. Phys. 4, 551-558 (1963); see appendix.
(23) Brillouin, L., Scientific Uncertainty, and Information (Academic Press, 1964).
(24) Birkhoff, B. D., Dynamical Systems (Amer. Math. Soc. Colloquium Pub., 1927).
(25) Birkhoff, G. D., Une généralisation a n dimensions du dernier théoreme de géométrie de Poincaré, C.R.Acad.Sci.Paris, Vol. 192, pp. 196-198 (1931).
(26) Birkhoff, G. D., Proof of Poincaré’s Geometric Theorem, Trans. Amer. Math. Soc. **14**, 14-22 (1913).
(27) Zeeman, E., “Stability of Dynamic Systems”, Nonlinearity 1 (1988) 115-155 - “A New Concept of Stability”, pp. 8-15 in Theoretical Biology (B. Goodwin and P. Saunders, eds., Edinburgh Univ. Press, 1989).
(28) Borel, E., Introduction geometrique a quelques theories physques, p. 94 (Gauthier-Villars, Paris, 1914).
(29) Margenau, H., The Nature of Physical Reality (McGraw-Hill, 1950)
(30) Margenau, H., The Miracle of Existence (New Science Library, Shambhala Pub., Boston, 1987); e.g., ff 12.
(31) Planck, M., Where is Science Going? (Ox Bow Press, 1981).
(32) Kline, M., Mathematics, The Loss of Certainty (Oxford Univ. Press, 1982).
(33) Hadamard, J., “Les surfaces à courbures opposées et leurs lignes géodésiques”, J. Math. pures et appl., 27-73 (1898); reprinted in Oeuvres de Jacques Hadamard (Paris: CNRS, 1968).
(34) Duhem, P., La Théorie physique: Son objet et sa structure (Paris: Chevalier et Riviére, 1906): section “Exemple de déduction mathématique à tout jamais inutilisable” [example of a mathematical deduction

forever unusable] In English translation: The Aim and Structure of Physical Theory (Princeton Univ. Press, 1991).
(35) Sir James Lighthill, F.R.S., The Recently Recognized Failure of Predictability in Newtonian Dynamics, Proc. R. Soc. Lond. A407, 35-50 (1986).
(40) Penrose, R., The Emperor's New Mind (Oxford Univ. Press, 1989).
(41) Casti, J. L., Reality Rules (John Wiley and Sons, 1992) chaper 2; on the controversy, 2.14.
(42) Fredkin, E., Digital Mechanics, Physica D 45, 254-270 (1990).
(42a) Fredkin, E. and Toffoli, T., Conservative logic, Int. J. Theor. Phys. 21, 219-253 (1982).
(42b) Margolus, N., Physics-like models of computation, Physics D 10, 81-95 (1984).
(44) Weisskopf, V. F., (1977), "The Frontiers and Limits of Science", Amer. Scientist 65, 405-411.
(44a) ibid., p. 411.
(45) Mayr, E., "The Growth of Biological Thought", (Harvard Univ. Press, 1982).
(46) Bauer, H. H., "Scientific Literacy and the Myth of the Scientific Method", (Univ. of Illinois Press, Urbana, 1992).
(47) Weisskopf, V. F., Nuovo Cimento Suppl. Ser.1 4, 465 (1966) Phys. Today 20 (No. 5), 23 (1967)
(48) Boorstin, Daniel, in "Discovery, Science, Technology, and the 'Illusion of Knowledge"', reported by Peter Grier in The Christian Science Monitor, pg. 3, Jan. 2, 1992.
(49) van Bendegem, J. P., "Finite Empirical Mathematics; Outline of a Model." Rijksuniversitent Gent (1987).
(50) Casti, J. L., "Paradigms Lost", pgs. 47,48 (Avon Books, NY, 1990).
(51) For a brief, but nice discussion of Bacon's contributions, see Moore, J.A., "Science as a Way of Knowing; The Foundations of Modern Biology" (Harvard Univ. Press, 1993). More extensive recent references are; "The works of Francis Bacon". (Edited by James Spedding, R. L. Ellis, and D. D. Heath. 14 vols. London: Longman (1857-1874). Reprinted 1968 by Garrett Press, New York); "The Great Instauration". (Edited by Gail Kennedy. Garden City: Doubelday, Doran (1937)); "New Organon and Related Writings". (Edited by Fulton Anderson. Indianapolis: Bobbs Merrill (1960)).
(52) Garfinkel, A., "Reductionism" p. 443 in "The Philosophy of Science" (R. Boyd, P. Gasper, and J.D. Trout, Eds., MIT Press, 1991). Garfinkel's analysis is in direct contrast with the hierarchical, but microreduction views of P. Oppenheim and H. Putnam, "Unity of Science as a Working Hypothesis", p.405, Ibid. Garfinkel's article comes from Chapter 2 of his book, "Forms of Explanation" (Yale Univ. Press, New Haven, CT, 1981).
(53) Jackson, E.A., "Equilibrium Statistical Mechanics", pg. 86 (Prentice-Hall, Englewood Cliffs, N.J., 1968)
(54) Needham, J., "Science and Civilisation in China", Vols. 1-11 (Cambridge University Press) - Vol. 11 is yet to be published. Of particular relevance to science is Vol.2, "History of Scientific Thought" (Cambridge Univ. Press, 1956), in which many interesting and valuable comparisons of the views of Nature in the ancient cultures of China, India, Greece, and the Middle East.
(55) Needham, J., Theories of the 'Ladder of Souls', Ibid., p. 21, Vol.2