PART I

THE UNITY OF SCIENCE - PLURALITY OF NATURE

The Plurality of Science

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What I have to say falls under four headings: What is unity of science, unity and reductionism, the search for certainty, and the search for completeness.

1. What Is Unity of Science Supposed to Be?

To answer this initial question, I turned to the introductory essay by Otto Neurath [5] for Volume 1, Part 1, of the *International Encyclopedia of Unified Science*. He begins this way:

> Unified science became historically the subject of this *Encyclopedia* as a result of the efforts of the unity of science movement, which includes scientists and persons interested in science who are conscious of the importance of a universal scientific attitude.
>
> The new version of the idea of unified science is created by the confluence of divergent intellectual currents. Empirical work of scientists was often antagonistic to the logical constructions of a priori rationalism bred by philosophico-religious systems; therefore, "empiricalization" and "logicalization" were considered mostly to be in opposition--the two have now become synthesized for the first time in history ([5], p. 1).

Later he continues:

> All-embracing vision and thought is an old desire of humanity. ...This interest in combining concepts and statements without empirical testing prepared a certain attitude which appeared in the following ages as metaphysical construction. The neglect of testing facts and using observation statements in connection with all systematized ideas is especially found in the different idealistic systems ([5], pp. 5-6).

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Later he says:

> A universal application of logical analysis and construction to science in general was prepared not only by the systematization of empirical procedure and the systematization of logico-empirical analysis of scientific statements, but also by the analysis of language from different points of view ([5], pp. 16-17).

In the same volume of the Encyclopedia, the thesis about the unity of the language of science is taken up in considerably more detail in Carnap's analysis of the logical foundations of the unity of science. He states his well-known views about physicalism and, concerning the terms or predicates of the language, concludes:

> The result of our analysis is that the class of observable thing-predicates is a sufficient reduction basis for the whole of the language of science, including the cognitive part of the everyday language ([1], p. 60).

Concerning the unity of laws, Carnap reaches a negative but optimistic conclusion--optimistic in the sense that the reducibility of the laws of one science to another has not been shown to be impossible. Here is what he has to say on the reduction of biological to physical laws:

> There is a common language to which both the biological and the physical laws belong so that they can be logically compared and connected. We can ask whether or not a certain biological law is compatible with the system of physical laws, and whether or not it is derivable from them. But the answer to these questions cannot be inferred from the reducibility of the terms. At the present state of the development of science, it is certainly not possible to derive the biological laws from the physical ones. Some philosophers believe that such a derivation is forever impossible because of the very nature of the two fields. But the proofs attempted so far for this thesis are certainly insufficient ([1], p. 60).

Later he has the same sort of thing to say about the reduction of psychology or other social sciences to biology.

A different and less linguistic approach is to contrast the unity of scientific subject matter with the unity of scientific method. Many would agree that different sciences have different subject matters; for example, in no real sense is the subject matter of astronomy the same as that of psychopharmacology. But many would affirm that in spite of the radically different subject matters of science there are important ways in which the methods of science are the same in every domain of investigation. The most obvious and simple examples immediately come to mind. There is not one arithmetic for psychological theories of motivation and another for cosmological theories of the universe. More generally, there are not different theories of the differential and integral calculus or of partial differential equations or of probability theory.

There is a great mass of mathematical methods and results that are available for use in all domains of science and that are, in fact, quite widely used in very different parts of science. There is a plausible prima facie case for the unity of science in terms of unity of scientific method. This may be one of the most reasonable meanings to be attached to any central thesis about the unity of science. However, I shall be negative even about this thesis in the sequel.

2. Unity and Reductionism

What I have said earlier about different sciences having obviously different subject matters was said too hastily because there is a historically important sense of unity. One form or another of reductionism has been central to the discussion of unity of science for a very long time. I concentrate on three such forms: reduction of language, reduction of subject matter, and reduction of method.

2.1. Reduction of Language

Carnap's views about the reduction of the language of science to commonsense language about physical objects remain appealing. He states his general thesis in such a way that no strong claims about the reduction of psychology to physics, for example, are implied, and I am sure much is correct about what he has had to say. On the other hand, it seems appropriate to emphasize the very clear senses in which there is no reduction of language. The reduction certainly does not take place in practice, and it may be rightly claimed that the reduction in theory remains in a hopelessly vague state.

There are many ways to illustrate the basis for my skepticism about any serious reduction of language. Part of my thesis about the plurality of science is that the languages of the different branches of science are diverging rather than converging as they become increasingly technical. Let me begin with a personal example. My daughter Patricia is taking a PhD in neurophysiology, and she recently gave me a subscription to what is supposed to be an expository journal, entitled Neurosciences: Research Program Bulletin. After several efforts at reading this journal, I have reached the conclusion that the exposition is only for those in nearby disciplines. I quote one passage from an issue [6] dealing with neuron-target cell interactions.

> The above studies define the anterograde transsynaptic regulation of adrenergic ontogeny. Black and co-workers (1972b) have also demonstrated that postsynaptic neurons regulate presynaptic development through a retrograde process. During the course of maturation, presynaptic ChAc activity increased 30- to 40-fold (Figure 19), and this rise paralleled the formation of ganglionic synapses (Figure 20). If postsynaptic adrenergic neurons in neonatal rats were chemically destroyed with 6-hydroxydopamine (Figure 24) or immunologically destroyed with antiserum to NGF (Figure 25), the normal development of presynaptic ChAc activity was prevented. These data, viewed in conjunction with the anterograde regulation

> studies, lead to the conclusion that there is a bidirectional flow of regulatory information at the synapse during development ([6], p. 253).

This is by no means the least intelligible passage. It seems to me it illustrates the cognitive facts of life. The sciences are diverging and there is no reason to think that any kind of convergence will ever occur. Moreover, this divergence is not something of recent origin. It has been present for a long time in that oldest of quantitative sciences, astronomy, and it is now increasingly present throughout all branches of science.

There is another point I want to raise in opposition to a claim made by some philosophers and philosophically minded physicists. Some persons have held that in the physical sciences at least, substantial theoretical unification can be expected in the future and, with this unification, a unification of the theoretical language of the physical sciences, thereby simplifying the cognitive problem of understanding various domains. I have skepticism about this thesis that I shall explain later, but at this point I wish to emphasize that it takes care of only a small part of the difficulties. It is the experimental language of the physical sciences as well as of the other sciences that is difficult to understand, much more so for the outsider than the theoretical language. There is, I believe, no comparison in the cognitive difficulty for a philosopher of reading theoretical articles in quantum mechanics and reading current experimental articles in any developed branch of physics. The experimental literature is simply impossible to penetrate without a major learning effort. There are reasons for this impenetrability that I shall not attempt to go into on this occasion but stipulate to let stand as a fact.

Personally I applaud the divergence of language in science and find in it no grounds for skepticism or pessimism about the continued growth of science. The irreducible pluralism of languages of science is as desirable a feature as is the irreducible plurality of political views in a democracy.

2.2. Reduction of Subject Matter

At least since the time of Democritus in the 5th century B.C., strong and attractive theses about the reduction of all phenomena to atoms in motion have been set forth. Because of the striking scientific successes of the atomic theory of matter since the beginning of the 19th century, this theory has dominated the views of plain men and philosophers alike. In one sense, it is difficult to deny that everything in the universe is nothing but some particular swarm of particles. Of course, as we move into the latter part of the 20th century, we recognize this fantasy for what it is. We are no longer clear about what we mean by particles or even if the concept as originally stated is anywhere near the mark. The universe is indeed made of something but we are vastly ignorant of what that something is. The more we probe, the more it seems that the kind of simple and orderly view advanced as part of ancient atomism and that seemed so near realization toward the end of the 19th century is ever further from being a true description. To reverse the phrase used earlier, it is not swarms of particles that things are made of, but particles that are made of swarms. There are still physicists about who hold that we will one day find the ultimate simples out of which all other things are made, but as such claims have been continually revised and as the complexity of high-energy physics and elementary particle theory has increased, there seems little reason that we shall ever again be able to seriously believe in the strong sense of reduction that Democritus so attractively formulated.

To put the matter in a skeptical fashion, we cannot have a reduction of subject matter to the ultimate physical entities because we do not know what those entities are. I have on another occasion [8] expressed my reasons for holding that Aristotle's theory of matter may be sounder and more sensible than the kind of simpleminded atomistic reductionist views dominating our thinking about the physical world for 200 years.

There is another appealing argument against reduction of subject matter in the physical sense that does not rest on the controversy about the status of mental events but on what has happened in the development of computers. Perhaps for the first time we have become fully and completely aware that the same cognitive structures can be realized in physically radically different ways. I have in mind the fact that we now have computers that are built on quite different physical principles; for example, old computers using vacuum tubes and modern computers using semiconductors can execute exactly the same programs and can perform exactly the same tasks. The differences in physical properties are striking between these two generations of computers. They stand in sharp contrast to different generations of animal species, which have very similar physical constitutions but which may have very different cultural histories. It has often been remarked upon that men of quite similar constitutions can have quite different thoughts. The computer case stands this argument on its head--it is not that the hardware is the same and the software different but rather that the hardware is radically different and the software of thoughts the same. Reduction in this situation, below the level of the concepts of information processing, seems wholly uninteresting and barren. Reduction to physical concepts is not only impractical but also theoretically empty.

The same kinds of arguments against reductionism of subject matter can be found even within physics. A familiar example is the currently accepted view that it is hopeless to try to solve the problems of quantum chemistry by applying the fundamental laws of quantum mechanics. It is hopeless in the same way that it is hopeless to program a computer to play the perfect chess game by always looking ahead to all possible future moves. The combinatorial explosion is so drastic and so overwhelming that theoretical arguments can be given that not only now but also in the future it will be impossible by direct computation to reduce the problems of quantum chemistry to problems of ordinary quantum mechanics. Quantum chemistry, in spite of its proximity to quantum mechanics, is and will remain an essentially autonomous discipline. At the level of computability, reduction is not only practically impossible but theoretically so as well.

An impressive substantive example of reduction is the reduction of large parts of mathematics to set theory. But even here, the reduction to a single subject matter of different parts of mathematics has a kind of barren formality about it. It is not that the fact of the reduction is conceptually uninteresting but rather that it has limited interest and does not say much about many aspects of mathematics. Mathematics, like science, is made up of many different subdisciplines, each going its own way and each primarily sensitive to the nuances of its own subject matter. Moreover, as we have reached for a deeper understanding of the foundations of mathematics we have come to realize that the foundations are not to be built on a bedrock of certainty but that, in many ways, developed parts of mathematics are much better understood than the foundations themselves. As in the case of physics, an effort of reduction is now an effort of reduction to we know not what.

In many ways a more significant mathematical example is the reduction of computational mathematics to computability by Turing machines, but as in the case of set theory, the reduction is irrelevant to most computational problems of theoretical or practical interest.

2.3. Reduction of Method

As I remarked earlier, many philosophers and scientists would claim that there is an important sense in which the methods of science are the same in every domain of investigation. Some aspects of this sense of unity, as I also noted, are well recognized and indisputable. The common use of elementary mathematics and the common teaching of elementary mathematical methods for application in all domains of science can scarcely be denied. But it seems to me it is now important to emphasize the plurality of methods and the vast difference in methodology of different parts of science. The use of elementary mathematics--and I emphasize elementary because almost all applications of mathematics in science are elementary from a mathematical standpoint--as well as the use of certain elementary statistical methods does not go very far toward characterizing the methodology of any particular branch of science. As I have emphasized earlier, it is especially the experimental methods of different branches of science that have radically different form. It is no exaggeration to say that the handbooks of experimental method for one discipline are generally unreadable by experts in another discipline (the definition of "discipline" can here be quite narrow). Physicists working in solid-state physics cannot intelligibly read the detailed accounts of method in other parts of physics. This is true even of less developed sciences like psychology. Physiological psychologists use a set of experimental methods that are foreign to psychologists specializing, for example, in educational test theory, and correspondingly the intricate details of the methodology of test construction will be unknown to almost any physiological psychologist.

Even within the narrow domain of statistical methods, different disciplines have different statistical approaches to their particular subject matters. The statistical tools of psychologists are in general quite different from those of economists. Moreover, within a single broad discipline like physics, there are in different areas great variations in the use of statistical methods, a fact that has been well documented by Paul Humphreys [2].

The unity of science arose to a fair degree as a rallying cry of philosophers trying to overcome the heavy weight of 19th-century German idealism. A half century later the picture looks very different. The period since the Encyclopedia of Unified Science first appeared has been the era of greatest development and expansion of science in the history of thought. The massive enterprise of science no longer needs any philosophical shoring up to protect it from errant philosophical views. The rallying cry of unity followed by three cheers for reductionism should now be replaced by a patient examination of the many ways in which different sciences differ in language, subject matter, and method, as well as by synoptic views of the ways in which they are alike.

Related to unity and reduction are the two long-standing themes of certainty of knowledge and completeness of science. In making my case for the plurality of science, I want to say something about both of these unsupported dogmas.

3. The Search for Certainty

From Descartes to Russell, a central theme of modern philosophy has been the setting forth of methods by which certainty of knowledge can be achieved. The repeatedly stated intention has been to find a basis that is, on the one hand, certain and, on the other hand, adequate for the remaining superstructure of knowledge, including science. The introduction of the concept of sense data and the history of the use of this concept have dominated the search for certainty in knowledge, especially in the empirical tradition, as an alternative to direct rational knowledge of the universe.

All of us can applaud the criticism of rationalism and the justifiable concern not to accept the possibility of direct knowledge of the world without experience. But it was clearly in a desire to compete with the kind of foundation that rationalism offered that the mistaken additional step was taken of attempting to ground knowledge and experience in a way that guaranteed certainty for the results. The reduction of the analysis of experience to sense data is itself one of the grand and futile themes of reductionism, in this case largely driven by the quest for certainty. Although it is not appropriate to pursue the larger epistemological issues involved, I would like to consider some particular issues of certainty that have been important in the development of modern scientific methods.

3.1. Errors of Measurement

With the development of scientific methodology and probability theory in the 18th century, it was recognized that not only did errors in measurement arise but also that a systematic theory of these errors could be given. Fundamental memoirs on the subject were written by Simpson, Lagrange, Laplace, and others. For our purposes, what is important about these memoirs is that there was no examination of the question of the existence or nonexistence of an exact value for the quantity being measured. It was implicit in these 18th-century developments, as it was implicit in Laplace's entire theory of probability, that probabilistic considerations, including errors, arise from ignorance of true causes and that the physical universe is so constituted that in principle we should be able to achieve the exact true value of any measurable physical quantities. Throughout the 19th century it was implicit that it was simply a matter of tedious and time-consuming effort to refine the measured values of any quantity one more significant digit. Nothing fundamental stood in the way of making such a refinement. It is a curious and conceptually interesting fact that, so far as I know, no one in this period enunciated the thesis that this was all a mistake, that there were continual random fluctuations in all continuous real quantities, and that the concept of an exact value had no clear meaning.

The development of quantum mechanics in this century made physicists reluctantly but conclusively recognize that it did not make sense to claim that any physical quantity could be measured with arbitrary precision in conjunction with the simultaneous measurement of other related physical quantities. It was recognized that the inability to make exact measurement is not due to technological inadequacies of measuring equipment but is central to the fundamental theory itself.

Even within the framework of quantum mechanics, however, there has tended to be a large conceptual equivocation on the nature of uncertainty. On the one hand, the claim has been that interference from the measuring apparatus makes uncertainty a necessary consequence. In this context some aspects of uncertainty need to be noted. It is not surprising that if we measure human beings at different times and places we expect to get different measurements of height and weight. But in the case of quantum mechanics what is surprising is that variation is found in particles submitted to "identical" experimental preparations. Once again a thesis of simplicity and unity is at work. Electrons should differ only in numerical identity, not in any of their properties. And if this is not true of electrons, there should be finer particles discoverable that do satisfy such a principle of identity.

The other view, and the sounder one in my judgment, is that random fluctuations are an intrinsic part of the behavior of microscopic phenomena. No process of measurement is needed to generate these fluctuations; they are a part of nature and lead to a natural view of the impossibility of obtaining results of arbitrary precision about microscopic physical quantities.

If we examine the status of theory and experiments in other domains of science, it seems to me that similar claims about the absence of certainty can be made. The thrust for certainty associated with classical physics, British empiricism, and Kantian idealism is now spent.

4. The Search for Completeness

Views about the unity of science, coupled with views about the reduction of knowledge to an epistemologically certain basis like that of sense data, are often accompanied by an implicit doctrine of completeness. Such a doctrine is often expressed by assumptions about the uniformity of nature and assumptions about the universe being ultimately totally ordered and consequently fully knowable in character. Unity, certainty, and completeness can easily be put together to produce a delightful philosophical fantasy.

In considering problems of completeness, I begin with logic and mathematics but have as my main focus the subsequent discussion of the empirical sciences.

4.1. Logical Completeness

Logic is the one area of experience in which a really satisfactory theory of completeness has been developed. The facts are too familiar to require a detailed review. The fundamental result is Gödel's completeness theorem that in first-order logic a formula is universally valid if and only if it is logically provable. Thus, our apparatus of logical derivation is adequate to the task of deriving any valid logical formula, that is, any logical truth. What we have in first-order logic is a happy match of syntax and semantics.

On the other hand, as Kreisel has emphasized in numerous publications (e.g., [4]), this match of syntax and semantics is not used in the proof of logical theorems. Rather, general set-theoretical and topological methods are continually drawn upon. One reason is that proofs given in the syntax of elementary logic are psychologically opaque and therefore in nontrivial cases easily subject to error. Another is that it is not a natural setting for studying the relation of objects that are the focus of the theory to other related objects; as an example, even the numerical representation theorem for simple orderings cannot be proved in first-order fashion. Completeness of elementary logic is of some conceptual interest, but from a practical mathematical standpoint useless.

4.2. Incompleteness of Arithmetic

The most famous incompleteness result occurs at an elementary level, namely, at the level of arithmetic or elementary number theory. In broad conceptual terms, Gödel's result shows that any formal system whose language is rich enough to represent a minimum of arithmetic is incomplete. A much earlier and historically important incompleteness result was the following.

4.3. Incompleteness of Geometric Constructions

The three classical construction problems that the ancient Greeks could not solve by elementary means were those of trisecting an angle, doubling a cube, and squaring a circle. It was not until the 19th century that these constructions were shown to be impossible by elementary means, thereby establishing a conceptually important incompleteness result for elementary geometry.

4.4. Incompleteness of Set Theory

In the latter part of the 19th century, on the basis of the work of Frege in one direction and Cantor in another, it seemed that the theory of sets or classes was the natural framework within which to construct the rest of mathematics. Research in the 20th century on the foundations of set theory, some of it recent, has shown that there is a disturbing sense of incompleteness in set theory, when formulated as a first-order theory. The continuum hypothesis as well as the axiom of choice is independent of other principles of set theory, and, as in the case of geometry, a variety of set theories can be constructed, at least first-order set theories.

The continuum hypothesis, for example, is decidable in second-order set theory, but we do not yet know in which way, that is, as true or false. Thus there is clearly less freedom for variation in second-order set theory, but also at present much less clarity about its structure. The results of these various investigations show unequivocally that the hope for some simple and complete foundation of mathematics is not likely to be attained.

4.5. Theories with Standard Formalization

The modern logical sense of completeness for theories with standard formalization, that is, theories formalized within first-order logic, provides a sharp and definite concept that did not exist in the past. Recall that the characterization of completeness in this context is that a theory is complete if and only if every sentence of the theory is either valid in the theory or inconsistent with the theory--that is, its negation is valid in the theory.

Back of this well-defined logical notion is a long history of discussions in physics that are vaguer and less sharply formulated but that have a similar intuitive content.

4.6. Kant's Sense of Completeness

Although there is no time here to examine this history, it is worth mentioning the high point of its expression as found in Kant's _Metaphysical Foundations of Natural Science_. Kant's claim is not for the completeness of physics but for the completeness of the metaphysical foundations of physics. After giving the reason that it is desirable to separate heterogeneous principles in order to locate errors and confusions, he gives as the second reason the argument concerning completeness.

> There may serve as a second ground for recommending this procedure the fact that in all that is called metaphysics the absolute completeness of the sciences may be hoped for, which is of such a sort as can be promised in no other kind of cognitions; and therefore just as in the metaphysics of nature in general, so here also the completeness of the metaphysics of corporeal nature may be confidently expected... .
>
> The schema for the completeness of a metaphysical system, whether of nature in general or of corporeal nature in particular, is the table of the categories. For there are no more pure concepts of the understanding, which can concern the nature of things. ([3], pp. 10-11).

It need scarcely be said that Kant's argument in terms of the table of the categories scarcely satisfied 18th-century mathematical standards, let alone modern ones. His argument for completeness was not subtle, but his explicit focus on the issue of completeness was important and original.

4.7. The Unified Field Theory

After Kant, there was important system building in physics during the 19th century, and there were attempts by Kelvin, Maxwell, and others to reduce all known physical phenomena to mechanical models, but these attempts were not as imperialistic and forthright in spirit as Kant's. A case can be made, I think, for taking Einstein's general theory of relativity, especially the attempt at a unified field theory, as the real successor to Kant in the attempt to obtain completeness. I do not want to make the parallel between Kant and Einstein too close, however, for Einstein does not hold an a priori metaphysical view of the foundations of physics. What they do share is a strong search for completeness of theory. Einstein's goal was to find a unified field theory defining one common structure from which all forces of nature could be derived. In the grand version of the scheme, for given boundary conditions, the differential equations would have a unique solution for the entire universe, and all physical phenomena would be encompassed within the theory. The geometrodynamics of John Wheeler and his collaborators is the most recent version of the Einstein vision. Wheeler, especially, formulates the problem in a way that is reminiscent of Descartes: "Are fields and particles foreign entities immersed <u>in</u> geometry, or are they nothing <u>but</u> geometry?" ([9], p. 361).

Had the program of Einstein and the later program of Wheeler been carried to completion, my advocacy of skepticism toward the problem of completeness in empirical science would have to retreat from bold assertion of inevitable incompleteness. However, it seems to me that there is, at least in the current scientific temperament, total support for the thesis of incompleteness. Grand building of theories has currently gone out of fashion in fields as far apart as physics and sociology, and there seems to be a deeper appreciation of the problems of ever settling, in any definitive way, the fundamental laws of complex phenomena.

As the examples I have mentioned--and many others that I have not--demonstrate, in most areas of knowledge it is too much to expect theories to have a strong form of completeness. What we have learned to live with in practice is an appropriate form of completeness, but we have not built this working practice explicitly into our philosophy as thoroughly as we might. It is apparent from various examples that weak forms of completeness may be expected for theories about restricted areas of experience. It seems wholly inappropriate, unlikely, and, in many ways, absurd to expect theories that cover large areas of experience, or, in the most grandiose cases, all of experience, to have a strong degree of completeness.

The application of working scientific theories to particular areas of experience is almost always schematic and highly approximate in character. Whether we are predicting the behavior of elementary particles, the weather, or international trade--any phenomenon, in fact, that has a reasonable degree of complexity--we can hope only to encompass a restricted part of the phenomenon.

It is sometimes said that it is exactly the role of experimentation to isolate particular fragments of experience that can be dealt with in relatively complete fashion. This is, I think, more a dogma of philosophers who have not engaged in much experimentation than it is of practicing experimental scientists. When involved in experimentation, I have been struck by how much my schematic views of theories also apply to experimental work. First one concrete thing and then another is abstracted and simplified to make the data fit within the limited set of concepts of the theory being tested.[2]

Let me put the matter another way. A common philosophical conception of science is that it is an ever closer approximation to a set of eternal truths that hold always and everywhere. Such a conception of science can be traced from Plato through Aristotle and onward to Descartes, Kant, and more recent philosophers, and this account has no doubt been accepted by many scientists as well. It is my own view that a much better case can be made for the kind of instrumental conception of science set forth in general terms by Peirce, Dewey, and their successors. In this view, scientific activity is perpetual problem solving. No area of experience is totally and completely settled by providing a set of basic truths; but rather, we are continually confronted with new situations and new problems, and we bring to these problems and situations a potpourri of scientific methods, techniques, and concepts, which in many cases we have learned to use with great facility.

The concept of objective truth does not directly disappear in such a view of science, but what we might call the cosmological or global view of truth is looked at with skepticism just as is a global or cosmological view of completeness. Like our own lives and endeavors, scientific theories are local and are designed to meet a given set of problems. As new problems arise new theories are needed, and in almost all cases the theories used for the old set of problems have not been tested to the fullest extent feasible nor been confirmed as broadly or as deeply as possible, but the time is ripe for something new, and we move on to something else. Again this conception of science does not mean that there cannot be continued correction in a sequence of theories meeting a particular sequence of problems; but it does urge that the sequence does not necessarily converge. In fact, to express the kind of incompleteness I am after, we can even make the strong assumption that in many domains of experience the scientific theory that replaces the best old theory is always an improvement, and therefore we have a kind of monotone increasing sequence. Nonetheless, as in the case of a strictly monotone increasing sequence of integers, there is no convergence to a finite value--the sequence is never completed--and so it is with scientific theories. There is no bounded fixed result toward which we are converging or that we can hope ever to achieve. Scientific knowledge, like the rest of our knowledge, will forever remain pluralistic and highly schematic in character.

Notes

[1] I am debted to Georg Kreisel for a number of penetrating criticisms of the first draft of this paper.

[2] This idea is developed in some detail in Suppes [7].

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