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BOUNDARIES AND BARRIERS

On the LIMITS to Scientific KNOWLEDGE

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Chapter 8

ON THE LIMITATIONS OF SCIENTIFIC KNOWLEDGE

Robert Rosen

There are no impregnable fortresses. There are only fortresses that are badly attacked – Les Liaisons Dangereuses

I. Introduction

I do not consider myself a philosopher. I am a biologist, attempting to grapple with the Schrödinger question, "What is Life?" It turns out that this is not an empirical question, to be resolved through observation in a laboratory. Hence I am a theoretical biologist, not a "practical" one. Many of my experimentalist colleagues, who tacitly feel that science is consubstantial with laboratory practice, accordingly reject the question itself as unscientific, and it does not bother them that they cannot answer it.

I believe, on the other hand, that the problem is not only perfectly scientific, but that it is central. I also believe that it is in some sense solvable. Thus, I have developed a different view of what "science" is and what it is about than most of my colleagues. I have had to spend much time in exploring the capabilities and the limitations of many methods which have been proposed to give an answer to the question, but which do not in fact do so. These methods all proceed by replacing the "real world" by one or another artificially circumscribed one, and by regarding "scientific" knowledge as what happens in that surrogate universe. Invariably, that surrogate turns out to be "too small" to accommodate organisms. In particular, this is true of the naïve and optimistic Reductionisms that have been asserted to answer the question, but which do not. In pursuing this process, I have acquired a great deal of "practical" experience about "limitations."

To me, the limitations of a method, or its inapplicability to a particular problem constitute neither a limitation of science, nor of the human mind. Nor are they inherent restrictions on the nature of the world. They mostly arise from replacing the "real world" by a small surrogate universe, a replacement that is made independently of a particular problem's exigencies. As we shall argue below, it is the essence of the *subjective* to do this.

In particular, Reductionisms in biology (and elsewhere) are adopted not because they answer the question, "What is life?", but because they emulate a method that has (sometimes) worked in dealing with inanimate nature. Reductionisms give us a small surrogate universe, consisting roughly of systems whose properties can be exhausted entirely on the basis of those of special subsystems that can be "fractionated" away from it, and studied entirely *in vitro*. These isolated fractions themselves are held to constitute a surrogate for the original system—indeed, they are the only kind of surrogate that is scientifically admissible. There are many reasons for wishing to believe that organisms fall into this class. But these reasons are irrelevant; organisms are outside such classes. I have used the word *complexity* (cf. Rosen 1991) to describe this situation. But complexity does not put organisms outside the pale of science, nor is there anything "vitalistic" about it. It is, as I have noted, simply the failure of a small surrogate universe to exhaust the real one. As such, it is essentially a mistake; an equivocation. It creates *artifacts,* as do all equivocations.

In short, a proposition like "all systems are simple (fractionable or reducible)" is not itself to be identified with "scientific knowledge." And, accordingly, failure of reductionism as a method, in the sense that there are problems to which that method does not apply, is not a limitation on science, or on scientific knowledge. Quite the contrary; we learn something basic and new about the world when it fails to be exhausted by a posited method.

II. What is "Scientific"?

To even address the question of whether or not there are "limitations of scientific knowledge," we must have an assessment of that qualifying adjective, *scientific*. It is already not so easy to characterize, or to reduce to others.

The earliest attempt to do so, or at least which I know of, goes back to Aristotle. He claimed that it was the entire job of science to account for "the why of things." This, in turn, led him to his doctrine of *causality,* in which he identified "scientific knowledge" about something

(call it X) with all the ways of answering the question "Why X?" If we can say "X because Y," or that "Y is at least a necessary condition for the effect X," then this is the kind of assertion that belongs to science (though of course such an assertion may still be true or false).

Another way of saying this is that an effect X is entailed by its causes Y, i.e., by answers to question "Why X?" Aristotle further tacitly assumed that the totality of these answers constitute a *sufficient* condition for the effect X. Thus, for Aristotle, the natural world constituted a web of (at least) such causal entailments that it was the job of science to illuminate. According to this criterion, it is clear what constitutes "scientific knowledge." What it has nothing to do with is the means adopted to answer a question "Why X?", or with characteristics like "objectivity"; these came much later.

Partly because this Aristotelian picture of science was independent of a stipulated method for obtaining answers to such "Why?" questions, and especially because it did not focus on empirical or observational procedures (which can rarely answer "Why?" questions, in any case), that picture of science has been slowly abandoned over the past few hundred years. Indeed, the Aristotelian view, in which science is content-determined in terms of the kinds of questions it must answer, has been replaced by method-based procedures. That is, something belongs to science according to how it was obtained, not by what it is about. This constitutes a truly massive shift in outlook. Indeed, as a result of it, the question as to what is "scientific knowledge" shifts from a semantic one to one of "scientific method." In particular, "limits to scientific knowledge" has shifted from something content-based to something quite different: the adequacy of an admissible methodology.

In the sections below, we shall argue that most (if not all) questions surrounding "limits of scientific knowledge" pertain primarily to the in-equivalences between the two ways we have sketched of characterizing science itself. As we have seen, the older Aristotelian view pertained to content rather than method; it consisted of answers to questions; to *information*. The second, more modern view, pertains not to content so much as to *method;* to process. The two pictures of science do not coincide. Still worse, there has never been any real consensus as to what methods are to be allowed as unquestionably producing "scientific knowledge."

III. About "Objectivity"

Probably the first thing one would say if asked to characterize "scientific knowledge" as opposed to other kinds of knowledge, is that it is *objective*.

By this is generally meant that the knowledge in question pertains to its object or referent alone, devoid of any information about how or when it was obtained.

The concept of "objectivity" already appears in the Aristotelian vision of science; it means that none of the "why?" questions about an effect X are answered by the processes involved in answering them. In more modern terminology, this asserts that an observation process, or an observer, plays no causal role in entailing what is observed.

However, the situation in Aristotle is confused by the presence of a causal category that he allows, but which has come to be rejected by every subsequent method-based view of what science is. That category is *finality*. Indeed, final causation has for a long time been regarded as the quintessence of subjectivity, and thus incompatible from the outset with science itself.

However, finality has no such connotation in the Aristotelian picture. In that picture, the concept of objectivity remains, even if final causation of an effect X is allowed; it is exactly as stated above: that X is simply not entailed by the process of answering "Why X?" That is, X may well have a final cause that entails it, but it is simply not the process of answering the question in that fashion. And the fact that X has a final cause can itself be perfectly objective, according to these criteria.

Nevertheless, the perceived need to exclude finality completely, in the name of preserving "objectivity," has been carried to ludicrous lengths in biology. For one thing, many have felt it necessary to do away with the concept of "function" entirely, one of the central concepts about organization, in general, on the grounds that it is finalistic and hence not "objective." It is astounding to watch adult physiologists twisting themselves into bizarre shapes to avoid saying things like, "the function of the heart is to pump blood." Carried still further, it is considered illegitimate for science to seek to understand anything about a part or subsystem in terms of a *larger* system or an environment with which it is interacting; hence, the Reductionistic idea that one must only seek to understand larger wholes in terms of "objective," context-independent parts, never the reverse.

In molecular biology, Jacques Monod is one of the few to actually commit to paper the frank conceptual processes at the root of it. In 1972, he states quite explicitly:

> The cornerstone of the scientific method is the postulate that nature is objective. In other words, the systematic denial that "true" knowledge can be got at by interpreting phenomena in terms of final causes—that is to say, of "purpose" ... science as we understand it today could not have been developed ... (without) the unbending stricture implicit in the postulate of objectivity—ironclad, pure, forever indemonstrable ... . The postulate of objectivity is consubstantial with science; it has guided the whole of its prodigious development for three centuries. There is no way to be rid of it, even tentatively or in a limited area, without departing from the domain of science itself.

In this passage, we see clearly the identification of "science" with a method, independent of any question, and the simultaneous (if subliminal) elevation of that method itself to a restriction on the material world (*"NATURE is objective"*). One need only ask if Monod's "Postulate of Objectivity" itself constitutes "scientific knowledge," and if so, about what? Indeed, to me, it is the "Postulate of Objectivity" itself that is subjective; the embodiment of a frank and clear intentionality, elevated to the status of a Natural Law. We shall have more to say about this, in a variety of contexts, as we proceed.

For the moment, let us retreat back to our original view of "objective knowledge," in the sense that acquiring it plays no role in its entailment. As we have seen, this has nothing to do with finality. But it does seem to imply a sharp dividing line between "objective knowledge" and other knowledge ("subjective knowledge"); a line based in entailment. As noted above, "subjective knowledge" cannot be entirely causally separated from the process by which it is acquired. Aristotle makes no value judgment on this division; he does not restrict or identify his view of "science" to just the "objective." This only came much later. But is there indeed such a line at all? Such a line would constitute, even for Aristotle, inherent limits to "objective knowledge." This is a deep question, which we shall discuss from a number of different angles in what follows.

IV. The Bohr-Einstein Debates

As I have argued elsewhere (e.g., Rosen 1993), physics strives, at least, to restrict itself to "objectivities." It thus presumes a rigid separation between what is objective, and thus falls directly within its precincts, and what is not. Its opinion about whatever is outside these precincts is divided. Some believe that whatever is outside is so because of removable and impermanent technical issues of formulation; i.e., whatever is outside can be "reduced" to what is already inside. Others believe the separation is absolute and irrevocable.

In either case, physics chooses a surrogate universe, bounded by criteria of "objectivity," and substitutes it for the "real" one. I will repeat here a citation (Bergman 1973) on this matter, which I have already used in *loc. cit.*:

> [Max] Planck designated in an excellent way ... the goal of physics as the complete separation of the world from the individuality of the structuring mind; i.e., the emancipation of anthropomorphic elements. That means: it is the task of physics to build a world which is foreign to consciousness, and in which consciousness is obliterated.

However interpreted, this is the "objective" world, which physics claims exclusively for itself, and which many physicists identify with "science." Anything outside can either be pulled inside ("reduction"), and hence becomes scientific as a special case of physics, or else cannot be regarded as belonging to science at all. But as we have already seen, it is already not clear whether "objectivity" is to be determined by content (the Aristotelian view) or by adherence to a method. In any case, we seem to have come a long way from science as "the why of things."

The Bohr-Einstein debates about complementarity, which occurred in the shadow of the "new" quantum theory, were precisely about what "objectivity" meant, and hence what physics itself was about. It was thus about how small (or how large) a surrogate universe it provides. A good reference about the substance of these debates are the volumes produced by Abraham Pais (1982, 1991, 1994), although Pais himself offers the opinion that, "I do not think these discussions affected the progress of physics in any way." In some trivial sense Pais is no doubt right about this (e.g., in the sense that most physicists find such issues irrelevant to what they do every day). But so much the worse for most physicists.

Both Bohr and Einstein were troubled (though in separate ways) by the wave-particle duality, revived by Einstein himself in 1905 via the photoelectric effect and his invocation of the *photon.* For over a century, it had seemed clear that light consisted of waves, on the basis of firm experimental evidence based on interference. However, beginning with the photoelectric effect, and growing ever greater as the 20th century progressed, an equally impressive body of experimental work could only be reconciled with a particulate (quantum) interpretation, and excluded waves. Indeed, some of Einstein's other work (special relativity) dispensed with the hypothetical medium *(ether)* in which waves of light had to propagate. The apparently irreconcilable wave-particle pictures, elevated to a universal status by de Broglie around 1925, paved the way for the wave mechanics of Schrödinger, and in a more roundabout way, for the matrix mechanics of Heisenberg, based on uncertainties and noncommutativities.

Bohr, who became in a sense the spokesman for new quantum theory and its interpretation through the next decades, proceeded to develop his views on complementarity to cope with the apparent contradictions between waves and particles. His position was somewhat analogous to Brouwer's position ("intuitionism") in the foundations of mathematics (an issue we will explore further subsequently). Namely, that the logical problems apparently manifested by mutually incompatible pictures really arise from trying to extrapolate classical ideas to quantum-theoretic situations. Even the idea of "mutually incompatible" is a classical idea (Brouwer said basically the same thing, in connection with the logical Law of the Excluded Middle, which held in finite realms, but not generally in infinite ones).

In particular, Bohrian complementarity argued that whether we see light as a particle or a wave was not inherent in light alone, but also depended on the way we measure or observe it. Classically, we could ignore the measurement procedure, and look at the outcome of a procedure as inherent in the light alone. But not in quantum theory. The "true" system to which the observation pertains, in a quantum realm, is the complex consisting partially of the light, but equally significant, of the apparatus or procedure that observes it.

Einstein could not accept this. He argued that something "belongs to reality" (i.e., is objective) only if it is independent of such things as how it is measured or observed. In particular, for Einstein, complementarity violates the stricture that we must not explain or describe a "real" system in terms of a particular larger one to which it might belong. This, it will be noted, is essentially Monod's "Postulate of Objectivity," that one must always look downward toward subsystems, and never upwards and outwards to a larger context.

For Bohr, then, the "objective situation" requires the specification of a means of interrogating a system, and not just the system being interrogated. The outcome of an "experiment" pertains to this larger composite system, and cannot be fractionated or separated into a part pertaining only to what is observed, and a remainder which pertains only to the means by which it is obtained. Indeed, the word "classical" pertains only to situations where such a fractionation can always be made.

So we can see that the Einstein-Bohr debates over complementary were really about what science (specifically, physics) is ultimately about; how (or even whether) objective knowledge, independent of an "organizing mind," and in which "consciousness is obliterated," can be obtained. Einstein believed that there was such knowledge, immanent alone in a thing, and independent of how that knowledge was elicited. Bohr regarded that view as "classical," incompatible with quantum views of reality, which always required specification of a context, and always containing unfractionable information about that context.

Actually, the debate over complementarity spreads far wider; it has profound causal correlates. As we saw above, Aristotelian causality involved dealing with questions or interrogatives of the form "Why X?" Bohr is in effect proposing that the variable X here is an inseparable pair, consisting of something being observed, and a procedure for describing or observing it. He is thus asserting that the original Aristotelian question, "Why X?", referring to X alone, apart from such a specific context, is ultimately meaningless, or at best can be given such a meaning only in limited "classical" realms.

In terms of surrogacy, Bohrian complementarity asserts that the classical world is too small, in itself, to provide a single coherent picture of material reality. Complementarity was his suggestion about how that world may be enlarged to accommodate quantum processes. He does so by, at root, changing the concept of "objectivity" itself from pertaining only to what is immanent entirely in a material system X to what is immanent in a system-observer pair (F, X); i.e, a larger system than X. Thus we get a bigger surrogate, which clearly cannot be "reduced" to the original "classical" world. Biological phenomena, however, raise the question whether this larger world, which is still method-based, is yet big enough. As I have extensively argued elsewhere, I do not think it is. But, as I have stated previously, this does not constitute a limitation on scientific knowledge; it is merely a failure or inapplicability of a method that has been substituted for science.

V. Some Mathematical Analogies

We have suggested above that the Aristotelian notion of causality, based on questions of the form "Why X?" establishes relations between material events, which it is the business of science to answer. In this view, the material world consists (to us) of a web of entailments between events X and Y, which take the form of such answers: "X because Y."

I have long argued that mathematics also consists of a web of entailments between propositions; *inferential* entailments. Thus, although the mathematical world is in a sense entirely subjective, in terms of its

subject matter (i.e., in terms of what it is *about*), it admits the *property* of objectivity. Indeed, ironically, it is often regarded as the most objective of realms—it has often been argued that mathematical truth is the highest truth, independent alike of the external world and of the mathematician. Even God could not make $2 + 2 = 3$

Systems of inferential entailments, i.e., mathematical systems, can be compared to systems of causal entailments in many ways. For one thing, such comparisons, through vehicles of encodings and decodings between them, provide the basis of modeling relations, which I have discussed at length elsewhere (cf. Rosen 1985, 1991). But at a deeper level, mathematics, as a specimen or embodiment of entailment, can tell us things about entailment itself, and hence throw a new light on the questions we have been considering. Indeed, there are exactly parallel questions, pertaining to "limitations of mathematical knowledge," which have been discussed for a long time. These constitute what is now called the "Foundations" of mathematics, and as in science (i.e., in causal entailments) are wrapped up in arguments over whether mathematics is content-based or method-based.

Thus, mathematics has undergone periodic "foundation crises," in which discrepancies between these two views have become unbearable. We are in one today, as we shall presently discuss. For now, because of its relation to the Bohr-Einstein debates over complementarity, we shall focus on an earlier one. One of its roots was in the attempt to extend arithmetic from finite to infinite realms.

The arithmetic operations, addition and multiplication, start as binary operations (between integers, say). That is, we can only give a meaning to expressions like $(a + b)$ and $(a \times c)$; uniquely determined new integers. These operations are commutative, so we can add or multiply two integers in either order and get the same answer. Products distribute over sums, so $a \times (b + c)$ is meaningful, and is the same as $(a \times b) + (a \times c)$. On the other hand, an expression like $a + b + c$ has no immediate meaning. Or rather, it has two different ones, which can be expressed as $(a + b) + c$, and $a + (b + c)$, each of which involve only binary operations. If we set these equal, i.e., impose the associative law, then we can unambiguously remove the parentheses in these expressions, and turn addition and multiplication into a single ternary operation $a + b + c$.

It is then a theorem (cf., Chevalley 1956), proved by mathematical induction, that given any *finite* set of integers $\{a_i\}$, there is a unique sum $\sum a_i$ that is independent of the way it is parenthesized; likewise, there is a unique product, and products distribute over sums in the familiar way. In other words, these unique values are independent of how they are calculated; the values appear *immanent* in their summands or factors alone; they are *objective.*

In the sixteenth century, mathematicians exuberantly began to explore what happens if this restriction of finiteness is removed, i.e., if we consider sums with an infinite number of summands, or products with an infinite number of factors. At first, in the hands of people like Euler and the Bernoullis, it seemed as if a whole new world of ineffable beauty was emerging, populated by relations like

$$1 + \frac{1}{9} + \frac{1}{25} + \cdots + \frac{1}{(2n-1)^2} + \cdots = \frac{\pi^2}{8},$$

or

$$\frac{2}{1} \times \left(\frac{4}{3}\right)^{\frac{1}{2}} \times \left(\frac{6}{5} \times \frac{8}{7}\right)^{\frac{1}{4}} \times \left(\frac{10}{9} \times \frac{12}{11} \times \frac{14}{13} \times \frac{16}{15}\right)^{\frac{1}{8}} \times \cdots = e.$$

At first, these results appeared just as "objective" as finite arithmetic. But then apparent absurdities began to creep in, slowly at first, then in a torrent. The worst of them was perhaps

$$1 - 1 + 1 - 1 + \cdots = ?,$$

which seemed to entail the ultimate absurdity, $1 = 0$.

So something was clearly wrong here, a Foundation Crisis. There seemed to be only three choices: (a) to restrict arithmetic to finite realms exclusively, where we were safe, (b) to venture into infinite realms, but to find a way to choose our factors and summands judiciously enough to retain "objectivity," or (c) to somehow enlarge the idea of "objectivity" itself.

As everyone knows, that Crisis was (temporarily) resolved by Cauchy around 1805 on the basis of (b). He introduced the notion of *convergence,* and a number of algorithmic criteria for deciding whether a given sequence or series converged. Convergence, in this sense, was a generalization of the "objectivity" automatically enjoyed in finite realms; the *value* of the limit of a convergent sequence was independent of how it was obtained, and inhered in the sequence alone. This stopgap rescued mathematical analysis as a secure discipline, and even had profound ramifications in theoretical physics, through the notion of the derivative

(velocity) as a well-defined variable, independent of how it was evaluated or measured. Without this, there could be no notion of *phase.*

A series like the last one displayed above is called *divergent,* and was regarded as meaningless; beyond the realm of mathematics itself. Such a sequence was thought of as having *no limit.* On the other hand, in a precise sense, "most" sequences thus diverge; only very special ones satisfy the Cauchy criteria. In fact, what happens with divergent sequences is that they have *many* distinct limiting values; values that depend precisely on how they are evaluated, and *not just* on the sequence alone.

Thus, a "limiting value" of a divergent sequence σ is a pair, which we may denote by (F, σ), where F is a specific evaluation process. In a sense, this is a perfectly objective number, determined by that pair. For instance, in the example of divergence given above, in which

$$\sigma = 1 - 1 + 1 - 1 + \ldots,$$

the apparent absurdity $1 = 0$ arises from not specifying F; or rather, by tacitly using two different Fs, and then equating the results, which we cannot do.

I believe the reader can immediately see the parallels between this situation and that involved in the Bohr-Einstein debates. In effect, Einstein was arguing that only convergent situations, in which limits are independent of how they are evaluated, are entitled to be called "real" or objective. It was Cauchy's position that only convergent sequences are allowable. Bohr's position was, rather, that the material world, in its quantum aspects, was like divergence; perfectly objective, but requiring further "information," pertaining to how it is evaluated, to determine a specific one of many complementary limits the sequence may have.

Let us pause to recapitulate what we have just said, since it is important. We were considering familiar operations of arithmetic, like addition, on ordinary numbers. We observed:

a. If we restrict ourselves to finite numbers of summands, a sum is uniquely determined, and independent of the way in which it is computed or evaluated. If we identify this independence with "objectivity," then this objectivity is in fact entailed by finiteness. That is, if σ denotes a finite set of summands, then it is a theorem that $F(\sigma) = G(\sigma)$, where F and G are distinct evaluation processes.
b. If we go to infinite realms, there is nothing to entail this "objectivity." It is no longer generically true that $F(\sigma) = G(\sigma)$ when σ is infinite.

c. If we generalize the "objectivity" condition $F(\sigma) = G(\sigma)$, we obtain a world of convergent sequences. This is, however, no longer a theorem, as it was before; rather, it is something *imposed,* or *presupposed,* in order to demarcate a world of infinite sequences that still manifest a property familiar from finite sums.
d. We then go on to identify the world so demarcated with "mathematics." What is left out of that world (which is, in fact, "most" sequences) is regarded as "nonobjective," since a limiting value depends on the choice of an evaluation procedure F, and not on a sequence σ alone.
e. Thus, what we call "mathematics" in this procedure becomes inherently method-dependent, and not content-dependent. As such, it has (pardon the pun) inherent limits.
f. Nevertheless, the only real subjectivity here is entirely in the decision to replace "arbitrary sequence" by "convergent sequence," and to replace a pair (F, σ) by σ alone. This choice is not mandated or entailed by anything in the content of mathematics itself. Rather, it is motivated by an intention, to retain customary habits, familiar from the finite realm, as intact as possible in the infinite one.

VI. A Word About Computability

Another good example of the phenomenon we have just described comes from our current mathematical Foundation Crisis, rather than from a previous one. I have written much about it in the past, so I will cover it quickly. Although its immediate historical roots are somewhat different, it is in fact rather closely related conceptually to the issue of convergence. It has to do with *computability,* viewed again as a generalization of finiteness.

It was long a hope, or expectation, that the entailment processes of mathematics could be equivalently replaced with word-processing algorithms. Mainly, the hope was that in such severely restricted, finitely-generated contexts, consistency of arbitrary mathematical theories, like set theory, could be *guaranteed; entailed.* This was the world of Formalism. It turned out, of course, that "most" mathematical systems were not formalizable; this was the upshot of the venerable Gödel Theorem, which has (among many other things) precipitated these workshops on "limits to scientific knowledge."

The theorem actually compares a method-based simulacrum of mathematics, based on formalizability (computability) with a content-based concept, and shows that the former is inevitably much smaller than the latter. The problem here is in a way much more acute than simply replacing "sequence" by "convergent sequence," as Cauchy did; it, in effect, sharply limits the evaluation processes (what we called F before) which allows limits of sequences to be evaluated at all. So the formalist world is very impoverished from the outset. In fact, it is limited to what can be done entirely by the iteration of rote processes; a limitation which will not carry you from the finite to the infinite, nor back again.

In this formalistic world, "objectivity" is interpreted yet again, as entailment arising from purely syntactic rules. Roughly, something is "objective" if and only if it could be carried out by a properly programmed machine; i.e., as a matter of software and hardware. This conception spills over into science, and especially into biology, by recasting Monod's dictum ("NATURE is objective") into the still more restrictive form "NATURE is computable."

Indeed, it might be noticed that Bohrian complementarity would be a hard thing to convey to a finite-state machine. Part of the problem here is that such a machine is, in material terms, entirely a classical device, both in its hardware and in its software; all it could see of a non-classical, quantum realm is noise.

VII. On Complexity

As I remarked at the outset, my interest in the problems dealt with above is a consequence of my scientific concerns with biology, and particularly with the question, "What is life?", the central question of biology. It became clear to me that Reductionism was not the way to find an answer. Indeed, the Laplacian *Geist,* the Reductionistic ideal, would make an extremely poor biologist. So too would his quantum-mechanical analog.

Since I did not regard myself as method-bound, I explored alternatives, especially under the rubric of "relational biology" (cf. Rosen 1991). I ended up with a class of systems ((M, R)-systems), which seemed to me to be (a) perfectly "objective," but (b) fell outside the category of "mechanisms," and could not be understood in terms of Reductionistic method alone. I cannot claim that these (M, R)-systems fully answer the question "What is life?", but I do claim that the answer must at least comprehend them.

The (M, R)-systems manifest inherent semantic properties, expressed in closed causal loops within them. Such closed loops, in inferential contexts, are called *impredicativities.* It is such impredicativities that Formalism rejects as "subjective," and which scientists like Monod *(vide* *supra)* claim are inconsistent with science itself, at least as they comprehend it

At any rate, my little (M, R)-systems are inherently unformalizable as mathematical systems. That means: not only do they have noncomputable models, but any model of them that is computable is not itself an (M, R)-system, and hence misses all of its biology.

You cannot "reduce" nonformalizability or noncomputability to formalizability. That is simply a fact; it is not a limitation. Likewise, you cannot, for example, "reduce" nonexact differential forms to single (potential) functions; again, that is a fact, and not a limitation. Nor can you "reduce" divergent sequences to convergent ones. Indeed, such "reducibility" is in itself a rare and nongeneric phenomenon; that too is a fact; a fact as "objective" as anything.

I originally called a (material) system *complex* if it had noncomputable models; otherwise, *simple.* People like Monod simply postulate that every *material system is simple;* i.e., there are no complex systems in nature, and identify this postulation with science itself. As noted above, this makes science method-based, rather than content-based, and inevitably limits its content only to what is consistent with the postulated method. This has always turned out to be a Procrustean bed, which inevitably creates limits to the science (or to the mathematics) so circumscribed.

This is how I view the Gödel theorem. It exhibits "mathematics" (or at least number theory) as a profound generalization of formalizability (or, alternatively, reveals formalizability to be a most severe specialization of mathematics). Mathematical rigor (i.e., "objectivity") does not reside in finitely-based syntactic rules alone. In my previous terminology, Gödel's Theorem demonstrated the *complexity* of number theory—its irreducibility to simple formalizations. I believe that biology does the same thing for our contemporary views of physics.

Indeed, one of the upshots of Monod's "Principle of Objectivity" is that one must never claim to learn anything new about physics (i.e., about matter) from a study of organisms; or, stated another way, that an observer would see exactly the same universe, governed by the same laws, whether life existed in it or not; the difference between them is a conceptually trivial difference in *state.* A mathematical analog of this assertion is that, e.g., one must never claim to learn anything new about set theory from a study of, e.g., groups. I believe that, in mathematics at any rate, Gödel's Theorem already refutes any such claim.

Set theory is currently the mathematical version of the physicist's search for a "theory of everything"; a theory from which everything is inferentially entailed. As noted above, in earlier centuries, the physicists' dream was embodied in the Laplacian *Geist,* who could know the motion of every particle and every force; who could formulate and solve every N-body problem. He might not be able to tell the difference between a universe with life in it and one without it; but then he could not conceive of a system which was not an N-body system. The limitations to what he could know are limitations in *him,* not in the universe he perceives; and he could never even know what they are.

I suggest that we humans are more fortunate than the hypothetical Ultimate Reductionist, in our ability to perceive complexity. That is, to recognize the necessity to pull ourselves outside the limitations of self-imposed methodologies, which create nonexistent "limits" to knowledge itself.

I believe, in short, that Aristotle was more correct in his view of science as a content-based thing, than the more currently orthodox views of science as constricted by a method. If this is so, then the problem of "limits" to science evaporates into mist.

Appendix: On Emergence

As a biologist, I have been engaged in various ways in a long-standing debate concerning the scientific status of the concept of "emergent novelty." The debate itself is of very broad currency, but it is perhaps sharpest in biological contexts. It touches directly on the issues we have been discussing; moreover, it is illuminated by the remarks made above. Hence it appears worth discussing the issue separately, if briefly, in this Appendix.

In a rough intuitive sense, "emergence" pertains to situations in which a system is, in some sense, "more than the sum of its parts." Another way of saying this is that a study of "parts" does not, by itself, entail properties of the whole from which the parts have been fractionated away. In the language we have used above, the "parts" do not suffice to answer all the "Why?" questions we can ask about the whole; those parts are not sufficient to entail the whole.

One can immediately see why methods of Reductionism, based precisely on the isolation and study of such "parts," look upon concepts of emergent novelty with great hostility. They tend to denounce the concept itself as unscientific and metaphysical.

On the other hand, the whole idea of "complexity" has always been tied up with the concept of emergence. For instance, von Neumann early argued that there was a "threshold of complexity" (I would prefer to use the word "complication"), below which automata (e.g., neural networks) could only deteriorate, but above which entirely new capabilities (learning, development, growth, reproduction, evolution) emerge. These are, of course, *biological* capabilities; pushing a material system across this threshold of complexity amounts to creating life. Indeed, in this view, the emergent property of "complexity" itself becomes a causal entity, a way of answering "Why?" questions: "Why is this system alive?" Because it is "complex"; over that presumptive threshold.

In the light of these considerations, let us reconsider the example of divergent sequences introduced in Section V above, and analogized with Bohrian complementarity. Clearly, from the standpoint of finite arithmetic, divergence is itself an emergent property. The dependence of a limit of an infinite sum on the way its summands are ordered and parenthesized is something without a counterpart in finite sums, and is unpredictable from them *alone*. Or, stated another way, finite arithmetic does not provide enough entailment to answer "Why?" questions about limits of divergent sequences.

This kind of situation is characteristic of emergence itself; a paucity of entailment in a world of "parts," and hence an inability to answer "Why?"-questions about systems from which the "parts" have been isolated.

As we have already seen above, if you make no concession to the additional modes of entailment on which the emergence itself depends, you get $1 = 0$.

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Chapter 9

UNDECIDABILITY EVERYWHERE?

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I. Physics After the Incompleteness Theorems

There is incompleteness in mathematics [1–7]. That means that there does not exist any reasonable (consistent) finite formal system from which all mathematical truth is derivable; and there exists a "huge" number [8] of mathematical assertions that are independent of any particular formal system. That is, these statements—as well as their negations—are compatible with the standard formal systems of mathematics. Take, for example, the Continuum Hypothesis or the Axiom of Choice. Both are independent of the axioms of Zermelo-Fraenkel set theory.

Can such formal incompleteness be translated into physics or the natural sciences, in general? Is there some question about the nature of things that can be proved to be unknowable for rational thought? Is it conceivable that the natural phenomena, even if they occur deterministically, do not allow their complete description?

Of course it is! Suppose there exists free will. Suppose further that an observer could predict the future. Then this observer could freely decide to act in such a way as to invalidate that prediction. Hence, in order to avoid paradoxes, one has either to abandon free will or accept that perfect and complete prediction is impossible.

The above argument may appear suspiciously informal. Yet, it makes use of the diagonalization technique, which is one of the royal roads to a constructive, rational understanding of undecidability in the formal sciences. What Gödel and others did was to encode the argument in a language suitable to their area of research. To translate and bring similar issues into mainstream natural science is, at least in the author's opinion, the agenda of present concern on rational limits to scientific knowledge.

Before discussing these questions further, we should first clarify our