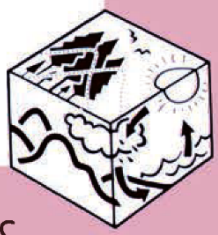
ESSAYS ON LIFE ITSELF

ROBERT ROSEN



COMPLEXITY IN ECOLOGICAL SYSTEMS

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Essays on
LIFE ITSELF

ROBERT ROSEN

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Preface

This volume is a collection of essays, intended primarily to enlarge upon a number of points that were touched upon in *Life Itself.* I believe they are of independent interest and importance, but I felt the ideas could not be pursued in that place, since they would detract from the main line of the argument to which *Life Itself* was devoted.

Thus this volume should be considered a supplement to the original volume. It is not the projected second volume, which deals with ontogenetics rather than with epistemology, although some chapters herein touch on ideas to be developed therein.

The essays presented here were mainly written after *Life Itself* was published. They were prepared for special occasions: meetings, seminars, conferences, and workshops. Some of them have appeared in print before, as contributions to the *Proceedings* volumes arising from these occasions. They were never intended for journals: the subject matter and the treatment required put them outside the scope of any journal known to me that would reach the appropriate audiences. I have on occasion submitted one or another of them to general journals such as *Science,* but the manuscripts were not even refereed.

Nevertheless, I feel there is sufficient interest in this material, and in the issues with which they deal, to justify bringing them together in one place, and in this way. Indeed, I have been surprised and gratified by the general reaction to *Life Itself.* I have received more correspondence relating to this volume than any other publication of mine, and from a broader spectrum of readers, from orthodox molecular biologists to software developers, linguists, and social scientists. For one reason or another, these correspondents expressed their covert uneasiness with previously presented paradigms; it was a very practical uneasiness, a feel-

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ing that their problems were not actually being addressed from those directions. They saw in *Life Itself* a language in which more satisfactory, and more practical, alternatives to current orthodoxies could be expressed. That is exactly what I had hoped.

This correspondence, in fact, reminded me of my fairly extensive travels in eastern Europe years ago. The orthodoxy there, at that time, was Dialectical Materialism, which also promised the solution to everything. Everyone avowed it: It was mandatory to do so. But no one really believed it.

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Part I

ON BIOLOGY AND PHYSICS

THE CHAPTERS in part I are essentially the text of a brief talk presented at a workshop on "Limits to Scientific Knowability," held at the Santa Fe Institute (SFI) in 1994, May 24 to 26. As described to me, the workshop was intended to explore the impacts (if any) of the famous Gödel Incompleteness results in mathematics upon the sciences. The workshop's tone was to be informal and exploratory, aimed at determining whether a more extensive effort by the Institute along these dimensions was warranted.

Accordingly, the workshop consisted primarily of roundtable discussion, with no formal papers delivered. However, the organizers requested a few participants to deliver brief position statements about the impacts of noncomputability results on their field. I was asked to do this for the field of biology. The following is, as best I recollect it, a reconstruction of what I said on this occasion. I have said it all before, but rarely so succinctly, rarely with such a feeling that I was saying exactly what I wanted to say. I include it here as a general introduction.

I was impressed by the general air of affability that pervaded this workshop. I had been rather critical of the SFI's activities, and most particularly of their programs in "complexity" and in "artificial life." My misgivings had arisen from a conviction that the future of graduate education and innovative basic science in this country rests on the development of private research institutes such as the SFI, and that limitless harm would be done if such a heavily promoted endeavor were to embark down unfruitful scientific paths. I do not want to see that happen—the situation is already fragile. However, my experience in this workshop reassured me that the SFI is basically healthy, that it has much

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to contribute, and that it should be supported by all those concerned with the principles involved.

To me, the basic question in biology, to which all others are subsidiary or collateral, is the one put most succinctly by the physicist Erwin Schrödinger: What is life?

Any question becomes unanswerable if we do not permit ourselves a universe large enough to deal with the question. $Ax = B$ is generally unsolvable in a universe of positive integers. Likewise, generic angles become untrisectable, cubes unduplicatable, and so on, in a universe limited by rulers and compasses.

I claim that the Gödelian noncomputability results are a symptom, arising within mathematics itself, indicating that we are trying to solve problems in too limited a universe of discourse. The limits in question are imposed in mathematics by an excess of "rigor," and in science by cognate limitations of "objectivity" and "context independence." In both cases, our universes are limited, not by the demands of problems that need to be solved but by extraneous standards of rigor. The result, in both cases, is a mind-set of reductionism, of looking only downward toward subsystems, and never upward and outward.

In science, for instance, it seems patently obvious that, whatever living organisms are, they are material systems, special cases drawn from a larger, more generic class of nonliving inorganic ones. The game is thus to *reduce,* to express their novel properties in terms of those of inorganic subsystems, merely subject to a list of additional conditions and restrictions. Indeed, one manifestation of this claim to the objectivity of reduction is that one must never, ever, claim to learn anything new about matter from a study of organisms. This is but one of the many forms of the protean Central Dogma (Judson 1979), expressed here as a limitation on material nature itself.

Despite the profound differences between those material systems that are alive and those that are not, these differences have never been expressible in the form of a list—an explicit set of conditions that formally demarcate those material systems that are organisms from those that are not. Without such a list, Schrödinger's question, and biology itself, become unanswerable at best, meaningless at worst. So we must probe more deeply into what the quest for such a list actually connotes.

No such list means there is no algorithm, no decision procedure,

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whereby we can find organisms in a presumably larger universe of inorganic systems. It has of course never been *demonstrated* that there is no such list. But no one has ever found one. I take seriously the possibility that there is no list, no algorithm, no decision procedure, that finds us the organisms in a presumptively larger universe of inorganic systems. This possibility is already a kind of noncomputability assertion, one that asserts that the world of lists and algorithms is too small to deal with the problem, too nongeneric.

Indeed, the absence of lists or algorithms is a generally recurring theme in science and mathematics, one that reveals the nongenericity of the world of algorithms itself, a world too unstable (in a technical sense) to solve the real problems. This was the upshot of the Gödel results from the very beginning.

It helps to recall the mathematical situation that Gödel inherited. It was a world still reeling from the discovery of non-Euclidean geometries almost a century earlier, geometries without number that were just as consistent as Euclid was. It was a world reeling from paradoxes within Cantorian set theory. There had to be something to blame for all of this; something to be expunged, to make everything right again; something not rigorous enough, which had to be found and eradicated.

Bertrand Russell, among others, argued that the fault lay in "impredicative" definitions and vicious circles, and he developed an elaborate and murky "theory of types" to replace them with predicative but equivalent counterparts. This was taken yet further by Hilbert and his school of formalists; they argued that rigor lay entirely in syntax, and that the difficulties at the foundations of mathematics arose entirely from unextruded, semantic residues of meaning. For them, a mathematical term (e.g., *triangle*) was not to be allowed any vestige of meaning; rather, there were to be formal production rules for manipulating *triangle* from one proposition to another. This drastic extrusion of semantics constituted true rigor; mathematics itself would be suspect as long as there was any vestige of meaning or semantics left in it. Hilbert sought this kind of formalization of all of mathematics, the reduction of mathematics to algorithms or lists.

It was this program that Gödel's results killed. Briefly, these results mean that a constructive universe, finitely generated, consisting of pure syntax, is too poor to do mathematics in. They mean that semantics and impredicativities and meanings are essential to mathematics; they

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cannot be replaced by more syntactic rules and more lists or algorithms. They mean that mathematical systems are generically unformalizable; hence it is the formalizable ones that are the rare special cases, and not the other way around. They mean that identifying rigor with formalizability makes most of mathematics unreachable.

I argue that biology teaches us that the same is true about the material world. Roughly, that contemporary physics is to biology as Number Theory is to a formalization of it. Rather than an organism being just a standard material system plus a list of special conditions, an organism is a repository of meanings and impredicativities; it is more generic than an inorganic system rather than less. If this is so, then the Schrödinger question, and indeed biology itself, is not exclusively, or even mainly, an empirical science; empirics is to it as accounting is to Number Theory.

If this is so, then organisms possess noncomputable, unformalizable models. Such systems are what I call *complex*. The world of these systems is much larger and more generic than the simple world we inherit from reductionism.

The main lesson from all this is that computability, in any sense, is not itself a law of either nature or mathematics. The noncomputability results, of which Gödel's was perhaps the first and most celebrated, are indicative of the troubles that arise when we try to make it such.

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CHAPTER 1

The Schrödinger Question, *What Is Life?* Fifty-Five Years Later

Erwin Schrödinger's essay *What Is Life?,* which first appeared in print in 1944, was based on a series of public lectures delivered the preceding year in Dublin. Much has happened, both in biology and in physics, during the half century since then. Hence, it might be appropriate to reappraise the status of Schrödinger's question, from a contemporary perspective, at least as I see it today. This I shall attempt herein.

I wonder how many people actually read this essay nowadays. I know I have great difficulty in getting my students to read anything more than five years old, their approximate threshold separating contemporary from antiquarian, relevant from irrelevant. Of course, in the first decade or two of its existence, as H. F. Judson (1979) says, "everybody read Schrödinger," and its impacts were wide indeed.

The very fact that everybody read Schrödinger is itself unusual, for his essay was a frank excursion into theoretical biology, and hence into something that most experimental biologists declare monumentally uninteresting to them. Actually, I believe it was mostly read for reassurance. And, at least if it is read superficially and selectively, the essay appears to provide that in abundance—it is today regarded as an utterly benign pillar of current orthodoxy.

But that is an illusion, an artifact of how Schrödinger's exposition is crafted. Its true messages, subtly understated as they are, are heterodox in the extreme and always were. There is no reassurance in them; indeed, they are quite incompatible with the dogmas of today. By the stringent standard raised in the Schrödinger title question, following these dogmas has actually made it harder, rather than easier, to provide an adequate answer.

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What Is Life?

Let us begin with the very question with which Schrödinger entitled his essay. Plainly, this is what he thought biology was *about,* its primary object of study. He thought that this "life" was exemplified by, or manifested in, specific organisms, but that at root, biology was not about *them*—it concerned rather whatever it was about these particular material systems that distinguished them, and their behaviors, from inert matter.

The very form of the question connotes that Schrödinger believed that "life" is in itself a legitimate object of scientific scrutiny. It connotes a noun, not merely an adjective, just as, say, rigidity, or turbulence, or (as we shall see later) openness does. Such properties are exemplified in the properties or behaviors of individual systems, but these are only *specimens;* the concepts themselves clearly have a far wider currency, not limited to any explicit list of such specimens. Indeed, we can ask a Schrödinger-type question, What is X? about any of them.

I daresay that, expressed in such terms, the Schrödinger question would be dismissed out of hand by today's dogmatists as, at best, meaningless; at worst, simply fatuous. It seems absurd in principle to partition a living organism, say a hippopotamus, or a chrysanthemum, or a paramecium, into a part that is its "life," and another part that is "everything else," and even worse to claim that the "life" part is essentially the same from one such organism to another, while only the "everything else" will vary. In this view, it is simply outrageous to regard expressions like "hippopotamus life" or "chrysanthemum life" to be meaningful at all, let alone equivalent to the usual expressions "living hippopotamus" and "living chrysanthemum." Yet it is precisely this interchange of noun and adjective that is tacit in Schrödinger's question.

This approach represents a turnabout that experimentalists do not like. On the one hand, they are perfectly willing to believe (quite deeply, in fact) in some notion of *surrogacy,* which allows them to extrapolate their data to specimens unobserved; to believe, say, that *their* membrane's properties are characteristic of membranes in general, or that the data from their rat can be extrapolated ad libitum to other species (Rosen 1983; see my *Anticipatory Systems* for fuller discussion). On the other hand, they find it most disquieting when their systems are treated as the surrogatees, and especially to be told something about *their* mem-

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brane by someone who has not looked at their membrane, but rather at what they regard as a physicomathematical "abstraction." When pressed, experimentalists tend to devolve the notions of surrogacy they accept on *evolution;* surrogates "evolve" from each other, and, hence, what does not evolve cannot be a surrogate. One cannot have the issue both ways, and that is one of the primary Schrödinger unorthodoxies, tacit in the very question itself.

A typical empiricist (not just a biologist) will say that the Schrödinger question is a throwback to Platonic Idealism and hence completely outside the pale of science. The question itself can thus be entertained only in some vague metaphoric sense, regarded only as a *façon de parler,* and not taken seriously. On the other hand, Schrödinger gives no indication that he intends only such metaphoric imagery; I think (and his own subsequent arguments unmistakably indicate) that, to the contrary, he was perfectly serious. And Schrödinger knew, if anyone did, the difference between Platonism and science.

Schrödinger and "New Physics"

Erwin Schrödinger was one of the outstanding theoretical physicists of our century, perhaps of any century. He was a past master at all kinds of propagation phenomena, of statistical mechanics and thermodynamics, and of almost every other facet of his field. Moreover, he viewed physics itself as the ultimate science of material nature, including of course those material systems we call organisms. Yet one of the striking features of his essay is the constantly iterated apologies he makes, both for his physics and for himself personally. While repeatedly proclaiming the "universality" of contemporary physics, he equally repeatedly points out (quite rightly) the utter failure of its laws to say anything significant about the biosphere and what is in it.

What he was trying to say was stated a little later, perhaps even more vividly, by Albert Einstein. In a letter to Leo Szilard, Einstein said, "One can best feel in dealing with living things *how primitive physics still is*" (Clark 1972; *emphasis added*).

Schrödinger (and Einstein) were not just being modest; they were pointing to a conundrum about contemporary physics itself, and about its relation to life. Schrödinger's answer to this conundrum was simple,

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and explicit, and repeated over and over in his essay. And it epitomized the heterodoxy I have alluded to before. Namely, Schrödinger concluded that organisms were repositories of what he called *new physics.* We shall turn a little later to his gentle hints and allusions regarding what that new physics would comprise.

Consider, by contrast, the words of Jacques Monod (1971), writing some three decades after the appearance of Schrödinger's essay:

> Biology is *marginal* because—the living world constituting but a tiny and very "special" part of the universe—it does not seem likely that the study of living things will ever uncover general laws applicable outside the biosphere. (*emphasis added*)

With these words Monod opens his book *Chance and Necessity,* which sets out the orthodox position. This idea of the "marginality" of biology, expressed as a denial of the possibility of learning anything new about matter (i.e., about physics) by studying organisms, is in fact the very cornerstone of his entire development.

Monod did not dare to attack Schrödinger personally, but he freely condemned anyone else who suggested there might be "new physics" wrapped up in organism, or in life, in the harshest possible way; he called them vitalists, outside the pale of science. Sydney Brenner, another postulant of contemporary orthodoxy, was even blunter, dismissing the possibility of a new physics as "this nonsense."

But Schrödinger, within his own lifetime, had seen, and participated in, the creation of more new physics than had occurred in almost the entire previous history of the subject. It did not frighten him; on the contrary, he found such possibilities thrilling and exhilarating; it was what he did physics for. Somehow, it is only the biologists it terrifies.

There is one more historical circumstance that should perhaps be mentioned here. Namely, biological thoughts were lurking very close to the surface in the cradles of the New Quantum Theory in the 1920s. Niels Bohr himself was always profoundly conscious of them. He had in fact grown up in an atmosphere of biology; his father (for whom the familiar Bohr effect, involving the cooperativity of binding of oxygen to hemoglobin, was named) was an eminent physiologist. Many of Bohr's philosophical writings, particularly those dealing with complementarity, are awash in biological currents (Pais 1991). In general, the creators of the New Quantum Theory believed they had at last penetrated the

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innermost secrets of all matter. I have been told, by numerous participants and observers of these developments, of the pervasive expectation that the "secrets of life" would imminently tumble forth as corollaries of this work.

That, of course, is not what happened. And indeed, Schrödinger's ideas about the new physics to be learned from organisms lie in quite a different direction, which we shall get to presently.

Genotypes and Phenotypes

We have seen in the preceding chapters just how radical and unorthodox Schrödinger's essay is, first in simply posing the question What is life? and second in tying its answer to new physics. Both are rejected, indeed condemned, by current dogmas, which cannot survive either of them. How, then, could this essay possibly have been read for reassurance by the orthodox?

The answer, as I have hinted, lies in the way the essay is crafted. Viewed superficially, it looks primarily like an exposition of an earlier paper by Schrödinger's younger colleague, Max Delbrück.[1] Delbrück, a student during the yeasty days in which the New Quantum Theory was being created, was deeply impressed by the ambiences I have sketched here. Indeed, he turned to biology precisely because he was looking for the new physics Schrödinger talked about, but he missed it. Delbrück's paper, on which Schrödinger dwelt at such length in his essay, argued that the "Mendelian gene" had to be a molecule (but see later section, "Order from Order").

Today, of course, this identification is so utterly commonplace that no one even thinks about it any more—a deeply reassuring bastion of reductionism. But it is in fact much more complicated than it looks, biologically and, above all, physically. As we shall see shortly, identifications require two different processes, and Delbrück argued only one. It was Schrödinger's attempt to go the other way, the hard way, roughly to deal with the question, When is a molecule a Mendelian gene? that led him to his new physics, and thence to the very question, What is life?

At this point, it is convenient to pause to review the original notion of the Mendelian gene itself, a notion intimately tied to the genotype-phenotype dualism.

Phenotypes, of course, are what we can see directly about organisms.

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They are what behave, what have tangible, material properties that we can measure and compare and experiment with. Gregor Mendel (*Life Itself,* section 11C) originally conceived the idea of trying to account for the similarities, and the differences, between the phenotypes of parents and offspring in a systematic way.

Mendel was, at heart, a good Newtonian. Newton's Laws[2] in mechanics say roughly that if *behaviors* are differing, then some *force* is acting. Indeed, one recognizes a force by the way it changes a behavior, and that is how one measures that force. In these terms, Mendel's great innovation was to conceive of phenotypes as *forced behaviors,* and to think of underlying "hereditary factors" (later called genes) as forcers of these phenotypes. In a more philosophical parlance, his hereditary factors constituted a new causal category for phenotypes and their behaviors; he was answering questions of the form, Why these phenotypic characters? with answers of the form, Because these hereditary factors. Mendel proceeded to measure the forcings of phenotype by genotype, by selecting a particular phenotype (the wild type) as a standard and comparing it to phenotypes differing from it in only one allele, as we would now say.

Exactly the same kind of thing was then going on elsewhere in biology. For instance, Robert Koch was also comparing phenotypes and their behaviors; in his case, the characters were "healthy" (his analog of wild type) and "diseased." The differences between them, the symptoms or syndromes marking the discrepancy between the former and the latter, were also regarded as forced, and the forcers were called germs. This constituted the "germ theory" of disease.

To anticipate somewhat, we can see that any such genotype-phenotype dualism is allied to the Newtonian dualism between states (or phases) and forces. The former are what behave, the latter are what make them behave. In a still earlier Aristotelian language, the states or phases represent material causation of behavior; the forces are an amalgam of formal and efficient causation. In biology, the phenotypes are the states and behaviors, the genotypes or germs are identified as forces which drive them.

On the other hand, it is all too easy to simply posit forces to account for the tangible changes of behavior that we can see directly. Critics of science have always pointed out that there is indeed something ad hoc, even ineluctably circular, in all this—to define a force in terms of ob-

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served behavior, and then turn around and explain the behavior in terms of that posited force. Indeed, even many scientists regard the unbridled invention of such forces as the entire province of theory and dismiss it accordingly, out of hand, as something unfalsifiable by observation of behavior alone. Worst of all, perhaps, such a picture generally requires going outside a system, to a larger system, to account for behaviors inside it; this does not sit well with canons of reductionism, nor with presumptions of objectivity or context independence in which scientists like to believe. Finally, of course, we should not forget fiascoes such as phlogiston, the epicycles, and the luminiferous ether, among many others, which were all characterized in precisely such a fashion.

For all these reasons, then, many people doubted the reality of the Mendelian genes. Indeed, for similar reasons, many eminently respectable physicists doubted the reality of atoms until well into the present century (Pais 1982).

It is precisely at this point that the argument of Delbrück, which Schrödinger develops in such detail in his essay, enters the picture. For it proposes an identification of the functional Mendelian gene, defined entirely as a forcer of phenotype, with something more tangible, something with properties of its own, defined independently—a *molecule.* It proposes, as we shall see, a way to realize a force in terms of something more tangible that is generating it. But, as we shall now see, this involves a new, and perhaps worse, dualism of its own.

On Inertia and Gravitation

What we are driving toward is the duality between how a given material system changes its own behavior in *response* to a force, and how that same system can *generate* forces that change the behavior of other systems. It is precisely this duality that Schrödinger was addressing in the context of "Mendelian genes" and "molecules," and the mode of forcing of phenotype by genotype. A relation between these two entirely different ways of characterizing a material system is essential if we are to remove the circularities inherent in either alone.

To fix ideas, let us consider the sardonic words of Ambrose Bierce, written in 1911 in his *Devil's Dictionary,* regarding one of the most deeply entrenched pillars of classical physics:

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> GRAVITATION, n. The tendency of all bodies to approach one another, with a strength proportioned to the quantity of matter they contain—the quantity of matter they contain being ascertained by the strength of their tendency to approach one another. This is a lovely and edifying illustration of how science, having made A the proof of B, makes B the proof of A.

This, of course, is hardly fair. In fact, there are two quite different quantities of matter involved, embodied in two distinct parameters. One of them is called *inertial mass:* it pertains to how a material particle *responds* to forces imposed on it. The other is called *gravitational mass:* it pertains rather to how the particle *generates* a force on other particles. From the beginning, Newton treated them quite differently, requiring separate Laws for each aspect.

In this case, there is a close relation between the *values* of these two different parameters. In fact, they turn out to be numerically equal. This is a most peculiar fact, one that was viewed by Einstein not merely as a happy coincidence but rather as one of the deepest things in all of physics. It led him to his Principle of Equivalence between inertia and gravitation, and this in turn provided an essential cornerstone of General Relativity.

We cannot hope for identical relations between inertial and gravitational aspects of a system, such as are found in the very special realms of particle mechanics. Yet, in a sense, this is precisely what Schrödinger's essay is about. Delbrück, as we have seen, was seeking to literally reify a forcing (the Mendelian gene), something "gravitational," by clothing it in something with "inertia"—by *realizing* it as a molecule. Schrödinger, on the other hand, understood that this was not nearly enough, that we must also be able to go the other way and determine the forcings manifested by something characterized "inertially." In more direct language, just as we hope to realize a force by a thing, we must also, perhaps more importantly, be able to realize a thing by a force. It was in this latter connection that Schrödinger put forward the most familiar parts of his essay: the "aperiodic solid," the "principle of order from order," and the "feeding on negative entropy." And as suggested earlier, it was precisely here that he was looking for the new physics. We shall get to all this shortly.

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Before doing so, however, we must look more closely at what this peculiar dualism between the inertial and the gravitational aspects of a material system actually connotes.

Newton himself was never much interested in understanding what a force *was;* he boasted that he never even asked this question. That was what he meant when he said, "Hypothesis non fingo." He was entirely interested in descriptions of system behaviors, which were rooted in a canonical state space or phase space belonging to the system. Whatever force "really" was, it was enough for Newton that it manifested itself as a *function* of phase, i.e., a function of something already inside the system. And that is true, even when the force itself is coming from *outside.*

This, it must be carefully noted, is quite different from *realizing* such a force with an inertia of its own, generally quite unrelated to the states or phases of the system being forced. This latter is what Schrödinger and Delbrück were talking about, in the context of the Mendelian gene, as a forcer of phenotype. As Newton himself did not care much about such realization problems, neither did the "old physics" that continues to bear his personality. Indeed, this is perhaps the primary reason that Schrödinger, who increasingly saw "life" as wrapped up precisely with such realization problems, found himself talking about new physics. It is the tension between these two pictures of force that will, one way or another, dominate the remainder of our discussion.

A central role was played in the original Newtonian picture by the *parameters* he introduced, exemplified by "inertial mass" and "gravitational mass." Roughly, these serve to couple states or phases (i.e., whatever is behaving) to forces. In mechanics, these parameters are independent of both phases and forces, independent of the behaviors they modulate. Indeed, there is nothing in the universe that can change them or touch them in any way. Stated another way, these parameters are the quintessence of objectivity, independent of any context whatever.

Further, if we are given a Newtonian particle, and we ask what kind, or "species," of particle it is, the answer lies neither in any particular *behavior* it manifests under the influence of one or another force impressed on it, nor in the states or phases that do the behaving, but rather precisely in those parameter values—its masses. They are what determine the particle's identity, and in this sense *they are its genome.* The particular behaviors the particle may manifest (i.e., how its phases or

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states are changing when a force is imposed on it) are accordingly only *phenotypes.* Nor does this identity reside in the behaviors of other systems, forced by it.

In causal language, these parameters constitute *formal cause* of the system's behaviors or phenotypes (the states themselves are their material causes, the forces are efficient causes).

Thus there is a form of the phenotype-genotype dualism arising already here, where *genome* (in the sense of species-determining or identity-determining) is associated with *formal causes* of behaviors or phenotypes. It arises here as a consequence of the dualism mentioned earlier, between the states or phases of a system and the forces that are making it behave. If these last remarks are put into the context of the realization problems that Schrödinger and, to a much lesser extent, Delbrück were addressing, it becomes apparent that the situation is not quite so straightforward as current dogmas would indicate. We will return to these matters shortly.

"Order from Order"

I will now digress from conceptual matters and look briefly at Schrödinger's essay into the realization problems I discussed earlier. In general, he was concerned with turning inertia into gravitation, a thing into a force, a molecule into a Mendelian gene. This is perhaps the most radical part of Schrödinger's argument, which ironically is today perceived as an epitome of orthodoxy.

Delbrück had argued that the Mendelian gene, as a forcer of phenotype, must be inertially realized as a molecule. The argument was as follows: Whatever these genes are, in material terms, they must be small. But small things are, by that very fact, generally vulnerable to thermal noise. Genes, however, must be stable to (thermal) noise. Molecules are small and stable to thermal noise. Ergo, genes must be molecules. Not a very cogent argument, perhaps, but the conclusion was satisfying in many ways; it had the advantage of being *anschaulich,* or visualizable. Actually, Delbrück's arguments argue only for *constraints,* and not just holonomic, Tinkertoy ones like rigidity; the same arguments are just as consistent with, for example, two molecules per "gene," or three molecules, or N molecules, or even a fractional part of a molecule.

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Schrödinger was one of the first to tacitly identify such constraints with the concept of order. Historically, the term *order* did not enter the lexicon of physics until the latter part of the nineteenth century, and then only through an identification of its negation, *disorder,* with the thermodynamic notion of entropy. That is, something was ordered if it was not disordered, just as something is nonlinear if it is not linear.

As I discussed in *Life Itself* (see section 4F), constraints in mechanics are identical relations among state or phase variables and their rates of change. If configurational variables alone are involved, the corresponding constraint is called *holonomic.* Rigidity is a holonomic constraint. The identical relations comprising the constraint allow us to express some of the state variables as functions of the others, so that not all the values of the state variables may be freely chosen. Thus, for example, a normal chunk of rigid bulk matter, which from a classical microscopic viewpoint may contain 10^{30} particles, and hence three times that number of configurational variables, can be completely described by only six. Such heavily constrained systems are often referred to nowadays as *synergetic.*[3] H. Haken (1977) calls the independently choosable ones controls, and the remaining ones slaved. We might note, in passing, that traditional bifurcation theory[4] is the mathematics of breaking constraints; its classic problems, like the buckling of beams and other failures of mechanical structures, involve precisely the breaking of rigid constraints as a function of changing parameters associated with impressed *forcings.*

Nonholonomic constraints, which involve both configuration variables and their rates of change, have received much less study, mainly because they are not mathematically tidy. However, they are of the essence to our present discussion, as we shall see.

The language of constraints as manifestations of order can be made compatible with the language of entropy coming from thermodynamics, but the two are by no means equivalent. Schrödinger took great pains to distinguish them, associating the latter with the old physics, embodied in what he called "order from disorder," marking a transition to equilibrium in a closed system. But by speaking of order in terms of constraints, he opened a door to radically new possibilities.

Schrödinger viewed phenotypes, and their behaviors, as *orderly.* At the very least, the behaviors they manifest, and the rates at which these behaviors unfold, are highly constrained. In these terms, the constraints

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involved in that orderliness are inherently nonholonomic, viewed from the standpoint of phenotype alone.

Delbrück had argued that a Mendelian gene (as a forcer of phenotype) was, in material (inertial) terms, a molecule, mainly on the grounds that molecules were rigid. Thus whatever order there is in a molecule entirely resides in its constraints. But these, in turn, are holonomic. As Schrödinger so clearly perceived, the real problem was to somehow move this *holonomic* order, characteristic of a molecule, into the *nonholonomic* order manifested by a phenotype (which is not a molecule). In the more general terms used in the preceding section, the problem is to realize an inertial, structural, holonomic thing in terms of a force exerted on a dynamic, nonholonomic thing.

This was the genesis of Schrödinger's conception of order from order, or, more precisely, large-scale, nonholonomic, phenotypic order being forced by small-scale, rigid, holonomic, molecular order. It was this kind of situation for which Schrödinger found no precedent in the old physics. This was why, in his eyes, organisms resisted the old physics so mightily.

Schrödinger expressed the holonomic order he perceived at the genetic end in the form of the aperiodic solid. In other words, not just *any* holonomic or rigid structure could inertially realize a Mendelian gene, but only certain ones, which both specialized and generalized conventional molecules in different ways. Nowadays, it is axiomatic to simply identify "aperiodic solid" with "copolymer," and indeed, with DNA or RNA, and the constraints embodying the holonomic order with "sequence." But this changing of names, even if it is justified (and I claim it is not), does not even begin to address the realization problem, the transduction of genomic inertia into gravitation that Schrödinger was talking about.

Schrödinger was perhaps the first to talk about this transduction in a cryptographic language, to express the relation between holonomic order in genome, and nonholonomic order in phenotype, as constituting a *code*. This view was seized upon by another physicist, George Gamow,[5] a decade later; after contemplating the then-new Watson-Crick structure for DNA, he proposed a way to use DNA as a template, for moving its holonomically constrained "order" up to another holonomically constrained but much less rigid inertial thing, protein. This is a very far cry from the code that Schrödinger was talking about; it is at

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best only an incremental syntactic step. The next big one would be to solve the protein-folding problem (see *Life Itself,* section 11F), something over which the old physics claims absolute authority. After three decades of fruitless, frustrating, and costly failures, the field is just beginning to move again. Ironically, this is being done by postulating that protein folding is a forced rather than a spontaneous process, and by trying to realize these putative forcers in inertial terms. Thus in a sense the Mendelian experience is being replayed in a microcosm. But this is another story.

In addition to the principle of order from order that Schrödinger introduced to get from genotype to phenotype, and the aperiodic solid that he viewed as constituting the genetic end of the process, and the idea of a cryptographic relation between holonomic constraints in genotype and the nonholonomic ones characterizing phenotype, Schrödinger introduced one more essential feature: the idea of *feeding* (on "negative entropy," he said, but for our purposes it does not matter what we call the food). This was not just a gratuitous observation on his part. He was saying that, for the entire process of order from order to work at all, the system exhibiting it *has to be open* in some crucial sense. In the next section, we shall look at this basic conclusion in more detail.

Molecular biologists, in particular, found reassurance in Schrödinger's essay, mainly because of his use of innocent-sounding terms in familiar contexts. However, whatever this essay may offer, it is not reassurance.

The "Open System"

Thus Schrödinger envisioned two entirely different ways in which biological phenotypes, considered as material systems, are open. On the one hand, they are open to forcings, embodied tacitly in the Mendelian genes. On the other hand, they are also open to what they feed on, what they "metabolize." The former involves the effects of something *on* phenotype; the latter involves the effects of phenotype on something else (specifically, on "metabolites" residing in the environment). Schrödinger was tacitly suggesting a profound connection between these two types of openness—namely, that a system open in the first sense must also be open in the second. Stated another way, the entire process of

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order from order that he envisioned, and indeed the entire Mendelian process that it represented, cannot work in a (thermodynamically) closed system at all.

Such thermodynamically open systems accordingly can be considered "phenotypes without genotypes." They are the kinds of things that Mendelian genes can force. So this is a good place to start, especially since, as we shall see, it is already full of new physics, even without any explicit genome to force it. To anticipate somewhat, we will be driving toward a new perspective on Schrödinger's inverse question, When can a molecule be a Mendelian gene? in terms of another question of the form, When can a thermodynamically open system admit Mendelian forcings?

The history of ideas pertaining to open systems is in itself interesting and merits a short statement.[6] The impetus to study them and their properties came entirely from biology, not at all from physics. Thinkers in the latter field preferred to rest content with closed, isolated, conservative systems and their equilibria, and to blithely assign their properties a universal validity.

The first person to challenge this, to my knowledge, was Ludwig von Bertalanffy[7] in the late 1920s. Ironically, he was attempting to combat the frank vitalism of the embryologist Hans Driesch, particularly in regard to "equifinal" embryological or developmental processes. Bertalanffy showed that these phenomena, which so puzzled Driesch, were understandable once we gave up the strictures of thermodynamic closure and replaced the concept of equilibrium by the far more general notion of steady state (*fliessgleichgewicht*) or the still more general types of attractors that can exist in open systems.[8]

Bertalanffy was a person whom Jacques Monod loathed, and whom he (among many others) castigated as a "holist." By their very nature, open systems require going outside a system, going from a smaller system to a larger one to understand its behaviors. Stated another way, openness means that even a complete understanding of internal parts or subsystems cannot, of itself, account for what happens when a system is open. This flies in the face of the "analysis," or reductionism, that Monod identified with "objective science." But this is another story.

In the late 1930s, Nicolas Rashevsky (see *Life Itself,* section 5B) discovered some of the things that can happen in a specific class of such open systems, presently termed reaction-diffusion systems. He showed

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explicitly how such systems could spontaneously establish concentration gradients in the large. This is, of course, the most elementary morphogenetic process, and at the same time it is absolutely forbidden in thermodynamically closed systems. It might be noted that another name for this process, in physiology, is active transport. Over a decade later, this process was rediscovered by Alan Turing[9] (1950) in a much simpler mathematical context than Rashevsky had used. A decade after that, the same phenomena were picturesquely characterized by Ilya Prigogine (a member of the Brussels School, writing from a base in irreversible thermodynamics), under the rubric of symmetry breaking. A huge literature on pattern generation, and self-organization in general, has arisen in the meantime, based on these ideas.

Bertalanffy himself was quite well aware of the revolution in physics that was entailed in his concept of the open system. Indeed, he said quite bluntly, "The theory of open systems has opened up an entirely *new field of physics*" (1952). Quite early in the game, Prigogine (1947) likewise said, "Thermodynamics is an admirable but *fragmentary* theory, and this fragmentary character originates from the fact that it is applicable only to states of equilibrium in closed systems. Therefore, it is necessary to establish a broader theory."

Even today there is no acceptable physics of open systems, which are not merely closed systems perturbed in a certain way (see, e.g., chapter 12). This is because closed systems are so degenerate, so nongeneric, that when opened, the resultant behavior depends on how they were opened much more than on what they were like when closed. This is true even for the classical theory of thermodynamics itself, and it is why this classical theory does not lend itself to expansion into a true physical theory of open systems. What passes for theory at this level is entirely phenomenological, and it is expressed in dynamic language, not thermodynamic. These facts are of direct and urgent concern to experimental analysis, particularly in biology, because the very first step in any analytic procedure is to open the system up still further, in a way that is itself not reversible. That is, roughly, why analysis and synthesis are not in general inverse processes (cf. later section, What About Artificial Life?).

In any case, Schrödinger himself *could* have known about these incipient revolutions in the old physics, tacit in systems that feed and metabolize. But he had fixed his attention entirely on molecules, and

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on biochemistry, and hence he missed a prime example of the very thing he was asserting, and which most biologists were even then denying, namely that organisms teach new lessons about matter in general.

Open systems thus constitute in themselves a profound and breathtaking generalization of old physics, based as it is on the assumption of excessively restrictive closure conditions, conservation laws, and similar nongeneric presumptions that simply do not hold for living things. Seen in this light, then, is it really biology that is, in Monod's words, "marginal," "a tiny and very special part of the universe," or is it rather the old physics? In 1944, Schrödinger suggested that it was the latter that might be the case. Today, fifty-five years later, that possibility continues to beckon and, indeed, with ever-increasing urgency.

The Forcing of Open Systems

The behaviors manifested in open systems, such as their capacity to generate and maintain stable spatial patterns, exemplify neither the classical thermodynamic notion of "order from disorder," as Schrödinger used the term, nor what he called "order from order." As I have said, open system behaviors look like phenotypes, but they are not forced, in any conventional sense, and certainly not in any Mendelian sense, even though they have "genomes" expressed in their parameters. Nevertheless, their behaviors can be stable without being rigid or in any sense holonomically constrained. Let us see what happens when we impose forcings on such a system and, especially, when we try to internalize those forcings.

The essence of an open system is, as we have seen, the necessity to invoke an "outside," or an environment, in order to understand what is going on "inside." That is, we must go to a larger system, and not to smaller ones, to account for what an open system is doing. That is why reductionism, or analysis, that only permits us to devolve system behavior upon subsystem behaviors, fails for open systems. And as we have seen, that is why there is so much new physics inherent in open systems. That fact, of course, does not make openness unphysical; it simply points up a discrepancy between the physics we presently know and the physics we need.

But there are many ways a system can be open. So far, I have dis-

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cussed only thermodynamic openness, characterized by energetic and material fluxes through the system. These are characterized by corresponding sources and sinks generally residing outside the system itself, in its environment. Inherent in this view is the notion of the system exerting forces on its environment, acting as a pump and driving the flow from sources to sinks.

However, an open system in this thermodynamic sense can itself be forced: the environment can impress forces on the system. This is what we have called a gravitational effect, and it is in general a quite different kind of openness to environmental influence than the thermodynamic openness we have just been considering. System behavior under the influence of such impressed forces has always been the lifeblood of classical particle mechanics and also, in a somewhat modified form, of what is today roughly called Control Theory.[10]

If there is already much new physics in the free behaviors of open systems, we should not be surprised to find much more in their forced behaviors, especially since our intuitions about how material systems respond to impressed forces are generally drawn from very simple systems, indeed generally linear ones. One of these intuitions, embodied in such things as servomechanisms and homeostats, is that a forced system will generally end up tracking the forcing. If this is so, it is correct to say that the relation between such an impressed force and the resulting system behavior is ultimately a cryptographic one; the explicit relation between the two is embodied in the familiar *transfer function*[11] of the system. That is already suggestive, but it is very risky to simply extrapolate such ideas to open systems.

A system that is open in *any* sense is one whose behaviors depend on something outside the system itself, whereas in a closed system, there *is* no outside. Thus it has always been a tempting idea to internalize the external influences in some way, to get a bigger system that is closed and deal with that. Unfortunately, the genericity of openness forbids it; genericity in this sense means that openness is preserved under such perturbations (i.e., physical openness is structurally stable). Indeed, what we end up with in this fashion is generally a bigger open system, which is in some sense even more open than the one we started with. This is, in itself, an important observation, which among other things underlies the familiar notion of the side effect (see the discussion of side effects in my *Anticipatory Systems*). At any rate, what one typically ends

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up with after carrying out such a strategy is the entire universe, which is not very helpful.

In general, the unforced or free situation in any system is one in which every force in the system is an internal force. In the language introduced earlier, it is a situation in which every gravitational aspect in the system can be assigned to a corresponding inertial aspect of that system. On the other hand, if a force is impressed on such a system from outside, that force has no inertial correlate within the system; there is in some sense an excess of gravitation over available inertia, an "inertial defect," if you will.

Thus if we wish to try to internalize such a force, we must augment our original system with more inertia; in practice, that means adding more state variables and more parameters to the system, in such a way that the forced behavior of the original system is now free behavior of the larger system.

Now, the effect of any force is to modify a rate, compared with what that rate would be in an unforced or free situation. That is, a force shows up in the system as an acceleration or deceleration of some system behavior (i.e., it acts as a *catalyst*). If we can internalize such a force in the manner we have described, in terms of inertially augmenting the original system with more state variables and more parameters, then it is not too much an abuse of language to call the new variables we have introduced (and of course the parameters we need to couple them to the original system) *enzymes.* (This usage, however, embodies a confusion between active sites and the molecules that carry them [see *Life Itself,* section 11F].)

In formal terms, such augmented systems must be very heavily constrained, with all kinds of identical relations between the new variables and parameters we have added (i.e., the "enzymes") and the tangent vectors that govern change of state in the system. That is, the new variables are doing a "double duty": they define state in the larger system, and they also participate in operating on that state, in determining the rate at which such a state is changing.

Without going into details, these constraints are strong enough to be expressed in an abstract graphical language. Primitive examples of this are the familiar representations of intermediary metabolism, in which the arrows (representing enzymes) correspond to the inertial variables and parameters we have added to internalize impressed forces, and

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the vertices roughly correspond to state variables of the smaller open system on which the forcings are impressed.

The existence of such a graph expressing the constraints is in fact a corollary of internalizing forces impressed on open systems, not only in biology, but quite generally. To a large extent, the converse is also true, but that is not of immediate concern. Note that the graph looks very much like an aperiodic solid, and indeed it possesses many of the properties Schrödinger ascribed to that concept. The novel thing is that it is not a "real" solid. It is, rather, a pattern of causal organization; it is a prototype of a *relational model* (see *Life Itself,* chapter 5).

Since the larger system is itself open, the "enzymes" will themselves have sources and sinks. They are not present in the diagram, but without them, the enlarged system, represented by the graph, is generally *not stable* as a free system. If we want it to be stable, *we need more forces* impressed on the system to stabilize it. This is, roughly, where the Mendelian genes enter the picture.

In a nutshell, stabilization of this kind is attained by modulating the rates that the "enzymes" impose on the original open system with which we started. This, in fact, is precisely what the Mendelian genes do: they correspond to accelerations or decelerations of the rates at which "enzymes" themselves control rates. We may further think to internalize impressed forces of this kind in the same way we just internalized the "enzymes" themselves—namely, add still more inertial variables of state, and still more parameters to couple them to what we already have, to obtain an even bigger open system, and one that is even more heavily constrained than before. As before, these constraints are strong enough to be expressed in graphical language, but the kind of graph that arises at this level is much more complicated. Instead of two levels of "function" embodied in the distinction we have drawn between the arrows of the graph and its vertices, we now have three such levels (the original metabolites, the "enzymes" that force them, and now the Mendelian genes that force the "enzymes"). If the original graphical structures are indeed thought of as aperiodic solids, so too are the new ones, albeit of quite a novel type.

Unfortunately, even thus augmented, the resulting open systems are still not in general stable. We could repeat the process: posit new impressed forces to modulate the Mendelian genes we have just internalized and seek to internalize them by means of still more inertia (i.e.,

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more state variables, more parameters to couple them to what is already in the system, and more constraints imposed upon them). At this point, we have a glimpse of an incipient infinite regress establishing itself.

The only alternative is to allow the sources and sinks for the internalized inertial forcers introduced at the N^{th} stage of such a process to have already arisen at earlier stages. A source for such an N^{th}-stage internalized forcer is *a mechanism for its replication,* expressed in terms of the preceding $N-1$ stages, and not requiring a new $N+1$ stage. Thus replication is not just a formal means of breaking off a devastating infinite regress, but it serves to stabilize the open system we arrived at in the N^{th} stage.

In biology, N seems to be a small number, $N = 2$ or 3, or perhaps 4 in multicellulars. But I can see no reason why this should be so in general.

Breaking off such an infinite regress does not come for free. For it to happen, the graphs to which we have drawn attention, and which arise in successively more complicated forms at each step of the process, must fold back on each other in unprecedented ways. In the process, we create (among other things) closed loops of efficient causation. Systems of this type cannot be simulated by finite-state machines (e.g., Turing machines); hence they themselves are not machines or mechanisms. In formal terms, they manifest impredicative loops. I call these systems *complex;* among other things, they possess no largest (simulable) model. The physics of such complex systems, described here in terms of the forcing of open systems (although they can be approached in many other ways) is, I assert, some of the new physics for which Schrödinger was looking.

When Is a Molecule a Mendelian Gene?

This was the real question Schrödinger was addressing in his essay, the inverse of the question Delbrück thought he answered by asserting that a gene is a molecule.

The question looks intriguing because, at its root, it embodies a Correspondence Principle between an "inertial" thing (e.g., a molecule) and a "gravitational" thing (a force imposed on an open system). But the question is much more context dependent than that; its answer involves

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not just inherent properties of a "molecule" in itself (e.g., "aperiodicity"), but also the properties of what system is being forced, and the preceding levels of forcing (of which "genome" is to be the last).

Thus very little remains of Schrödinger's simple cryptographic picture of "order from order," in which rigid molecular structures get transduced somehow into nonrigid phenotypic ones. Rather, the initial "order" appears as a pattern, or graph, of interpenetrating constraints, which determines what happens, and how fast it happens, and in what order, in an underlying open system. The arrows in such graphs, which I suggest constitute the real "aperiodic solid," are operators; they express "gravitational" effects on the underlying system. In terms of inertia, it is much more appropriate to speak of active sites than of molecules. The two are not the same.

Indeed, much simpler questions, such as, When is a molecule an enzyme? are hard to approach in purely inertial terms. These are all structure-function questions, and they are hard because a function requires an external context; a structure does not.

In a sense, if all we want to talk about is an active site (i.e., something gravitational), and we find ourselves talking about a whole molecule (i.e., something inertial), we run a severe risk of losing the site in the structure. There is much more inertia in a whole molecule than in a functional site. Unlike the impressed forces imposed from the environment of a system, which constitute an inertial defect, structure-function problems tend to involve a dual inertial excess of irrelevant information.

There is some new physics here too, I would wager.

What Is Life?

In this penultimate section, I shall review the Schrödinger question in the light of the preceding discussions, and in terms of a number of subsidiary questions either raised directly by Schrödinger himself or that have come up along the way.

Is What Is Life? *a Fair Scientific Question?*

My answer is, "Of course it is." Not only is it fair, it is ultimately what biology is about; it is the central question of biology. The question itself

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may be endlessly rephrased, but to try to eliminate it in the name of some preconceived ideal of mechanistic "objectivity" is a far more subjective thing to do than that ideal itself allows.

Does the Answer Involve New Physics?

Once we admit questions of the Schrödinger type, which treat an adjective or predicate as a thing in itself, we are already doing new physics. More formally, the old physics rests on a dualism between phases or states, and forces that change the states, which make the system "behave." Predicates, or adjectives, typically pertain to these behaviors, which are what we see directly. Moreover, the emphasis here is overwhelmingly skewed in the direction of what I have called the inertial aspects of a system (how it responds to forces) at the expense of its gravitational aspects (how it exerts forces). In biology, this shows up in terms of structure-function problems, where structure pertains to inertia, and function to gravitation.

Many biologists, indeed the same ones who would deny the legitimacy of the Schrödinger question, assert that function is itself an unscientific concept;[12] in effect, they assert there is only structure. Hence, biology can be scientific only insofar as it succeeds in expressing the former in terms of the latter. That is why Delbrück's argument, that a functionally defined Mendelian gene comprises a familiar chemical structure, a molecule, was received so enthusiastically, while the converse question (When can a "molecule" manifest such a function?), with which Schrödinger's essay is really concerned, was not even perceived.

Schrödinger's new physics, embodied generally in his initial question, and specifically in his appraisal of the relation between genes and molecules, rests in his turning our inertial biases upside down, or at least suggesting that inertial and gravitational aspects of material systems be granted equal weight. Once this is done, new physics appears of itself.

Is Biology "Marginal"?

Jacques Monod used this word in expressing his belief that organisms are nothing but specializations of what is already on the shelf provided by old physics, and that to claim otherwise was mere vitalism. He buttressed this assertion with his observations that organisms are in some sense rare and that most material systems are not organisms.

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This kind of argument rests on a confusion about, or equivocation on, the term *rare,* and identifying it with *special* (see *Life Itself,* section 1A). An analogous argument could have been made in a humble area like arithmetic, at a time when most numbers of ordinary experience were rational numbers, the ratios of integers. Suddenly, a number such as π shows up, which is not rational. It is clearly rare, in the context of the rational numbers we think we know. But there is an enormous world of "new arithmetic" locked up in π, arising from the fact that it is much too general to be rational. This greater generality does not mean that there is anything vitalistic about π, or even anything unarithmetical about it; indeed, the only vitalistic aspects show up in the mistaken belief that "number" means "rational number."

Schrödinger's new physics makes an analogous case that organisms are more general than the nonorganisms comprehended in the old physics, and that their apparent rarity is only an artifact of sampling.

What Is This "New Physics"?

The new physics involves going from special to general, rather than the other way around. At the very least, it means going from closed systems to open ones, discarding specializing hypotheses such as closure conditions and conservation laws. There is still no real physics of such open systems, largely because the formalisms inherited from the old physics are still much too special to accommodate it.

Most significant, I feel, will be the shifting of attention from exclusively inertial (or structural) concepts to gravitational aspects. This can be expressed as a shift from concerns with material causations of behavior, manifested in state sets, to formal and efficient causations. As I have suggested, these are manifested in graphical structures, whose patterns can be divorced entirely from the state sets on which they act. The mathematical precedent here lies in geometry, in the relation between groups of transformations tied to an underlying space, and the abstract group that remains when that underlying space is forgotten. To a geometer, concerned precisely with a particular space, this discarding of the space seems monstrous, since it is his very object of study; but to an algebraist, it throws an entirely new perspective on geometry itself, since the same abstract group can be *represented* as a transformation group in many different ways (i.e., an underlying space restored, which can look very different from the original one from which the group was ab-

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stracted). In the same way, it would look monstrous to a biologist, say, to throw away his state spaces (his category of material causation, his "inertia") and retain an abstract graphical pattern of formal and efficient causation, but that is what is tacit in Schrödinger's concern with gravitation.

What Is Life?

The lines of thought initiated in Schrödinger's essay lead inexorably to the idea that life is concerned with the graphical patterns we have discussed. The formal metaphor I have suggested, namely, dissociating a group of transformations from a space on which it acts, shows explicitly a situation in which what is a predicate or an adjective from the standpoint of the space can itself be regarded as a thing (the abstract group) for which an underlying space provides predicates. This is analogous to the inversion of adjective and noun implicit in Schrödinger's question itself; as we saw at the outset, it involves partitioning an organism into a part that is its life and a part that is everything else. Seen from this perspective, the "life" appears as an invariant graphical pattern of formal and efficient causation, as a gravitational thing; the "everything else" appears in the form of material causation (e.g., state sets) on which such a graph can operate.

Such a system must be *complex*. In particular, it must have nonsimulable models; it cannot be described as software to a finite-state machine. Therefore, it itself is not such a machine. There is a great deal of new physics involved in this assertion as well.

To be sure, what I have been describing are necessary conditions, not sufficient ones, for a material system to be an organism. That is, they really pertain to what is not an organism, to what life is not. Sufficient conditions are harder; indeed, perhaps there are none. If so, biology itself is more comprehensive than we presently know.

What about "Artificial Life"?

The possibility of artificial or synthetic life is certainly left wide open in this discussion. However, the context it provides certainly excludes most, if not all, of what is presently offered under this rubric.

The first point to note is that, in open systems generally, analysis

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and synthesis are not inverse operations. Indeed, most analytic procedures do not even have inverses, even when it comes to simple systems or mechanisms. For instance, we cannot solve even an N-body problem by "reducing" it to a family of N_k-body problems, whatever k is. How much more is this true in the kinds of material systems I have called complex, a condition that I have argued is necessary for life? Indeed, no one has ever really studied the problem of when an analytic mode possesses an inverse, that is, when an analytic mode can be run backward, in any physical generality.

A second point is that what is currently called artificial life, or A-life, primarily consists of exercises in what used to be called biomimesis. This is an ancient activity, based on the idea that if a material system exhibits *enough* of the behaviors we see in organisms, it must *be* an organism. Exactly the same kind of inductive inference is seen in the "Turing Test" in artificial intelligence: a device exhibiting enough properties of intelligence *is* intelligent.

In this century, biomimesis has mainly been pursued in physical and chemical systems, mimicking phenomena such as motility, irritability, and tropisms in droplets of oils embedded in ionic baths. Previously, it was manifested in building clockworks and other mechanical automata. Today, the digital computer, rather than the analog devices previously employed, is the instrument of choice as a finite-state machine.

At root, these ideas are based on the supposition that some finite number of (i.e., "enough") simulable behaviors can be pasted together to obtain something alive. Thus that organisms are themselves simulable as material systems, and hence are not complex in our sense. This is a form of Church's Thesis, which imposes simulability as, in effect, a law of physics, and indeed, one much more stringent than any other. Such ideas already fail in arithmetic, where what can be executed by a finite-state machine (i.e., in an "artificial arithmetic"), or in any finite (or even countably infinite) collection of such machines, is still infinitely feeble compared to "real" arithmetic (i.e., Gödel's Theorem).

Schrödinger himself, in the last few pages of his essay, quite discounted the identification of "organism" with "machine." He did this essentially on the grounds that machines are rigid, essentially low-temperature objects, while phenotypes were not. This provocative assertion, more or less a small aside on Schrödinger's part, is well worth pursuing in the context of the material basis of artificial life.

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Conclusions

Schrödinger's essay, published nearly a half-century ago, provides little comfort to an exclusively empirical view of biology, certainly not insofar as the basic question What is life? is concerned. On the contrary, it removes the question from the empirical arena entirely and in the process raises troubling questions, not only about biology but about the nature of the scientific enterprise itself. However, Schrödinger also proposed directions along which progress can be made. The consignment of his essay to the realm of archive is premature; indeed, it is again time that "everybody read Schrödinger."

NOTES

1. The paper that motivated Schrödinger was a joint work, which he himself referred to as "Delbrück, N. W. Timoféëff [Ressovsky] & K. G. Zimmer, *Nachr. a.d. Biologie d. Ges. d. Wiss. Göttingen,* vol. 1, p. 89. 1935." The main impetus of that paper was what in those days was called target theory (*treffertheorie*). It attempted to determine the cross section or physical size of "genes" by counting the number of mutations induced by standard doses of high-energy particles. It turned out (artifactually) that the answers obtained by such methods were of the order of molecular sizes. The stability arguments arose from attempts to understand or interpret these "target-theoretic" results.

2. This is the substance of Newton's Second Law of Motion. In this limited mechanical context, *behavior* means change of state (or phase). Force in this picture is always a function of phase alone; changing the force means changing that function, but the *arguments* of that function remain fixed, determined entirely by the system on which the force is acting. Thus in a strong sense, the same force acting on two different mechanical systems gets two different and unrelated descriptions. Even in mechanics, this view was strongly challenged, especially by Ernst Mach. The Machian challenge is rather closely related to the subject of this chapter.

3. The term *synergy* is used in different ways by different authors. It was used by Russian theorists, especially by I. Gelfand, S. V. Fomin, and their collaborators, to describe complicated coordinated neuromuscular activities like walking or running. It has been used, especially by H. Haken, in a way essentially synonymous with mechanical constraints. It connotes the control of many apparently independent degrees of freedom via a much smaller number of controls. Many different kinds of problems, such as the folding of proteins, can be regarded as synergetic in this sense.

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4. At its essence, bifurcation theory concerns situations in which distinct systems that are close to each other according to one criterion may behave arbitrarily differently according to another. It thus concerns situations in which an approximation to a system, however good the approximation, fails to be a model or a surrogate for it. The original mathematical setting for bifurcation theory studies the interplay between a metric topology on a set and an equivalence relation ("similarity") on that set. Several of the essays in this volume deal with bifurcation-theoretic kinds of problems (see especially chapters 11 and 12). In biology, a wide variety of problems are of this type—for example, the distinction between micro- and macroevolution, and the extrapolation of experimental data from one species to another (Rosen 1959).

5. Physicist George Gamow was the first to suggest a cryptographic relation between linear copolymer sequences (primary structures) in DNA (or RNA) and polypeptides. Part of this was based on Schrödinger's use of the term *code* in his essay of 1944. Gamow's idea dated from early in 1953. It had apparently never occurred to any of the empiricists who considered themselves closest to the problem.

6. Intuitively, in physical terms, an open system is one that is exchanging matter and energy with its environment. Openness is harder to characterize mathematically; it is not the same as autonomy, the time-independence of imposed forces. It rather means the absence of symmetries or conservation conditions imposed on the system itself. These kinds of conditions tend to translate into the exactness of differential forms, which is very nongeneric (see chapters in part III).

7. Von Bertalanffy has become well known as the father of General System Theory. He came to develop this as an alternative to reductionist, Cartesian ideas, which he felt were not only scientifically inadequate for biology but had deplorable social and ethical side effects for humanity at large. His system-theoretic ideas were essentially relational in character. (See *Life Itself* for fuller discussions.)

8. Driesch, an eminent experimental embryologist, discovered in 1891 that if the two blastomeres arising from the division of a fertilized sea urchin egg are separated, each will develop into an entire organism and not into a half-organism. This behavior seemed to him so counter to any possibility of mechanical explanation, even in principle, that he invented a noncorporeal concept of *entelechy,* or "wholeness," to account for it. Behaviors such as this were often called equifinal, connoting that the same end-state is attained however much an intermediate state is perturbed or mutilated. (For an interesting discussion, see Waddington [1950].)

As far back as 1928, Bertalanffy identified "equifinality" with what we would today call the stability of point attractors in dynamical systems. He also recognized early that the equilibria of closed systems are not attractors at all. Hence, to a certain extent, Driesch was right, but he confused "physics" with "the physics of closed

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systems." Bertalanffy also recognized that open systems were not amenable to conventional, purely reductionistic modes of empirical analysis. This, ironically, led to the ever-increasing denunciation of Bertalanffy as a vitalist.

9. Turing's contribution is in *Philosophical Transactions of the Royal Society of London [B]* (1952;237:37). It should be noted that something more than equifinality is involved here: The stability of a steady state (point attractor) can change (in Turing's case, from a stable node to a saddle point) as a consequence of how open the system can be. This change of stability is manifested as a change from spatial homogeneity to spatial inhomogeneity; the latter behavior is the essence of self-organization. This is a bifurcation phenomenon between two classes of behaviors, each by itself equifinal.

10. Control Theory can be looked at, in a way, as the study of system behaviors as a function of state-independent, time-dependent forces. It thus involves a large conceptual departure from the completely state-dependent, time-independent kinds of forces envisaged by Newton (see note 2, above).

11. Roughly speaking, the transfer function of a system describes how it transduces input or forcing into output or behavior. It is the central concept of linear system theory. But it makes sense only for linear systems, and it does not generalize.

12. Expressions of the form "the function of the heart is to pump blood" or "the function of an enzyme is to catalyze a reaction" are regarded as teleological, hence vitalistic, hence unscientific (see *Life Itself,* section 5D). Recall the discussion of the Mendelian gene, initially defined purely in functional terms, and the concomitant denial of its reality on such grounds.

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CHAPTER 2

Biological Challenges to Contemporary Paradigms of Physics and Mimetics

The following remarks are intended to address two problems: (a) the role of contemporary physics in dealing with the nature and properties of living systems, and (b) the role of mimetic approaches (usually prefaced by the adjective *artificial*) in dealing with these same matters. Both approaches are offered as (quite distinct) ways of making biology scientific, or objective, by in effect making it something other than biology. And they are both, in a historical sense, ancient strategies; in their separate ways, they appear to embody a mechanistic approach to biological phenomena, whose only alternative seems to be a discredited, mystical, unscientific vitalism. They are alike in that they suppose biology to be a specialization of something inherently more general than biology itself, and the phenomena of life to be nothing but very special embodiments of more universal laws, which in themselves have nothing to do with life and are already independently known. In this view, whatever problems set biology apart from the rest of science arise precisely because organisms are so special.

One prevailing manifestation of such ideas is the naive *reductionism* that passes today as the prevailing philosophy underlying empirical approaches to organisms. The very word connotes that living things are special cases of something else, and that we learn everything there is to know about them by reducing them, treating them as mere corollaries of what is more general and more universal.

However, organisms, far from being a special case, an embodiment of more general principles or laws we believe we already know, are indications that these laws themselves are profoundly incomplete. The universe described by these laws is an extremely impoverished, nongeneric one, and one in which life cannot exist. In short, far from being a special

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