

CHAPTER O N E

It Is Possible to Reduce Biological Explanations to Explanations in Chemistry and/or Physics

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Despite all the dramatic successes we have witnessed in recent years in unraveling the physics and chemistry behind biological phenomena, we still have a very long way to go. The most fundamental question of what is the difference between living and non-living matter remains unanswered. Unlike most physical and chemical systems, living systems must—at the very least—be endowed with *function*, a concept that to this day remains indispensable to biology, yet is effectively missing from the vocabulary of physics and chemistry. Accordingly, we need an account of the evolution of function out of simple physical and chemical dynamics which we do not as yet have. Nevertheless, I retain a guarded optimism: we are, I believe, moving closer to answering this basic question. Yet doing so, I argue, requires fundamental transformations in the conventional approaches of both physics and chemistry.

1 Introduction

My task in this paper is to respond to the question “Is it possible to reduce biological explanations to explanations in chemistry and/or physics?” Forced to choose between only two alternatives—yes or no—I opt for the positive response. But I could as easily have gone the other way. Indeed, I suggest that the question is not well-posed. To the extent that a negative response would imply that I believe there is something beyond physical and chemical processes involved in the formation of living beings, I am obliged to respond affirmatively. I am an unambivalent materialist and do not harbor any such belief. But if by that proposition one means that biological explanations can be reduced to the theories of matter currently available

in physics and chemistry, then my response is no. And not, as Niels Bohr (1932/1958) once argued, because the study of biology can be expected to bring the discovery of new laws. But rather, paraphrasing the arguments put forth by Nancy Cartwright (1983), because laws of physics, by definition, have been developed with reference to a narrow range of possible physical and chemical phenomena; and, one might add, necessarily so. For that is the way with laws: they are developed to describe (or explain) the lowest common denominator of physical and chemical processes, or that which is said to *underlie* the manifest variety of these processes.

How is that lowest common denominator identified? In the actual practice of the physical and chemical sciences, this has generally been translated (and for good pragmatic reasons) into the assumption that that common denominator is coextensive with the simple. Thus, for example, highly oversimplified models, conspicuous for their lack of realism, are often justified by the tacit expectation that, despite their lack of realism (perhaps even by virtue of their simplicity), they contain the features of greatest importance (see Keller, 2002, Ch. 3). In this fashion, a tradition has developed in which concepts of *underlying*, *basic*, *fundamental*, and *simple* are all expected to cluster together.

Such an expectation has worked astonishingly well in physics; not so, however, in biology. As has often been observed, biology simply does not conform to these expectations. Whatever the meaning of *fundamental* in biology, it clearly cannot be equated with *simple*, nor is it at all obvious that it is common to all biological entities. For example, DNA might be argued as fundamental to biological systems, but DNA is an enormously complex molecule. Even in the early days, when molecular biologists believed that its primary function was to code for proteins, no one would have thought of DNA as simple. Today, biologists have learned that this molecule has a repertoire of capacities far richer than had originally been thought, with new dimensions emerging every day. Also, they have learned that DNA is not universal: some organisms make do with RNA, and some entities (e.g., prions) manage to sustain themselves and reproduce without either DNA or RNA. Furthermore, not only does biology *not* admit of a clustering of *fundamental*, *common denominator*, and *simple*, but also a number of philosophers (starting with Jack Smart in 1963) have concluded from this fact that biology, therefore, does not have laws of its own, at least not in the sense that physics is said to have laws. As Ernst Mayr (1982) famously put it, the only universal law in biology is that “All biological laws have exceptions” (p. 38; also see Beatty, 1995; Mitchell, 2000; Smart, 1963).

Which is not to say that biology is not a science (although Smart seems to have thought it was to say exactly that), but rather that it is a science in which the search for “laws” has not, at least so far, played much of a role. True, one finds the term used to describe many important biological discoveries: Mendel’s laws of segregation, power laws (referring to the distribution of nodes in biological networks), and even the central dogma is sometimes referred to as a law. But crucially, these ought properly be called *generalities*, not laws. They apply over a broad range of conditions; but there is neither anything necessary about them (as things could have been otherwise), nor need they be free of exceptions. Furthermore, where they do apply, they may or may not be either fundamental or simple. They are simply empirical statements about what generally seems to be the case. As such, they may (at least in some cases) serve as explanations of what follows, but to the extent that they

come with no account of how such generalizations emerge from the vicissitudes of the evolutionary pathways that gave rise to such properties, the generalities themselves remain unexplained.

Where, then, is the role of physics and chemistry in the construction of biological explanations? As a materialist, I am committed to the position that all biological phenomena, including evolution, require nothing more than the workings of physics and chemistry. But this position requires neither a belief in the adequacy of our current understanding of physics and chemistry, nor any expectation that biology will lead us to the discovery of new laws. New *phenomena*, to be sure, but regarding the question of new *laws*, I concur with Jean-Baptiste Lamarck (1809/1984): “Nature has no need for special laws” (p. 132). I believe that analysis of the specific physical and chemical phenomenology involved in biological processes should, in principle, suffice for an understanding of what endows biological systems with the properties of life. I say “in principle” because there is no telling whether or not the complexity of these systems might, in practice, exceed our analytic capacities. But in any case, the more important question is, “What kinds of analysis, in fact, are required?”

2 Systems Biology

Efforts to develop explanations of biological phenomena based on the physical and chemical properties of highly idealized biological systems were not infrequently made over the course of the twentieth century, but they failed to persuade working biologists of the value of such analyses (see Keller, 2002). Eric Ponder (Director of the Cold Spring Harbor Institute from 1936 to 1941) summed up the sentiments of an entire community when, in his response to such early efforts by Nicolas Rashevsky (1934), he argued:

[I]t is futile to conjure up in the imagination a system of differential equations for the purpose of accounting for facts which are not only very complex, but largely unknown. . . . It is said that if one asks the right question of Nature, she will always give you an answer, but if your question is not sufficiently specific, you can scarcely expect her to waste her time on you . . . what we require at the present time is more measurement and less theory. (cited in Wilson, 1934, p. 201)

Rashevsky, it must be said, was only proceeding as mathematical physicists generally did—indeed, given the analytical technologies available at the time, he had little choice but to begin with a highly idealized (and hence, simplified) model. Similarly, biologists’ disdain for such models, and their demand for more measurement, was also based on the realities of their discipline; in particular, on the recognition of both the complexity of biological systems and their rootedness in evolution.

In any case, among biologists it was the demand for more data that prevailed throughout most of the twentieth century. But at the century’s end, the dramatic successes of the reductionist program of molecular biology and, perhaps especially, the development of new technologies (e.g., for nucleotide sequencing, molecular labeling, and visual tracking) led to an avalanche of data that threatened to overwhelm the ability of experimental biologists to make sense of their findings using their traditional modes of analysis. In the physical and mathematical sciences, conditions

were also changing, primarily due to the development of powerful high-speed computers and, together, these quite different developments created the conditions for a new rapprochement between mathematical and biological science. Support for interdisciplinary collaborations skyrocketed and, in the early years of the twenty first century, new institutes, programs, and departments under the name of *systems biology* have proliferated like wildfire.

Far from being a unified endeavor, systems biology encompasses a great variety of mathematical, computational, and physical approaches to biological problems, and there is substantial disagreement among researchers as to how far computational analysis will enable one to proceed by working from the bottom up (i.e., from what is known about the relevant molecular elements and their interactions), and to what extent it is necessary to work downwards from known systemic properties to the identification of the relevant actors. Nonetheless, virtually all are agreed on the importance of grounding analysis in the data that have become available, and on the need to develop analytic techniques that would enable one to move beyond a list of parts (e.g., genes, proteins, etc.). There is also widespread agreement on the extent to which the analysis of systems with a large number of components yields (emergent) properties that could not have been predicted from the behavior of the components themselves. Of particular importance is the recognition of such key biological properties as robustness, evolvability, and quorum sensing as properties that emerge only on the systems level. Indeed, in less than a decade, work in systems biology has reconfigured the frontier of biological inquiry, bringing issues to the fore that had previously gone virtually unnoticed.

Thus far, the main strength of such analyses has come not from the application of simple, highly idealized models, but from close attention to the kinds of experimental data that recent biological research has provided. Through imaginative deployment of theoretical and computational techniques from the physical, mathematical, and computer sciences, from the application of methods and insights from the engineering sciences, and from the use of sophisticated technologies developed in experimental physics to obtain the kinds of micro-molecular data that analysis of the physical underpinnings of molecular processes requires, we are beginning to acquire the kinds of understanding that would permit the formulation of biological explanations in terms of physical and chemical processes. But it is becoming clear that these accounts need not resemble the kinds of explanations in physics and chemistry to which we are accustomed.

Among the most obvious differences are: (1) the large number and dense interactivity of parts; (2) the hierarchical (and multi-scale) organization of biological systems; (3) the dependence of the identity of parts, and the interactions among them, on higher-order effects; and (4) the robustness and adaptability of biological structures. It is commonplace to observe that biological systems are vastly more complex than physical systems (they might be said to be maximalist, in contrast with the minimalism of physical systems), and the question has recently surfaced as to why these systems are so complex. As Arthur Lander (2004) puts it, "What purpose does all this complexity serve?" (p. 713). Kunihiro Kaneko (2006) raises a similar question, observing that: "For the purpose of simply responding to the external environment, a simpler reaction consisting of a single level would be sufficient." Kaneko's answer, once again, points us to evolution. He writes that "the great diversity or complexity we

observe is a result of the history of organisms that experienced a great variety of circumstances. . . . The past experiences have come to be embedded in the structure and function of organisms” (p. 9), and that seems correct.

3 Function: A Minimalist Conception

But there are clearly qualitative, as well as quantitative, differences between biological and physical systems, and perhaps foremost among these is that biological systems are endowed with *function*, a concept effectively missing from the vocabulary of physics and chemistry yet, to this day, indispensable to biology. To avoid confusion, let me make clear from the start that my use of the term “function” departs from the convention that has developed in recent years among philosophers of science, whereby function is defined in terms of natural selection. Proper function—as first argued by Ruth Millikan (1984), and now widely asserted—should be understood solely in the context of natural selection: the function of *X* is that “which caused the genotype, of which *X* is the phenotypic expression to be selected by natural selection” (Neander, 1998, p. 319). This is *not* how I am using the term. Indeed, both logically and historically, the notion of function with which I am concerned must *precede* the onset of natural selection, at least as that term is usually construed.

Natural selection—at least since the neo-Darwinian synthesis and, probably, ever since Darwin—has been conventionally understood as requiring the prior existence of stable, autonomous, and self-reproducing entities. Minimally, it presupposes the existence of single celled organisms, or, simply, of stable, autonomous, cells capable of dividing. But these first cells contained, of necessity, numerous sub-cellular entities (or modules), endowing the primitive cell with the functions that would be minimally required for the cell to sustain itself and reproduce. In other words, even if the first cells lacked many features of the modern cell, they had to have had primitive mechanisms to support metabolism, cell division, etc., and there needed to have come into being primitive embodiments of function that would work keep the cell going and to protect it from insult.

To further clarify what I mean by function, let me turn to Michael Ruse’s (2003) argument against the use of the term for inanimate systems and, more specifically, against the sufficiency of circular causality for an understanding of biological function. Ruse offers the familiar example of the cyclical process by which rain falls on mountains, is carried by rivers to the sea, evaporated by the sun, whereby it forms new rain clouds which, in turn, discharge their content as rain. The river is there because it produces or conveys water to form new rain clouds. The rain clouds are a result of the river’s being there. But Ruse argues that we would not want to say the function of the river is to produce rain clouds, and he is right. What is missing, he claims, is the means by which “things are judged useful.”

I will not follow Ruse in his deployment of such worrisomely adaptationist and intentional notions as *value* and *desire*. Instead, I want to salvage his observation by re-describing what he calls *judgment* as a measurement of some parameter, or, if you like, as an evaluation performed by a mechanical sensor that, when exceeding some pre-set limit, is fed back into a controller which is able to restore the proper range of parameter. In other words, reverting to another philosophical tradition, I use the

term *function* in the sense of a simple feedback mechanism. Like a thermostat. As William Wimsatt (2002) puts it, “To say that an entity is functional is to say that its presence contributes to the self-regulation of some entity of which it is a part” (p. 177). Once such a mechanism is added to the rain–cloud–river cycle (say, a mechanism that triggers a change in evaporation rates when the water level falls too low), we can, in this sense of the term, quite legitimately speak of function and say, for example, that the function of such a mechanism is to maintain the water level within a certain range of parameters. Furthermore, I argue, we can similarly refer to the many different cellular mechanisms (proof-reading and repair, chaperones, cell-cycle regulation) that maintain various aspects of cellular dynamics.

To make my minimalist conception of function more precise, I want to focus on those effects that contribute to the stability of such properties that are essential to the system’s continuance: its water level, or perhaps its physical integrity. *Stability*, here, means little more than constancy, the fixity of some property of either the internal or external milieu, but it is important to note that maintaining a property within a certain range does not necessarily contribute to the survival of the overall system. For example, maintaining the water level within certain bounds does contribute to the stability of the rain–cloud–river cycle, whereas maintaining a constant temperature of the system need not have anything to do with the stability of the system as such. In order to distinguish the stability or constancy of a particular aspect of the system from the stability of the system as a whole, I will use the term *persistence* for the latter. The crucial point is that, when the stability of a property *does* contribute to the persistence of the overall system, a kind of selection—i.e., selection of the most stable (or most persistent)—will operate automatically. Here, if you will, is selection without reproduction. Like natural selection, it is entirely agnostic with respect to the forces that threaten the stability of a system; it cares only about persistence.

Indeed, if we think of reproduction as just one strategy for ensuring persistence—as Frederick Bouchard (2004), Rosenberg and Kaplan (2005), and others have advocated—we can think of natural selection as a subset of this more general category of *selection for persistence*. And just as in the case of natural selection, the more general selection process that kicks in with variable stability/persistence provides us with an explanation of why the arrangement of device-cum-system is likely to be common. It might also provide us with an explanation of progressive refinements of organization leading to increases in stability. It will not, however, contribute to our understanding of how such an arrangement of parts—an arrangement in which one part exerts an effect that enhances the stability of the larger system in which it is embedded—first came to be.

Put in these terms, the obvious question is how such a mechanism might arise. It is striking that, for all the promise of recent attempts to treat biological systems as instances of the kinds of non-linear dynamical systems encountered in statistical physics, and for all the power of non-linear dynamics to generate emergent patterns, biological function—as I am using the term—seems not to be on the horizon. Apparently, the emergence of function requires rather more than just non-linear interactions, and the question is what that more consists of.

Addressing just this question, Walter Fontana and Leo Buss (1996) have concluded that “the traditional theory of ‘dynamical systems’ is not equipped for dealing

with constructive processes. Indeed, the very notion of 'construction' requires a description that involves the structure of objects. Yet, it was precisely the elimination of objects from the formalism that make dynamical systems approaches so tremendously successful" (p. 59). They argue that the main problem is that, although conventional dynamical systems theory is "well-suited to treat changes in the magnitudes of quantitative properties of fixed object species," it is still "ill-suited to address interactions that change the objects themselves" (p. 63). And this, of course, is precisely what characterizes the objects of biological systems, which are necessarily subject to change by the internal dynamics of the system, and not only by external insult. As they put it: "Mutation is to construction like perturbation is to dynamics" (p. 63). Fontana and Buss call their own very interesting effort to expand the traditional theory to include objects, their internal properties, their construction, and their dynamics, *constructive dynamical systems*.

4 Kant and As-If Purpose

Of course, function is not the only property that distinguishes living from non-living systems (or, as a *Nature* (2005) editorial put it, that distinguishes a live cat from a dead one), but it lies at the heart of that question of just what kind of science a science of life might be. Certainly, it lay at the heart of Immanuel Kant's (1724-1804) concern about the prospects for a science of life. "Organisms," he wrote in the second Part of the *Critique of Judgment* (1790/1993), are the beings that "first afford objective reality to the conception of an *end* that is an end of *nature* and not a practical end. They supply natural science with the basis for a teleology . . . that would otherwise be absolutely unjustifiable to introduce into that science seeing that we are quite unable to perceive a priori the possibility of such a kind of causality" (pp. 558, 66). For Kant, it was precisely here that the fundamental difficulty for a scientific approach to life is to be found: in the fact that organisms are systems that behave *as if* having a purpose, goals, ends (or functions) of their own. (It is true that the term Kant actually used was not function, but rather *Naturzwecke*, translated as "an end of nature" or "natural purpose," but I would argue that all of these terms are equally effective in capturing what was essential to his concerns.)

Kant did not believe that organisms really were teleological, but rather that we have no way of understanding them other than by thinking of them as teleological. Perhaps he should have written, "They supply natural science with the basis of an *as-if* teleology"—something totally missing from natural science as he knew it (which basically meant Newtonian mechanics). Indeed, this was the reason that he famously (or infamously) concluded that "there will never be a Newton for a blade of grass." The kind of organization instantiated by organisms cannot be obtained either from Newton's laws themselves, or from any other set of laws of that kind. As he wrote, it is "contrary to reason that crude matter on its own should have structured itself originally in terms of mechanical laws, that life could have sprung from the nature of what is lifeless, and that matter could have molded itself on its own into the form of a self-preserving purposiveness" (Kant, 1987, p. 311). Accordingly, if biology is to be a science, it would clearly have to be a different kind of science.

As Kant saw it, the difference between the life and the physical sciences lay precisely in the peculiar part-whole organization of living beings, or, as he suggested (in what was, in fact, the first use of the term), in their “self-organization”:

In such a natural product as this every part is thought as *owing* its presence to the agency of all the remaining parts, and also as existing *for the sake of the others* and of the whole, that is as an instrument, or organ . . . the part must be an organ *producing* the other parts, each, consequently, reciprocally producing the others. . . . Only under these conditions and upon these terms can such a product be an *organized* and *self-organized being*, and, as such, be called a *physical end*. (Kant, 1987, p. 557)

An organism, in short, is “an organized natural product is one in which every part is reciprocally both end and means. In such a product nothing is in vain, without an end, or to be ascribed to a blind mechanism of nature” (p. 558). In fact, Kant does not exclude the possibility that such products are intrinsically mechanical, but only the possibility that we can ever fully explain them in terms of blind mechanism, given the limits to our powers of judgment. Which is not to say that we should not try, but only that we must accept our own cognitive limits. As Alix Cohen (2007) explains:

It is reasonable, even praiseworthy, to try to explain natural products in terms of natural mechanism as long as there is some probability of success. Indeed, if we give up this attempt, we must not do so on the ground that it is *intrinsically* impossible to find the purposiveness of nature by following this route, but only on the ground that it is impossible *for us* as human beings. (p. 115)

Indeed, Kant’s argument is that it is precisely because of our limitations that we must use the concept of teleology in our investigations of biological systems.

This work *Critique of Judgment*, written in 1790, might well be regarded as the first philosophical treatise on systems biology, even though that term never appears as such. Nor does it appear in any of the many research efforts that Kant’s discussion inspired over the next half-century, especially in Germany. What these efforts shared was a commitment to finding a way of integrating teleology and mechanism in the analysis of biological systems, qualifying in Tim Lenoir’s (1982) view as a research tradition that he dubbed “teleo-mechanist.” But whatever the successes of this tradition, their unifying goal soon fell out of favor, effectively drowned out by the increasing polarization between mechanistic science and a purposeful divine architect, on the one hand, and by Darwin’s great success, on the other. Indeed, even Kant’s essay on teleological judgment, the second part of his *Critique*, fell into neglect. But with the rise of systems biology over the last few years, it is hard not to hark back to this earlier tradition, and, more specifically, to the concerns of Kant’s inquiry.

5 Cybernetics and Bernard Machines

The chief difference between then and now, between Kant’s despair of ever being able to provide a mechanistic account of natural ends, and the optimism of our own time, came out of the development of mechanisms that promised a self-directed

functioning, in the middle of the twentieth century. “Out of the wickedness of war,” as Warren Weaver (1948, p. 540) wrote shortly after the end of World War II, emerged not only a new machine, but also a new vision of a science of the inanimate: a science based on principles of feedback and circular causality, and aimed at the mechanical implementation of exactly the kind of purposive behavior that so worried Kant. I am referring to the birth of *cybernetics*, a science that would repudiate the very distinction between organism and machine on which Kant’s concept of self-organization was predicated.

Certainly, the early vision associated with cybernetics promised far more than it delivered, conspicuously failing to fill the gap between self-piloting aircraft and living organisms. Nevertheless, since then, appreciation of the central role of feedback in the design (or organization) of functional units has never flagged and, in recent years, this concept has come to play a more prominent role in the analysis of regulatory systems in biology. It seems to me that contemporary methods of analysis of feedback in cybernetics just might be able to fill that critical gap, providing a mechanistic (i.e., physical and chemical) account of, at least, some cases of biological function.

Scott Turner (2007), borrowing from his colleague Cosma Shalizi, has recently introduced a felicitous term into the literature that I believe will serve well for the minimalist conception of function I proposed above. Turner is interested in “agents (or organs) of homeostasis” and, referring to the famous physiologist most associated with homeostasis, Claude Bernard (1813–1878), he calls these agents *Bernard machines*. Bernard machines are devices for creating (and maintaining) a fixed milieu either inside the organism or immediately surrounding it; they are the mainstay of physiology, or, rather, they are embodiments of physiology.

In terms of my earlier discussion of function, a Bernard machine is exactly what is required to turn the rain–cloud–river cycle into a system with function; a Bernard machine is the mechanism that introduces function into such a system. Such devices are ubiquitous in biological systems, yet extremely complex from an evolutionary point of view (even when considered simple from an engineering perspective). Furthermore, and clearly attesting to the power of contemporary systems biology (especially of those efforts that proceed, simultaneously, from the bottom up and the top down), there are now a number of examples in which a naturalist account of such mechanisms is either available or close to available. I have chosen two cases to illustrate.

My first case is bacterial chemotaxis, the ability of bacteria to sense and move toward an attractant (e.g., a nutrient), or away from a repellent (e.g., a toxin). Over the last few years, biologists have unraveled virtually the entire structure of this system—indeed, bacterial chemotaxis might be said to be a poster child of systems biology. The machinery that has evolved to make chemotaxis possible, itself, is built out of relatively sophisticated, and pre-existing, structures, the main components of which are the flagella (the motor system) and the chemical receptors (the sensors). The paradigmatic example is *E. coli*, where each bacterium has several flagella, capable of rotating clockwise and counter-clockwise. Given the helical structure of the individual flagellum, counter-clockwise motion has the effect of aligning them into a single rotating bundle, leading to motion in a straight line; conversely, clockwise rotation disrupts the bundle, causing the bacterium to tumble in place. The net

effect is a random motion of alternating *straight swims* and *tumbles*. But the addition of a simple feedback between the receptors and flagella that regulates the direction of rotation according to chemical concentration leads, straightforwardly, to directional motion.

The actual system of feedback that *E. coli* employs is at least one step up from the simplest possible. In particular, it employs a mechanism allowing temporal comparison of concentrations, linked to the motor system in such a way that the time between tumbles is reduced if the bacterium is going in the “right” direction, thereby leading to a more direct chemotactic response than would otherwise have been possible. Over the course of evolution, still further feedback loops have been added, resulting in an extraordinarily robust and efficient device for maintaining a favorable environment for the modern bacterium—an environment free of toxins in the case of negative chemotaxis, or rich in nutrients in the case of positive chemotaxis.

This is a Bernard machine *par excellence*, but it is one that regulates the environment of the bacterium by moving the organism until its local environment is improved, rather than by directly changing that environment (as, for example, a thermostat would). It works by means of a hierarchy of feedback loops between the sensory and motor system that are now well-understood, and that have been effectively modeled in terms of the chemical and physical processes involved. How such a machine came to be built, however, is another question, and that process is not well-understood. Yet it can safely be said that it, too, depended on a kind of feedback; more specifically, on feedback between variations in chemical processes, changes in the physical, material environment these variations lead to, and shifts in the selective pressure operating on the chemical innovations caused by the changed physical environment. Here, function is built, as it were, by a kind of evolutionary transduction of the interactions between chemical reactions and the physical dynamics of the spatial-temporal-material medium in which these reactions occur.

My second example of a Bernard machine is one of Turner’s own examples: the mound, built by a colony of termites that serves as a wind tunnel, regulating the level of CO₂ (and oxygen) in the termite nest. Here, the environment of the colony is maintained at a more or less constant level of comfort, not by moving the colony, but by the work of individual termites in constructing a physical device that regulates the flow of wind, and that, by so doing, works to stabilize the temperature and concentration of gases in the nest. Unlike the machinery of chemotaxis, this mechanism is not part of the body of the individual organism, but rather part of the organism’s environment. Equivalently, it might be said to be a component (indeed, a necessary component) of the extended body of the colony. Without it, the colony could not survive.

Here is the problem the termites face: A typical colony begins with a single queen and king, housed under the surface of the soil, with impressive reproductive powers. In a short time, the colony reaches a population of approximately a million termites, with the respiratory requirement roughly that of a rabbit (Turner tells us that when you add in the respiratory needs of the accompanying fungi, the respiratory need of the colony approximates that of a goat). Yet, throughout this explosive growth in population, the atmosphere of the nest—viz., the temperature, concentrations of O₂ and CO₂—remains quite stable. The question is, “How is this accomplished?”

Turner answers the question as follows. The behavioral repertoire of the individual termite includes two actions: picking up grains of soil and gluing them down. Which action is performed depends on CO₂ level, with a high probability of picking up the grain of soil when the level of CO₂ is high concentration, and of gluing it down at low concentrations. In other words, just as with the bacteria undergoing chemotaxis, each termite is equipped with a sensor that is coupled to its behavior. The link between the levels of CO₂ recorded by the sensor, and the activity of the termite, guides the construction of a mound in which the rate of air circulation is effectively matched to the metabolic needs of the colony. The mound, one would have to say, has a function, namely, to maintain the atmosphere of the nest at comfortable levels. We might call this an *atmospherostat*, built not by intentional agents, but by mindless termites who know only how to pick up and deposit grains of dirt at rates that depend on the concentration of CO₂. But this simple sensor-behavior link in the individual termite, no more complex than that found in the bacterium, leads, on the level of the colony, to an ongoing and apparently purposive rearrangement of the soil between the nest and the open. Indeed, the net result is the construction and continual reconstruction of a mound that, at any given moment, operates as a well-designed wind tunnel adapted to the respiratory needs of the colony.

Once again, the presence of feedback loops is of key importance. The physical structure of the mound determines (feeds back on) the flow of air through the mound (the air, of course, includes CO₂), the distribution of CO₂ determines (feeds back on) the activity of the termites in their subsequent building activities, and the activity of the termites results in higher levels of CO₂. The net result of all this feedback is a regulatory system of air-mound-termites with exactly the same logic as that of the thermostat in your living room, with the striking difference that no engineer was required to either design, build, or install the termite mound.

Our understanding of the functioning of this Bernard machine is less well-developed than that of bacterial chemotaxis, but the broad outline is clear. And here too, nothing as yet has been said about the origin (or evolution) of such a machine. Yet, it seems safe to presume the operation of the same kind of feedback as I hypothesize for the evolution of the mechanism for bacterial chemotaxis, namely, between: variations in chemical processes (genetic mutations); changes in the physical, material environment to which these variations lead; and shifts in the selective pressure operating on the chemical innovations caused by the changed physical environment. Here too, I suggest that the coupling between sensor and behavior required for function emerged by evolutionary transduction of the interactions between chemical reactions and the physical dynamics of the spatial-temporal-material medium in which these reactions occur.

6 A Guarded Optimism

Still, we are not home free. At best, the examples I offer provide evidence of partial successes in the effort to “reduce biological explanations to explanations in chemistry and/or physics.” Indeed, they reveal an ambiguity in the very question. It is one thing to explain how chemotaxis works, or how termites are able to construct wind tunnels that regulate their environment in terms of chemistry and physics, but quite

another to explain how such complex and exquisitely organized systems could have first arisen. Or, as Kant put the question, how “crude matter on its own should have structured itself originally in terms of mechanical laws, that life could have sprung from the nature of what is lifeless, and that matter could have moulded itself on its own into the form of a self-preserving purposiveness.” I have put forth some very general suggestions, but the task of constructing concrete models that would actually be capable of generating such devices is enormously difficult, and efforts appropriate to the task have only begun. Such efforts would have to take into account processes of self-assembly and self-organization in multi-level systems, operating on multiple spatial and temporal scales, in which (as Fontana and Buss have emphasized) the internal structure and properties of the component elements are themselves responsive to the dynamics of the system.

Yet, it seems to me that the task is now defined with sufficient clarity to support some optimism. What kind of explanation such models will yield, whether it would fall within the range of human cognitive capacities to grasp, or whether it would require reliance on computers that are so much better able than we are to handle all this complexity—these, of course, are other questions altogether. It is just possible that, in the end, if and when we succeed in “reducing” biological explanations to physics and chemistry, Kant will still have proven to be right about the relation between such accounts and human judgment.

Postscript: Counterpoint

As I expected it would be, it is difficult to find much difference between John Dupré’s position and my own. But there are a couple of places that indicate some small disagreement.

First, I would resist his paraphrase of Bedau’s discussion of weak emergence; rather, I would say that it is the interactions among the parts—and *not* their behavior—that (apart from stochastic input) “determine” the behavior of the whole. In the next paragraph, he refers to the “properties of the parts.” But, in my view, it is important to distinguish *interactions*, *behavior*, and *properties*, especially in constructing models of complex systems.

Second, our uses of the term *downward causation* probably need distinguishing. I use it to refer to a wide range of influences that global properties have on the parts, including not only their activity (in the case of genes), but their very identity. The properties of a cell, for example, are at least partly determined by transcription of DNA but, in turn, cellular properties also determine which sequences are to be transcribed, in which combinations, and in what order. Indeed, the very definition of what (if anything) a gene *is* depends on the properties of the cell in which the DNA is embedded.

Finally—and here is probably our biggest difference—I find Dupré’s distinction between *context* and *interactions* problematic. Or, perhaps it would be better to say *artificial*. For example, I agree that one might argue that chaperones can be seen as embodying yet another kind of interaction between protein, and I can see nothing wrong with such an argument. Indeed, I would suggest that what Dupré calls *context* is simply all those other factors/molecules whose interactions with the object or system in question have not been made explicit and, hence, have not been included in the description.

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