Chapter Two

Radical Provincialism in the Life Sciences

A Review of Rupert Sheldrake's A New Science of Life

When Sheldrake's book *A New Science of Life: The Hypothesis of Formative Causation* first appeared in 1981, it received considerable attention from both scientists and laypersons. Some hailed the book as a radically new and viable approach to a vast range of scientific issues. Its opponents, on the other hand, denounced it strongly as (at best) a work of indefensible eccentricity. Naturally, this polarization of opinion caught my attention. Because the work excited such strong contrasting positions, I figured it was likely to be interesting, and *A New Science of Life* was certainly that. However, my own assessment was that Sheldrake's staunchest supporters and detractors were both wrong: Sheldrake's view of formative causation was neither viable nor as radical as it seemed. But it wasn't crazy either; in fact, Sheldrake's proposal revealed considerable intelligence, insight, and originality. Nevertheless, it was seriously flawed, and to my surprise I found it to be flawed for the same reasons as the theories Sheldrake was concerned with rejecting.

Shedrake's treatise on formative causation is now in its third edition. In fact, the book's cover describes its latest reincarnation as a "fully revised edition" (it appeared in the United States under the title *Morphic Resonance*).[1] However, although Sheldrake did modify the original text somewhat and add some new material, the fundamental flaws of the book remain intact. So I believe the time has come for a reappraisal. But before launching into my critique, I want to make something very clear, especially since in other chapters I frequently express my contempt for various of my academic or scientific colleagues, and also since I know that many people—outside of

philosophy at least—have trouble distinguishing sharp and sustained intellectual criticism from personal attacks. I know Rupert Sheldrake. I like him personally—in fact, I consider him a friend—and I hold him in very high esteem. I think he's exceptionally smart, intellectually serious and probing, and unusually creative and innovative as an experimentalist. I just think he's dead wrong about the view he propounds in his book.

Contrary to what some readers might think, there's a good reason for discussing the topic of formative causation in the context of the present volume. That's because the errors I believe Sheldrake commits are standard mechanistic errors, crucially similar to those undermining more familiar theoretical programs, particularly in the behavioral sciences. These errors are often very seductive (and by no means foolish), and some of them I discuss in other chapters and in other publications. My hope is that by seeing how classic mechanistic confusions manifest in and sabotage Sheldrake's theorizing about formative causation, readers will be better able to appreciate both the nature and scope of those errors, and hopefully they'll be better able to detect inevitable new versions of them when they appear.

BACKGROUND

Sheldrake argues that the life sciences have been strikingly unable to account satisfactorily for some fascinating and obviously important organic phenomena. The reason, he claims, is that those sciences have simply adopted the mechanistic assumptions common to other areas of the physical sciences. In particular, Sheldrake wants to resolve certain outstanding problems of *biological morphogenesis*, which he defines (following Needham) as "the coming-into-being of characteristic and specific form in living organisms."[2] From Sheldrake's viewpoint, the main puzzles of biological morphogenesis are as follows: (a) How do biological forms develop from the relatively simple structures present in the egg at the start of development? (b) How are systems able to *regulate*? That is, what explains the fact that "if a part of a developing system is removed (or if an additional part is added), the system continued to develop in such a way that a more or less normal structure is produced"?[3] (c) How are organisms able to *regenerate*—that is, replace or restore damaged structures? (d) How are we able to explain *reproduction*, in which "a detached part of the parent becomes a new organism; a part becomes a whole"?[4]

Sheldrake argues that a mechanistic science, which attempts to explain all the phenomena of life (including human behavior) in terms of physics, is impotent to solve the above puzzles. But he also thinks that a *vitalistic* alternative to mechanism is likewise inadequate. The fatal flaw of vitalism, Sheldrake claims, is that it posits an unbridgeable causal gap between two

radically different kinds of thing—namely, a nonphysical entelechy (or vital force) and the physical world. According to Sheldrake, the only remaining alternative to mechanism and vitalism—and the approach he endorses—is a form of *organicism*. Rather than attempting to explain the physical facts of morphogenesis in terms of a nonphysical entelechy, the organicist posits evolutionary biological principles as primitive (rather than emergent) features of nature, extending even to the domains carved out by physics and chemistry. Sheldrake cashes out his version of organicism by describing the relevant biological regularities in terms of *morphogenetic fields*.

But before considering the details of Sheldrake's theory, I must say that I find his rejection of vitalism unconvincing. Whether or not vitalism is tenable, it won't fail for the reason Sheldrake suggests.

I've argued elsewhere that causal links are essentially explanatory links.[5] Relating two states of affairs as cause and effect is, like giving directions, a way of systematically leading a person—in this case, conceptually—from one place to another. But nothing prohibits cause and effect from being of different ontological types. As I'll illustrate in more detail below, in some contexts it's perfectly appropriate and illuminating to posit causal connections between different *kinds* of states of affairs, in much the same way as context determines what sorts of directions are appropriate to a request for directions.

Sheldrake's criticism of vitalism seems merely to be a variant of a familiar criticism of Cartesian interactionistic dualism. In both cases, critics attack the view in perhaps its least plausible form. According to Descartes's version of dualism, mind and body are two distinct kinds of *stuff* or *substance*—the latter extended in space and the former unextended. But Descartes's model of causality was that of billiard-ball collisions—that is, causation by contact. Accordingly, he took causes to be spatiotemporally contiguous with and immediately prior to their effects. So critics correctly noted that something extended could neither push nor be pushed by something unextended.

However, this objection to Cartesian interactionism applies only to a *substance*-dualism, according to which causal interactions are supposed to occur between two distinct kinds of *thing*. Moreover, it relies on a model of causality suitable only for extended things, and then only for billiard-ball-type interactions between them. But other—and usually more sophisticated—forms of dualism are immune to this criticism. For example, one could adopt a *property* (rather than substance) dualism and also admit the legitimacy of causal explanations not fitting the billiard-ball model. That is, one could argue that psychological (or mentalistic) and physicalistic *descriptive categories* don't reduce one to the other, and then one can posit causal (explanatory) links between the two domains without having to explain how two different kinds of stuff can impinge on each other.

For instance, we could say that "the mind" is merely a general term for the class of mental events (or a certain *aspect* of what a person does), just as "the weather" is a general term for the class of meteorological events (or a certain aspect of planetary phenomena) and "the economy" is a general term for a class of financial transactions and institutions. Neither the mind, the weather, nor the economy need be construed as a *substance*. And clearly, nothing prohibits causal links between different kinds of events, identifiable on different levels of description. In fact, not only do we draw such causal connections all the time with a philosophically clear conscience, but we often find them extremely illuminating as ways of drawing conceptual links between different domains of phenomena. For example, we can find causal links between meteorological and sociological or economic phenomena, as when a hurricane leads to looting and a severely damaged local economy. To draw this kind of connection, we don't need to maintain that the three classes of phenomena are all expressible on some single level of description. Similarly, we're free to draw causal links between mental and physical events, even if statements about one don't reduce to statements about the other (or even if the two types of statements fail to reduce to some other common level of description).

Now as I see it, vitalism may have been articulated in a confused way, analogous to Descartes's version of dualism. Neither entelechy nor the mind need to be reified—that is, construed as a kind of (nonphysical) stuff. Vitalism, in its classic formulations, may be no more than a confused form of the view that, on the level at which we describe organic phenomena, there are facts and regularities unique to that level—that is, not reducible to (or translatable without residue into) another level of description. These *vital* facts and regularities would simply have no analysis in the terms appropriate to mechanical or impersonal forces or processes. But in that case, vitalists wouldn't need to explain how a nonphysical force can impinge on a physical organism (especially considering that not all causality is billiard-ball causality).

FORMATIVE CAUSATION: THE BASICS

In any case, Sheldrake proposes an organicist alternative to vitalism. Its main features are the following.

1. In addition to the familiar forms of energetic causation posited in physics, a further type of causation, *formative causation*, "imposes a spatial order on changes brought about by energetic causation."[6] In other words, this type of causation helps determine the internal and external structure of things in nature. Moreover, although formative causation is a kind of physical pro-

cess, it "is not itself energetic, nor is it reducible to the causation brought about by known physical fields."[7]

This last point, I should add, seems especially peculiar in light of Sheldrake's rejection of vitalism. He seems to be arguing for the kind of "action of unlike on unlike"[8] he considers fatal to that approach.

2. Each kind of *morphic unit* (or identifiable *thing* of a particular kind) in nature has its own characteristic *morphogenetic field*. These fields affect material systems when a characteristic part of a morphic unit—a *morphogenetic germ*—"becomes surrounded by, or embedded within, the morphogenetic field of the entire morphic unit. This field contains the morphic unit's virtual form, which is actualized as appropriate component parts come within its range of influence and fit into their appropriate relative positions."[9]

3. Morphogenetic fields affect morphic units by a process called *morphic resonance*. "This influence takes place through the morphogenetic field and depends on the systems' three-dimensional structures and patterns of vibration."[10] Morphic resonance can act on a morphic unit across space and time, as when the form of a morphic unit is determined by the forms of previous similar systems.

Sheldrake ingeniously applies these ideas to a vast range of organic phenomena, and he develops his theory in considerable detail. But the three proposals just mentioned are the heart of the theory, and their weaknesses are enough to sabotage it beyond salvation. Once these are brought into the open, the poverty of the hypothesis of formative causation becomes clear. It also becomes clear that Sheldrake's approach is fundamentally that of the mechanistic theories he wants to repudiate.

FORMATIVE CAUSATION: THE PROBLEMS

We can get a first glimmer of the problems with Sheldrake's theory by asking: Are there morphogenetic fields for every possible parsing of nature? In principle, of course, there are endless ways of dividing nature into object-kinds and event-kinds. Is each resulting type correlated with its own characteristic morphogenetic field? Sheldrake seems to think so. First, he claims that morphic units will be found in "biological and physical systems at all levels of complexity,"[11] and then he says explicitly, "Each kind of morphic unit has its own characteristic morphogenetic field."[12]

But the problem with a claim like this—as I've pointed out elsewhere[13] and in chapters 1 and 3 of the present volume—is that forms, objects, events, and kinds are not intrinsic to nature. There's no absolute inventory of things in nature. *We* decide, relative to some guiding purpose or set of interests, how to parse nature or history into objects, events, and kinds (including form-kinds—morphic units, if you will). Apart from some guiding set of interests

and their associated standards of relevance and importance, there's no reason to parse nature one way rather than another. In that respect, nature is intrinsically undifferentiated; no method of parsing enjoys inherent (or context-independent) priority over any other. But if objects, events, and so on aren't items from a prefabricated ontological storehouse and are instead merely elements of constantly evolving and shifting conceptual grids that we place over nature, then morphogenetic fields don't exist inherently *in* nature either.

To avoid misunderstanding, I should emphasize that none of this shows that nature has no structure or that we're not entitled to impute a structure or structures to nature. But nature doesn't have *a* structure, some *one* parsing into and arrangement of elements that enjoys inherent priority over all others. Rather, it has an infinite number of parsings and orderings, some better than others—and then only in relation to some goal or set of interests and needs. For example, what justifies our table of elements and their associated structures is that the list and analysis of elements fits into a successful scientific theory. And it's only against a background of inquiry or discussion that this scientific theory is our guide in determining the relevant inventory of things. As with any conceptual grid through which we view the world, our choice of descriptive categories (and the resulting structuring of nature) is justified by its utility, as a tool for systematizing our experience. (I'll return to this point shortly.)

But Sheldrake's view seems considerably less sophisticated than a pragmatic defense of kind terms and structural descriptions. Sheldrake seems to take the hard-line Platonist (i.e., essentialist) view that morphic units and their associated morphogenetic fields are *natural kinds*—that is, items in an interest-, purpose-, or context-independent set of natural furniture. He seems to think there is a final or preferred inventory of things or kinds (one including morphogenetic fields), not merely different inventories justified relative to their utility as intellectual tools. In fact, as we'll see shortly, that's the only way Sheldrake can make sense of his claim that morphogenetic fields can select other systems in order to act on them. If what counts as a thing, or system, or structure, is not built into nature but is instead determined only against a background of shifting organic concerns and goals, then there's no context-independent, privileged, or inherent basis for something even to be a morphogenetic field (or one rather than another) or for some morphogenetic fields (rather than others) to be causally active in nature. Another sign of this essentialism emerges in Sheldrake's remarks about similarity (which I discuss below). His views on that topic are really just another aspect of the same deep, Platonist error.

To see this more clearly, we should ask: How is it that "all past systems act upon a subsequent similar system by morphic resonance"?[14] Actually, we can ignore the temporal complexities of this claim and ask simply: How does a system's morphogenetic field *select* similar systems on which to exert its

influence? Sheldrake wants to say: by morphic resonance, which he conceives according to a *vibratory* or *tuning* model. But the answer to the question can't be given in purely structural (or topological) terms. *Similarity* is a concept that is neither formally definable nor analyzable independently of a context.

The appeal to morphic resonance involves a not-too-subtle retreat back to mechanistic thinking. Yet Sheldrake's theory can't work without it. Apparently, Sheldrake fails to see that similarity is no more inherent in nature than are our divisions or inventories of things. In fact, he makes the classic mistake that undermines (among other things) memory-trace theory (see chapter 1). I'm especially surprised to see the error occupy such a prominent place in Sheldrake's thinking, given his apparent grasp (at least when he wrote the first edition of his book) of H. A. Bursen's attack on trace theory.[15]

Anyway, Sheldrake explains morphic resonance by means of a tuning model, according to which objects resonate with each other when they vibrate at the same or similar frequencies. But this maneuver is doomed from the start. Not only is similarity not built into things, but the analogy between similarity and closeness of frequency is deceptively straightforward and greatly oversimplified. Actually, not even closeness of frequency can be as straightforward as Sheldrake suggests. The context-relative nature of closeness of frequency is illustrated clearly in cases of musical performance. Contemporary practice usually dictates tuning to A440, but so-called period instrument or "historically informed"[16] groups often tune instead to some frequency below that—for example, A438, A436, or an even lower pitch (e.g., A415). Nevertheless, they see themselves as playing the same piece *and in the same key* as ensembles tuning to A440. So here's a context in which A436 (say) could count as close to A440. But if some renegade member of an A436-tuned period instrument ensemble defiantly tuned instead to A440, in that context A440 and A436 would not count as similar or close. Differences in frequency can matter greatly on an even finer-grained scale—for example, whether one is tuning according to a tempered scale or a just-toned scale. I'll return to this issue below in connection with geometric congruence.

Sheldrake concedes that similar systems may differ in their specific features.[17] But he claims that a process of *automatic averaging* will bring their common features into alignment. This process, Sheldrake maintains, is analogous to that of producing composite photographs. Now first of all, Sheldrake fails to see that the *common features* that are brought into alignment may not be exactly the same from one system to the next. So what would explain how *these* differences get adjusted for? Another appeal to automatic averaging would start Sheldrake on just the sort of vicious regress that's fatal to memory-trace theory (as Sheldrake at one time seemed to realize). Second, the process of producing composite photographs is clearly not one of auto-

matic averaging. Composite photos are produced by a *person*, by someone who first must *decide* which features are relevant and similar and who then determines a method by which to align them. The process doesn't simply happen by itself; criteria or standards of relevance exist only against both local and global backgrounds of organic needs and practices.

At one point, Sheldrake remarks that absolute size is irrelevant to what a thing's form is. But the fact that he must point this out is a tacit concession that, under certain conditions, differences in size *might* lead us to classify two things as different in form. And here the poverty of the concept of morphic resonance stands out starkly. We must ask: What in nature (i.e., independent of human needs and interests) determines size limits on forms? The answer, of course, is *nothing*: *we* determine those limits, in different ways for different purposes. But then morphic resonance isn't a phenomenon built into nature, operating according to internal, inherent, structural principles or criteria.

My favorite example from geometry (elaborated more fully in chapter 1) should help clarify this point. Mathematicians often speak of *congruence* of different figures. But congruence—merely another name for "similarity of geometric form"—is widely recognized to be relativized to some rule of projection, to some mapping function we *choose* to adopt. Different standards of congruence are suitable for different purposes, and no one of them is inherently preferable to the others. For example, we might map a triangle only onto other triangles with the same horizontal orientation and the same internal angles, or we might allow triangles to be mapped onto triangles with a different horizontal orientation or with different angles. We might map isosceles triangles only onto isosceles triangles. But we could also regard isosceles and right triangles as congruent. We could regard all triangles as congruent and decline to map any triangle onto rectangles or circles; but we could also map a triangle onto these other types of figures and even onto lines. What sanctions any of our rules of projection is always something about the context of inquiry; it's never determined simply by the objects themselves. A question like "What other figures are congruent with an isosceles triangle?" has no answer whatsoever apart from an actual context of inquiry. But then, if we can only give a context-relative or *positional* account of similarity for geometric figures (and, by the way, for frequencies), things certainly look bleak for the concept of morphic resonance, especially when we move on to the more complex domains (e.g., human behavior) in which Sheldrake tries to deploy it.

I'll return shortly to the topic of behavior, since Sheldrake commits additional crucial errors in discussing that. First, however, let's look at another reason why Sheldrake's proposal fails to account for the phenomena he thinks are in urgent need of explanation. One genuinely interesting case he considers is how a certain region *r* of a developing organism could develop

more than one way—say, into either an eye or a limb. Sheldrake's explanation is that *r* comes under the influence either of the morphogenetic field associated with eyes or of the one associated with limbs. But how could this be, by Sheldrake's own account? Why should a given morphogenetic field pick out the right part, or *any* part, of the developing organism? We can't just say (as Sheldrake does) that *r* comes under the eye (or limb) field and that's why *r* develops into an eye (or limb). The reason is that the morphogenetic field is supposed to apply *mechanically* (by morphic resonance) to things of the appropriate structure. But *r*, *by hypothesis*, doesn't yet *have* that structure; rather, it's supposed to be *given* that structure by the morphogenetic field that selects it. So Sheldrake has offered no reason why the eye field, say, should influence a part of the developing organism that's not yet distinguished structurally from the region that develops into a limb (or not yet distinguished so much that it can develop in only one way). That part of the organism is not yet structured so that it can resonate only with the eye field; by hypothesis, it's still morphically flexible or labile. Before region *r* comes under the influence of the eye field, it *could* also come under the influence of, and resonate with, some quite different morphogenetic field. But if *r*'s structure is developmentally indeterminate—that is, if it's compatible with (and presumably equally similar to, resonance-wise) the structure of different sorts of morphogenetic fields, then Sheldrake's appeal to resonance between similar structures doesn't explain why *r*'s development should follow one course rather than another.

Sheldrake seems to propose one possible solution to this problem.[18] He appeals to *morphogenetic germs*, the characteristic parts of morphic units, and he says that *primary* morphogenetic fields determine characteristic germs on which different *secondary* morphogenetic fields can then act in different regions of the organism. But this simply won't do. Why should a primary field pick out—presumably by morphic resonance—a part of the organism not yet distinguished enough structurally to be a characteristic morphogenetic germ? Clearly, the selectivity problem mentioned in the previous paragraph has just been pushed back a stage. There's no reason, on Sheldrake's own principles, for an undeveloped and structurally indeterminate part of the organism to resonate with *any* more specific or highly structured morphogenetic field. So long as morphogenetic fields resonate with, and thereby affect, only those items having a similar structure, this problem is insuperable. Yet without the process of morphic resonance, Sheldrake's theory can say nothing. It can only point to the phenomena or regularities to which morphogenetic fields are supposed to correspond.[19]

MORPHOGENETIC FIELDS AND BEHAVIOR

When Sheldrake finally turns to the topic of behavior, the errors mount and become more striking. Here, interestingly, Sheldrake's views are anything but novel; behavioral scientists frequently commit the same errors in one form or another. In fact, Sheldrake's comments on behavior are often as disappointingly elementary as those of your average behaviorist (see chapter 3 for more on that topic). Nevertheless, Sheldrake's discussion has the virtue of bringing his underlying explanatory principles close to the surface, where their flaws stand out clearly.

First of all, Sheldrake repeatedly blurs the critical distinction between *action* and *movement*. Briefly stated, all actions involve movement, but not every movement is an action. Moreover, actions can be described only in mentalistic or intentional terms, whereas movements are describable without reference to intent, in purely physical or mechanical terms. You might say that actions are movements intended a certain way. For example, the raising of one's eyebrow is a movement (or series of movements); but as an action, it may be a sign of astonishment, a sexual invitation, or a way of yielding to an opthamologist's examination. Movements, then, are functionally and intentionally indeterminate; the same movement may be (or be a part of) different actions. If any part of organic activities is describable in purely physical terms, movements are the most promising candidates. But which action results from a movement can only be described both intentionally and relative to the movement's *position* in a context. And even then, nothing intrinsic to a situation determines which action (rather than another) occurs. Likewise, a given structure may be associated with an indefinite number of different functions.

But Sheldrake seems not to appreciate these points, although subtle changes between the original and latest edition of the book have obscured this somewhat. Nevertheless, the changes from the first edition are merely cosmetic. Originally, Sheldrake classified both heart-beating and mating behavior as movements; in the new edition, he says searching for a mate is a form of behavior. But in his new edition, Sheldrake still maintains that action (or behavior) is explicable and analyzable as a series of movements, something to which a resonating field corresponds and whose characteristic resonance mechanically captures the distinctive pattern of the behavior.

Now even if similarity of movement could be analyzed formally, so that the concept of morphic resonance could apply to movements, similarity of action certainly can't be analyzed that way (I'll develop this point below). However, not even similarity of movement can be analyzed formally. As my earlier geometric example shows, no kind of similarity exists intrinsically between two things—that is, merely in virtue of their inherent properties. But it's easy to generate additional and more germane examples focusing on

movements specifically. Consider: What natural law or rule of projection could determine, say, whether a flea and an elephant display a similar movement or whether a young beginner's golf swing contains the same movements as that of Tiger Woods? Obviously, whether those items count as similar or not depends on prevailing, but ephemeral and not even remotely universal, standards of relevance and importance. For example, the golf swings of Tiger Woods and the novice might count as similar in a context where we're comparing golf swings to tennis swings, but not when the focus is on fine differences between the techniques of different golfers. The flea and the elephant example is equally obvious; sometimes size matters, and sometimes it doesn't. And it's really that simple, no matter how scientists and philosophers pretentiously hide their confusion beneath imposing technical terms. It's also important to realize that Sheldrake's error on this issue isn't even remotely scientific. He's once again making a deep philosophical mistake about the nature of similarity—namely, assuming that similarity or dissimilarity is an inherent, rather than positional and context-relative, relation between two things.

But Sheldrake goes further. He actually proposes that there could be morphogenetic fields for behavioral types, including searching for a mate and courtship, as well as for habits generally. Apparently, he fails to grasp (a) that nothing done by an organism is inherently of a given behavioral type, and (b) that behavior, described topologically or structurally (say, in terms of movements), is functionally indeterminate. This means, among other things, that there's no limit to the range of activities that can exemplify a given behavioral type; virtually any activity, given the right surrounding history, can exemplify any behavioral type. Moreover, any activity that does exemplify a type does so because of the way *we* construe its position in a bit of history or against a background of human activity, needs, and traditions. The exemplification of a behavioral type isn't inherent in nature. It's inexorably relativized to shifting standards of relevance imposed by conscious agents in a living context. But that means there can be no purely structural or formally specifiable *essence* to that type—certainly nothing like a specifiable frequency with which some things but not others may resonate.

The procrustean and impoverished nature of Sheldrake's conception of behavior emerges especially clearly in his chapters 9 and 11. He claims that while human behavior is more flexible than that of other organisms, "this flexibility is confined to the early stages of a behavioural sequence, and especially to the initial appetitive phase; the later stages, and in particular the final stage, the consummatory act, are performed in a stereotyped manner as fixed action patterns."[20] So, for example, with regard to feeding, "people obtain their food by all sorts of different methods. . . . Then the food is prepared and cooked in many different ways, and placed in the mouth by a variety of means. . . . But there is little difference in the way the food is

chewed, and the consummatory act of the whole motor field of feeding, swallowing, is similar in all people."[21]

Although feeding is a more uniform and ritualized behavior than many (giving the example of feeding behavior at least superficial plausibility here), it's still very easy to demonstrate the inadequacies of Sheldrake's position. Sheldrake suggests that feeding culminates in stereotyped processes. What makes a given sequence of movements a case of feeding is that, like behavioral fields generally, it terminates in one of the "limited number of [characteristic] goals given by [inherited] motor fields."[22] In the case of feeding, these goals are apparently chewing and swallowing (the latter being the "consummatory act of the whole motor field of feeding"). I doubt that Sheldrake realizes how he commits himself to a very unscientific thesis: that there exists a defining set of goals for feeding, a Platonic essence that can be formally described in topological terms and that permits resonance only between things of the appropriate type (or essence). Had Sheldrake described this in more archaic Platonic terms (e.g., a thing's feeding-ness), his book might have attracted even more derision than it has already received. However, by couching the view in more obscurantist technical terms, appealing to fields and resonance and morphic units, it sounds far more sophisticated. However, just as appeals to memory traces are no more intelligible or plausible than Plato's suggestion that memories are like impressions in wax (see chapter 1), Sheldrake's view is only a cosmetically altered twist on a deservedly disreputable and deeply nonsensical position.

This is easy to see even in the case of feeding. First, there's no antecedently specifiable and physically describable set of goals that distinctively defines the feeding process. And second, there's no such goal that is inherently a feeding goal. To demonstrate the first point, it's enough to consider the ways *all* organisms feed. For one thing, not all organic feeding processes terminate in chewing and swallowing. The range of organic methods of feeding is enormous, and in principle it's unlimited. Yet they're all ways of feeding. And it *is* fair, incidentally, to discuss the entire range of feeding activities rather than just human feeding behavior. If the behaviors are all ways of feeding, then according to Sheldrake's own principles there's an organic morphic unit of feeding behavior that falls under a grand morphogenetic field for feeding. But of course the varieties of human feeding alone assume many forms, even in the final stages. People may be fed intravenously or may eat nothing but liquids; so neither chewing nor swallowing is necessary for human feeding. Moreover, if some human were to feed in a currently unprecedented fashion (say, by absorbing nutrients through the skin in a food "bath," by inhaling nutritional smoke, or by using food suppositories), these acts would still be acts of feeding, despite their failing to conform to whatever limited set of goals we happen—by what amounts to nothing more than a historical accident—to specify for that behavior. So human

feeding isn't defined relative to empirically necessary or fixed—much less inherited—goals.

With regard to the second point above, no human activity like chewing followed by swallowing inherently terminates a feeding event. Other sorts of events may also end with chewing and swallowing—for example, ingesting hallucinogenic mushrooms (hardly a case of feeding: it's engaged in for reasons other than organic sustenance or satisfaction of hunger), taking an appetite-suppressant candy (something intended to *frustrate* the eating process), chewing and swallowing an emetic, or chewing and swallowing a cyanide capsule in an act of patriotic suicide.

Furthermore, it's preposterous to think that there could be a nonpositional formal or structural correlate to the chewing or swallowing processes, something that—independently of a context—determines whether two such events are similar. But that's precisely what's presupposed by Sheldrake's claim that there are morphogenetic fields for those activities. Again, Sheldrake relies on the unacceptable view that similarity is built into nature. But just as geometric congruence isn't a static or inherent relation obtaining between two figures, whether or not chewing or swallowing events are similar likewise depends on context-relative standards of relevance. Even if Sheldrake is correct in claiming that "there is little difference in the way . . . food is chewed," that claim is true only relative to a certain detached and global perspective appropriate to Sheldrake's theoretical inquiry. In other familiar situations, Sheldrake's claim would be considered obviously false. More generally, whether the differences *make* a difference, and *which* differences matter, is always something we decide relative to a background of needs and interests. It's never a formally or inherently specifiable property of the activities themselves.

For example, a mother may reprimand her child for chewing on just one side of the mouth, for chewing too quickly, or for gulping down food in a boorish fashion. Implicit in the reprimand is the assertion of a *dissimilarity* between the child's activity and the mother's allegedly more correct procedures. Dentists, too, might make analogous observations for patients who (say) need to correct their methods of chewing for the sake of oral welfare. And of course, nonhuman organisms exhibit a further variety in ways of chewing whose differences would be precisely the point of a PhD thesis on chewing styles across species. Now if geometric congruence isn't an inherent, static relation between geometric figures, why should similarity of chewing activities be a relation we can specify independently of shifting background standards of relevance? The ease with which Sheldrake supposes all chewing events to be inherently similar shows either (a) that he's unaware of the context-relative standards of relevance and similarity on which he relies (those appropriate to his wide-ranging scientific inquiry but not to the perspective of the disapproving mother, dentist, or PhD candidate) or (b) that he

thinks his criteria are somehow privileged or inherently fundamental. The former would be an example of methodological myopia; the latter, an example of metaphysical chauvinism.

I've learned over the years that my position on the nature of similarity is often, and somewhat reliably, misconstrued as an attack on the *concept* of similarity. Sheldrake himself did this (though, of course, in a characteristically sophisticated way) in correspondence over my criticisms,[23] and perhaps readers will be better able to avoid this misunderstanding if we examine that correspondence briefly. Sheldrake wrote to me: "You argued that similarity was a *slippery concept with no basis in nature*, rather something created in an *arbitrary* way by our own minds" (italics added). First, I never said that similarity had no basis in nature. I claim merely that whether two things count as similar is not something *intrinsic* to nature—that is, forced on us by nature itself. In other words, I've claimed only that similarity isn't a static or inherent two-term relationship between the similar things. Furthermore, I never said that determinations of similarity are arbitrary. On the contrary, determinations of similarity can have all sorts of reasonable pragmatic justifications. It's clearly a non sequitur to conclude that because similarity relations aren't intrinsic and static that determinations of similarity are arbitrary.

Sheldrake also wrote:

> Your attack on the idea of similarity is not specific to my own work, but to all of science as we know it. It is a fundamental tenet of physics that all hydrogen atoms are similar, and behave in a similar way. That is why the spectral lines of distant galaxies can be interpreted by astronomers to give information about parts of the universe that are billions of light years away. Chemists assume that all acetone molecules are similar, and hence exhibit similar properties. Biologists assume that all cell membranes are similar in their general structure involving lipids and proteins, that all genes of a given kind are similar, that all protein molecules of a given kind are similar, and that all members of species are similar. Species are defined in terms of similarity. Genera depend on similarity too, but literally less of a specific kind. Families depend on remoter forms of similarity, but one sufficiently strong to suggest a common ancestor.

I'm grateful to Sheldrake for having presented this objection so clearly and persuasively. But again, I'm not attacking or criticizing the concept of similarity. I'm trying to clarify it. I'm not saying that similarity claims are never correct, or that they're never appropriate or justified relative to a background of assumptions and theoretical interests. To repeat, I'm saying only that similarity is not merely a static two-term relation between the things said to be similar. For example, I grant that two things can each have seven grams of mass, and if so, they might count as similar in virtue of the fact that the predicate "seven grams of mass" can appropriately apply to both. But those two things may also count as dissimilar in virtue of one of their many other

properties—say, if one is red and the other green, or if only one is organic, metallic, brittle, poisonous, or an appendage, or rectangular, manufactured, or valuable. The two things aren't intrinsically similar or dissimilar, and nature doesn't decide for us which kind of property trumps the other and therefore which things (according to Sheldrake) should resonate with which other things.

To return once again to my geometric example, whether or not two things count as similar depends on matters of relevance, and that's settled only against a background of organic intentions and activity, both local and global—never simply by or between the objects themselves. To put this in Sheldrake-like terms, nothing in nature, by itself, can select which other kinds of objects a given triangle should resonate with. Any triangle? Or only those with the same angles, or the same orientation, or sides of similar length? Or any geometric figure? *We* can plausibly posit similarity (or rules of projection) in any of those situations, but independently of some background there's no reason to assert that any object is similar to any other. In fact, the only background that would allow positing relations of similarity independently of some human or other organic needs etc., would be a divine background. God's perspective would, I suppose, trump that of any of his creatures. But my guess is that this deistic move is not one Sheldrake is prepared to make. In any case, it's not what the hypothesis of formative causation asserts.

Moreover, when trying to get a handle on the nature of similarity, in some ways it's potentially misleading to focus on a property like having seven grams of mass or on examples like the one from geometry. Those examples can be deceptively simple. In certain instances it's more helpful (or to the point) to consider (as Sheldrake eventually does) behavioral similarities. Because we can't characterize so neatly the relevant properties in virtue of which two behaviors count as similar, these cases may be more instructive exemplars of what similarity amounts to.

It should take only a moment's reflection to see why behavioral similarities are difficult to capture crisply in some set of necessary and sufficient topological features (as Sheldrake attempts by appealing to resonance). Consider, for example, the behavioral category of courtship (one of Sheldrake's own examples), and consider the enormous variety of things that can count as instances of courting behavior—for example, a caveman clubbing a cavewoman, a woman playing dumb so as not to threaten her chauvinistic and insecure date, erotic conversation over dinner, placing an ad in a newspaper's "Personal" column or through an online dating service, bragging to a date about one's possessions, flaunting one's exceptional physical endowments, lying to a date to conceal one's sordid past, writing poems to one's sweetheart, serenading one's beloved, purchasing an expensive gift, fighting a duel, clowning around at a fraternity party, merely combing one's hair, wear-

ing clean clothes, or cutting back on one's conversational reliance on expletives, and on and on. Moreover, each of these subsets of courting behavior (and only human courting behavior at that) can be exemplified in endless ways. And in the present context, what also matters is that there are endless numbers of backgrounds against which any two of these courting behaviors would count as *dis*similar.

PREDICTION AND EXPLANATION

Some might protest that Sheldrake's theory predicts occurrences different from those predicted by rival theories—for example, concerning the rapidity of learning a task or the maintenance and proliferation of new forms (including learned behaviors). For instance, Sheldrake predicts that if a large number of rats are trained to learn a new behavior, then any subsequent training of similar rats will be easier as a result. That is, similar rats—no matter how remote geographically from the original—will learn the new ability faster than the original group of rats. Unlike Lamarckism, Sheldrake's theory predicts this result for *all* similar rats—not just progeny of the original trainees. So wouldn't a successful test of these predictions vindicate Sheldrake's theory?

The answer, of course, is that even if the predictions turn out to be correct, that wouldn't be sufficient to warrant accepting the hypothesis of formative causation. It takes more than predictive utility to justify a theory; false (and even incoherent) theories can make true predictions. Besides (and surprisingly), Sheldrake makes no effort to rule out the rival hypothesis of experimenter expectancy effects in such cases, some of which offer striking parallels to the experiments Sheldrake discusses and which also have the virtue of connecting to a large and varied body of replicated results (see chapter 7 for more on expectancy effects).[24] Equally curiously, Sheldrake refrains from making conjectures about possible paranormal interactions between experimenters and nonhumans or between members of nonhuman species. I can understand why Sheldrake might have wanted to avoid that option, considering the already apparently radical nature of his proposals. But in the years since he first published *A New Science of Life*, Sheldrake has done a great deal of innovative and interesting experimental work in parapsychology. So his interest in the topic is hardly a secret (he even discreetly mentions some of his recent work in a few places—mostly footnotes). And appeals to ESP or PK would be obvious alternative explanations to that of formative causation—not simply (as Sheldrake entertains briefly) phenomena that might be explained in terms of morphic resonance.

Nevertheless, Sheldrake may have performed a service to science in drawing attention (for whatever reason) to phenomena or regularities that

merit our attention and which other theories have ignored or missed. But the fact remains that the conceptual underpinning of Sheldrake's theory is deeply defective, no matter how serendipitous some of its predictions may be. So Sheldrake's theory may have the virtue of pointing science in new and important directions. But it remains a false start nevertheless.

There's simply no reason to posit morphogenetic fields if we're forced to accept false or absurd presuppositions in order to explain how they work. But then there's no reason to posit morphogenetic fields at all. Unbuttressed by a plausible (or even coherent) mechanism of operation, the positing of morphogenetic fields adds nothing to the regularities they were designed to explain. Morphogenetic fields would merely be a new and technically imposing name for old or hitherto unappreciated phenomena.

So keep in mind that my objections to Sheldrake are compatible with the facts he alleges and predicts. That is, even if his theoretical explanations don't work, it may well be that there's some causal connection between, say, the widespread learning of a given ability and the greater ease with which subsequent populations learn it. All I'm arguing is that *if* Sheldrake's alleged facts *are* facts, and *if* they have an analytical explanation in terms of lower-level processes, then Sheldrake's proposed explanation is unsatisfactory. It could also be that the alleged facts are genuine facts but have no explanation (or analysis). I've already considered such a possibility in connection with Sheldrake's criticism of vitalism, but I'll say more below about the kinds of facts we can take to be primitive—that is, at scientific ground level.

EXPLANATORY LIMITS

In light of Sheldrake's avowed opposition to mechanistic theories in the life sciences, it's very interesting that his own view should turn out to be so classically mechanistic. Perhaps one reason is that Sheldrake doesn't understand what a mechanistic theory is. At this point, I need to review several issues considered repeatedly throughout this book. The overlap with other chapters is unavoidable, because the mechanistic thinking I want to attack appears in many different guises and in different theoretical arenas.

First of all, a mechanistic explanation is not simply one that explains a phenomenon in the language of physics. In fact, mechanistic theories can be dualistic or idealistic; the differences in these variants are merely differences in "hardware." What makes an explanation mechanistic is that it explains a system's or structure's function entirely in terms of the operations, interactions, and organization of its component parts. Moreover, this feature of a mechanism is something that can be captured in a *generalization*. That is, mechanisms exhibit regularities in their behavior or operation, and we can express those regularities by stating in a general way which initial conditions

or properties of the system lead to which results. For example, that's the way we explain how a machine's inner workings produce a certain output. In addition to specifying the underlying processes leading to that output, one requirement of this approach is to be able to state or define in a general way what the output is. But to do that in a manner that scientists find acceptable and useful, a loose characterization of the output or function of the mechanism isn't enough. Something more specific and precise is needed; in fact, what's needed are characterizations that specify *necessary and sufficient conditions* for something's being an instance of the mechanism or an instance of its function (or output).

To this extent, Sheldrake's theory is disappointingly conventional. By appealing to morphic resonance and to the alleged existence of essences or defining structures for *kinds* (including behavioral kinds), he commits the same errors that undermine memory-trace theory—in fact, all theories (including cognitive or computational psychological theories) positing physical correlates or mechanisms for kinds of organic or psychological phenomena. In fact, Sheldrake's theory attempts the reduction of *types* of behavioral states which even hard-core philosophical physicalists recognized long ago to be untenable. It's why sophisticated physicalists many years ago abandoned comparatively naive type-identity reductions of the mental to the physical in favor of token-identity theories—not that that actually helped.[25]

What else might account for Sheldrake's lapse into traditional mechanistic thinking? One likely possibility is that (in addition to erroneously equating mechanism with physicalism) Sheldrake failed to recognize a fundamental presupposition of most mechanistic theories—what I've called the *small-is-beautiful assumption*. According to this assumption, there can't be unanalyzable phenomena or facts at the observable level. Scientists agree, reasonably, that explanation by analysis (i.e., into constitutive lower-level processes) can't continue indefinitely. In other words, they admit that some regularities in nature are primitive in the sense that we can't go behind them and profitably ask *how* they occur. *That* they occur is simply a basic fact about the way the universe works, and there are no deeper corresponding regularities that explain why. In this way we arrive at one kind of scientific ground level. Now so far, this is fine; there's no problem in holding that some facts or regularities in nature should be considered as ultimate or primitive. However, most scientists go further and assume that these fundamental regularities can exist only at the level of the very small—say, the atomic, microscopic, biochemical, or neurological level, and never at the level of observable behavior. But that's simply an assumption, not an empirically established fact, and I believe antimechanists have marshaled powerful arguments against it, similar in many ways to the arguments I deployed against memory-trace theory in chapter 1.

In any case, had Sheldrake been willing to abandon this small-is-beautiful assumption, he would never have had to look *beneath* the surface of the phenomena of morphogenesis for an account of how they work or occur. He could have let the phenomena stand as primitive and unanalyzable biological or organic regularities, and then he wouldn't have needed to postulate morphogenetic fields and the literally unintelligible mechanism of morphic resonance by which they work. Moreover, stopping the search for vertical explanation at this point is neither unscientific nor a failure in understanding. In fact, it's a *victory* of understanding to figure out where analysis comes to an end and where regularities can't be analyzed further in terms of more primitive constitutive processes. Besides, not all explanation stops once we identify ground-level phenomena; only vertical explanation (explanation by analysis) grinds to a halt. Scientific explanations take many forms; explanation in terms of lower-level processes is only one of them. And in the realm of behavior (as I consider in more detail in chapter 3), explanation by analysis is especially wrongheaded. The discussion in that chapter helps illustrate the crucial point that some patterns emerge first at the observable level (including the level of behavior).

It's interesting, then—and I suppose ironic—that many view Sheldrake's theory as radical. In most important ways it's thoroughly traditional. Sheldrake has adopted wholesale the standard assumption that observable phenomena should be analyzed in terms of unobservable lower-level processes and mechanisms. He's accepted the parochial and received view that only by offering such analyses can a discipline be scientific or provide an understanding or explanation of the phenomena. And that's precisely the mistaken view which has failed so conspicuously in the behavioral sciences.

Granted, at times it seems as if Sheldrake wants to move in a different theoretical direction. He often seems eager to oppose the common view that nature operates according to immutable laws (presumably, those uncovered by an adequate physical theory). At least at these points in his book, Sheldrake embraces the nonstandard view that the physical sciences should be modeled to some extent on evolutionary (if not teleological) principles fundamental to the life sciences, of which formative causation is arguably the most important. But what Sheldrake seems not to appreciate is that his analysis of formative causation is merely the old mechanical view of nature in new garb.

Nevertheless, despite all my reservations concerning the tenability of Sheldrake's theory, I still consider *A New Science of Life* to be a serious, interesting, and thought-provoking work. For one thing, Sheldrake deserves to be commended for his care and ingenuity in working out the details of his hypothesis and for extending its scope to many domains. And even more important, his book has the virtue of pointing out the possible existence of hitherto unrecognized or underappreciated phenomena and regularities.

But (as I've insisted repeatedly throughout this book) no scientific theory is thoroughly empirical, and like many theories in science, Sheldrake's looks more empirical than it is. First of all, like all scientific theories, it rests on philosophical presuppositions. Every scientific theory starts from some assumptions or other about what nature is like, what observation is, and what properties are, as well as methodological assumptions about which investigative and explanatory procedures are appropriate to which domains. And no matter how carefully the superstructure of a theory is worked out, the theory can only be as strong as its foundations. Regrettably, Sheldrake's theory is fatally vulnerable to the criticisms noted earlier, and those errors are thoroughly philosophical, not empirical. They concern very abstract (and apparently unrecognized) presuppositions about what *must* be the case (what nature *must* be like) for the theory to work as well as assumptions concerning the nature of science itself. Nevertheless, Sheldrake has done a first-rate job of presenting and describing a range of phenomena and problems which the life sciences must confront, but which they haven't yet dealt with adequately.

My own view is that no *science* in the traditional sense of the term can do the job. We need something much more radical than a new but methodologically and conceptually conventional scientific theory. We must be prepared to describe and explain many organic phenomena in ways currently regarded as nonscientific or prescientific. We must radically reconstrue the goals of science and aim for a more robust, balanced, and enlightened view of what *understanding* and *explanation* are. I sympathize with Sheldrake's rejection of many theories in the life sciences; but as I see it, Sheldrake hasn't carried his rejection far enough. The failures of the current life sciences and behavioral sciences are due less to problems specific to particular theories and more to their underlying shared presuppositions about what a science is and what a life science can be.

As I argue throughout this book, different domains demand different methodologies and modes of explanation. Because of Sheldrake's failure to appreciate this point, his theory must ultimately be consigned to the ash heap along with many others hailed as revolutionary but which at their core are merely crude mechanistic views in fancy new clothing—for example, sociobiology, Pribram's holographic analysis of memory, and (more generally) information-theoretic and computational analyses in the so-called cognitive sciences. As I've tried to show to some extent here and also in chapters 1 and 3, the assumptions underlying these approaches to the behavioral and life sciences are fundamentally incoherent at worst and transparently false at best. Despite their provocative (and only superficial) novelty, the theories really have nothing to stand on.

Only when the life sciences stop trying to mimic the methods of physics, only when they recognize that there's more than one way to be scientific, will we begin to see theories adequate to the domains of organic phenomena. Of

course, that insight alone would force a profound change in the life sciences as we know them. It would lead to an awareness that the life sciences may never be scientific in the ways theories of physics have traditionally been. But unless science experiences a change of that magnitude, it will never competently address the problems and phenomena Sheldrake discusses—or for that matter, organic phenomena generally.

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NOTES

1. Sheldrake, 1981, 2009a, 2009b. Since no two editions have the same pagination for the passages they have in common, for simplicity I'll limit my references to Sheldrake, 2009b.
2. Sheldrake, 2009b, p. 34.
3. Ibid.
4. Ibid., p. 36.
5. Braude, 1997. See also Scriven, 1975.
6. Sheldrake, 2009b, p. 143.
7. Ibid.
8. Ibid., p. 65.
9. Ibid., p. 143.
10. Ibid, p. 144.
11. Ibid., p. 95.
12. Ibid., p. 143.
13. For example, Braude, 1997, 2002, 2007.
14. Sheldrake, 2009b, p. 144.

15. Sheldrake, 2009b, pp. 31–32; Beloff, Emmet, Morgan, Sheldrake, & Thompson, 1981.
16. A self-serving term if ever there was one.
17. Sheldrake, 2009b, p. 122.
18. Ibid., pp. 135ff.
19. Recently, Stan McDaniel has also called attention to this flaw in Sheldrake's account, but from a slightly different angle. See McDaniel, 2010.
20. Sheldrake, 2009b, p. 232.
21. Ibid., p. 233.
22. Ibid.
23. Personal communication, Nov. 22, 2007.
24. See also Rosenthal, 1976, 1977 and Rosenthal & Rubin, 1978, and compare the results with the experiments Sheldrake describes in chapter 11.2.
25. See, e.g., Braude, 2002; Goldberg, 1977; Yalowitz, 2011.