Radical Provincialism in the Life Sciences: A Review of Rupert Sheldrake's *A New Science of Life*[1]

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Sheldrake's book, subtitled "The Hypothesis of Formative Causation," has recently received considerable attention from both scientists and laypersons. Some hail the book as a radically new and viable approach to a vast range of scientific issues—with a potential bearing on parapsychological theory. Its opponents, on the other hand, either dismiss it mildly as a merely wrongheaded theoretical program, or denounce it strongly as a work of indefensible eccentricity. Any work that excites such strong contrasting opinions promises to be interesting, and *A New Science of Life* is certainly that. My own view, however, is that Sheldrake's staunchest supporters and detractors are both wrong: Sheldrake's new theory is neither viable nor as radical as it seems. But it is not crazy either—and in fact I consider it flawed for essentially the same reasons I object to a good portion of received scientific theory. Even more interesting, the theory is flawed for the same reasons as the theories Sheldrake is most concerned to reject.

I

Sheldrake feels that certain gaping lacunae in our understanding of organic phenomena exist because of inherent weaknesses in the prevailing approaches of the life sciences. He maintains that certain fascinating phenomena cannot plausibly be accounted for in terms of orthodox mechanistic scientific theory. In particular, Sheldrake thinks we must explain certain outstanding problems of *biological morphogenesis*, which he defines (following Needham) as the "coming-into-being of characteristic and specific form in living organisms" (p. 19). 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 devel-

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oping system is removed (or if an additional part is added), the system continues to develop in such a way that a more or less normal structure is produced" (p. 19)? (c) How are organisms able to *regenerate*—i.e., replace or restore damaged structures? (d) How are we to explain *reproduction,* in which "a detached part of the parent becomes a new organism; a part becomes a whole" (p. 21)?

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. Such a theory would fail because it posits an unbridgeable causal gap between two radically different kinds of thing—namely, a nonphysical entelechy and the physical world. The only remaining alternative to mechanism and vitalism—and the approach he endorses—is a form of *organicism.* Rather than attempt to explain the physical facts of morphogenesis in terms of a nonphysical entelechy, the organicist appeals to *morphogenetic fields.*

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

As I have pointed out elsewhere (Braude, 1979a, 1982), causal links are essentially *explanatory* links. 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 (see also Scriven, 1975). But nothing prohibits cause and effect from being of different ontological types. What kinds of states of affairs may appropriately be related causally depends on the context of inquiry, just as context determines what sorts of directions are appropriate to a request for directions.

Sheldrake's criticism of vitalism seems merely to be a version of the standard criticism of Descartes' interactionistic dualism. In both cases the view attacked is conceived in perhaps its least plausible form. Descartes thought mind and body were two distinct kinds of *stuff* or *substance*—the former unextended and the latter extended. But Descartes' model of causality was that of billiard-ball collisions, and critics were correct in pointing out that something unextended could neither push nor be pushed by something extended.

Notice, though, that this objection to Cartesian interactionism applies only to a *substance*-dualism, in 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 among them. If one were instead to maintain a kind of *event-* or *level-of-description*-dualism, rather than a substance-dualism, and also admit the legitimacy of causal explanations not fitting the billiard-ball model, then the standard objection to interactionism would have no force. That is, if one argues that psychological (or mentalistic) and physicalistic descriptive categories are not reducible one to the other, one is free to posit causal (explanatory) links between the two domains without having to explain how two different kinds of stuff can impinge on each other. "The mind" need only be 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). Neither the mind nor the weather need be construed as a *substance*. And surely 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 do not need to maintain that the three classes of events are all expressible on some one level of description. Similarly, we are free to draw causal links between mental and physical events, even if statements about one do not reduce to statements about the other (or even if the two types of statements fail to reduce to some other level of description).

Now vitalism, as I see it, may have been articulated in a confused way, analogous to Descartes' version of dualism. Entelechy needn't be reified any more than the mind need be construed as a kind of 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—i.e., not reducible to another level of description. These *vital* facts and regularities would simply have no analysis in the terms appropriate to mechanical or impersonal forces. But in that case, vitalists would not be constrained to explain how a nonphysical force can impinge on a physical organism (especially considering that not all causality is billiard-ball causality).

II

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" (p. 116). In other words, this type of causation helps determine the internal and external structure of things in nature. Moreover, formative causation "is not itself energetic, nor is it reducible to the causation brought about by known physical fields" (p. 116).

This last point, I hasten to add, seems especially peculiar in light of Sheldrake's rejection of vitalism. He would appear to be arguing for the kind of "action of unlike on unlike" (p. 49) he considered fatal to that approach.

2. Each kind of *morphic unit* (i.e., identifiable *thing*) 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" (p. 116).

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" (p. 117). 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 develops his theory in considerable detail. But the aforementioned proposals are the heart of the theory, and their weaknesses are sufficient to sabotage it beyond salvation. Apparently Sheldrake is unaware of some deep philosophical issues that his theory implicitly addresses. Once these are brought into the open, it becomes clear not only that the hypothesis of formative causation deals inadequately with them, but that its approach is fundamentally that of the mechanistic theories Sheldrake wants to repudiate.

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, since he claims (on p. 73) that morphic units may be found at all levels of complexity, and then says explicitly, "Each kind of morphic unit has its own characteristic morphogenetic field" (p. 116). But the problem with a claim like this—as I have pointed out before in the pages of this journal and elsewhere (see, e.g., Braude, 1979a, 1979b, 1979c, 1981, 1982)—is that forms, objects, events, and kinds are not intrinsic to nature. There is 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. But if these things are not items from a prefabricated ontological storehouse and are instead merely elements of constantly-evolving conceptual grids that we place over nature, then morphogenetic fields are not *in* nature either.

To avoid misunderstanding, I should emphasize that none of this shows that nature has no structure, or that we are not entitled to impute a structure or structures to nature. But nature does not have *a* structure, some one parsing into and arranging of elements that enjoys inherent priority over all others. It has, rather, 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 element-kinds fits into a successful scientific theory. As with any conceptual grid we place over nature, our choice of descriptive categories (and the resultant structuring of nature) is justified by the success of the grid as an intellectual tool in systematizing our experience. 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 view that morphic units and their associated morphogenetic fields are *natural kinds*—i.e., 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. No doubt Sheldrake is not clear enough about these issues to put his position in these terms. But when one considers his remarks about similarity (which I discuss below), one can see that Sheldrake's tacit (and apparently unwitting) assumption that nature has a preferred description is merely another slice of the same philosophical pie.

We must next ask: How is it that "all similar past systems act upon a subsequent similar system by morphic resonance" (p. 117)? Actually, we may 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 cannot be given in purely structural (or even quasi-structural) terms. *Similarity* is a concept that is neither formally definable nor analyzable independently of its context.

The appeal to morphic resonance involves a not-too-subtle retreat back to mechanistic thinking. Yet Sheldrake's theory cannot work without it. Apparently, Sheldrake fails to see that similarity is no more inherent in nature than are our parsings or inventories of things; in fact, he makes the classic mistake that vitiates, among other things, memory trace theory. I'm surprised to see the error occupy such a prominent place in Sheldrake's thinking, given his apparent grasp of H.A. Bursen's attack on trace theory (Beloff et al., 1981).

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, as I mention below in connection with geometric congruence, not even closeness of frequency can really be as straightforward as Sheldrake suggests.) Sheldrake wants his theory to extend to forms of *behavior*. But there are fatal disanalogies between similarity of behavior and similarity of frequency. For example, although we can lay down antecedently specifiable limits for similarity of frequency (and some other things)—e.g., by saying that any frequency between 438 Hz and 442 Hz is similar to A 440—we cannot do this for behavioral forms (or most other forms, for that matter). As an illustration of the disanalogy to closeness of frequency, compare, as instances of the behavioral form *courtship* (one of Sheldrake's own examples): a cave man clubbing a cave woman, a woman playing dumb so as not to threaten her chauvinistic and insecure date, erotic conversation over dinner, placing an ad in the "Personal" column of a newspaper, bragging to a date about one's possessions, lying to a date to conceal one's sordid past, writing poems to one's sweetheart, clowning around at a fraternity party, and on and on. Moreover, each of these subsets of courting behavior (and just human courting behavior at that) may be exemplified in endless ways.

Sheldrake concedes that similar systems may differ in their specific features (p. 98). 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. 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 is to explain how *these* differences are adjusted for? Another appeal to automatic averaging would start Sheldrake on just the sort of vicious regress he seemed to recognize was fatal to memory trace theory. Second, the process of producing composite photos is not one of automatic averaging. There is nothing automatic about the process. Composite photos are produced by a person, by someone who *decides* which features are relevant and similar and who then determines a method by which to align them.

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, difference 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 is not a phenomenon built into nature, operating according to internal structural principles or criteria.

An example from geometry may help make this point clear. In geometry one often speaks 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 criteria of congruence are suitable for different purposes, and no one of them is inherently preferable to the others. Thus, 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 is never a feature of the objects themselves. A question like "What other figures are congruent with an isosceles triangle?" has no answer apart from an actual context of inquiry. But then, if we can only give a positional account of similarity for geometric figures (and, by the way, for frequencies), things look bleak indeed for the concept of morphic resonance when we move on to more complex domains—including that of human behavior.

I will return shortly to the topic of behavior, since Sheldrake commits another crucial error in discussing that. First, however, let us look at another reason why Sheldrake's proposal fails to account for the phenomena he thinks are in urgent need of explanation.

One type of phenomenon Sheldrake wants to explain is how a certain region *r* of a developing organism could develop in more than one way—say, into an eye or into 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 say 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*, *ex hypothesi,* doesn't yet *have* that structure; it is *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 is 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 with the eye field; by hypothesis, it is still morphically flexible. 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 simple and developmentally indeterminate—if, that is, it is compatible with (and presumably equally similar to) the structure of different sorts of morphogenetic fields, then Sheldrake's appeal to resonance between similar structures does not explain why *r*'s development should follow one course rather than another.

Sheldrake seems to propose one possible solution to the problem (p. 110). He appeals to *morphogenetic germs,* the characteristic parts of morphic units, and says that *primary* morphogenetic fields determine characteristic germs on which different *secondary* morphogenetic fields may 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? Apparently, the selectivity problem mentioned in the previous paragraph has just been pushed back a stage. There is no reason for an undeveloped 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.

When Sheldrake finally turns to the topic of behavior, he commits further serious errors. Here, interestingly, Sheldrake's views are anything but novel; behavioral scientists frequently commit the same errors in one form or another. Sheldrake's discussion, however, is helpfully perspicuous. It has the virtue of bringing the errors close to the surface, where their flaws may stand out clearly.

To begin with, Sheldrake fails to appreciate the distinction between *action* and *movement*. To put it briefly, all actions involve movement, but not every movement is an action. For example, the raising of one's eyebrows is a movement (or series of movements); it may be a sign of astonishment, a sexual invitation, or a way of yielding to an ophthalmologist's examination. Movements, then, are functionally indeterminate: the same movement may be (or be a part of) different actions. If any part of organic activities may be described in purely physical terms, movements are the most promising candidates. But which action results from the movement can only be described relative to the movement's *position* in a context. And even then, nothing intrinsic to the 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 grasp these points: not only does he classify heart-beating and mating behavior as movements, but (in section 11.4) he maintains that actions are—in a sense he never tries to develop—ultimately explicable as sequences of movements. (Of course, Sheldrake doesn't articulate the point in this way, since he doesn't recognize the distinction between action and movement. This is simply what his view boils down to.) Now even if similarity of movement could be analyzed formally, so that the concept of morphic resonance could apply to movements, similarity of action cannot. (I will develop this point below.) But clearly, if even geometric figures are not inherently similar or dissimilar, movements are not either. Consider: what natural law or rule of projection could determine, say, whether a flea and an elephant display a similar movement, or whether the batting swing of a Little League player contains the same movements as the swing of Reggie Jackson? Sheldrake's error here is not even remotely scientific. He is once again making a deep philosophical mistake about the nature of similarity—namely, assuming that similarity or dissimilarity is an inherent, rather than positional, property of things.

Lamentably (and rather naively), Sheldrake actually proposes that there could be a morphogenetic field for behavioral types, including *searching for a mate* and *courtship*, as well as for *habits* generally. He fails to grasp (a) that nothing done by an organism is inherently of a given type, and (b) that anything that *is* of a given type could, in some other context, be of a different type. This means, among other things, that there is no limit to the range of activities that can exemplify a given behavioral type: virtually any activity, given the appropriate surrounding history, can exemplify any behavioral type. Moreover, any activity that does exemplify a type does so because of the way in which *we* construe its position in a bit of history. The exemplification of a behavioral type is not inherent in nature. It is inexorably relativized to *criteria of relevance* imposed by human agents in a context of inquiry. But then there can be no structural or formally specifiable *essence* to that type—certainly nothing like a specifiable frequency with which some things but not others may resonate.

Just how procrustean and impoverished a conception of behavior Sheldrake endorses emerges clearly in his penultimate chapter. 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" (p. 194). 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 men" (pp. 194–195).

Although feeding is a more uniform and ritualized behavior than many (giving the example of feeding behavior at least superficial plausibility here), it is 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" (p. 195). 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 permits resonance between things of the appropriate type, and only between things of that type (or essence).

But (a) there is no antecedently specifiable set of goals that defines the feeding process, and (b) there is no goal that is inherently a feeding goal. To demonstrate (a) it should be sufficient to consider the ways *all* organisms feed. Certainly not all organic feeding processes terminate in chewing and swallowing. The range of organic methods of feeding is enormous and in principle unlimited. Yet they are all ways of feeding. And it *is* fair, incidentally, to discuss the entire range of organic feeding activities, rather than just human feeding behavior. If the behaviors are all ways of feeding, then according to Sheldrake's own principles there is an organic morphic unit of feeding behavior which 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 specify for that behavior. So human feeding is not defined relative to fixed—much less inherited—goals.

With regard to point (b), no human activity like chewing followed by swallowing inherently terminates a feeding event. Other sorts of events also may end with chewing and swallowing—for example, ingesting hallucinogenic mushrooms (hardly a case of feeding: it is 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.

Moreover, it is preposterous to think that there could be a formal or structural correlate to the chewing or swallowing processes. But the existence of such a correlate is 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 is not inherent in figures, whether or not chewing or swallowing events are similar depends on context-relative criteria of similarity. Even if Sheldrake is correct in claiming that "there is little difference in the way . . . food is chewed," whether the differences *make* a difference, and *which* differences are relevant, is always relative to a context. It is never a formally specifiable property of the activities themselves. Thus, 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. And of course nonhuman organisms exhibit a further variety in ways of chewing. Now why, if geometric congruence is not an inherent property of geometric figures, should similarity of chewing activities be a property that is specifiable independent of the context of those activities? The ease with which Sheldrake supposes all chewing events to be similar shows either (a) that he is unaware of the context-dependent criteria 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) or (b) that he thinks his criteria are somehow inherently fundamental, or preferable to all others. The former would be an example of shortsightedness; the latter, an example of metaphysical chauvinism.

Some might protest that Sheldrake's theory predicts occurrences different from those predicted by rival theories—for example, concerning the maintenance and proliferation of new forms (including learned behaviors). Sheldrake predicts, for instance, 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, this would not be sufficient to warrant acceptance of the hypothesis of formative causation. It takes more than predictive utility to justify a theory; false (and even incoherent) theories may make true predictions. Sheldrake may well have performed a service to science in drawing attention (for whatever reason) to phenomena or regularities which are worthy of attention, and which other theories have ignored or missed. The fact would remain, however, that the conceptual underpinning of Sheldrake's theory is deeply defective, no matter how serendipitous some of its predictions may be. Sheldrake's theory, then, may have the virtue of pointing science in new and important directions. But it remains a false start nevertheless.

There is simply no reason to posit morphogenetic fields if we are forced to accept false or absurd presuppositions in order to explain how they work. But then there is no reason to posit morphogenetic fields at all: unbuttressed by a 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 phenomena.

I should emphasize that my objections to Sheldrake are compatible with the facts he alleges and predicts. That is, even if his theoretical explanations do not work, it may well be that there is some causal connection between, say, the widespread learning of a given ability and the greater ease with which subsequent populations learn it. All I contend is that *if* Sheldrake's alleged facts *are* facts, and *if* they have an analytical explanation (in terms of subsidiary processes), then Sheldrake's proposed explanation is unsatisfactory. Of course it could 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 which facts may be at scientific ground level.

III

Considering Sheldrake's avowed opposition to mechanism in the life sciences, it is very interesting that his theory should turn out to be so classically mechanistic. Perhaps one reason is that Sheldrake does not fully understand what mechanism is. A mechanistic explanation is not simply one that explains a phenomenon in the language of physics. A mechanistic theory needn't even be a physicalistic theory. Dualist and idealist theories can be mechanistic. The differences would merely be differences in hardware. A mechanistic explanation is one that attempts to explain a phenomenon in the way we explain how the inner workings of a machine produce a certain output. In addition to specifying the subsidiary processes leading to the output, one requirement of this approach is to be able to state or define in a general way what the output is. To this extent, Sheldrake's theory is disappointingly conventional. His appeals to morphic resonance and to the alleged existence of essences or defining structures for *kinds* (including behavioral kinds) involve the same errors that underlie the brain-radio or energy-transfer theory of telepathy, and for that matter all theories (including all cognitive or computational psychological theories) that posit physical correlates or mechanisms for kinds of organic or psychological phenomena (see Braude, 1979a). Sheldrake's theory, in fact, attempts the kind of reduction of *types* of behavioral states that even hard-core philosophical physicalists recognize is untenable.

What else might account for Sheldrake's lapse into traditional mechanistic thinking? I submit that (in addition to erroneously equating mechanism and physicalism) Sheldrake has not recognized a fundamental assumption of mechanistic thinking—namely, that there cannot be unanalyzable phenomena or facts at the observable level. All scientists concede that explanation by analysis (i.e., into subsidiary processes or mechanisms) cannot continue indefinitely. They admit, in other words, that some phenomena are primitive in the sense that we cannot go behind them and profitably ask of them *how* they occur; they are simply some of the ways the universe works. In identifying or describing these phenomena, we arrive at a kind of scientific ground level. Most scientists assume, however, that these fundamental phenomena can exist only at the level of the very small, that is, the atomic or microscopic level, and never at the level of observable phenomena. But this is merely an assumption, not an empirically established fact; and antimechanists have powerful arguments that (I believe) conclusively demonstrate its falsehood.

In any case, had Sheldrake been willing to abandon this 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 phenomena at the biological or organic level, and then he would not have had 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 would be no more unscientific, nor more of a failure in understanding, than conceding that phenomena at the quantum level are primitive and unanalyzable into subsidiary processes and mechanisms. In fact, it may be a *victory* of understanding to figure out where analysis comes to an end. Besides, not *all* explanation stops once we identify ground-level phenomena: only vertical explanation (explanation by analysis) will grind to a halt. Other forms of explanation, such as horizontal explanation (e.g., by analogy) and covering-law explanation, will still be possible and may still prove profitable.

It is interesting, then, that many view Sheldrake's theory as radical. In most important ways it is thoroughly traditional. Sheldrake has adopted wholesale the standard assumptions about what a scientific explanation of observable phenomena should look like (i.e., that the phenomena should be analyzable in terms of unobservable subsidiary processes and mechanisms). Along with that, he tacitly accepts the received view that only by offering such analyses of phenomena can a discipline be *scientific* or provide an *understanding* or *explanation* of the phenomena. This is precisely the parochial attitude that underlies most experimental and theoretical work in the behavioral sciences, and which (as I have argued elsewhere) renders most of their results worthless.

Nevertheless, despite all my reservations concerning the tenability of Sheldrake's theory, I would recommend *A New Science of*

Life as a serious, interesting, and thought-provoking work. Sheldrake deserves to be commended for his care and ingenuity in working out the details of his novel hypothesis and extending its scope to many domains, as well as for pointing out the possible existence of hitherto unrecognized phenomena.

But no scientific theory is thoroughly empirical, and, like many theories in science, Sheldrake's looks more empirical than it is. Like all scientific theories, however, it rests on philosophical presuppositions. Every scientific theory starts from some assumptions or other about what nature is like and what observation is, as well as from methodological assumptions about which investigative and explanatory procedures are appropriate to which domains. And no matter how carefully the superstructure of a theory may be worked out, the theory can only be as strong as its foundations. Regrettably, Sheldrake's theory is fatally vulnerable to philosophical criticisms and a priori objections. His errors concern very abstract (and apparently unrecognized) assumptions 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. Still, Sheldrake has done a fine job of presenting and describing a range of phenomena and problems which the life sciences must confront, but which they have not 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 conventionally scientific theory. We must be prepared to describe many organic phenomena only in ways currently regarded as nonscientific or prescientific. We must radically reconstrue the goals of science, and aim for a more 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 has not 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.

A very simple point to which most scientists (including parapsychologists) seem blind—but which Aristotle pointed out more than two thousand years ago—is that 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—for example, sociobiology, Pribram's holographic analysis of memory (and cognitive phenomena generally), and information-theoretic and computational analyses of cognitive and paranormal phenomena. As I have argued before, the mechanistic assumptions underlying these approaches to the 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 is more than one way to be scientific, will we begin to see theories adequate to the domains of organic phenomena. Of course this 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 way theories of physics have traditionally been. But it seems to me that unless science experiences a change of this magnitude, it will never competently address the problems and phenomena that Sheldrake discusses—or, for that matter, the phenomena that interest parapsychologists.

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