

## **REFLECTIONS ON SHELDRAKE, WILBER, AND "NEW SCIENCE"**

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I find myself provoked in many ways by Wilber's article (1984). As Sheldrake seems destined to become another new-age celebrity, Wilber's discussion seems timely and welcome.

The strength of Wilber's article, it seems to me, is that it gets quickly and concisely to the heart of the problem with Sheldrake's hypothesis—namely, there are alternative theories, well within the known "laws of nature," which can potentially account for the phenomenon known as morphogenesis. These theories cannot be ruled out just because the enormous amount of experimental work needed to distinguish between competing possibilities has not yet been completed. Sheldrake admits he can point to little or no direct experimental influence for his thinking. For a radical thesis such as Sheldrake's to be taken up by evolutionary and developmental biologists, it must be able to demonstrate its superiority over all other possible explanations.

Despite the fact that Wilber's discussion is well argued and knowledgeable, I do not agree with some of his conclusions. I don't agree, for example, when he says that scientists have been "unfair" when they criticized Sheldrake's hypothesis because it does not account for the origin of new forms. It doesn't seem to me to be unfair to ask about first causes when Sheldrake is trying to advance a thesis that is an alternative to theories which do attempt to account for both emergence and establishment of new forms. What if the same principles which contribute to the emergence of a new form are also responsible for its stabilization, that is, the tension between biological variation and natural selection, which is the basic hypothesis of most evolutionary biologists? '

Another point of disagreement is with Wilber's position that Sheldrake's work must be regarded as "scientific" on the basis of the belief that what "alone defines a scientific hypothesis is testability." This is an idea which Sheldrake himself promotes enthusiastically. Karl Popper notwithstanding, this is a surprising position on both their parts in these "post-critical" days.

Any useful definition of what constitutes the scientific has to be much more complex than

that. What lay people refer to as science is not some definable abstraction but is a culture, a nonformalizable web of distinct but "over-lapping neighbourhoods," as Polanyi calls them, or "discourse communities" in the words of Geertz, or "disciplinary matrices" if you follow Kuhn. There are no simple, formalizable rules of scientific method which can be extended over territory as vast as that which Sheldrake encompasses with his theories. Unsatisfactory as it may be, the best we can say is that what qualifies a hypothesis as scientific is that it is deemed so by at least a segment of a community which calls itself scientific. I do not wish to say with this that Sheldrake's work is not scientific but only that its claim to be so does not stand or fall on the basis of testability or falsifiability.

To his credit Wilber recognizes that it is absurd to extrapolate from physics to human reality and he compliments Sheldrake for not doing so. It is strange, then, that he does not seem to recognize Sheldrake's attempt to extrapolate from hypotheses of "morphic resonance" and "formative causation" at molecular and biological levels, into the fields of human learning and consciousness, as another example of the same thing. This seems particularly surprising in view of his criticism of Sheldrake's liberal use of analogies from energetically law-abiding systems in support of his ideas about nonenergetic morphogenetic fields.

Sheldrake, throughout his work, shifts levels in an arbitrary way. On page 71, for example, in making his case for causation of form by nonenergetic fields, he uses the analogy of the way in which an architect's plan "causes" the ultimate form of the house, yet not in any direct energetic way. This is simply a cognitive dodge. He uses the word "cause" as if it has the same meaning whatever the realm of reality. But we know this is not so. When we say the house is caused by the plan we do not mean the same thing as when we say heat causes a stew to boil.

A major problem with a "grand theory" like Sheldrake's is that in order to fit a vastly variable universe into one inclusive framework, simplifications must be made which obliterate not only idiosyncratic variation, but also significant differences. For example, Sheldrake's hypothesis requires evidence that forms "average out" into a morphic rut due to the influence of past forms. He uses, as an analogy, composite photographs made by superimposing images from distinct individuals in a multiple exposure process (p. 98). The problem with this analogy is that it depends for its power on our forgetting that the process begins with images which are quite different, that is, illustrative of the enormous variety of forms found in nature; and it is only through a technical device that these differences can be averaged out in such a way.

Just as we can apparently "average out" marked actual differences between people through the technical device of composite photography, we can also apparently "average out" the multiplicity of scientific data about the real world through cognitive devices such as selecting data that fit our theory and ignoring data that do not. Obviously all theory makers (including me in this discussion) must decide what is real variation and what is mere perturbation, but "grand theorists" are much more vulnerable to arriving at grossly distorted pictures from such selection.

Sheldrake's vulnerability is never so evident as when he ventures into the realm of human life. On page 194 of his book, Sheldrake (1981) proposes that the "varied patterns of human behaviour" lead to a limited number of consummatory acts, such as feeding and copulation. He recognizes that the human species has a far wider range of ways to arrive at these consummatory acts than have other animals, but, whatever the variety, "feeding and

swallowing is similar in all men." He admits that "not all human activity is subordinated to the motor fields which canalize it towards biological or social goals" (are they the same?), but he dismisses this objection as follows:

Some is explicitly directed towards transcendent ends. This kind of behaviour is shown in its purest form in the lives of saints. But clearly most of the behaviour of most human beings has no such transcendent direction.

Does he wish to suggest that most non-saintly human activity--' music, art, poetry, politics, conversation, mathematics, science, religion, etc., etc,-are to be best understood as nothing but preparatory play to be eventually consummated in such basic functions as copulation and feeding? This conceptual composite picture is a clear example of the kind of gross reduction which Wilber calls stupid when the reduction is carried back to physics. *Biologism* is no more appropriate a paradigm for human life than is *mechanicism*. Reductionism is reductionism.

Neither do I share Wilber's enthusiasm for a new-age science which he sees as attempting a synthesis of empiricism and traditional meditational practices. I understand the disenchantment with positivism and empiricism when zealously applied to the world of the conscious mind but I disagree that the solution lies in such a synthesis as Wilber proposes. For me the most exciting and challenging direction in today's intellectual life is in the recognition that different realms of reality require different methods of enquiry, different modes of thinking-different paradigms.

Empiricism has adequately demonstrated its superiority over more introspective and interpretive epistemologies in the study of the material world, -realms of reality accessible to direct observation and experimentation. This can include some aspects of human reality such as anatomy, physiology, genetics, and even some pre-meaning events in psychological life. But as we cross the threshold into the language-mediated, intersubjective worlds of meaning, culture, ethics, religion, aesthetics, and so on, empiricism fails us. The world of conscious human existence-or being seems to be a noncausal, nonmaterial, nonspatiotemporal realm of reality. In this world, determinism and positivism do not hold. What empirical evidence could ever help us decide what Bach's influence was upon Beethoven, determine if Cuban influence caused the war in El Salvador, tell us "where" King Lear is or "what" love is?

It is in these realms that we need traditional meditational practices, along with philosophy, connoisseurship, semiotics, conversation, humanistic psychology, introspection, interpretation, anthropology, political analysis, and so on.

It seems clear to me that in the search of a science of persons the limits of empiricism have been reached, but the step I suggest we need to take is to recognize that the intersubjective social world of minds requires an entirely new kind of science; one that is discontinuous with thought forms developed about the material world of causes and effects. It may be viewed from Sheldrake's reductionistic biologism that a violent rape and an elegant courtship may ultimately result in ejaculation, but what can possibly be gained (or considered "new-age") in classifying them together as variable motor-fields leading to a consummatory act?

Most of human life is made up of events and phenomena which are irreducible to physics, biology, or even behavior. They have meaning in and of themselves, It seems to me that the most courageous thing we humanistic psychologists could do at this time would be to abandon our infatuation with nuclear physicists, behaviorists, developmental botanists, and anyone else who wants to extrapolate from nonconscious realms of reality, and assert that human life is paradigmatic of itself. Natural scientists do not hesitate to insist that anthropomorphism should be avoided. I would like to see humanistic scholars similarly insist that biologism has little to offer the study of human conscious life.

#### REFERENCES

- Sheldrake, R. (1981). *A new science of life*. Los Angeles: J. P. Tarcher.
- Wilber, K. (1984). Sheldrake's theory of morphogenesis. *Journal of Humanistic Psychology*, 24 (2)1"