Appendix

A.1 Comments and controversies

At the time the first edition of A New Science of Life was published in June 1981, the New Scientist published an article of mine summarizing the hypothesis of formative causation. This was introduced by Colin Tudge in the following way.

Scientific proof that science has got it all wrong

Rupert Sheldrake is about to publish a book that says, in arguments that *could* be refuted, that Western science has sadly misconstrued the world and all the creatures therein. Sheldrake believes that the form of things - crystals or organisms - and the ways they behave are not determined solely by the physical 'laws' and principles that science has so far identified. The view that everything that every creature does could, in the end, be explained by the properties of its constituent molecules - that biology in the end is chemistry, and chemistry (if knew enough) would be physics - is, he humbly suggests, junk.

Instead he posits (or rather re-posits, for the term is expanded from embryology) the idea of 'morphogenetic fields', which implies that when any one thing forms (a crystal, say) or any animal learns a new form of behaviour, it will influence the subsequent learning or formation of all other crystals or animals of the same kind. Indeed, he suggests, the world is kept on course not because 'laws' prescribe what must happen, but because of all the things that could happen, only one does - and that one then influences all things of the same kind that come afterwards,

Of course, within the context of modern science, such an idea is completely scatty. Blond and Briggs, who publish Sheldrake's *A New Science of Life* next week, and we, who are running a brief essay by him on page 766, have clearly flipped our respective lids, and we should get back to our test-tubes but for three things.

The first is that Sheldrake is an excellent scientist; the proper, imaginative kind that in an earlier age discovered continents and mirrored the world in sonnets. He is a plant physiologist, once a scholar of Clare College, Cambridge, then a fellow, and, for good measure a Rosenheim Research Fellow of the Royal Society. By the early 1970s he was all set to attain the dream of most career scientists, a professorship in the university of his choice, when he decided to look at whole plants growing in fields rather than

bits of plants in the laboratory, He went to Hyderabad, India, in 1974. And there he wrote the first draft of *A New Science of Life*.

The second reason for taking Sheldrake seriously is that the science in his ideas is good. To absorb what he says involves what Thomas Kuhn off-puttingly termed a paradigm shift, which means putting aside our assumptions on how the world works. This is an uncomfortable thing to do. However the notion of modern mechanistic science that we have indeed identified all the major forces and fields at work in the world is astonishingly presumptuous. It is also untestable and therefore, by Karl Popper's criterion, unscientific. Thus, if anything is unexplained (as a remarkable number of biological phenomena are), the mechanists suggest that a little more knowledge, a little more research along the same lines, will produce the answer. The underlying argument, that the future will sort out all the problems, is untestable, and therefore unscientific.

In contrast, what Sheldrake proposes is scientific. This does not mean that it is right, but that it is testable, for example by finding out whether a rat learning a trick in New York makes it easier for a rat of the same strain subsequently to learn the same trick in London. (Sheldrake would like to have done this experiment, but it is hard for a plant physiologist to get a Home Office licence to work on rats, especially when the idea at stake involves a 'paradigm shift'.)

The third reason for taking Sheldrake seriously is that other people do. The argument that we should all be Catholics because the Pope is a clever man might not cut a lot of ice. But as Sheldrake says, the scientists who take him most seriously and sit up at nights working through the implications are the ones who ought to be the most affronted: the physicists. No-one who has worked with particles within particles, or distances and temperatures that are so out of scale with what humans can comprehend, could doubt that the modest 'morphogenetic' fields that Sheldrake proposes could indeed exist.

If the experiments that Sheldrake suggests will test his ideas do not work, and go on not working, he will be shown to be wrong. That's life, he says. More to the point, that's science.

Colin Tudge (New Scientist, 18 June 1981)

The ensuing correspondence was summarized by Roy Herbert in the New Scientist's *'Forum' section*.

Action at a distance

Readers accusing each other of pomposity, of perpetrating nonsense and sophisticated methodological falsificationism are the stuff of life to correspondence columns, so the epistolatory consequences of Rupert Sheldrake's article on 'formative causation' in *New Scientist* (18 June p 766) and the later review (16 July, p 164) of his book on the theory brought a welcome liveliness. To follow the exchange of argument and, polite invective may have proved something of a strain on the vocabulary with words such as 'entelechy' and 'organismic' flying about, not to mention phrases such as 'paradigm shift', but it has been rewarding to see the mixture of dismissal, caution, non-commitment and outright approval displayed in letters over the past few weeks. Not the least interesting was a claim from Ninian Marshall that the theory, or an extremely similar one, was put forward 20 years ago, though the suggestion that this is evidence for the theory is dubious. Perhaps it simply means that most people can read.

Colin Tudge, who originally said, approximately, that because Sheldrake's theory could be tested it was, therefore, science, may have expected to get into deep water but Lewis Wolpert is inclined to go deeper. He has a hypothesis that Tudge is being subverted by Martians and in order to test this would like Tudge to agree to being kept in isolation in a room surrounded by water for six months, adding sinisterly that other journalists would benefit from this by cosmic resonance (Letters, 30 July, p 306).

The magazine, of course, cannot print all the letters on the subject, many of which were satirical, including near-positive proof that the price of gold is influenced by the electronic impact spectrum of propyne. Some charged wildly off into the thickets of semantics and others into rare systems of philosophy, mostly personal, one stating that the whole subject was easily explainable if you admitted the existence of the soul.

One point made by a correspondent is that human beings are concerned in crystallisation experiments and in observing the behaviour of rats and so may be influenced by what they know. So might the rats be influenced, by telepathy from humans or, indeed, from other rats. An experiment with rats in Y-mazes could be mounted, says another letter, in which rats are tested in London, once only, for right or left preference. Then a huge number of rats in Buenos Aires could be trained to prefer the left and an equal number of rats in Jakarta trained to do the opposite on a date kept secret from the London researchers. The writer says that he is prepared to bet that nothing will happen to the preferences of the British rats. The suspicion that the Buenos Aires and Jakarta rats would cancel each other out might be a factor he has overlooked.

A number of correspondents remarked that if formative causation were true it would by now have become not simply testable, but blindingly obvious and quoted examples in evidence against it. Why, since so many people in history have learned to speak Chinese, can't we all learn it with ease, or even be born able to speak it? Or are we not ‘similar' to people in China? The language question was quoted more than once, one writer saying that as far as he could see, formative causation would have had us all speaking the same language long before now. A terse letter from Plymouth said never mind about rats; there is no acceleration whatsoever in children's rate of apprehension in schools, though successive generations learn the same tricks year after year.

From the Chaire de Genetique et de Microbiologie at the Ecole Nationale Supérieure Agronomique de Montpellier, Professor J.F.T. Spencer suggests that as the hypothesis of formative causation leads logically to the expectation that physical events affecting one group of living organisms should influence the development of other groups of the same species we should take a closer look at circumcision. As the Jews have practised this for thousands of years, the foreskins of Caucasian peoples, In particular, should have been getting shorter over that time. Either the hypothesis is incorrect, or, long ago in history, human beings had tremendously long foreskins. Remote groups might have been less affected, in which case measurement of foreskin length among say, Easter Islanders or Australian Aborigines might provide useful data and employment for anthropologists. [*Note*: see p. 136.)

It is relatively easy to be scornful about formative causation and most letters were. In some people's eyes this is a point in its favour, as they can then cry that lots of other ideas have been lampooned and later found to be correct. Only one letter took exception to Rupert Sheldrake's analogy with the television set, Analogy is always dangerous because it is almost impossible to find an accurate example. The television set, the reader knows is influenced from outside and he is therefore predisposed to acceptance of the idea of formative causation. But a television set is a television set and not a model of the Universe.

Roy Herbert

(*New Scientist*, 6 August 1981)

Meanwhile, A New Science of Life *had been reviewed in most of the major British newspapers, generally favourably. Here, for example, is the review from the Sunday Times, by Dr Bernard Dixon.*

Cabbages or kings?

'If vitalism is such a valuable method of thinking, would you give us a valuable thought?' Sir Peter Medawar once quizzed one of his fellow participants during a scientific symposium. The remark (unanswered) was delivered with an understandable blend of irritation and humour. For over a century now, there have been those who have resisted scientists' pretensions to describe the marvels of nature in exhaustive detail. Something additional to physics and chemistry is required, these thinkers argue, if we are to understand the categorical difference be-tween organic and inorganic matter. Approaches such as vitalism and holism have been formulated accordingly while countless authors, from J.C. Smuts to Hans Driesch, have offered us more profound alternatives to 'reductionism'.

During these same decades, however, the triumphant development of modern biology and medical science has owed everything to that very same mechanistic stance in the laboratory. Materialism, rather than belief in supra-material forces, has spawned life-saving antibiotics, lifted intolerable burdens from severely depressed patients and revealed the structure of DNA. Sir Peter's impatience is well-founded.

But unease persists in the other camp too - among those who are logically or intuitively unable to accept thorough-going materialist orthodoxy. A merit of Rupert Sheldrake's extraordinary book is that it begins by spelling out coolly and accurately the grounds for such dissatisfaction. With a welcome lack of evangelical fervour, Dr Sheld-rake reminds us that, despite the cracking of the genetic code, biological science retains a substantial portfolio of unsolved problems.

The moving forces behind evolution, for example, are by no means comprehensively described. The origin of life - notwithstanding confident assertions to the contrary - remains an open question. Above all, as far as Sheldrake is concerned, we have the challenge of trying to understand what shapes the form, development and behaviour of living organisms.

One answer to the persistence of these riddles is simply that they concern very, very complex systems. Although we can describe in exquisite detail parts of the hereditary blueprints which determine characteristics of cabbages and kings, tortoises and tapeworms, vastly more remains to be learned. But given the conceptual and practical returns already won via a mechanistic approach, we have every justification for confidence that even the most perplexing problems of brain and behaviour will succumb to the same investigational strategy. Alterna-tive analyses are irrelevant.

Perhaps, though, some hefty difficulties remain. A particular challenge is posed by the phenomenon of differentiation. How does a fertilised egg metamorphose, stage by stage, into a baby, a blackbird or a Bombay duck? Orthodox theory holds that DNA in the egg functions like a computer program, specifying the creature concerned and directing its development. As identical DNA copies are passed on to all cells, however, it is difficult to see how this process can generate tissues as utterly different as heart muscle and toe-nails, white blood cells and dental pulp.

So, while much has indeed been learned about differentiation, scientists trying to explain its causes soon retreat into what Sheldrake calls ‘vague suggestions about physico-chemical interactions somehow structured in time and space. The problem is merely restated.' His answer, with due respect to vitalism and materialism but concurrent with neither, is that living organisms are shaped by 'morphogenetic fields' of a type unknown to science. These fields impose particular patterns and are derived from those associated with previous similar creatures.

In other words, things develop one shape rather than other possible shapes because similar systems were organised in that way in the past. In addition to well-understood forces, Dr Sheldrake's fields help determine the structures not only of cabbages and kings, but also of non-living objects such as chemical crystals - and indeed patterns of behaviour too. Via what he calls 'morphic resonance', these influences carry across both space and time.

Being a serious scientist, Sheldrake must expect most of his colleagues to dismiss vague suggestions of this sort as gratuitous metaphysics, But Occam's Razor is not a particularly appropriate weapon to use against him, in an area which could well benefit from new perspectives. His tightly argued case does merit attention.

I have two complaints, one of them closely linked with the book's principal virtue. First Sheldrake gets into terrible trouble when, though placing his morphogenetic fields beyond the understanding of contemporary science, he tries to picture them. Second, this popular book presenting a heterodox scientific thesis is perplexingly short on experimental data. Unlike many works seeking to lay bare the limitations of mechanistic biology, it does contain some hard evidence. Most remarkable are the results of tests conducted in Harvard, Edinburgh and Melbourne suggesting that if rats are trained to perform a particular task, other rats elsewhere in the world will develop a tendency to learn the same skill more easily. But the most recent of these investigations - hotly controversial at the time - was completed three decades ago. Why, with research facilities at his command, has Dr Sheldrake not pursued this matter practically as well as philosophically?

Despite intermittent criticisms, Sir Karl Popper's standards of testability and even falsifiability still hold sway in determining the status of a scientific theory. To Rupert Sheldrake's considerable credit, he has suggested several elegant, inexpensive ways in which some bizarre notions could be assessed experimentally. By the same token, his decision to write this seductive and plausible book *before* doing the necessary benchwork must be counted a grave defect.
(*Sunday Times*, 28 June 1981)

Reviews also appeared in a wide variety of periodicals. These are some extracts.

Dr Dennis Summerbell, *The Biologist* (November 1981)
Not just another crank book but an attractive and sound presentation of an improbable hypothesis. The author argues that all life phenomena are not explicable in terms of the currently known physical and chemical constraints, and then presents a hypothesis, 'formative causation', that bridges the gap. The principle of the hypothesis is that the structure of pre-existing systems affects the genesis of subsequent similar systems by a cumulative influence that acts across time and space so as to conserve form. The mechanism he evocatively calls ‘morphic resonance'.

Sheldrake does not dismiss or attack the laws of science but presents his theory within an orthodox scientific framework, accepting the value of the reductionist approach and providing a strong counter to vitalism. The book is well written, provocative and entertaining, and Sheldrake's scholarly approach includes excellent summaries of current beliefs in many fields of life science.

Improbable? Yes, but so was Galileo.

Lois Wingerson, *World Medicine* (July 1981)
Wild as they seem, Sheldrake's ideas are difficult to refute on logical grounds when they are read in detail. At the least, they provide a good test case of the creative flexibility of biologists' minds.

Dr P. E. Hodgson, *The Tablet* (25 July 1981)
In spite of many impressive advances in genetics and molecular biophysics, we still lack a detailed understanding of the evolution and development of living organisms. Darwin's theory of natural selection provides a plausible mechanism for microscopic evolution but the processes responsible for major changes remain obscure. How, for example, can an extremely

complicated system like an eye or a feather develop in this way when intermediate forms would probably hinder survival? In addition to these problems of the origin of forms, there are other difficulties in accounting for their growth and behaviour.

Such problems have sometimes been solved by postulating divine intervention, a move both theologically objectionable and scientifically suicidal. It is an impoverished conception of God's creative power that requires him to be continually tinkering with creation in this way; in such situations the remedy is to intensify scientific research.

For many years biologists have postulated organising factors, entelechies or morphogenetic fields that guide the development of new structures. By itself this is not helpful unless we can say how such fields work. Now Dr Sheldrake carries this idea further ...

The hypothesis of formative causation refers to objectively observable regularities and so can be tested in physical, biological and psychological systems. It provides a new way of looking at many puzzling phenomena and if confirmed could greatly contribute to the unification of the sciences.

Professor Mary Hesse, *Theology* (Vol. 85, 1982)
The hypothesis is a startling and exciting one, and it is to be hoped that its potentialities outside biology will be explored, as well of course as its implications within biology, upon which its credentials must ultimately rest.... Sheldrake's hypothesis has potentialities for the description of a sequence of system-levels even, perhaps, reaching up to the perennial religious problem of how to express God's providential dealings with the world.

Stephen Clark, *Times Literary Supplement* (12 March 1982)
What is the theory's importance? Perhaps none: for it may be that its predictions are not confirmed. Other, more mathematical theories may survive a refutation or two - Sheldrake's probably won't survive many failed predictions, though some form of organicism seems incumbent on us. If it does perform better than its rivals, we may set about detecting these fields by other means. What metaphysic will go along with the theory seems uncertain: it is compatible with a modified materialism, or with dualism, animism or theism, or even with idealism. Those questions must still be answered at their own level. If no one bothers to try at all, it should not too easily be assumed that this is because the good sense of scientists is to

ignore fatuous hypotheses. More likely, there isn't any money, and too much ill-judged metaphysics.

Dr Brian Goodwin, *New Scientist* (16 July 1981)

The extraordinary variety of plant and animal forms, and the orderly transformations which organisms undergo during their development, are what most distinctively characterise the biological realm. So it is not surprising to find that some of the most animated debates among biologists over the past two centuries have been concerned with the appropriate way to understand how specific forms and their variants emerge generation after generation. The last two or three decades have, however, witnessed little real dialogue on the problem of biological form because it has been generally assumed that, between them, genetics and molecular biology would give solutions to the problems of inheritance and morphogenesis. The genetic programme, the repository of instructions encoded in the DNA and selected for the capacity to generate organisms with morphologies and behaviour patterns adapted to specific environments, seemed the answer.

However, answers in science mean specific theories defining necessary and sufficient conditions for particular processes, with specific, testable predictions, and no one has yet defined exactly how the genetic programme is supposed to generate organisms of specific form and behaviour. What it can do is control the production of specific molecules and macromolecules in particular parts of the organism at particular times in its development. The assembly of large molecules into the characteristic structures of cells, and these into organisms of specific form, is generally assumed to follow either from the known laws of physics and chemistry, or from yet-to-be-discovered extensions of these.

What Rupert Sheldrake offers in his clearly written book is a radically different solution....

The concepts developed by Sheldrake and used to explain morphogenesis and behaviour sound thoroughly vitalistic. However, he argues that his approach is not so much vitalistic as organismic, in the sense of A. N. Whitehead's philosophy of organism, since he applies the principle of formative causation to the physical as well as the biological realm. Thus physical morphogenesis such as condensation of subatomic particles to form atoms, atoms to form molecules, and molecules to form liquids and crystals, are all treated in terms of the same causal principles as those assumed to be operating in living organisms. Furthermore, his specific predictions about the outcome of clearly-defined experiments contrast with the absence of

such tests from previous vitalist theories in biology. Therefore Sheldrake considers that his approach to biological form and behaviour is rigorously scientific and that its experimental consequences are spelt out much more clearly than those dependent on the concept of a genetic programme.

The virtues of this book lie in the clarity of treatment and the deductive rigour which Sheldrake brings to the examination of a central biological problem. Whether or not one agrees with his proposals, and I find myself reluctant to share his views because of the dualism he introduces into science in the form of energetic and non-energetic fields, it is quite clear that one is dealing here with an important scientific inquiry into the nature of biological and physical reality.

The hypothesis was discussed on several radio programmes in Britain, theUnited States, Australia and Western Europe, and was widely publicized in newspapers and magazines, ranging from The Times *to the* Evening Standard, *from* Science Digest *to* Harper's and Queen, *and from* Omni *to the* Spectator. The Guardian *devoted an editorial to the subject:*

Darwin revisited

In spite of its long tenure at the centre of the scientific stage, the neo-Darwinian theory of evolution - mutation regulated by natural selection - remains only a tantalising hypothesis. Tantalising because it squares with so many of the facts as known, hypothetical because it does not square easily with all of them. There is nothing uncommon about that: most scientific principles are hypotheses awaiting disproof or improvement. In the neo-Darwinian case, however, it was long assumed, at least among laymen, that biologists were happy with what they had got and were not expecting radical changes. That is not so. There is something of a ferment of ideas attempting to grapple with the central doubt about mutation and natural selection, namely that even over long stretches of time they seem an inadequate means of accounting for the diversity of life and the high specialisation of the cells and organisms composing it. Hence biologists, or at least the sceptics among them, will not let go of Lamarck, who pioneered the heretical belief that acquired characteristics can be inherited.

Lamarck's is not an easy theory to grapple with because it requires the experiences undergone by an organism to change the structure of that organism's genes so that the experience is passed on to an offspring. A timely contribution to this debate of which a good deal more is likely to be

heard has now come from a British biochemist, Dr Rupert Sheldrake, in his book A New Science Of Life: The Hypothesis Of Formative Causation (Blond and Briggs, C12.50). Dr Sheldrake suggests that there is more than one influence on the development of an organism; that its physical and chemical structure as determined by the gene, in the case of animals and plants, is supplemented by a behavioural pattern supplied by previous and contemporary members of the same species. Thus if rats learn a trick by the usual methods of reward and punishment other rats, not necessarily only the offspring, should learn the trick faster.

According to Doctor Sheldrake experiments have shown that they do indeed learn faster. Or consider the cuckoo: it never sees its parents; adult cuckoos gather and migrate long distances towards the end of summer; a month later young cuckoos gather and migrate to the same place. It is hard to explain such phenomena by the mechanistic rules of neo-Darwinism, which is why the debate keeps breaking so absorbingly forth.

(*The Guardian*, 27 July 1981)

This publicity (which I had not at all expected) formed the background to an extraordinary editorial attack in Nature:

A book for burning?

Books rightly command respect, even affection. They are the products of sustained creative work, and even if their authors' ambitions are not fully realized, find a niche somewhere as pebbles on the beach of scholarship and literature. And even bad books should not be burned; works such as *Mein Kampf* have become historical documents for those concerned with the pathology of politics. But what is to be made of Dr Rupert Sheldrake's book *A New Science of Life*, published in the summer (Blond and Briggs, London, 1981)? This infuriating tract has been widely hailed by the newspapers and popular science magazines as the ‘answer' to materialistic science, and is now well on the way to being a point of reference for the motley crew of creationists, anti-reductionists, neo-Lamarckians and the rest. The author, by training a biochemist and by demonstration a knowledgeable man, is, however, misguided. His book is the best candidate for burning there has been for many years.

The argument is simple. If physicists cannot confidently calculate crystal structures or molecular configurations *ab initio* from a simple knowledge of their ingredient atoms, surely it is unthinkable that molecular biologists will be able accurately to relate the genetic struc-ture of an

organism to the successive phenotypes that arise during the course of its development? And the difficulty is not merely computational. Do not the cells of arms and legs contain exactly the same DNA, Sheldrake asks? And how can it be that the very similar genomes of human beings and chimpanzees give rise to very different phenotypes when, in *Drosophila*, much larger differences of genotype specify flies that are very similar in form? Sheldrake's way of dealing with these and other observations is to borrow from embryology the vague notion of the morphogenetic field, the name given to the largely unidentified influences that give shape to a developing organism, and then to exalt this into a kind of universal agency. All aggregations of matter, animate or otherwise, are supposed to be exposed to a great variety of morphogenetic fields which fill the whole of space and which travel forwards in time. The effects of these morphogenetic fields (which are otherwise not described) are to ensure that particular aggregations of matter take on the form assumed by similar aggregations of matter elsewhere or at some earlier time. This is the 'hypothesis of formative causation' – the subtitle of the book. The inheritance of acquired characteristics is at a stroke explained. Jung's collective unconscious is made inevitable. Instinctive animal behaviour becomes a non-problem, for there will be a morphogenetic field that ensures that each set of neurones in each central nervous system is put into an appropriate correspondence.

This argument would not be taken seriously were it not that others have done so. As things are, however, Sheldrake's book is a splendid illustration of the widespread public misconception of what science is about. In reality, Sheldrake's argument is in no sense a scientific ar-gument but is an exercise in pseudo-science. Preposterously, he claims that his hypothesis can be tested - that it is falsifiable in Popper's sense - and indeed the text includes half a dozen proposals for experiments that might be carried out to verify that the forms of aggregations of matter are indeed moulded by the hypothetical morphogenetic fields that are supposed to pervade everything. These experiments have in common the attributes of being time-consuming, inconclusive in the sense that it will always be possible to postulate yet another morphogenetic field to account for some awkwardly inconclusive result and impractical in the sense that no self-respecting grant-making agency will take the proposals seriously. This, however, is not the most serious objection to Sheldrake's attempt to endow his argument with an appearance of orthodoxy. The more serious objection to his argument is that it says nothing of any kind about the nature and origin of the crucial morphogenetic fields and contains no proposals for investigating the means by which they are propagated. Many readers will be left with the impression

that Sheldrake has succeeded in finding a place for magic within scientific discussion - and this, indeed, may have been a part of the objective of writing such a book. But hypotheses can be dignified as theories only if all aspects of them can be tested. Sheldrake's hypothesis is no better than the hypothesis that a person equipped with a water-divining rod is able to detect subterranean water as a consequence of some intervening 'field' generated by the presence of water, and his proposals for experimental tests no better than the argument that since water-diviners succeed in making money, there must be something in the theory.

It is naturally distressing that these plain truths are not more widely understood. It is also a misfortune that the public expectation of what science can accomplish, now coloured by Dr Sheldrake's argument, should so conspicuously lack patience. Sheldrake's argument takes off from his catalogue of the ways in which the molecular biologists, no doubt the shock-troops of the reductionists, have so far been unable to calculate the phenotype of a single organism from a knowledge of its genotype. But so what? Have not the past twenty years shown clearly enough that molecular explanations of biological phenomena are, contrary to some earlier expectations, possible and powerful? And who says that molecular biology must be counted as a failure for as long as embryology is not a branch of mathematics? Dr Sheldrake, whose background should have enabled him to know better, has done a great disservice by helping to give currency to these misconceptions and false expectations. His book should not be burned (nor even confined to closed shelves in libraries) but, rather, put firmly in its place among the literature of intellectual aberrations.

(*Nature*, 24 September 1981)

However, far from consigning the hypothesis to oblivion, this attack caused even more public interest, and provoked immediate reactions. The editor of the New Scientist, *Michael Kenward, responded as follows*:

Burning editorials

They are at it again in Little Essex Street. Last week's Nature has an editorial entitled 'A book for burning?' While most 'popular science magazines', (like ours) to use a phrase culled from the editorial, prefer to review books with the reviewer's name appended, Nature slams anonymously into Dr Rupert Sheldrake's book *A New Science of Life*....

Readers - of both *Nature* and *New Scientist* - with memories will recall a similar episode when *Nature* launched an attack on a scientist with somewhat unorthodox ideas not long ago. Then the battered scientist was Ted Steele. Does this mean that Nature has abandoned the scientific method whereby ideas are launched on to the world to be tested by the scientific community? Or are we to have trial by editorial?

Michael Kenward (*New Scientist*, I October 198 1)

The controversy was reported on The World Tonight *(BBC Radio 4) on 30 October 1981; and the programme included a 15-minute discussion between myself and the editor of* Nature, *John Maddox, together with Janet Cohen. This is how it concluded:*

Maddox: If I might put it like this, Dr. Sheldrake, what you are saying if I understand your book correctly, and if I have understood you just now correctly, is that because you start off with a fertilized cat's egg, a fertilized cat's ovum, and because somewhere in space and time there is a 'Cat Field' that egg will be guided towards the shape of first of all an infant cat - a kitten, and then towards the shape of an adult cat. Is that right?

Sheldrake: Yes. What I am saying is that these things that give rise to patterns in nature, particularly the patterns or forms or structures of living organisms, are not really governed by timeless laws or simply by the chemicals they inherit, although the chemicals they inherit are very important, but that they are more like habits and they depend on what has happened in the past and how often it has happened.

Maddox: One of Dr. Sheldrake's problems is that not merely does he require a morphogenetic field for cats, but he requires a morphogenetic field for Abyssinian cats, Siamese cats, for ordinary tom cats in the alley, and the sheer complexity of this interpenetrating set of fields really does make the mind boggle. Particularly when one recognizes that cats are only one of a few million species.

Sheldrake: The thing that is mind-boggling is the sheer diversity and complexity of nature. I mean there are all these different kinds of animals and plants. And if we are to understand why there can be so many different kinds of cats and so many other different kinds of animals and plants and birds and so on, then the complexity of nature is a fact: the problem is how to explain it.

Maddox: Well, Dr. Sheldrake, there is a very elementary, and I admit qualitative, but nevertheless suggestive explanation. You have talked

about the molecules of DNA, the constituents of the chromosome of every living creature, and the molecular biologists since the late 1940s have been pointing out that the number of variations you can play on the chemical constitution of a single DNA molecule of the size one finds in a human organism, is quite sufficient to explain far more than the diversity of species now alive.

Sheldrake: I am perfectly well aware of course that the conventional view among molecular biologists is that this in time will be solved in conventional ways. But my point is simply this, this is an act of faith on their part, and through the whole history of developmental biology and embryology there have been people who have dissented from that kind of view.

Maddox: The conventional scientific view, which I think is entirely proper, is that there is no particular point in inventing theories which in themselves require a tremendous feat of imagination and constitute an assault on what we know about the physical world as it stands, when there is at least a chance, and in this case a good chance, in my opinion, that conventional theories will in due course provide an explanation.

Could I explain to Dr. Sheldrake why I feel so strongly about his book? I do think these interesting questions you describe are actually non-questions. By that I mean that I know of no convincing evidence to suggest that the phenomena - the collective unconscious, the paranormal, and so on - that these are *real* phenomena. And while it is entirely right and proper that people in their arm-chairs should sit back and wonder how it is that this or that spoon bends in the hands of somebody, but not in the hands of somebody else, I do think it is the business of serious, sober scientists, if you like - and that is where I acknowledge that I am sounding a bit crusty - to concern themselves with problems that are real. I am very worried indeed at the way that this will have comforted all kinds of anti-science people.

In Nature's *Correspondence columns, letters on this subject continued to appear for months. Here are some of them.*

Incendiary subject

SIR - Perhaps you are right, after all, to describe Dr Rupert Sheldrake's *A New Science of Life* as a book for burning. For after seeing the disastrous effect Sheldrake's book has wrought upon the detachment, not to say the

common sense, of one with the responsibilities of the editorship of *Nature*, I shudder to contemplate the effect upon the ordinary man,

But perhaps it is the influence of a pulpit from which to denounce scientific heresies that is the danger, rather than the book itself. For surely there is nothing in the book to raise excitement to the point of lumping together 'creationists, anti-reductionists, neo-Lamarckians and the rest'. For scientists, the worst a book can do is to waste their time. You could have served us better by arranging for the publication of two careful and opposed critical reviews. For non-scientists, unhelpful books abound. Their ability to mystify science is as nothing compared with strenuous attempts to declare an orthodoxy.

ROBERT HEDGES *Oxford University, UK*

SIR - I must voice my grave concern that, in the influential editorial pages of *Nature*, reasoned argument has given way to the emotional outburst of your comment 'A book for burning?' Amid many heated adjectives you condemn Dr Sheldrake's *A New Science of Life* as 'the best candidate for burning there has been for many years' because (a) his claim that it can be tested is 'preposterous', and (b) the theory is incomplete regarding the 'nature and origin' of the morphogenetic fields postulated by Sheldrake and 'the means by which they are propagated'. The second reason is, you say, 'more serious', adding that 'hypotheses can be dignified as theories only if all aspects of them can be tested'.

This second objection, if it were partial grounds for making a publication a candidate for burning, would prevent the publication of any hypothesis until it had been articulated to its last detail - a sure method of stifling all innovation.

For the first objection, (a) above, three arguments are advanced: (i) the experiments are time consuming; (ii) it would be possible to explain away negative results; (iii) no grant-making agency would support the experiments. Argument (i) would condemn all research into inheritance, not just that proposed by Sheldrake; argument (ii) applies in principle to any experiment, but is in this case vacuous since Sheldrake clearly states that he would regard failure as disproof; and argument (iii) is equally empty in its appeal to 'higher authority' without any indication as to why no agency would support the experiments.

I share *Nature's* concern expressed in the comment that the public should not gain the impression that science contains irrational elements. But the way to combat this impression is by displaying rationality.

C.J.S. CLARKE *University of York, UK*

SIR - In a leading article you reject Dr Sheldrake's morphogenetic fields as 'pseudo science' on the grounds that he does not prescribe their nature or origin, or discuss how their laws of propagation might be discovered. But the properties of heat, light and sound were investigated long before there was any understanding of their true nature, and electricity and magnetism originally had exactly the status that you criticized in the hypothetical water-divining example. Were such investigations pseudoscience?

You claim that hypothesis can be dignified as theories only if *all* aspects of them can be tested. Such a criterion would bar general relativity, the black hole and many other concepts of modern science from being regarded as legitimate scientific theories.

The discussion of Dr Sheldrake's proposed experiments and their falsifiability is rendered void since it assumes *a priori* that the experi-ments will fail.

The rapid advances in molecular biology to which you refer do not mean very much. If one is on a journey, rapid progress on the way implies neither that one is close to one's destination, nor that the destination will be reached at all by continuing to follow the same road.

By referring to 'self-respecting grant-making agencies' you show a concern not for scientific validity but for respectability. The fundamental weakness is a failure to admit even the possibility that genuine physical facts may exist which lie outside the scope of current scientific descriptions. Indeed, a new kind of understanding of nature is now emerging, with concepts like implicate order and subject-dependent reality (and now, perhaps formative causation). These developments have not yet penetrated to the leading journals. One can only hope that the editors will soon cease to obstruct this avenue of progress, and instead encourage reviews of the field.

B. D. JOSEPHSON
University of Cambridge, UK
(15 October, 1981)

Sheldrake's truth

S I R - I read with interest and alarm your editorial 'A Book for Burning?', in which you criticized Rupert Sheldrake's new book *A New Science of Life* (Blond & Briggs, London, 1981). In particular, a strong, sometimes even hysterical, attack was directed at Sheldrake's alleged belief in the 'failure' of molecular biology and at his 'vague notion' that the idea of

morphogenetic fields, developed by embryologists such as Conrad Waddington and elaborated mathematically by theoreticians such as Rene Thom, can find wider application in the life sciences. Sheldrake's views were denounced as 'pseudoscience', 'popularist' and as introducing 'magic' into science. It was implicit in the editorial that Sheldrake is to be considered as an exponent of the intellectually bank-rupt nineteenth-century doctrine of vitalism, which has quite rightly been forced to yield place to the subsequently productive (but arguably no less mystical) reductionist schools of thought.

If I understand Sheldrake correctly, it seems that his conception of molecular biology is not as a sterile failure, but as an important and crucial contribution to the analysis of problems of intracellular organisation on which the physiology of the whole organism clearly depends. All Sheldrake appears to have done, then, is to state that the whole is not just the sum of its constituent parts and that higher organizational states cannot be understood in reductionist terms. Although his book is not without its scientific solecisms, Sheldrake has raised many stimulating arguments, and presents an important landmark in the application of a formal geometry to living things as begun by Waddington and Thom. Certainly, I feel that the book is too important to dismiss easily, and in conclusion I should like to recall Milton's dictum that '. . . Truth never comes into the World but like a Bastard, to the ignominy of him that brought her forth'. (*The Doctrine and Discipline of Divorce, 1643-44*).

M.T. ISAAC
St Bartholomew's Hospital Medical College,
London, UK
(29 October 1981)

Harsh words

SIR - Your comments on Dr Rupert Sheldrake's book *A New Science of Life* are very harsh. One can hardly pretend that our current understanding of development and its genetic control is adequate. We should not even believe that it is oriented in the right direction. The concept of morphogenetic fields is neither unscientific nor without support from some distinguished embryologists. If the same concept is extended to all aggregations of matter, animate or otherwise, it may be considered as a bold, hazardous, foolhardy or unwarranted generalisation, depending on one's attitude towards new thought. Just a hundred years ago if somebody suggested that the sequence of nucleotides in DNA constitutes the information required to make a fly, a

frog or a man from a small bit of cytoplasm it would have been dismissed as preposterous. If there is no obvious way of testing experimentally Dr Sheldrake's hypothesis of formative causation, it does not preclude its possibility in the future.

Many embryological theories depend on recourse to morphogenetic fields in spite of the recent progress in genetics and molecular biology. No real progress can be made in understanding the importance of these fields without identifying the nature of the variables along the gradients. Perhaps the only measurable variable to which the origin of a gradient can be traced is the concentration of diffusing substances. Positional information which adds a new dimension to the concentration of diffusing substances is also now accepted as a fundamental mechanism of morphogenesis. However, even these powerful theoretical tools have not successfully accounted for any single embryological event. In fact it would be preposterous to assume that development of complex organisms can be explained solely on the basis of the current theoretical platform on which molecular biology stands. Recourse to new conceptual framework and unconventional mechanisms is imperative to understanding complex biological processes such as morphogenesis.

K. VASUDEVA RAO
Department of Zoology, University of Delhi, Delhi, India.□(12 November 1981)

Morphic field sports

SIR - The rather intemperate reaction of *Nature* to Sheldrake's hypotheses, against which Josephson has protested, is in part the result of Sheldrake's own choice of Bergsonian - or Paracelsan – explanations for the effect he postulates, and in part the result of noncommunication between biology and physics.

Had Sheldrake said that the quantum interconnectedness might extend to macrosystems, including biological systems, I do not think that *Nature* would have felt that its virginity was in peril. A model of interconnectedness does in fact flow from Bohm's idea of explication. The experimental agenda is to see how far beyond the subatomic level this patterning extends. If particles correspond to the asteroids and spaceships of a video game, appearing to behave as objects subject to cause-effect, but being in fact virtual displays built up from pulses which bear no translational resemblance

to the 'display', Darwinian evolution might well be (some would say, 'must be') a video game of the same order, appearing to follow simple selection-adaptation principles, as the game-pieces appear to collide, explode and so on - but in fact determined by information from an implicate substrate.

The relevance of interconnectedness to middle-order systems looks like a prime candidate for confirmatory research. If it were confirmed, Sheldrake would be both right in principle and wrong in his postulated mechanism of 'morphic fields'; viewed in this way, his suggestion is far from absurd.

One awaits with interest *Nature's* reaction to the first book which points out that Bohm's model also blows up the convention of Helmholtzian mind. That suggestion might prove even more alarming than a physics-based neovitalism.,

ALEX COMFORT
UCLA Neuropsychiatric Institute,
California, USA
(14 January 1982)

Nature *returned to the attack with a review by Professor D.R. Newth.*

A haunted house of cards

The title of this book is misleadingly modest. The author is not content to propose only a new science of life, for he reassesses many features of the real world that have been revealed by natural science, and proposes that there exists a great conservative principle making itself felt as much, or more, by sub-atomic particles as in developing embryos or in the behaviour of human beings. The principle is that what happens, or has happened, can exert an influence that is without decrement in space or time upon future events of a similar kind. This influence acts to promote a repetition of what has gone before. The degree of similarity qualifying a living organism to respond to these is conspecificity. Not all decisions or events, however, are susceptible to the principle of 'formative causation'.

The immediate recipient of the messages is a 'morphogenetic field' which guides formal change in its associated 'morphogenetic germ' until its prescriptions have been met and the 'morphic unit' is finally co-extensive with the field. The morphogenetic field blends the experience of all previous similar morphic units by a process of 'morphic resonance'. Neither morphic resonance nor the obedience of the morphogenetic germ to the dictates of its morphogenetic field involve exchanges of matter or energy.

This, I understand it, is the burden of Dr Sheldrake's argument.

It is, of course, brave to expound in little more than 200 pages so revolutionary a denial of everything that empirical science has made seem probable. Nor should we deny some leniency to the holders of really way-out ideas. They lack the support of a comforting background of assumptions held in common with their readers. But they should at least try to be clear, at whatever cost to their credibility.

Dr Sheldrake writes as Mrs Bloom daydreamed, with no one theme rigorously explored before it sets off another which before resolution gives way to something else again. It would be unkind to suggest that this is a device for escaping from difficulties, but even readers who are wholly unsympathetic might welcome a clearer view of the author's position. For example, in discussing the limitations of morphic resonance he assumes that while past events can be effective now, future events cannot. While conceding that it is logically conceivable that they might be, he excludes the future on the ground of simplicity and remarks severely that 'only if there were persuasive empirical evidence for a physical influence from future morphic events would it become necessary to take this possibility seriously'. Apart from the ambiguity of introducing *physical* influences into a discussion of extra-physical phenomena, one is left wondering why the same severity is not applied to the past.

Indeed, Dr Sheldrake does believe that his ideas are capable of receiving support from experiment, but his proposals for experiments are curiously tentative and unsatisfactory. Thus ... *if thousands of rats were trained to perform a new task in a laboratory in London, similar rats should learn to carry out the same task more quickly in laboratories everywhere else. If the speed of learning of rats in another laboratory, say in New York, were to be measured before and after the rats in London were trained, the rats tested on the second occasion should learn more quickly than those tested on the first*.

Well, yes, but so they should without the London intervention, and any quantitative predictions in the operation of this hypothetical principle are so wholly arbitrary that the design of such experiments would be difficult indeed. Dr Sheldrake concedes this in his rather casual suggestions for a handful of investigations in each of which he describes a possible result supporting formative causation, but the opposite result is inconclusive. It would be a help if he could offer us predictions the failure of which would end the matter.

Anyone tempted to take formative causation or morphic resonance seriously should ask themselves why. A world haunted by messages from

the past, some, like those from morphic units of extinct species, destined to vibrate eternally and in vain while seeking a morphic germ with which to resonate, may have a poetic appeal. Unfortunately it may also appeal to a perverse fear of scientific understanding. Dr Sheldrake explains early in the book that while some outstanding biological problems are difficult, others are, in principle, insoluble – for example, those associated with evolution and with the origin of life. Neither, as it happens, is suitable for the operation of formative causation, since they are creative and unique rather than repetitive. But by the end of his exposition one reader had the distinct impression that intrinsic insolubility had its own attractions for him and that the hypothesis of formative causation was his contribution to a happy state of confusion.

D.R. NEWTH
(5 November 1981)

But in Nature's *Christmas Books Supplement, where eminent contributors were asked to name the books they particularly enjoyed in 1981, Lord Ashby, F.R.S. selected* A New Science of Life, *describing it as 'an astonishing challenge to orthodox theories of plant and animal development'.*

Articles about this hypothesis and the debate it has provoked appeared over the succeeding three years in newspapers, magazines and books inBritain, Australia, India, Japan, South Africa, the U.S.A. and throughout Western and Eastern Europe, and these ideas have been discussed on radio and television in at least 10 countries. The great majority of these discussions have been sympathetic to the new lines of investigation suggested by the hypothesis; but of course the controversy has not died down.For example, following an article about the hypothesis in the Guardian, *Professor Lewis Wolpert, F.R.S. took up arms again*:

A matter of fact or fancy?

The scientific analogy to the artist starving in the garret is that of the visionary outside the conventional laboratory whose ideas initially arouse scorn, but eventually become accepted. Certainly this is the view that the media so often give. Brian Inglis perpetuates it with his article (Body and Soul, December 28) in which he gives full support to the ideas contained in A New Science of Life by Rupert Sheldrake.

Sheldrake has put forward the notion that in the biological world processes such as embryonic development, evolution and learning involve a mystical process called formative causation. Central to his view is morphic resonance. He suggests that the ways a hand, or eye develop are only understandable in terms of morphogenetic germs in the cells which resonate with past forms. He thus rejects as inadequate the whole of genetics, embryology and evolutionary theory (not to mention learning theory) and claims to have found the answer to all these inadequacies in his new theory.

Sheldrake's views are just those of an updated Vitalist, mystical and useless as ever. The core idea is that the nature of living organisms can never be explained in terms of physics and chemistry. It is singularly bizarre that Vitalism should be revived at present, when molecular biology is being ever more successful. That it should be so welcomed must reflect a deep and genuine need in many people for mystical explanations that leave the soul intact and make our mortality more tolerable. Like religious beliefs, these have nothing to do with science.

We know a great deal more about the development of pattern and form than Sheldrake acknowledges. His discussion of regeneration is typical of his general approach. Many animals are able to regenerate lost parts. Sheldrake asserts that such biological regulation cannot be accounted for by any machine-like system. But this is to ignore our current understanding of, for example, one of the simpler regenerating systems - hydra.

We do not have a complete understanding of how hydra regenerates its head when it is removed or how a small but normal hydra regenerates from a fragment of a larger one, but there is a wealth of experimental detail and, more important, there are models which provide a very good account of the process of regeneration. They show unequivocally how a physico-chemical system could provide the basis for regeneration of pattern. None of this work in any way requires concepts like morphic resonance. It is also true that there are many aspects that we do not understand, But this is not to say that the processes present an impenetrable mystery: quite the contrary, we have a very good idea as to what questions we wish to answer.

For a new theory to be taken seriously, it must at the very least deal with the current experimental data as well as current theories. Morphic resonance is totally hopeless in this respect since it does not even touch the data. It merely asserts the action of unmeasurable, nonquantifiable forces. But, cry its defenders, the theory is testable and therefore conforms to Popper's criterion for science. This view completely misunderstands the nature of science. It is possible to hold absurd theories which are testable, but that does not make them science. Consider the hypothesis that the poetic

Muse resides in tiny particles contained in meat. This could be tested by seeing if eating more hamburgers improved one's poetry.

One of the characteristics of pseudo science is its reliance on singular observations. Single cases can disprove a theory, but in order to give up the whole of genetics and development one would have to be very sure that the counter observations were correct. You could be persuaded that the Queen was a Russian agent, but because this would require you to give up all those beliefs and evidence that you had to the contrary, you would have to find the evidence overwhelming. So it is in science.

Science does not advance by the work of cranks being initially rejected and then accepted. Quite the contrary. Science advances by means of people doing laborious and often difficult experiments, arguing with each other, changing their minds, thinking up new theories and modifying them in a large social enterprise. Certainly new ideas are often treated with scepticism and so they should be. But these ideas are within a common framework of scholarship.

It is not surprising that Inglis suggests that Sheldrake's ideas may provide a basis for understanding the paranormal. That would be explaining the unreal with the non-real: like multiplying zero by zero.

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(11 January 1984).

The, theoretical models of regeneration in Hydra *to which Professor I Wolpert refers concern possible physical or chemical factors which might influence cells in different positions within the tissues to synthesise different proteins. However, this does not undermine the discussion of morphogenesis in this book, which in fact assumes for the Purpose of argument that such factors could not only be postulated theoretically, but actually identified by empirical research (p. 40).*