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Science: A ‘Dappled World’ or a ‘Seamless Web’?

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Nancy Cartwright, *The Dappled World: Essays on the Perimeter of Science* (Cambridge: Cambridge University Press, 1999), ISBN 0-521-64411-9, viii + 247 pp.

In their much discussed recent book, Alan Sokal and Jean Bricmont (1998) deride the French deconstructionists by quoting repeatedly from passages in which it is evident even to the non-specialist that the jargon of science is being outrageously misused and being given meanings to which it is not remotely relevant. Their task of ‘deconstructing the deconstructors’ is made far easier by the evident scientific illiteracy of their subjects.

Nancy Cartwright is a tougher nut to crack. Her apparent competence with the actual process of science, and even with the terminology and some of the mathematical language, may lead some of her colleagues in the philosophy of science and even some scientists astray. Yet on a deeper level of real understanding it is clear that she just does not get it.

Her thesis here is not quite the deconstruction of science, although she seems to quote with approval from some of the deconstructionist literature. She seems no longer to hold to the thesis of her earlier book (Cartwright, 1983) that ‘the laws of physics lie’. But I sense that the present book is almost equally subversive, in that it will be useful to the creationists and to many less extreme anti-science polemicists with agendas likely to benefit from her rather solipsistic views. While allowing science some very limited measure of truth—which she

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defines as truth of each ‘model’ in its individual subject area within its ‘shield’ of carefully defined conditions set by the scientists themselves—she insists that there is no unity, no sense in which there are general laws of physics with which all must be compatible. She sees all of the laws of physics as applying only *ceteris paribus*, all other things being equal. Whenever other things are not equal, she seems to abandon the laws as any guide. In this sense, she advocates a ‘dappled world’ in which each aspect of reality has its separate truth and its separate ‘model’.

Reading further, one senses that the problem may be that she is bogged down in eighteenth- and nineteenth-century (or even older) epistemology while dealing with twentieth-century science. The central chapters seem to depend heavily on such outdated, anthropomorphic notions as cause and effect, recurrent regularities, capacities, etc. To me, the epistemology of modern science seems to be basically Bayesian induction with a very great emphasis on its Ockham’s razor consequences, rather than old-fashioned deductive logic. One is searching for the simplest schematic structure which will explain all the observations. In particular, what seems to be missing in the thinking of many philosophers of science—even the great Tom Kuhn, according to Steven Weinberg—is the realisation that the logical structure of modern scientific knowledge is not an evolutionary tree or a pyramid but a multiply-connected web.

The failure to recognise this interconnectedness becomes obvious when we are presented with ‘classical Newtonian mechanics, quantum mechanics, quantum field theory, quantum electrodynamics, Maxwell’s electromagnetic theory’ and, in a separate place, ‘fluid dynamics’, as logically independent and separate rather than as, what they are, different aspects of the same physical theory, the deep interconnections among them long since solidly cemented.

Another part of the problem with this book is that the two dominant examples chosen are physics and economics, the rationale being that both sciences have ‘imperialistic ambitions’, the physicists aiming to provide a ‘theory of everything’ in the physical world, and some economists claiming universal validity in the social sphere. These two sciences, however, are on different levels of the epistemological ladder. Physicists search for their ‘theory of everything’, acknowledging that it will in effect be a theory of almost nothing, because it would in the end have to leave all of our present theories in place. We already have a perfectly satisfactory ‘theory of everything’ in the everyday physical world, which only crazies such as those who believe in alien abductions (and perhaps Bas van Fraassen) seriously doubt. The problem is that the detailed consequences of our theories are often extraordinarily hard to work out, or even in principle impossible to work out, so that we have to ‘cheat’ at various intermediate stages and look in the back of the book of Nature for hints about the answer. For instance, there is nothing in the quantum mechanics of the chemical bond which implies the genetic code in its detailed form, yet there is equally nothing in the operations of molecular biology which is incompatible with our quantum-mechanical understanding of

the chemical bond, intermolecular forces, and so on. In fact in the defining discovery of the field, the double helix, that understanding played a crucial role. Thus the consequences often imply the laws without the laws implying a particular set of consequences. Physics is well embedded in the seamless web of cross-relationships which is modern physical science.

Economics, on the other hand, is an example of a field which has not yet achieved interconnection of enough related information to have real objective validity. It resembles medicine before the germ theory, or biology before genetics: there are a lot of facts and relationships, but there are probably some unifying principles and connections to other sciences which are yet to be found. Yes, the Chicago school makes ambitious claims—so did the Marxists in the past. Both, to my mind, qualify as ideologies rather than theories and belong in the political sphere. There are also serious economists—with several of whom I happen to have worked, including one of the creators of the mainstream Arrow–Debreu theory—who are doing their best to discover the deeper realities that may be there, and are in conflict with the dominant school. In science as in every other field of human endeavour the rule must be *caveat emptor*: science as a whole cannot be responsible for the temporary gullibility of the public to the claims of cold fusion, Freudianism or monetarism: these are just bad, or at best incomplete, science. In sum, whenever a school of scientists creates an intellectually isolated structure which claims validation only within its own area and on its own terms—that is, does exactly what Cartwright is claiming *all* scientists do—that science no longer has the force of dynamic, self-correcting growth which is characteristic of modern science. Cartwright's 'cocoon' are an excellent description of Freudianism or behaviourism in psychology, or of the response of electrochemists to cold fusion, but do not describe healthy science.

I have some particular reason for unhappiness about the message of the book: in a very early chapter she quotes me as being opposed to my clearly stated position. My best-known work on these subjects begins with these words: 'The reductionist hypothesis may still be a topic for controversy among philosophers, but among the great majority of scientists it is accepted without question. The workings of our minds and bodies, and of all matter [...], are assumed to be controlled by the same set of fundamental laws, which [...] we know pretty well'. Since it is clear that I was and am one of that 'great majority', it is disingenuous of Cartwright, who is one of those 'controversial philosophers', to quote succeeding paragraphs in such a way as to arrogate me to the opposite side.

There was a second place where I can fairly competently fault her understanding. In Chapter 8 she states that she will 'take as the central example' the BCS theory of superconductivity, an area which has been extensively studied by the 'London School of Economics Modelling Project'. I have been involved with the theory (and practice) of superconductivity for 43 years—for instance, I supplied a crucial proof which is referred to in the original BCS paper as a personal communication. In 1987 I gave a lecture

studying an important and neglected part of the history of this theory, which was written up and published by Lillian Hoddeson as part of the American Institute of Physics's history of solid-state physics (Hoddeson, 1992).

My contribution was called 'It Isn't Over Till the Fat Lady Sings'. I used that crude American metaphor from the world of sport to characterise the somewhat confused period (1957–1965) between the original BCS paper, which indeed proposed a 'model', and the approximate end of the verification and validation process which made the model into what physicists properly call the 'theory' of phonon-mediated (ordinary, or BCS) superconductivity. (My usage of 'model' may be rather different from that of the LSE modelling project. What I, and physicists in general, usually mean by the term is a simplified example which is in the right universality class—for the meaning of this term, read on.) At the end of that period we were in possession (i) of a microscopic theory controlled by a small parameter, hence described by a convergent perturbation expansion about a mean-field solution which is rather like BCS, and (ii) of a detailed understanding of a much wider range of phenomenology than the Ginsburg–Landau equations could provide. This is such that the theory is no longer confined to its 'cocoon' but deals well with all kinds of messy dirt effects. (The best books describing this outcome may be Parks' two-volume compendium (1969) and de Gennes' slim book (1966), both published in the late 1960s.)

Apparently, the LSE project accepts, for much of its account of BCS and G-L, a late pedagogical description (Orlando and Delin, 1990), by two engineering-oriented authors who had no part in the above history. It is known to many historians of science that textbooks tend to caricature the real process of discovery and validation, and this is an error I regret finding here. The only original literature quoted (except for BCS and for Gor'kov's early, model-based derivation of G-L from BCS) are unsuccessful previous attempts at a theory by Bardeen himself and by Frohlich, as well as others by such luminaries as Heisenberg, Salam, Wentzel and Tomonaga. (In the process, she renames my old friend Herbert Frohlich 'Hans'.)

So: in 1957 BCS may have been describable as a *ceteris paribus* model, with no adequate account of a wide range of phenomena, or of its own limitations. It was made, by 1965, into an enormously flexible instrument with a high degree of *a priori* predictive power, and even more explanatory power. In fact, one of the deep difficulties of theorists of the new high T_c superconductors is persuading the community that, flexible as BCS is, new principles of physics must be invoked to explain the new phenomena. But as is almost always the case, the new ideas do not destroy, but instead supplement, the old. Just as the discovery of quantum chromodynamics left quantum electrodynamics firmly in place, no sensible theory of high T_c will displace BCS from its validity in ordinary metals.

The story that Cartwright misses entirely, however, is the unifying and interleaving effect the theory of superconductivity had on very widely separated areas of physics. Far from being an isolated 'model' applying only in its

shielded cocoon (as in the misfit metaphor she uses of the SQUID magnetoencephalograph in its shielded room) it was an explosive, unifying, cocoon-breaking event. First, in its own field: it showed us solid-state physicists that we could no longer safely ignore the general structure of physical theory: our familiar electrons were no longer little particles but unquestionably were quanta of a quantum field. True, in accepting the exclusion principle we should long since have realised how implausible it would be for 'particles' to be *absolutely* identical, but we had come to make casual assumptions about them. Then our particle physics friends began speculating how the vacuum itself might be a BCS state, a speculation ending in the electroweak theory. Finally, the nuclear physicists realised that we might have found the explanation for a series of puzzling phenomena observed in nuclei, and made the nucleus into a paired state. Yet another epiphany came when we predicted, and found, that the rare isotope of He would be a BCS superfluid of a new kind. So even though, in terms of the fundamental, unifying, microscopic laws, BCS made not the slightest change, it taught us a new way in which quantum fields could act, and also called our attention to the very general phenomenon of broken symmetry which is one of the key ways in which complexity can emerge from those laws.

Let us get back to the book. One of the basic epistemological points on which I differ radically from Cartwright is a very common misconception. Like many others, she maintains that the primary goal of science is prediction, prediction in the sense of being able—or at least wishing—to exactly calculate the outcome of some determinate set of initial conditions. But that is not, for instance, what an archaeologist is doing when he measures a carbon date, or a fluid dynamicist when he studies the chaotic outcome of convection in a Benard cell. Rather, each is searching for understanding. In the one case, he wishes to correlate different measurements to get some idea of the sequence of past events, which surely could never have been predicted in the quantitative sense but may enlighten him as to fundamental human behaviours. In the second case, he knows to a gnat's eyelash the equations of motion of his fluid but also knows, through the operation of those equations of motion, that the details of the outcome are fundamentally unpredictable, although he hopes to get to understand the gross behaviour. This aspect is an example of a very general reality: the existence of universal law does not, in general, produce deterministic, cause-and-effect behaviour.

Of course, in some sense there is always an aspect of prediction, in the sense that correct predictions—but by no means detailed ones—are very strong validations of the theory. If the archaeologist sees a particular kind of pottery, he may predict and then verify the carbon date; then next time he will not have to check the date. But in the epistemology which describes at least the natural sciences, I believe that the goal is exactly what Cartwright is trying to convince us is impossible: to achieve an accurate, rational, objective, and unified view of external reality. In the final section of her Chapter 2, asking 'Where Do Laws of Nature Come From?', she gives as her answer, 'always the source must be

the books of human authors and not the book of Nature'. On crucial matters she is a social constructionist.

I have argued elsewhere that this is not a tenable position, at least unless one is willing to accept total solipsism. Our perception of the everyday world is based on fragmentary, unreliable data which we only put together by creating a 'schema' or theory about the actual nature and objective existence of the various objects—chairs, mothers-in-law, teddy bears, or whatever—which we hypothesise to be in it. Then we correct, verify and validate the theory by making predictions from it (if I reach out and touch that brown thing, it will be fuzzy). Or I ask someone else to confirm my idea. Thus if we reject the inductive methods of science, we reject our only way of dealing with reality. In order to maintain our daily lives we have to accept the objective reality of the world and that it is the same world for everyone.

Why is this necessarily the case? Because we have so many cross-checks, so many consistency conditions. In the end, the schema contains many, many fewer bits of information than the data our senses gather, so we can be sure that no other theory could possibly fit. Now, we see that we can think of science as simply a somewhat more abstract, somewhat more comprehensive extension of our schema, describing the external world and compressing the enormous variety of our observations into a comprehensible whole.

The process of deconstructing the rest of the book in detail is beyond my budget of patience. The last chapter, in which she deals with the quantum measurement problem, for instance, seems to advocate one of the thousands of alternative incorrect ways of thinking about this problem that retain the quantum-classical dichotomy. My main test, allowing me to bypass the extensive discussion, was a quick, unsuccessful search in the index for the word 'decoherence' which describes the process that used to be called 'collapse of the wave function'. The concept is now experimentally verified by beautiful atomic beam techniques quantifying the whole process.

Another key word missing from the index and from the book—I checked—is renormalisation. This is not just a technical trick but a central concept in the philosophy of physics, underpinning the physicists' use of model Hamiltonians, the passage to the limit of continuum equations, and even the modern view of statistical mechanics. A 'modelling project' which has anything to do with physics should hardly ignore the way in which we build and justify our models. The renormalisation group is a way to expand the scale from the atomic to the macroscopic which shows that often the result is an enormous simplification, a division of systems into 'universality classes' all of which behave the same way in this limit; hence we may pick the simplest model for further study.

Returning to Cartwright's other exemplary subject, there is another contrast here. It is a great advantage of physics over economics that we physicists can often actually justify our use of models in this way, whereas use of the same idea in economics is almost never justifiable. An economy cannot be sorted out into macroscopic vs microscopic, with the former constructed by simply

aggregating the latter: the individual agents have foresight and are of such widely different sizes and characteristics that averaging is meaningless, even if they behaved in any mechanistic or even rational way.

There is an attack on the entire science of molecular biology in the Introduction, making the hardly very philosophical plea that the allocation of funds for genetics should be slashed in favour of preventive medicine, childcare, and other worthy causes. I could agree that a very bad glitch in the patent laws—based on not very good science—has led to a frantically accelerated search for the 'gene for this and that disease', where almost all phenomena involve the collective contributions of many genes and perhaps even of the entire genome. But while we are being feminist, are we willing to give up DNA testing? Or the heavily molecular and surprisingly successful research programme on AIDS? These are political and moral questions and have no place in a book about epistemology. Science advances by looking under the streetlight where the light is, not by 'crusades' against socially acceptable targets. The political direction of scientific strategy which she appears to advocate here has a very bad historical record in which Lysenko is only the worst recent disaster.

In summary, this book seems to show that what may have happened in the philosophy of science—or at least in this corner of the field—is precisely the kind of intellectual isolation from outside sources which elsewhere leads to bad science. There is a reluctance to accept the fact that science has become a dynamic, growing web of interrelationships both within and across fields, and that even philosophers can no longer make do without taking into account modern insights into the workings of nature and of our own mentalities. The description in this book of the process of scientific discovery, in the chapter called 'Where Do Laws of Nature Come From?', is just false from the point of view of one who has participated in it. Scientists have increasingly, and in some cases consciously, had to invent for themselves the epistemology they actually use. Scientists are not particularly able philosophers, as the case of Bohr demonstrates, but at least they are in touch with reality at first hand, and their insights into the matter have profoundly changed our understanding of how we make discoveries. In the modern state of science, no discovery lives in a cocoon, rather it is built within and upon the entire interconnected structure of what we already know.

In a sense, this is a valuable book, in that it serves as a wake-up call telling me that it is time scientists themselves examined epistemology in the light of their experience of the reality of scientific discovery. When challenged on these subjects, many of us cite Popper's ideas. Though basically right as far as they go, these now seem out of date and naive. Two scientists who have addressed these matters are Murray Gell-Mann and E. O. Wilson, and my remarks above are strongly influenced by what they have had to say. Gell-Mann, in *The Quark and the Jaguar* (1994), and even more in remarks at various workshops of the Santa Fe Institute, has emphasised the role of 'compression', while Wilson (1998) proposes the term 'consilience' for the web of interrelationships. But it is

time to take a more definitive look at why—and, of course, when—science is right.

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For her, the world presented by the laws of physics is largely a fiction. The world in which we live, unlike that inside the laboratory, is a messy, unpredictable place, marked by discontinuities and fractures. The regularities promised by physics are rarely apparent. It is a more dangerous "in some ways a more interesting place" than the supposedly absolute and eternal laws of physics would suggest. Cartwright, unusually for a philosopher of science, displays a concern not only for science as knowledge but for its potentiality to change the world. If we are practically concerned with science our main interest is less with conceptual purity than with getting things to work. She cites the example of building a superconducting device which would allow us to detect the victims of strokes.

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