Being and becoming

H. M. Collins

The Construction of Social Reality. *By John R. Searle. Free Press/Allen Lane: 1995. Pp. 241. $25, £20.*

JOHN Searle is a philosopher at the University of California at Berkeley. In 1984 he delivered the BBC Reith lectures, cementing his reputation as one of the clearest and most forceful thinkers around. He is probably best known outside the professional heartland for his 'Chinese room' argument against artificial intelligence, which he repeated in his Reith lectures. Searle imagined a person in a room managing conversational interchanges in Chinese by manipulating Chinese word-symbols according to a rule book; the person in the room might or might not understand Chinese. Thus Searle proved to nearly everyone's satisfaction that symbol manipulation is not the same as understanding.

In his latest book, Searle looks at the social sciences. The title is provocative in that it contrasts with that of a famous book by Peter Berger and Thomas Luckmann, *The Social Construction of Reality* (1967). Searle aims to show the difference between what can and what cannot be socially constructed. On the way he develops a refreshingly clear exposition of the problems of the social sciences, in the positive sense of problems that are difficult and interesting.

Searle explains that the social sciences, as opposed to the natural sciences, have to deal with things that exist only because we think they exist. Take paper money: on the one hand there is the actual paper and printing; on the other hand there is the value that resides in money only so long as everyone continues to believe, act and talk as though it is valuable. (Searle includes an excellent discussion of the role of language in the creation of 'institutional facts'.) Once people stop thinking, talking and acting collectively as though money is valuable, it stops being valuable. This is a philosophical puzzle because money is at least at real in its effects as subatomic particles — as the frustrated builders of the Superconducting Super Collider know. Searle thinks, then, that there is social construction of social things and that these things are nevertheless real.

Once one sees that things that exist only because we think they exist affect all our lives in a way that is as concrete as can be, the recent arguments between natural and social scientists are put into context. Social scientists are surprised that natural scientists have difficulty with this kind of idea. For example, Richard Dawkins insists that there are no social constructivist at 30,000 feet who aren't hypocrites, yet if he has money in his pocket he is a social constructivist himself.

Where Searle differs from what he perceives to be the view of social constructivists is that he thinks the existence of social things presupposes a class of things that are there whether we think about them or not. I leave the details of the argument to the reader. Agree with him or not, in putting the matter so clearly, Searle shows the way to the interesting questions. Is it true that social things are based on nonsocial things? If it is true, where is the boundary between social and nonsocial? How do we tell where the boundary is? What constraints do nonsocial things place on the construction of social things and vice versa? If some social scientists have overstepped the boundary — and this may be the cause of the heat in recent debates — how can we argue the matter sensibly? How can we investigate the way in which facts come into being without each side simply trying to impose its authority? □

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Science evolving

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Evolutionary Naturalism. *By Michael Ruse. Routledge: 1995. Pp. 316. £35, $49.95.*

MICHAEL Ruse aims to describe what scientists actually do in their research and how they arrive at their theories — a mixed bag of false starts, fallacious reasoning, the cultivation of followers, the marketing of ideas and so on. His approach, evolutionary naturalism, rejects the traditional distinction between the normative and the descriptive analysis of science. For him the path of discovery to, say, Darwin's theory of natural selection makes a difference to the theory itself, whereas for the normative analyst it is just history. Normative analysts (who probably include most readers of *Nature*) would say that the logical structure of the theory, its truth or falsity and its relevance to the objective problem can all be assessed independently of the route of discovery.

A scientist's problem is to produce an explanatory theory of greater truth and depth than any rival theory; a look at the path of discovery might give us hints about how to interpret this objective problem situation. But, having said that, it is important to distinguish between Kekulé's tail-biting snakes and his problem situation (how to explain benzene phenomena), a distinction one wishes Ruse had explored systematically.

It is worth stressing that problems, which Ruse (following the philosopher Larry Laudan) believes introduce an obviously subjective element, can be treated as objective abstract entities. Anyone who doubts this can consult the surprisingly interesting *Guidelines for Examination in the European Patent Office* (European Patent Office, 1994). It is clear from this document that a person's subjective conception of an objective problem may be wrong and may fail to be decisive in the eventual solution. Ruse writes as if Karl Popper never said a word about the evolution of scientific theories from objective problems.

Ruse's use of Thomas Kuhn to undermine Popper's falsification theory is a weak assault on normative analysis. In the *Structure of Scientific Revolutions* (University of Chicago Press, 1962), Kuhn says that "[no] process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature". This passage is the start of the myth that Popper was a naive falsificationist (that is, someone who believes in conclusive falsification) and of the confusion that his falsificationism was an historical thesis. Falsificationism was meant as a normative proposal based on a logical analysis of the situation facing the scientist eager to learn from mistakes made in blindly groping for the truth. Only secondarily was it meant to suggest what actually happens in science. Nevertheless, there are many interesting examples that conform to the pattern of Popper's conjecture and refutation, for example Rutherford's refutation of J. J. Thomson's theory of the atom in 1911. (For more, see Popper's *Realism and the Aim of Science*, Hutchison, 1983).

There are two strange things about the above passage from Kuhn. First, we are supposed to regard it as a falsification of falsificationism. But why should we, if, as Ruse insists, scientists ignore falsifications? The naturalist does not have an answer, because he cannot tell you what you should or should not do. Second, rhetorically the argument trades on the tacit assumption that scientists mostly get things mostly right (and if there is a best method, then they will be using this soon if not now). But, being fallible, all of them may one day get it not just mostly, but completely wrong (or at least overlook a better method). And in fact, they have. The naturalist defines away this possibility. The normative analyst can also ask: how can we promote the growth of scientific knowledge? What method(s) should the scientist adopt if this is his aim? How should we control error? All these questions are lost in naturalism.

Ruse does shy away from a crude scientism that says that all problems can be