eing and ecoming

M. Collins

e Construction of Social Reality. *By n R. Searle. Free Press/Allen Lane: 95. Pp. 241. $25, £20.*

n Searle is a philosopher at the Unisity of California at Berkeley. In 1984 delivered the BBC Reith lectures, enting his reputation as one of the rest and most forceful thinkers nd. He is probably best known out- the professional heartland for his nese room' argument against artificial elligence, which he repeated in his ith lectures. Searle imagined a person room managing conversational interges in Chinese by manipulating nese word-symbols according to a rule ok; the person in the room might might not understand Chinese. Thus rle proved to nearly everyone's satistion that symbol manipulation is not same as understanding.

n his latest book, Searle looks at the ial sciences. The title is provocative in t it contrasts with that of a famous ok by Peter Berger and Thomas Luckn, *The Social Construction of Reality* 67). Searle aims to show the difference ween what can and what cannot be ially constructed. On the way he devel- a refreshingly clear exposition of the blems of the social sciences, in the itive sense of problems that are ficult and interesting.

Searle explains that the social sciences, opposed to the natural sciences, have to l with things that exist only because we k they exist. Take paper money: on the hand there is the actual paper and ting; on the other hand there is the e that resides in money only so long as ryone continues to believe, act and talk though it is valuable. (Searle includes excellent discussion of the role of guage in the creation of 'institutional ts'.) Once people stop thinking, talking l acting collectively as though money is able, it stops being valuable. This is a losophical puzzle because money is at st at real in its effects as subatomic ticles — as the frustrated builders of Superconducting Super Collider ow. Searle thinks, then, that there is ial construction of social things and t these things are nevertheless real.

Once one sees that things that exist ly because we think they exist affect all r lives in a way that is as concrete as can the recent arguments between natural d social scientists are put into context. cial scientists are surprised that natural entists have difficulty with this kind of a. 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 solved by science, but there are many forms of naturalism and it is unclear exactly where Ruse stands (see Susan Haack's *Evidence and Enquiry* (Blackwell, 1993) for clear distinctions and refutations of some forms of naturalism).

In the section on evolutionary epistemology, Ruse argues that scientific thinking (indeed, all thinking) is to be understood with the help of "epigenetic rules". These are rules of thinking that we are disposed to follow because of our evolutionary history, such as the law of non-contradiction. If our ancestors were inclined to regard sabre-toothed tigers as both dangerous and not dangerous at the same time, they would not have been our ancestors. The approach is much more promising than that of, say, the philosopher David Hull, who sees the scientific success of a theory simply in terms of the proliferation of memetic copies, without considering the rational filters at work. But, although promising, Ruse's approach shows its defects when he goes on to say that the necessity of logical rules is based on nothing but our feelings of necessity shaped by evolution. He neglects the fact that conforming to the law of non-contradiction saves us from moving in an argument from true premises to a false conclusion, and also enables us to test the implications of our theories (because a false conclusion of a valid argument implies a false premise). Logical necessity and validity are emergent properties of language and cannot be defined in terms of feelings (evolved or not).

Such a view also neglects the most interesting thing about evolution — the emergence of radically new structures and properties not fully derivable from, contained in or explicable by those that came before. Once we have language, for instance, we can build theories that no one person can fully grasp: they are no longer just a part of our psychology. Perhaps the ability to count, for example, was selected phylogenetically, but after Godel's famous incompleteness proof, we must say that the linguistically formulated theory of natural numbers has unfathomable content and that this is a fact independent of the way we feel about the proof. If this is not enough to convince Ruse that we are capable of producing systems that have independent properties, I would suggest looking at Danny Hillis's work, which suggests that self-evolving computer programs may produce perfectly working airline navigation systems containing millions of lines of program that nobody could possibly understand.

There are surely elements of marketing in the selection of scientific theories, but, if one wants to know about that, it is wise to consult advertising and marketing specialists. At the end of the day we have still to analyse what is the best method for enhancing the growth of scientific truth. We can teach students how to falsify theories and how to obtain jobs, and with enough ingenuity, there need be no conflict.

I have focused mainly on a few problems in *Evolutionary Naturalism*, but it covers in an informative way many other interesting areas. In the discussion of evolutionary ethics, for example, Ruse argues that our morals are, like our logic, based on biological evolution. The book is a readable and forthright presentation of his views on the relevance of evolution to philosophy and could serve as a useful introduction for students. □

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Relative values

Sean Nee

Macroecology. *By James H. Brown. University of Chicago Press: 1995. Pp. 269. $42.50, £33.95 (hbk); $15.95, £12.75 (pbk).*

FROM 1736, generations of the Marsham family, who lived near Norwich, England, each year meticulously recorded facts such as the dates of the first snowdrops and cuckoos, resulting in an invaluable data set for studying how species respond to variations in climate. Darwin provides the best example of how endless catalogues of facts about natural history, each fairly dull in itself, can be illuminating when looked at, imaginatively, as a whole.

This stamp-collecting instinct is rife in ecology. Starlings weigh 80 grams and four million of them live in Sweden. Ecologists do not of course stare blankly at these facts, mesmerized. Rather, they ask how and why starling numbers have changed over time, and how a starling's weight affects its chances of surviving the winter. These questions take us in one direction, towards the particular. James Brown invites us to go in the other direction and to ask, for example, about the relationship between body size and abundance of birds in general — that is, to put all the facts into one big data set, to find empirical generalizations and to try to understand them.

Clearly, there are many permutations. If we just look at the shape of the distribution of bird body sizes, we see that it is skewed, with many more small species than medium or large ones. This is true not only for birds but for mammals, fish, trees, bacteria and insects as well, suggesting that there is a general principle to be unearthed. The main variables for which macroecologists have data are the body size, abundance, habitat specificity and geographical range of species, providing us with spaces of up to four dimensions in which to plot species points and look for interesting features in the resulting cloud. Brown demonstrates that with imaginative use of these data, meaningful hypotheses can be posed and many of their implications can be tested.

This is not a "radical new research agenda", as the publishers would have us believe, although it is interesting and important, and this is the first book to package the field for wide consumption. But it does contain some radical ideas. For example, Brown offers an argument to explain the distribution of animal body sizes that defines fitness as the rate at which animals invest energy in reproduction. Because this invites us to discard current theories of life-history evolution, the author indeed "makes a major intellectual leap", but perhaps into the abyss.

Our thinking about allometry is also challenged. It is well known that larger mammals live longer, bear fewer young, have higher metabolic rates and so on. In other words, the relationship between body size and just about everything else is mono-tonic. Brown suggests that many of these relationships may in fact be triangular, with very small-bodied species showing the opposite overall trend. If true, this would be important. But so far there is only anecdotal evidence, with one exception. Certain data sets reveal a triangular relationship between body size and abundance, with the most abundant species having intermediate sizes. But it has been argued in the literature that the relationship is merely a sampling artefact, although Brown does not mention this.

In fact, there is much that Brown does not discuss, and other macroecologists reading this book will, quite rightly, be annoyed by the extent to which their work is ignored. One is given no hint, for example, that people have been using species–area relationships to predict extinctions for decades before the publication of a paper coauthored by Brown in 1992, which gets a chapter all to itself. Nevertheless, Brown is a dominant figure in this field and his work is a suitable vantage point for an overview of the macroecological research programme. □

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■The *Chambers Dictionary of Science and Technology* is 55 years old this year and was last revised seven years ago. The seventh edition, edited by Peter Walker, has just been published as the *Larousse Dictionary of Science and Technology.* Its 1,236 pages contain 49,000 entries, over 500 illustrations and many appendices. Larousse, £45 (hbk), £19.99 (pbk).