in influence between genes and social environment was about 70:30. But doubt has been cast on this research: it has been suggested that some of the data were "made up" to buttress Burt's view that genes encode the social order.

Peter Neubauer left less to chance in his study. Working through the Louise Wise Adoption Agency in New York, he carried out a long-term social and psychological experiment. He arranged for twins — and on one occasion triplets — to be separated and adopted by families from different social backgrounds. Neither the families nor the children were told. The 'experiment' came to light when two of the triplets accidentally discovered each other, and then found the third, a story suspiciously like Ken Follett's novel *The Third Twin*.

Thomas Bouchard of the University of Minnesota made his reputation by studying the similarities and differences of separated twins whom he reunited. Extreme cases of similarity exist in which the twins' lives seem to match in uncanny detail, even down to the clothes they wore when they first met as adults.

Although these findings appear astonishing at first sight and make great newspaper stories, what do they really mean? How many are just coincidences? Some are probably secondary to physical characteristics, which certainly are genetic in origin. Should similar research not be carried out using people less closely related? If scientists believe that genes do predispose people to wearing particular clothes, they should use techniques similar to those used to identify the genes suspected of predisposing towards inherited conditions such as asthma. According to Wright, Bouchard thinks it is too much effort to look at such control populations: "What could it tell you?"

What do scientists mean by environment? A lot of research concentrates on the differences between the family and the wider world. But environment before birth may be decisive, a point made forcefully by Bernard Devlin *et al.* (*Nature* **388,** 468; 1997). These findings were based on a meta-analysis of more than 200 studies of the heritability of IQ. As IQ and its heredity feature strongly in *Twins*, Wright should have said more about its provenance as a psychological and social indicator and the robustness of its measurement. And, if all or part of it can be inherited, what exactly are the genes coding for?

One plausible suggestion is that they control the speed at which the brain processes information. Wright does not say just how great the genetic differences are between people. In fact the differences appear to be rather subtle: about one base in a thousand in the three billion bases in the human genome, but with a tendency for differences to be concentrated in non-coding DNA.

Psychology ought to be the science of individuals: it should examine what makes people different, even unique. Too often twins are used as convenient models for studying the human condition in its generality. Perhaps psychologists might study twins for themselves, and look at their developing personalities, their mutual social interaction and the social dynamics both inside and outside their families. It would probably be better for twins; it might even be better science.

The book does not quite work either as journalism or as a critical survey. What characterizes good journalism is that it makes its point on a first reading by reducing and simplifying the information content to the minimum. There is too much information here for that, but too little to give a clear perspective of the field. The science Wright describes is at best tentative and provisional, a point that may be lost, sometimes deliberately, on society in general and its leaders in particular. And this is a field where, as Wright argues persuasively, science has a tendency to lead to social theory and sometimes politics only too readily. It is worth remembering the words Bertolt Brecht put into the mouth of Galileo: "The aim of science is not to open a door to infinite wisdom but to set a limit to infinite error." □

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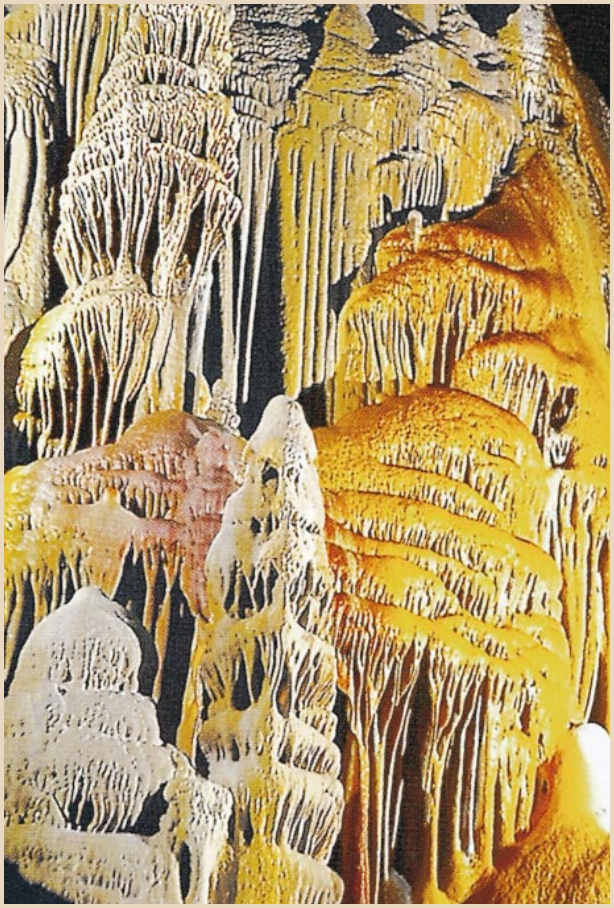
Underground revelations



The Fiery Temple of the Cave God, a colourful 'flowstone' cascade in Three Fingers Cave, Guadalupe Mountains, New Mexico, is one of more than 250 mineral-deposit formations that see photographic light of day in the second edition of *Cave Minerals of the World* (National Speleological Society, $70) by Carol Hill and Paolo Forti. The book describes in detail the wide range of mineral types found in caves, and provides a guide to the classification of secondary mineral deposits, or 'speleothems'. The authors then stick their necks out and draw up a list of what they consider to be the top ten caves in the world in terms of mineral deposits.

True stories

A Novel Defense of Scientific Realism

by Jarrett Leplin

Oxford University Press: 1997. Pp. 204. $39.95, £35

Stathis Psillos

Modern science has transformed the way we think of the world. Nature is no longer taken to be as our senses tell us it is. Entities and mechanisms invisible to the naked eye, such as electromagnetic waves, electrons, protons, neutrinos and DNA molecules, to mention but a few, are said to populate the world and cause the phenomena we observe.

But why should we take scientific theories to be true, or nearly so? Why can we not just take them to be mere instruments for the systematization and prediction of observable phenomena, without attributing reality to the invisible entities they posit? Or could we not just suspend our judgement as to the truth of the assertions the theory makes about invisible entities, and believe only that the theory is empirically adequate, that is, that whatever it says about the observable phenomena — and

only this — is true?

Scientific realists argue that mature and genuinely successful scientific theories should be accepted as nearly true. The main defence of this optimistic attitude is known as 'the no miracle argument' because it is based on Hilary Putnam's slogan that "scientific realism is the only philosophy of science that doesn't make the success of science a miracle".

Modern defenders of scientific realism, most notably the philosopher Richard Boyd at Cornell University, have based their defence on the idea that the impressive predictive and explanatory successes of scientific theories would remain unaccounted for unless we accept that the entities, processes and causal mechanisms they posit to operate behind the phenomena are real. They dismiss instrumentalist accounts of scientific theories by pointing out that they leave the success of science unexplained. If theories are merely 'black boxes', the only virtue of which is that they offer the most economical classification of the observable phenomena, then there is no reason to expect that they can be, as the French philosopher and scientist Pierre Duhem put it, "prophets for us". To counter that these black boxes are empirically adequate — that they save all phenomena — would not be much of an improvement on the instrumentalist position. For what needs explanation is precisely the fact, if it is a fact, that scientific theories save the phenomena. To say that they do is merely to assert what needs to be explained.

The realist explanation of the success of science does not go unchallenged. One line of criticism is that it is too easy to obtain empirical success: just 'write into' the theory the right observational consequences. Then the theory would not fail to predict them. But realists are quick to regiment their argument. There is a kind of prediction that can support only a realist understanding of the theory that entails it: the prediction of novel phenomena. For there is no explanation of why the theory predicts the existence of a novel phenomenon, other than to say that the phenomenon is brought about by the theoretical mechanisms posited by the theory. A novel prediction is typically taken to be the prediction of a phenomenon whose existence is ascertained only after a theory suggests its existence.

But this cannot be the whole story because theories also receive support from explaining phenomena that are already known. So some philosophers argue that the 'temporal view' of novelty is inadequate and should be replaced by a 'novelty in use' view: a prediction of an already known phenomenon can be 'use novel' with respect to a theory provided that information about this phenomenon was not used in the construction of the theory. Yet it has been notoriously difficult to make precise the intuitive idea of 'use novelty'.

A Novel Defence of Scientific Realism takes up this challenge and meets it beautifully. Jarrett Leplin analyses 'novelty' by reference to two requirements: independence and uniqueness. The core idea is that a prediction of a phenomenon O, be it already known or hitherto unforeseen, is novel for a theory T if no information about O is necessary for the prediction of O by T and if there is no other theory available that explains why O should be expected.

Leplin puts his analysis of novelty to work in his defence of scientific realism. His argument is not new. It is a variant of the known line that a realist understanding of theories that entail novel predictions offers the only explanation of their capacity to yield such predictions. But I think this line is right, and here Leplin does a thorough job in developing the realist position and blocking well-known counter-arguments.

However, the realism he wants to entertain is rather weak. His 'minimal epistemic realism' is committed merely to the claim that "there are possible empirical conditions that would warrant attributing some measure of truth to theories — not merely to their observable consequences, but to theories themselves". As he knows, many realists would aspire to more. They would, for instance, try to defend the more substantive thesis that the distinctive mode of inference involved in the generation and acceptance of explanatory hypotheses in science, known as inference to the best explanation, is conducive to truth. Here Leplin parts company with his fellow realists and wishes them *bon voyage*. Their voyage has to be successful if the rationality of belief in unobservable entities is to be defended. Consequently, more work needs to done to defend a full-blooded realism. But this book is a splendid starting point. □

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At a glance

Sex, Color, and Mate Choice in Guppies

by Anne E. Houde

Princeton University Press: 1997. Pp. 210. $49.50, £35 (hbk); $19.95, £14.95 (pbk)

It is hard not to like a book with an appendix titled "How to build a better bordello", although the subjects on display here are male guppies. Building an effective bordello, or at least a reliable way to determine mating preferences, is important to biologists who study guppies, and, like much of the rest of this book, turns out to be of interest to researchers into sexual selection in other species as well.

For studies of adaptation in the wild in general and of mate choice in particular, guppies have been a model system for many years, and the author synthesizes the results of decades of research. She carefully distinguishes between speculation and fact, and provides one of the most lucid analyses of current mate-choice models I have read. Literature cited on sexual selection is impressively broad for vertebrates, less so for insects and other invertebrates.

Is the book just for 'guppy people'? Yes and no; researchers anxious to keep up with the nuances of the latest experiments on the significance of the amount of orange area relative to the intensity of orange will find details of limited interest to the rest of us. The last few chapters, however, particularly the concluding section, provide an inspiring guide to future directions in behavioural ecology.

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The Handicap Principle: A Missing Piece of Darwin's Puzzle

by Amotz Zahavi and Avishag Zahavi

Oxford University Press: 1997. Pp. 286. $30, £18.99

At first sight, the peacocks tail emblazoned across the dustjacket is an affront to evolution by natural selection. Surely such a cumbersome trait could not arise by 'survival of the fittest'? Such naive criticisms were pre-empted by Darwin with his theory of sexual selection.

In the mid-1970s, Amotz Zahavi published a controversial series of papers in which he claimed that all previous theories of sexual selection were flawed. Instead, he proposed his handicap principle. Moreover, he asserted that Darwin's demarcation between natural and sexual selection was incorrect, the true distinction being between natural and 'signal' selection.

This insightful yet accessible book explains the handicap principle and applies it to the analysis of a wide variety of behavioural phenomena: altruism, mate choice, the signalling between predator and prey, to name but a few. Many of the Zahavis' ideas are inspired and should be required reading for all zoologists and interested lay people. Unfortunately, this high praise must come with at least one caveat: the book is extremely tendentious. Although the authors provide a rigorous defence of their own position, rival theories are often unfairly dismissed with glib and specious arguments.

Overall, this is a beguiling tale, pleasantly illustrated, which is of genuine and general importance to our understanding of evolution and animal behaviour.

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