down challenges to the Christian establishment. The move to the tranquil ex-vicarage of Down in 1842 was an attempt to emulate his favorite role-models, the clerical naturalists, so respectable and secure. It was his parson's lifestyle and parish duties here that Moore now brings out so vividly. He shows us the little-seen Darwin: the village patron, the squire discharging his duties, running benefit societies and clothing clubs, complaining about curates making off with school money or village maids.

In short, Moore provides another fix for locating Darwin in Secord's "social geography." The improving squire becomes a transitional figure in an industrializing culture, supporting the rural clergy and old order even while redesigning creation along more competitive, naturalistic lines. Darwinism was to help sustain the switch in power from the Church-and-corporation élite to the rising liberal-industrial order in Britain. How it was then adapted to other European political contexts is shown in part 3 by Paul Weindling and Pietro Corsi (on the German and Italian reactions) and Francesco Scudo and Michele Acanfora (on the Russian response). Ultimately, Moore's work raises a fundamental question: how could a respectable Whig educated to tradition and privilege knowingly commit a treasonable act against the old Oxbridge clerisy? In showing us "parson" Darwin Moore makes the problem more acute than ever.

For the future, Kohn suggests that we switch the focus from Darwin himself to the "conceptual debates and institutional structures" of his day, in order to understand the contemporary meaning of evolution. How much remains to be done in this respect is evident from *The Darwinian Heritage*. It is surprising, for example, that despite a 76-page bibliography there is no listing for Morris Berman's study of the Royal Institution, *Social Change and Scientific Organization* (1978), which explores the utilitarian ethos of Darwin's London—a book that surely gives an insight into Darwin's potential audience. Correlating the political and scientific outlooks of London's various social groups will certainly enable us to put Darwin into better perspective.

For the moment, though, no one can doubt that *The Darwinian Heritage* stands as a monumental achievement. Not least it is a superb feat of organization on Kohn's part. With its seminal papers, state-of-the-art techniques, commentaries, and stock-taking, it is sure to remain a centennial landmark.

ADRIAN DESMOND
Department of Zoology, University College London, London WC1E 6BT, United Kingdom

A Debate in Solar Physics

Confronting Nature. The Sociology of Solar-Neutrino Detection. TREVOR PINCH. Reidel, Dordrecht, 1986 (U.S. distributor, Kluwer, Norwell, MA). xii, 268 pp., $49.50. Sociology of the Sciences Monographs.

In *Confronting Nature*, Trevor Pinch presents a sociology of the detection of solar neutrinos. Working within the research program of "social constructivism," he wants to "learn something about the social processes whereby consensus is reached in science" (p. 3) and to evaluate the "interpretative flexibility" of evidence and theory. The notions of "negotiation," "interpretative flexibility," and "evidential context" used by Pinch are important, for they make it possible to address in sociological terms questions usually discussed (by philosophers) in epistemological terms like "theory-laden" or "truth-value." Thus, the ultimate goal of constructivists—or relativists—is to show that in science epistemological problems are in fact social problems, "the social grounding of beliefs" being not, according to them, "predicated upon their truth-status" (p. 3).

With this project (presented in the first two chapters) in mind, Pinch reconstructs the history of an important experiment in modern physics that led to the measurement of the flux of neutrinos coming from the sun. Obtained in 1967, the first results of the experiment indicated a flux much lower than the one predicted by theory, and the discrepancy—still unexplained—has given rise to an important debate on the quality of the experiment and on the value of theoretical models of the sun used to make the prediction. It is this state of debate and uncertainty that makes the history of the "solar-neutrino problem" an interesting research site for a sociologist who wants to "observe," so to speak, how scientists achieve consensus in science.

To structure the narrative, Pinch introduces an important distinction between the apparatus *per se* and the "evidential context"—that is, the "context in which the results of the experiment are held to gain significance" (p. 49). In this way the link between the experiment and, for example, the theory to be "tested" cannot be taken for granted and must be established by the actions and interactions of the scientists involved. In the case of neutrino detection, the apparatus—constructed by Ray Davis, a chemist at the Brookhaven National Laboratory—was essentially a tank of perchloroethylene in which the chlorine-37 isotope could interact with a neutrino to produce a radioactive isotope of argon. Collected after a certain period of time, the argon atoms were then counted by a Geiger counter, which detected the Auger electrons emitted during their decay. The resulting number was then used to infer the number of neutrinos interacting in the tank. Davis began to work on this project in 1949 and thought he could use an appropriate tank to detect free neutrinos. He soon realized, however, that the sensitivity of the apparatus was not adequate given the large cosmic-ray background. He then decided to use his apparatus to set an upper limit to the flux of neutrinos that, according to nuclear astrophysics, was coming from the sun. Though his first result, published in 1955, was many orders of magnitude higher than the predicted flux and thus not really useful, it put Davis on a potentially important track, for his apparatus could serve to test nuclear reactions going on in the sun.

The link between Davis's apparatus and nuclear astrophysics was forged in 1958 when new experimental results on nuclear cross sections suggested that the flux of neutrinos coming from the sun might be higher than expected. It was consolidated in 1964 when a $600,000 grant was obtained from the Atomic Energy Commission (AEC) to build a 100,000-gallon tank to test the new predictions. It was during this period that Davis came into contact with the nuclear astrophysicists of the Kellogg Radiation Laboratory at Caltech, especially William Fowler, whose reputation did much to give credibility to the enterprise, and John Bahcall, who made the necessary calculations and stayed in close contact with Davis. Like an ethnographer, Pinch describes in detail, in chapters 4 and 5, the many activities of Davis and the Caltech group to secure the realization of the experiment. He shows that the activities of the experimenter and the theoretician are not limited to nuts and bolts or calculations and include such activities as, in this case, negotiating with companies for the construction of the apparatus, lobbying to get funds from the AEC, and visiting the experimental site. For Pinch, all these activities consolidated the multiple links between Davis and Bahcall (personal, scientific, and professional) that were the social basis of the "intellectual" links between the experiment and the theory and that made possible the success of the whole enterprise.

Having shown how the links between theory and experiment are socially constructed, Pinch addresses the question of the "interpretative flexibility" of experimental results and theoretical predictions by examining the fate of the data and models produced by the collaboration of Davis and Bahcall. Here again, the analysis is very fine-grained, and to make a long story short, let us say that while, at the end of 1967, Davis

interpreted his results as being in conflict with the theory (that is with Bahcall's calculations) Bahcall maintained (for more than a year) that there was no contradiction betwen the theory and the data. Pinch uses this episode to argue that the notion of contradiction is flexible and negotiable and consequently that philosophies of science that consider theories to be confirmed or refuted by experiments are in trouble. However, one must be careful with the notion of contradiction. In logic, it applies to a given proposition, whereas in the case discussed here the scientists use the term in the general sense of "disagreement" with a theory, which is always a complex system of propositions. For this reason Bahcall can easily maintain that there is no contradiction by pointing to the fact that some of the input data have wide error margins and predicting that the disagreement between theory and experiment will vanish when better input data are obtained. Thus the discussion going on between Davis and Bahcall does not show any flexibility in the logical notion of contradiction but rather a flexibility in the possible predictions of the theory made possible by the flexibility of the input data. Besides, Pinch explicitly notes in the conclusion that interpretative flexibility varies from area to area. This variation seems closely linked with the possibility of playing with the error margins of the input data in a credible way. After all, the fact that Bahcall finally admitted that there was a real problem with the predictions of the model is clearly related, as Pinch himself admits, to the fact that after the publication of new experimental results on nuclear cross sections and solar opacities it became more difficult to play with the main input data. So, the fact that an experiment can be seen at a given time and by different scientists as both compatible and incompatible with a theory has nothing to do with the logical notions of contradiction and consistency, which apply only to a single proposition enunciated by a given individual.

In his research, Pinch relied heavily on interviews, most of them conducted in 1978. Accordingly, it would have been useful to present a more critical discussion of the status of these "data." It seems clear that the scientists' reminiscences cannot be taken at face value, for they are heavily influenced by the scientists' position as "being-interviewed-by-a-sociologist," to use the language of phenomenology. For instance, when Bahcall explains his close relationship with Davis by saying "I had staked my career on my ability to predict the response of the instrument" (p. 116) or that "scientific advancement depended in a large measure on my correctness" (p. 118) he is talking like a sociologist, but quoting these extracts is not a proof that the explanation in terms of interests is true. The effect of the interview is even more evident in the case of Goldhaber. In a paper published in 1967, he had written that he had "often suspected that the theory overshot a little." Intrigued by this remark, Pinch asked him, in 1978, to "elaborate" on it. He answered: "I thought they had a bit oversold it" (p. 124). The change in language is clear, and though it makes more evident the "strategic" aspect of the actions, it is an artifact of the interview situation. In any case, the sociologist does not need to see his analysis "repeated" by the the actors for it to be convincing. The plausibility of his arguments rather comes from the coherence they give to the series of events described, and this even though some actors may disagree with the proposed interpretation.

Fortunately, Pinch's arguments are most of the time convincing, and he has produced an important book that will be useful to historians of science, who will find a sensitive and detailed history of an important episode of modern physics, as well as to sociologists and philosophers of science who want to find a common ground to discuss the hypothetical specificity of science vis-à-vis other cultural activities.

YVES GINGRAS
Département de Sociologie,
Université du Québec à Montréal,
Montréal, Québec, Canada H3C 3P8

Ecological Comparisons

Are Australian Ecosystems Different? J. R. DODSON and M. WESTOBY, Eds. Ecological Society of Australia, [no place], 1985 (distributor, Blackwell Scientific, Carlton, Australia). iv, 250 pp., illus. Paper, $A25. Proceedings of the Ecological Society of Australia, vol. 14. From a symposium, Sydney, Aug. 1984.

Are Australian ecosystems different? Of course they are. They are full of *Eucalyptus*, kangaroos, and flower-pollinating parrots. It is much less clear that they differ significantly in ecological characteristics from ecosystems elsewhere. To produce a preliminary assessment, the Ecological Society of Australia convened a symposium in 1984. The motivation for the symposium was, first, to determine the relevance of overseas research for practical management decisions in Australia, and, second, to explore the extent to which the properties of ecosystems are determined by their present-day environments as opposed to the idiosyncrasies of history. Determining the predictability of the evolution of ecosystems is no easy task. Characteristics of the physical environments of the ecosystems to be compared need to be matched. The length of time current environments have existed is also important; even the most ardent ecological determinist does not expect extremely rapid ecological convergence in regions whose climates have only recently become similar. Once ecosystems have been well matched, appropriate features to be compared must be decided on. Previous assertions about the uniqueness of Australian ecosystems have been based primarily on taxonomic comparisons, which take into account only one of many features of interest.

Among the interesting features for ecological comparison are life history traits, patterns of species richness, species diversity, guild structure, productivity, and successional patterns. Identification of the best units to compare is difficult, and data for some of these units are sparse, partly because community ecology has increasingly focused on species belonging to particular taxonomic groups or guilds within them. Even within one taxon ecosystems may be similar in one trait of interest, say species richness, but not in others, say guild structure. Intertaxon differences are even more pronounced.

The papers published in this volume span the range of comparisons from single families of organisms to communities of species belonging to many different taxa. The environmental traits compared are similarly diverse: marine, freshwater, and terrestrial ecosystems are all treated. The topics were evidently selected on the basis of availability of investigators with something to say about comparisons between Australian ecosystems and those elsewhere.

Not surprisingly, the comparisons differ markedly in the extent of information available, the wisdom displayed in choice of traits to compare, and the goodness of match of sites. Some papers do not really compare Australian ecosystems with those elsewhere. Others compare Australian ecosystems with ecosystems that bear similar names but occur in dramatically different climates. In some cases the communities being compared are taxonomically very similar, making it difficult to separate effects of convergence, if any, from features due to common ancestry.

Among the papers in which the sites are well matched and the community components compared are ecologically interesting and taxonomically distinct are those on macrobenthic animal communities of Tasmania and the Holarctic (B. V. Timms), Australian and Northern Hemisphere streams (P. S. Lake *et al.*), plant species diversity on small scales (B. Rice), tropical insect herbivory levels (M. D. Lowman), phytotelmata (R.