umphs of the germ theory of disease, the discovery of filterable viruses, the gene theory, and the enzyme theory of life each in its own way refashioned the arena of debate. In fact, it is Farley's view that the issue disappears in the 1960's not so much because of a final resolution but because biochemistry and molecular biology transformed the debate into one between a gradualist and a saltationist view of the appearance of life.

Farley pushes out the sphere of relevant concerns even beyond related scientific theories. This brings us to the most controversial aspect of the book. Throughout, the author endeavors to associate particular scientific positions with religious and philosophical commitments. Historians of science have been attempting such bridge-building for some time with only moderate success. The recent interpretations of Pasteur developed by Farley and Geison stand out as some of the most successful efforts. Farley's chapter on Pasteur, which is one of the best in this book, follows closely this revisionistic line and is going to upset many an uncritical Pasteur admirer. In short, Geison and Farley have argued that much of our historical understanding of spontaneous generation arguments has been dictated in the first instance by Pasteur's own highly subjective history of events. They also show that Pasteur's strong commitment to Catholicism and to the Second Empire fanned to the point of intolerance his reaction to opponents. They show that the judgments of the Académie des Sciences on the Pasteur-Pouchet and Pasteur-Bastian controversies were far from the evenhanded evaluations expected from the scientific community. These demonstrations may say little about the context of Pasteur's discoveries, but they suggest a lot about the justification of scientific ideas. Farley attempts similar "political" interpretations of the Tyndall-Bastian controversy (p. 141), of the views of the Haldanes, father and son (pp. 164–165), of the views of Oparin (pp. 171–173), and of the outcome of the First International Symposium on the Origin of Life of 1957 (pp. 179–181), among others. This reviewer finds these latter efforts at an "externalist" treatment of scientific ideas less developed and consequently less convincing than the exposé of Pasteur.

In short, in putting together this complex story so effectively Farley has shown us that the history of the life sciences has unexploited riches. By promoting a multifaceted history, which includes social as well as intellectual elements, he has attempted one of the most difficult of syntheses in our discipline. In places, to be sure, he remains tantalizingly brief in his analysis, for example in his discussion of the relation between the acceptance of the germ theory of disease and the spontaneous generation issue (pp. 144–146); occasionally he glosses over the contributions of major personages, for example Von Baer (p. 34); and now and then he slips into inadequately documented assertions, as in the account of the Ralph Spitzer episode at Oregon State (p. 178). But these are the shortcomings to be expected in any project so ambitious and comprehensive. This book is enormously rewarding to read and will be the necessary starting place for any future work on the subject; it can serve as a good survey of much of the history of 19th- and early 20th-century biology.

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Science as Problem-Solving

Progress and Its Problems. Toward a Theory of Scientific Growth. LARRY LAUDAN. University of California Press, Berkeley, 1977. x, 258 pp. $10.95.

What constitutes progress in science, and how is scientific progress to be explained? These are the large questions Laudan tackles in this ambitious book. He starts out with an aggressive prologue, contrasting his own approach to these questions with that of "most contemporary philosophers of science." Where others have defined scientific progress in terms of increase in the power of successive theories to explain facts, he proposes to define it in terms of increase in problem-solving capacity. And where others have treated the scientific rationality that may be invoked to explain progress as a quest for truth based on confirmation and refutation of theories, he proposes to show that scientific rationality can be defined in terms of choice based on assessment of the problem-solving capacities of theories, without appeal to the notion of truth. He promises to show that his approach can "avoid many of the paradoxes which previous models have generated, and make some sense of the historical data."

The rest of the book falls into two parts. In the first part the promised definitions of scientific progress and scientific rationality are spelled out. As a preliminary we are offered a taxonomy of the conceptual and empirical problems a theory may have to solve, each type being illustrated with snippets from the history of science. Next it is argued that individual theories should not be considered the units between which rational choices have to be made. Rather the scientist must decide between the "research traditions" that are associated with series of theories. Research traditions are "sets of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain." After these preliminaries we are presented with the author's new criteria of scientific rationality. The "acceptability" of a research tradition is to be judged by the problem-solving effectiveness of the most recent theories associated with it. Its promise or "rational pursuitability" is to be judged by the "progress or rate of progress" in solving problems that it has exhibited in the past. In the second part of the book the author attempts to justify his earlier claim that his account of scientific progress and rationality can "make some sense of the historical data." He presents two tests for any proposed account of scientific rationality. As far as I am able to understand his argument, these are: first, it should enable us to show that those developments in the history of science that we all intuitively judge to be rationally motivated were in fact so; and second, it should provide the historian of science with adequate guidelines for selecting and weighting his material, without committing him to insensitive and anachronistic attempts to impose our present-day criteria for theory assessment and choice on past scientists who held to very different criteria. The author insists that on these tests his account fares better than traditional accounts but offers no evidence for this claim.

The mathematician Hardy is said to have remarked of a friend's fallacious proof, "There is less in this than meets the eye." Here too the remark applies. To start with, the author fails to show that his "new" approach is original. The illusion of originality is sustained by repeated contrast of his own views with a naive caricature that is variously described as "the traditional analysis," "the conventional wisdom," and "the standard view." This is misleading, for in fact the approach to the study of science the author defends, an approach customarily known as "instrumentalism," has a long history and many well-known recent exponents. The author appears to be unaware of this history and makes no attempt to answer any of the

well-known objections to the instrumentalist position. Further, he offers no convincing reasons for supposing that his own account of scientific progress and rationality in terms of problem-solving differs from the accounts in terms of confirmation and explanatory power that he rejects. Instead of an argument we are offered a list of alleged differences between "facts" and "problems" (pp. 16 and 17); for example, "There are many facts about the world which do not pose problems because they are unknown" and "many known facts do not necessarily constitute empirical problems." These observations are, alas, perfectly consistent with the claim the author is out to refute. For unknown facts seem to correspond to unrecognized empirical problems, and recognized empirical problems that do not challenge a theory seem to correspond to known facts that are not relevant to the theory. The author's subsequent treatment of the various types of problems that a theory may face reinforces the suspicion that we have here merely a new jargon, not a new philosophical position.

This would be a minor criticism had the author succeeded in his primary aim, that of defining scientific rationality in a way that resolves the problems raised by the accounts of such philosophers as Carnap, Popper, and Lakatos. In the course of his discussion Laudan does indeed make some telling points—for example, he has many interesting things to say about the factors that affect the weight attached by scientists to different kinds of unsolved problems, and he presents a good argument for the interesting claim that the scientist who would make a rational choice among current theories on grounds of "promise" must be prepared to consider at least the recent history of his subject. But the definition of scientific rationality that finally emerges is vacuous. For the notion of a scientific problem is extended to cover almost anything anyone could conceivably consider relevant to the assessment of a theory; the "research traditions" between which scientists are supposed to make their rational choices are so vaguely defined as to cover almost any imaginable conglomeration of theories; and there are no definite restrictions placed on the relative weights to be assigned to different kinds of problem-solving in the estimation of problem-solving effectiveness. In consequence, anything goes: the criterion of rationality is apparently compatible with a vast range of strategies for choosing theories, including the very models of rationality the author rejects. No wonder the author is so confident that his model of rationality will do justice to the history of science. It is hard to imagine any historical development in science, however bizarre, that would not be shown to be rationally motivated and thus "explained" if we adopt this all too generous formula. And the scientist, faced with Laudan's account of rationality, may well complain, as Leibniz did of Descartes's analytic method, that it amounts to little more than "Take what you need; do what you should; and you will get what you want."

The instrumentalist approach to the history and philosophy of science, the approach that in the hands of Ernst Mach, Pierre Duhem, and the American pragmatists once yielded remarkable insights, is, I think, still a promising approach today. But to establish the credentials of instrumentalism painstaking historical case studies and careful philosophical arguments are needed. Laudan has offered us only gradiose promissory notes.

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Rotating Neutron Stars

Pulsars. F. G. SMITH. Cambridge University Press, New York, 1977. xii, 240 pp., illus. $19.50. Cambridge Monographs on Physics.

Pulsars. RICHARD N. MANCHESTER and JOSEPH H. TAYLOR. Freeman, San Francisco, 1977. xiv, 282 pp., illus. $19.95. A Series of Books in Astronomy and Astrophysics.

In 1934 Walter Baade and Fritz Zwicky suggested that supernova explosions would produce compact stars with extraordinary properties—stars as dense as the atomic nucleus. The concept of "neutron stars" was received with great skepticism by most astrophysicists. At most it was usually thought that though such a state of matter was theoretically possible it was highly unlikely to exist in nature. The sudden and dramatic vindication of Baade and Zwicky's ideas, which were promoted most vigorously by Zwicky, occurred almost exactly ten years ago with the discovery of pulsars. The appearance of pulsars on the astronomical scene is an example of a breakthrough caused by new observational techniques. In this case the breakthrough resulted from the combination of a newly accessible waveband—radio frequencies—and the development of receivers with short time constants and their use in a repetitive observing mode. There was a sudden flood of complex observational detail followed by attempts at decoding the observations with physical theory. The rapid identification of the objects as rotating neutron stars is a well-known story, but the subsequent development of the complex interaction of observational details and theoretical models is not so well known. The time is ripe, the publishers of these two books tell us, for the appearance of a book pulling all this material together into easily digestible form.

The two books are quite similar in scope. They cover almost exactly the same material and in more or less the same depth. No topic covered in one is omitted in the other. Observational material is organized in both along obvious lines (integrated pulse profiles, individual pulses, timing, dispersion and scattering in the interstellar medium, distances, galactic distribution, the Crab Nebula and its pulsar, x-ray pulsars, and binary systems). Discussions of theoretical matters are divided between the characteristics of neutron stars and theories of the pulse emission mechanism (about which surprisingly little is known with any degree of certainty). The major difference in approach is that Smith has chosen to integrate the theory with the observational results more than have Manchester and Taylor, who survey the observations first and devote the last two chapters to theoretical material. Of course in both books there is much cross-referencing, and both are successful at synthesizing observational detail with interpretation. If forced to choose between the two, I would lean toward Manchester and Taylor because of their more complete subsidiary material: pulsar table (that has more data per object than Smith's and coverage of 149 objects compared to Smith's 105), reference list, and indexes (plus a list of symbols). A good index is of immense value when a volume is used for reference rather than bedtime reading. Manchester and Taylor also have more complete discussions of the more recently discovered binary pulsar and of the evolution of binary systems with mass transfer.

Both books have succumbed to the regrettable convention of omitting the titles of papers referred to. It is frustrating to dig out an obscure reference, vaguely cited in an important discussion, only to find out from the title alone that it is irrelevant to one's particular interests.

Both books are well written and have succeeded in providing a unified summary of varied and complex material, with little of the flavor of a batch of synopses of journal articles slapped togeth-