the separate-agency decision was reached nor the Executive role in congressional deliberations is recounted with the sort of insight one would expect the author's 60 interviews to produce.

But if the book has shortcomings as political analysis, it is far superior to most case histories in its grasp of the substantive issues involved and in its demonstration of how the perceptions and misperceptions of political actors shape their initiatives. Not surprisingly, Rettig finds that experience with the program has led to more modest expectations among erstwhile crusaders and to revised ideas about research priorities. He is eager for Congress, at the end of the program's first decade, to glean the results of this experience and to tailor the program accordingly. Congress, to be sure, might have little "incentive to conduct such a review. There are political benefits in favoring the cancer crusade and perceived costs in criticizing it." And, Rettig might have added, even if "criticism" is forthcoming, one could hardly expect it to depart radically from the expectations that members of Congress have brought to medical research for three decades. This is simply to say that, despite the aging and passing of the remarkable Lasker circle, the basic conflicts of perception and priority that have surrounded medical research may change relatively little. The leadership (and protection) of the research community will continue to require sensitive interpretation and adaptation to those who, quite rightly, perceive the public's stake in the research enterprise.

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The Yerkes Tradition

Progress in Ape Research. Proceedings of a conference, 1976. GEOFFREY H. BOURNE, Ed. Academic Press, New York, 1977. xiv, 300 pp., illus. $16.

On looking at the table of contents and fanning the pages of this volume one notes that its title does not convey its full or its special character, which is that it is a festschrift honoring the 100th anniversary of the birth of Robert Mearns Yerkes. The contributions, which are 29 in number, were originally presented at a two-day memorial conference at the Yerkes Primate Research Center, with sessions entitled Historical Beginnings of Research on Great Apes, Communication and Language in Great Apes, Chimpanzees as Biomedical Models, and Comparative Perspectives of Human Origins. The contents are not completely representative of anthropoid research because the scope of the conference was restricted by lack of funds. It comes as no surprise, then, that the bulk of the papers on communication and language are contributed by Rumbaugh's group in the program of study of the communication skills of the chimpanzee Lana. The biomedical-model section conveys the probably unintended impression that the research limitations are so great that this and the other anthropoid species are best studied for their particular biological characteristics rather than as substitutes for humans in high-risk experiments. The history of research with the great apes reflects these constraints.

What distinguishes the book from most other "Progress in" books is the section euphemistically entitled Historical Beginnings. Mostly, this section consists of reminiscences of persons who were associated with Yerkes many years ago, including his son, David, and daughter, Roberta Yerkes Blanchard. The other chapters of this short section are appreciations and personal memories of what life was like in New Haven and in Orange Park, Florida, and testimonials to Yerkes's greatness. He was obviously brilliant, warm and nurturant, and supportive of the younger scientists who came under his direction.

Building a laboratory and identifying a domain of research that could continue and progress for 50 years is a more than adequate accomplishment for one person. Yet, as Meredith Crawford reminds us, Yerkes had profound influence on the development of psychology, playing major roles in the development of tests for selection and classification of military personnel and in the establishment of a committee for research on sex. He also headed the National Research Council's Emergency Committee on Psychology to organize the efforts of psychologists as might be required for World War II.

Reading these brief testimonials, one gets the impression that the establishment and operation of a research laboratory for the psychobiological study of great apes were a perfectly natural phenomenon that was simply part of the zeitgeist. It took someone less closely associated with psychology (Gordon Hewes) to point out that the river of psychological science was going down an entirely different channel in the early days of the laboratory. The impact of Pavlov and associationism on the rest of psychology as well as psychology's idolatry of theories that could be tested by experimental design with results neatly arrangeable into rows and columns meant that the program at the Yerkes Laboratories had to be a product of a remarkably independent scientist. The prevailing notion that animals were simply interchangeable units required to fill certain cells of an experimental design lasted for many years, and the study of a single animal or of a small group of animals to see what their inclinations and behavior were was alien to American psychology. The situation got to the point where one site-visit team to the Yerkes Laboratories at Orange Park was dismayed by the naming of animals: surely any laboratory that named its animals would be reluctant to face the hard decisions that had to be made in the name of objective science. Others asked Henry Nissen, its director, if he was running a haven for ancient apes. This group of skeptics and the field of psychology generally were totally embarrassed by the animal-behavior studies that developed after World War II under the name of ethology and that, growing totally and somewhat defiantly outside of established doctrine of psychology, yet changed the content and emphasis of every comparative psychology textbook.

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Geology as History

The Structure of Geology. DAVID B. KITTS. SMU Press, Dallas, 1977. xx, 180 pp. Paper, $8.95.

In this book Kitts has collected together eight essays published in various journals between 1963 and 1974 which analyze the complex inferential context in which statements about the past are derived and tested. The essence of his method is to apply the ideas of a number of leading philosophers of science to the special case of geology and paleontology, and he is well qualified to do so, holding joint professorial appointments in geology and history of science in the University of Oklahoma.

There is a considerable amount of overlap in the subject matter of the essays, and rather than discuss each in turn I shall attempt to précis the principal thesis expounded. Geology differs fundamentally from the physical sciences in being concerned with the inference of specific events in the past and not with the more theoretical matter of establishing laws of nature. It is indeed essential for geological methodology that such laws are taken for granted. Geologists

are obliged to cope with nature in all its complexity through the additional dimension of time because history never exactly repeats itself, hence the subject must always have a large descriptive component. Geologists have more intellectual kinship with historians than with physicists, and there are severe limits to a reductionist approach. Furthermore (p. 47), "Geology is not a predictive historical science. It is not even an immature predictive historical science. It is the most highly developed retrodictive historical science."

To illustrate how Kitts applies his basic thesis to particular subjects it may prove useful to consider his attitude to the claim made by a number of people, myself included, that the widespread acceptance by geologists a few years ago of plate tectonics provides an admirable illustration of Kuhn's conception of a scientific revolution. Kitts argues that the comparison is somewhat misleading because Kuhn's examples of changing paradigms are concerned with fundamental scientific laws or principles, whereas continental drift is a specific historical hypothesis that does not challenge basic physical theory. Now this is true enough, but I for one persist in my belief that both in its more general and its more restricted Kuhnian usage the word revolution is a succinct and accurate description of what took place in the earth sciences. Perhaps this is because my fundamental criterion is social rather than theoretical, as I am more interested in what produces mass conversion of scientists from one set of beliefs to another. I would rather adapt Kuhn to geology than drop the term revolution. After all, Kuhn's influential work has been criticized in various quarters for being an oversimplified and to some extent distorted version of what actually goes on in the scientific community.

Although Kitts is basically right in maintaining that geologists cannot challenge fundamental physical laws, and instead depend on them absolutely to make some kind of sense of the past, he provoked me to wonder by what criterion a fundamental, inviolable law of nature is to be established. Presumably we are all happy to preserve the constancy of the velocity of light, but what about the universal gravitational constant? A small minority of geologists believe that the earth has expanded through time, and it has been seriously argued that this could have been achieved simply by a reduction in the value of G. Are we to reject such an interpretation outright because geologists have no license to tamper with such a basic physical principle?

Kitts's essay on paleontology and evolutionary theory provokes a different sort of general question. Is the relation of paleontology to biological theory exactly parallel to that of geology to physical theory? The answer must surely be no, if the most general biological theory is that of Darwinian evolution. This is because evolutionary theory has an essential historical component and fossils must provide critical evidence. The extent to which paleontologists must bow to the interpretations of geneticists, ecologists, and molecular biologists in formulating their own hypotheses of macroevolution is still far from decided, however.

Whether in discussing historical explanation or the establishment of degree of certitude in geology or in evaluating the methodological proposals of a leading 19th-century geologist such as G. K. Gilbert, Kitts rarely fails to be stimulating and thought-provoking. Only his essay on geological time left me absolutely none the wiser. I can warmly recommend Kitts's little book both to philosophically minded geologists and to those philosophers and historians of science who wish to extend their horizon from the very different world of physics and chemistry.

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Processing Solar Energy

Biological Solar Energy Conversion. Papers from a conference, Miami, Nov. 1976. AKIRA MITSUI, SHIGETOH MIYACHI, ANTHONY SAN PIETRO, and SABURO TAMURA, Eds. Academic Press, New York, 1977. xiv, 454 pp., illus. $18.50.

Interest in the potential of biological systems for solar energy conversion has generated numerous meetings, workshops, and symposia in the last few years. This volume of proceedings summarizes much of what has been happening since an earlier workshop in September 1973, and it is recommended reading for specialists as well as the general public. Because the report of the 1973 meeting was available only as a government publication, the release of the present volume with its expected more general distribution is welcome. Further, the book contains considerably expanded discussions and some presentations of new results bearing on topics only tentatively mentioned previously.

What emerges is an overall impression of much potentially promising work on the use of enzyme systems, particularly hydrogenases, as well as photosynthetic organisms for solar energy conversion, although in a concluding chapter an evaluation of the constraints inherent in the use of such systems underscores the need for cautious optimism concerning their eventual applicability in large-scale energy provision. A more realistic attitude is to regard bioconversion as an important component in a many-factorial solution to the energy problem.

The four sections of the book cover all aspects of bioconversion, ranging from algal metabolism, the enzymology of hydrogen activation, and photohydrogen production through the photosynthetic production of organic compounds and nitrogen fixation to large-scale engineering development. Much in the later sections is a rehash of old material, but there are enough new data, as well as descriptions of ongoing research, to justify publication of the book. A particularly attractive feature is the inclusion of papers on research in Japan and, to a lesser extent, in Germany, giving the reader a perspective that includes some appreciation of effort worldwide.

There are clear indications of lacunae. The material presented in the first section is an example. The origin of photohydrogen in algal systems remains uncertain despite considerable experimentation. The much-needed surveys of marine organisms for alternative sources of hydrogenase, as well as whole-cell photohydrogen production, are still in a preliminary stage. Encouraging results with a marine blue-green algal strain that shows comparatively large light-dependent hydrogen production underscore the need to promote such efforts. Applications of aquaculture using algal and bacterial mixed cultures as food sources are still potentially intriguing but are hardly developed beyond the pilot stage. Characterization of hydrogenases, which, along with extensive surveys of source materials, is a necessary step in attempts to achieve stabilization, has moved slowly. Little more is provided about the catalytic and structural properties of hydrogenases than was known some years ago.

However, an attitude of reasoned enthusism is indicated. One recalls the old saying that "everyone talks about the weather, but no one does anything about it." About bioconversion of solar energy there is likewise much talk, but also some do.

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