Book Reviews

Fatigue in Aircraft Structures. Proceedings of the international conference held at Columbia University January 30–February 1, 1956. Alfred M. Freudenthal, Ed. Academic Press, New York, 1956. 456 pp. Illus. $12.

The collection of papers and discussions which constitute this book represent the proceedings of an international conference held at Columbia University early in 1956 under the sponsorship of the Office of Scientific Research, Air Research and Development Command, U.S. Air Force, and the Guggenheim Institute of Flight Structures, Columbia University. The individual contributions have been assembled under three main subject headings: (i) basic physical mechanisms and theories of fatigue; (ii) fatigue testing and methods for predicting fatigue life; and (iii) actual design techniques for the prevention of fatigue failures in aircraft structures.

As a group, the five papers dealing with the basic mechanism of fatigue in metals provide a comprehensive summary of recent experimental and theoretical work in this field. Significantly, most of this work has been carried out in England and Australia. It is also evident that some of the latest theories which have been advanced to account for the origin of fatigue cracks in metals have reached a rather high level of sophistication, incorporating as they do modern concepts regarding the generation, motion, and interactions of dislocations and point defects—that is, vacancies. Although much of the information contained in these papers has been published elsewhere, those who are interested in the basic physical behavior of metals under cyclic stresses will nevertheless find this series of articles instructive.

The next six papers are devoted to the more applied or engineering aspects of the fatigue problem. Among the topics given special treatment here are the mechanics of fatigue crack propagation, the development of testing methods in relation to design needs, the interpretation of fatigue data and statistical prediction of fatigue life, and so on. The remaining eight contributions are concerned primarily with the design procedures which have been developed by the aircraft industry for the prevention of fatigue failures in civilian and military aircraft. These papers are especially interesting since they offer a comparison between the design practices adopted in England, Sweden, France, Australia, and the United States.

To the metallurgist and physicist, as well as the test engineer and aircraft designer who must deal with the effect of metal fatigue on the performance and safety of modern aircraft, this book therefore contains much of interest and value.

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Philosophy of Science. The link between science and philosophy. Philipp Frank. Prentice-Hall, Englewood Cliffs, N.J., 1957. 394 pp. $7.65.

I had intended to say that this book was thirty years out of date, but that would not be quite right. There are references to, and in some cases extended treatments of, contemporary topics scattered throughout the book. It is rather Professor Frank's knowledge of the issues that *concern* philosophers of science and the reasons *why* they concern philosophers of science that is thirty years out of date. Anyone who still thinks that the issue in philosophy of science is between "operational definition" and "metaphysical interpretation" might enjoy reading this book. Afterwards he should learn some *real* philosophy of science.

To take up the issue of operational definition: logicians of science have had considerable difficulty in finding out just what it is that operationalists are contending. Is the operationalist view that talk about molecules is "really" talk about human experiences with measuring apparatus? The only argument for such a view would be that molecules don't "really" exist and that measuring apparatus does "really" exist. This would be itself a metaphysical position (a species of subjective idealism). To make the thesis of operationalism more amenable to scientific discussion it has in general been identified with the *translatability thesis*. The translatability thesis is simply that every sentence in theoretical science is synonymous with some sentence in the observational vocabulary of science—that is, some sentence that does not contain any term purporting to refer to unobservable objects such as "molecules." The translatability thesis is, however, false, as some thirty years of logical investigation have shown. Frank mentions neither the attempts to make the thesis of operationalism precise (for example, Carnap's work on reduction sentences) nor the difficulties that have been shown to arise with the thesis when it is made precise. Rather he says that every term in theoretical science must be "operationally defined" and then proceeds to construe anything and everything as an "operational definition." For instance, he regards the requirement that the forces postulated by physical theory must be expressible as "simple functions" of the distances and velocities as part of the operational meaning of "force," and he says, without blushing, "the Newtonian law of force asserts that there is in every specific case a formula that would be recognized as "simple" by the scientists of our period" (page 111). It is odd to find someone seriously maintaining that Newtonian physics is *about scientists!* More generally, one may reply to Frank as follows: even a realist would admit that a good scientific theory should lead to successful predictions and that it should be as simple as is consonant with success in its predictive and explanatory function. This much can be said for the scientific theory embracing any term or terms whatsoever, not just the term "force." This is a far cry from saying that terms like "force" are *translatable* by means of terms referring to operations, or that when we talk about force, molecules, atoms, and so on, we are "really" talking about scientists or about the operations they perform.

The book might be used as a source book for some fairly standard ("Logical Empiricist") views in philosophy of science. For instance, chapter 3 is a fairly good presentation of the more or less "orthodox" view of geometry, and chapters 13 and 14 represent a surprisingly responsible discussion of current inductive logic. The "orthodox" view of geometry which Frank defends seems to me, however, to be a mistaken one. On this view, pure geometry is regarded as an uninterpreted calculus, and the principle that light travels in straight lines, which is used when we try to derive testable predictions from geometric axioms, is regarded as some sort of operational definition. This seems a distortion of the situation. It would be more accurate to say that even pure geometry is a meaningful theory which, however, does not

have any testable consequences in isolation. It is not unusual for a perfectly meaningful scientific theory to lack testable consequences when considered in isolation. We may say that what we test when we revise or confirm geometrical postulates by reference to physical experiments is not the geometry itself but rather the conjunction of geometry with a certain portion of optics and mechanics. On this account, "light travels in straight lines" is no more a "definition" than the parallels postulate is. It is just a principle of optics, nothing more nor less than that. And if "something goes wrong," we can revise either the geometry or the principles of optics, depending on the nature of the trouble. In certain worlds it might be best to retain Euclidean geometry and say that light does not travel in straight lines. In other worlds it might be best to abandon Euclidean geometry and retain this much of optics. The usual operationalist account hides, where it should emphasize, the fact that optical and geometrical principles figure on a par in scientific inquiry.

Since Professor Frank's book is thirty years out of date (at least in philosophic sophistication), the reader of this review might be interested in knowing where he can find a more contemporary account. Perhaps the best single book on philosophy of science currently available is *Scientific Explanation,* by Braithwaite (Cambridge University Press). For readers with a smattering of symbolic logic there is also a very good monograph by Professor Hempel: "Fundamentals of Concept Formation in Empirical Science" [*International Encyclopedia of Unified Science* (University of Chicago Press), vol. II, No. 7]. Those interested in the issues surrounding operationalism and the so-called verifiability theory of meaning might also read the pioneering articles by Carnap: "Testability and Meaning" and "The Interpretations of Physics" [both of these articles may be found in Feigl and Brodbeck's *Readings in the Philosophy of Science* (Appleton-Century-Crofts), a volume which also contains a large number of other good articles in contemporary philosophy of science]. A critique of Carnap's views by C. G. Hempel was published in a philosophic journal and has been republished in Linsky's *Semantics and the Philosophy of Language* (University of Illinois); a lengthy reply by Carnap, giving his current position, may be found in vol. I of *Minnesota Studies in the Philosophy of Science* (University of Minnesota). Those interested in more general issues in the philosophy of science might read *Philosophy of Science* by Stephen Toulmin (Home University Library)—a book which I do not think is a sound introduction to the philosophy of science, taken by itself, but which supplies some needed corrective to the stark "deductivism" of the Carnap-Hempel-Braithwaite account of scientific theories. And for the detailed examination of specific physical theories there is still no better work available than the writings of the late Hans Reichenbach.

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An Educational History of the American People. Adolphe E. Meyer. McGraw-Hill, New York, 1957. xx + 444 pp. Plates. $6.

A. E. Meyer states that his new book, *An Educational History of the American People,* is written mainly for novices. The book indeed is a light, interesting history. The author traces his theme from colonial America up to the present. The text has three major divisions: "The Beginnings," describing educational practices in colonial America; "Growth and Evolution," presenting the development of a distinctive public school system; and "Coming of Age," analyzing the emergence of modern educational practice and theory. The last section, which clearly describes the controversies in modern American educational philosophy, is by far the best part of the book.

Undergraduates should enjoy reading this work. Yet a *caveat* must be entered: This is not an impartial history—indeed it is not really a history but, rather, a running commentary. If one looks carefully beneath the intriguing literary style (sometimes Meyer is even too coy in his word usage), one can discover biases and personal judgments. The criticism of public education is a case in point (page 326). Thus, for the more serious layman this story may do more harm than good, for the author's subjectivity often distorts the history of American education.

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Job Attitudes: Review of Research and Opinion. Frederick Herzberg, Bernard Mausner, Richard O. Peterson, and Dora F. Capwell. Psychological Service of Pittsburgh, Pittsburgh, Pa., 1957. xii + 279 pp. $7.50.

"There is a serious discrepancy between our technological success in creating the machinery for production of goods and services and our understanding and dealings with the people who operate and manage these modern tools of production," asserts the preface to this volume. As the contents make clear, this discrepancy cannot be ascribed to lack of publications; rather, it relates to the restricted quality of the research available.

Research on workers' attitudes has been hampered by a variety of factors. Much of it has been naive or has been motivated by a wish to prove the wisdom of management. Even more has been limited by the difficulty of getting managerial permission to study employees on the job. Problems which should be tackled with a multidisciplinary team and large resources have been studied by one person without even access to high-speed computers. The result is a haystack of bibliographical references in which the wheat is truly hidden by the chaff. Unfortunately, the authors have not been very critical in identifying, for the reader, which items fall into which category.

The book contains a tremendous amount of information which can be valuable to researchers in psychology, medicine, economics, and sociology, as well as to managers and union officers. It should serve an especially useful purpose in spotlighting the gaps in our knowledge of phenomena which might well wreck our technological-economic system if we do not learn to deal more understandingly with them.

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First Symposium on Host Specificity Among Parasites of Vertebrates. Institut de Zoologie, Université de Neuchâtel, Neuchâtel, Switzerland, 1957. 324 pp. Illus.

One of the most outstanding characteristics of parasitism is the specificity for certain kinds of hosts. For a very long time the facts concerning this specificity have been recorded, organized, and filed away with little attempt to analyze the bases upon which it rests, its significance, or its relationship to other biological problems. This symposium records an attempt to pose some of these problems before a group of parasitologists who have concerned themselves with various phases of host specificity and zoologists who are interested in speciation, evolution, phylogeny, and taxonomy of the hosts.

After establishment as a working hypothesis of the existence of some parallelism between the phylogenies of hosts and parasites, the various groups of hosts (mammals, birds, reptiles, and fishes) and of parasites (ectoparasites and helminths) were examined to determine the extent of this parallelism between the