BOOK REVIEWS

O. K. Buros, (ed.). *The Fourth Mental Measurements Yearbook.* Highland Park, New Jersey: The Gryphon Press, 1953, pp. 1163 + xxiii, $18.

The Fourth Mental Measurements Yearbook follows the same plan as its predecessor, *The Third Mental Measurements Yearbook*, and, like it, is intended to supplement, rather than supplant, previous volumes of the series. The two main sections of the book are the Tests and Reviews section, with 830 entries, and the Books and Reviews section, with 429 entries. Other useful features are a list of contributing reviewers, a periodical directory and index, a publishers directory and index, an index of titles, an index of names, and a classified index of tests.

The eight objectives stated by the Editor for the Tests and Reviews section might be characterized in terms of three general purposes:

1. To provide a bibliography of published tests and research which has been done on them;
2. To give test users valuable information about specific tests;
3. To exert an influence toward improving the quality of tests.

The above ordering seems to this reviewer to be that of the extent to which the objectives have been attained. As stated in the preface: "The yearbook attempts to list all commercially available tests—educational, psychological, and vocational—published as separates in English-speaking countries in the four-year period 1948–1951. The commercially available tests also include older tests selected for review and tests published during the nineteen-year period (1933–1951) covered by this series of yearbooks and bibliographies but not previously listed." A new feature of this volume is the listing (but only rarely the reviewing) of tests which are available only through certain restricted sources, such as: Association of American Medical Colleges, College Entrance Examination Board, Educational Testing Service, Life Insurance Agency Management Association, National League of Nursing Education, and Psychological Corporation.

The bibliography of publications related to the various tests contains a total of 4,417 titles and the attempt has been made to include all references published and unpublished "on the construction, validity, use, and limitations of each test . . ." through 1951. This bibliography certainly constitutes an invaluable source of information for anyone planning extensive work with any test on which research findings are available.

For each test listed there is given, in addition to title, author, and publisher, a description of the groups for which the test is intended; copyright or publication date; what part scores, if any, are obtained from it; whether the test is an individual or a group test; whether it is machine scorable; cost (as of early 1952); and working time and total time required. If data on reliability and validity are absent from the manual, this fact is mentioned also.

The distribution of the entries in the Tests and Reviews section over the various content fields, and the incidence of reviews for them, are shown in Table 1. Inasmuch as there are 277 items which are not reviewed either in the *Fourth Yearbook* or in previous books in the series, it seems pertinent to make the suggestion that information about how the test was standardized and validated and some quantitative statement of its reliability and validity should be included in the descriptive material provided by the Editor. This is information which every test user should want to have anyway, and it is in fact omitted from some of the reviews themselves.

TABLE 1

Contents of Tests and Reviews Section of The Fourth Mental Measurements Yearbook

Main Heading	Subheadings	Total No. of Entries	Number of Entries			Secondary Entries* Reviewed
			Reviewed in Fourth Yearbook	Reviewed in Previous Vols. Only	Not Reviewed	
Achievement Batteries		26	20	1	5	
Character and Personality	Nonprojective	75	54	3	16	2
	Projective	46	22		3	21
English	Compos., Lit., Spell., Voc.	71	32	7	26	6
Fine Arts	Art, Music	13	8	1	3	1
Foreign Languages	Engl., Fr., Ger., Gr., Ital., Latin, Span.	35	18	1	16	
Intelligence	Group	67	41	8	18	
	Individual	31	24	3		4
Mathematics	Alg., Arith., Geom., Trig.	76	34	5	35	2
Miscellaneous	Agric., Bus., Educ., Comp. & Scoring Devices, Etiquette, Handwrit., Health, Home Econ., Ind. Arts, Phil., Psychol., Record & Report Forms, Relig. Educ., Safety Educ., Test. Prog.	87	33	8	46	
Reading	Miscell., Oral, Readiness, Spec. Fields, Study Skills	61	40	4	12	5
Science	Biol., Chem., Gen'l. Sci., Geol., Misc., Phys.	55	20	10	25	
Sensory-Motor	Hearing, Motor, Vision	18	9	1	7	1
Social Studies	Econ., Geog., Hist., Polit. Sci., Sociol.	48	12	6	28	2
Vocations	Cler., Interests, Man. Dex., Mech. Abil., Misc., Specific Vocations	121	76	7	37	1
Total		830	443	65	277	45

* This column includes books, etc., related to specific tests, and subtests of batteries which are listed separately but reviewed only with the batteries.

The reviews vary in quality, some of them being thorough and factually-oriented discussions of points of importance to test users, while others are principally expressions of opinions based on the reviewers' study of the content of the items of the tests. In some cases, usually tests which have been reviewed in previous volumes of the series, only certain aspects of the tests are considered. This circumstance points up what seems to this reviewer to be a serious obstacle in the use of the book: For many tests, evaluation by a prospective user requires reference to two, or even more, of the volumes in the series. It is to be hoped that in subsequent Yearbooks, Buros may find it possible to consolidate all the reviews which have currency, so that reference to but one source is necessary.

Despite the variability in quality just referred to, the reviews are on the whole serviceable and in many instances furnish the kind of information relative to applications and limitations of the tests which may go far toward preventing their misuse. Quite a number of reviews are so adversely critical (and justifiably so) that it is hard to believe that the tests in question will receive enough support to justify their publication, if the *Mental Measurements Yearbooks* have any influence at all.

The second main section of the *Yearbook*, Books and Reviews, includes 429 titles, 141 of which are reviewed. To quote from the preface: "An attempt has been made to list all measurements books published in English-speaking countries in the four-year period 1948–1951. In addition, a few older books are listed when accompanied by review excerpts not previously published in this series. . . . Books on statistical methods in education and psychology published in the eleven-year period 1941–1951 are included but without accompanying reviews. Instead, cross references are given to reviews in *Statistical Methodology Reviews, 1941–1950.*"

Buros is quite frank in expressing his concern over the prospect that it may not be possible to publish subsequent Yearbooks unless the sale of the *Fourth* makes the venture a good financial risk. It is difficult to see how the market can be greatly expanded if each new volume requires for its full utilization the availability of previous *Yearbooks* in the series. Perhaps the solution to the problem may be in the adoption of some kind of loose-leaf format, so that additional reviews of a test could be inserted next to the original ones for that test. Such an arrangement would also permit the establishment of a continuing review service, so that critical reviews of tests could be made available as the tests themselves appeared or as new findings about their uses were brought out. It would indeed be regrettable if this valuable service which Buros is performing should have to be discontinued.

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HANS REICHENBACH. *The Rise of Scientific Philosophy*. Berkeley and Los Angeles: University of California Press, 1951, pp. xi. + 333.

Those who have even a slight acquaintance with contemporary academic philosophy are aware of the movement or school which is variously called Logical Positivism or Logical Empiricism. It had its beginnings in Vienna and Berlin during the middle twenties, and its influence spread quickly. Largely as a result of European political changes during the past two decades, most of the surviving leaders of its early period are settled now in the United States. Hans Reichenbach, author of *The Rise of Scientific Philosophy*, was one of the earliest members of the Logical Empiricist group in Berlin, and is now Professor of Philosophy in the University of California at Los Angeles. His book is the first to present a popular exposition of the new philosophy, which is said by the author to be itself a science. The Preface asserts that, " . . . this book is written with the intention of showing that philosophy has proceeded from speculation to science." (p. vii).

Reichenbach's book is divided into two parts, the first, occupying one-third of the book, is headed "The Roots of Speculative Philosophy," and takes up the first six chapters. The second part consists of twelve chapters and occupies two-thirds of the book, being headed "The Results of Scientific Philosophy." The first part is devoted to an extended condemnation of the vast bulk of traditional philosophy. Traditional philosophies are divided by Reichenbach into just two camps, which he calls *rationalism* and *empiricism*. The first is condemned for its belief that factual knowledge can be obtained from sources other than sense perception. The second is criticized somewhat more mildly for its failure to agree that certainty is not a criterion for knowledge, and that so-called certain knowledge, like pure mathematics, for example, is not *factual*. Their belief in the possibility of achieving a priori knowledge which is factual or synthetic led rationalist philosophers, we are told, to the construction of theories which provide only pseudo-explanations for the problems with which they were concerned. And the failure of older empirical philosophers to recognize that probability rather than certainty is all that knowledge need possess is what led them into the wasteland of barren scepticism, like Hume. Reichenbach's criticism is only partly devoted to refutation, however. For the most part he is interested more in the psychological basis of the older philosophies than in their logical shortcomings. Here, as in Dewey, the "quest for certainty" is the villain.

The second part of the book is more constructive, although in every case the author repeats his claim that progress could be made only after the synthetic a priori was abandoned and the demand for certainty was relinquished in favor of probability. Here Reichenbach discusses such varied topics as the nature of geometry, the philosophy of time, causality, the atomic theory, evolution, and what he calls "the functional conception of knowledge." His treatment of many of these topics is illuminating and very readable, much in the tradition of Eddington and Gamow. Reichenbach's discussion of geometry covers familiar ground, for the most part, describing briefly how earlier empirical rules of thumb were codified into a deductive system by Euclid, and then describing modern developments at somewhat greater length. The modern view, developed subsequent to the introduction of non-euclidean geometries, is that one axiomatic system is as "good" as another, regarded merely as mathematics, but that the question of the nature of real or physical space is an empirical question for the physicist to determine by observation or experiment. Some stimulating remarks are made in criticism of the "conventionalist" doctrine of Poincare, but the argument is not carried far enough to be completely convincing.

Included also is a longish chapter on ethics, in which Reichenbach accepts without criticism the view of C. L. Stevenson that so-called moral judgments are neither true nor false, being what he calls "volitional decisions" rather than propositions or assertions proper.

An interesting feature of Reichenbach's book is the parallel treatment he gives to the history of philosophy and the history of science. The last chapter of Part One is on classical physics, and several chapters in Part Two are more concerned with the development of modern science itself than with philosophy. The account is valuable in showing the influence of scientific progress on philosophizing—the "old" as well as the "new" philosophy was subject to this influence. Of course the parallel treatment is not merely an expository device, but an essential part of Reichenbach's argument to the effect that the *significant* questions considered by the older philosophers were of such a nature that they could only be answered by science itself, not by philosophy.

What then is the proper task and the appropriate method of philosophy? According to Reichenbach, "It is the clarification of meanings through logical analysis . . ." (p. 145), that is, "Philosophy is logical analysis . . ." (p. 308). Since it is Reichenbach's view that "Logic formulates rules of language . . ." (p. 222) logic and philosophy seem to have much in common with, perhaps even reduce to lexicography and grammar, which are the traditional disciplines dealing with the correct use of words and their meanings. But far more

interesting conclusions are drawn by Reichenbach than by any grammarian. For example, "The question of the existence of the mind is a matter of the correct use of words but not a question of facts." (p. 272). And again, "There is the question of the existence of the external world . . ., which is found to be a question of correct use of language . . ." (p. 307). These quotations should serve to indicate that Reichenbach has in mind some criterion for "the correct use of words" other than the facts, including, presumably, the facts of language usage, to which mere lexicographers and grammarians are bound. Not even a hint is given, however, of what these criteria consist.

The author's conception of analysis is not altogether clear. When a scientist speaks of analysing his data, what he usually has in mind is the formulation of some hypothesis or theory which will account for or explain the observed facts. In *this* sense of analysis, however, it is the traditional method of philosophizing used by the great philosophers from Plato through Whitehead. The classical philosophers, of course, "taking all knowledge for their province," sought to formulate very general theories to account for data not lying wholly within the scope of any of the special sciences. Since Reichenbach wishes to distinguish sharply between his own and the traditional conception of philosophy, he must have some *other* sense of "analysis" in mind. But the reviewer is unable to discover what this new sense of "analysis" might be.

Professional psychologists who read this book will be somewhat taken aback to be told that " . . . the human mind . . . is essentially passive in the act of perception." (p. 66). And they will perhaps be puzzled as to how to go about fulfilling the new tasks assigned them by Reichenbach, for example " . . . predetermination through fate is a conception to be explained by psychology . . ." (p. 105). Some of Reichenbach's "demonstrations" strike this reviewer as being somewhat less than convincing. Consider, for example, the following: "The mind is inseparable from a certain state of bodily organization. It follows that mind and bodily organization of a certain kind are the same thing." (pp. 271–2). This argument introduces a new principle, which we might dub "the identity of indivisibles." We might as well argue that since Damon and Pythias are inseparable, they are therefore "the same thing," which does not seem to be very logical, and yet follows the same pattern as Reichenbach's own argument.

It should be remarked, moreover, that many doctrines which Reichenbach presents as though they were universally accepted results of positivistic philosophy would find few adherents even from the ranks of other Logical Empiricists. His remarks on probability theory, for example, have been presented and argued for by him in numerous publications in the past, and yet they seem to have won but little acceptance. The situation is similar with respect to his claims for the utility of many-valued logics in quantum theory.

It is regrettable that Reichenbach felt obliged to refute all previous philosophers before presenting his own views, because some of his refutations seem to fall far wide of their marks. Reichenbach suggests that had non-euclidean geometries been discovered earlier, "Plato's doctrine of ideas would have been abandoned as lacking its basis in geometrical knowledge." (p. 142). Yet Platonic or mathematical realism is strongly defended by such mathematicians and logicians as Frege and Gödel, who presumably were at least as well acquainted with non-euclidean geometries as Reichenbach himself. And to say that the philosopher Berkeley was a solipsist is as much a historical error as it is a philosophical one to say that he refuted his own solipsism by writing books; but Reichenbach says both these things (p. 267).

Yet despite its shortcomings in matters of detail, Reichenbach's book is a provocative and forceful argument for a very important contemporary philosophical position.

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W. Ross ASHBY. *Design for a brain.* New York: Wiley & Sons, 1952, pp. ix + 260.

The analogy is a device greatly admired by scientists. At least there never seems to be a dearth of them. Sometimes the analogy provides momentary interest, but quickly fades out of popularity because of a lack of any real contribution to the advancement of science. At other times the analogy becomes so useful as to assume the role of a precise theory or even a factual description of the area of knowledge analogized.

The analogy has many possible uses in science. Sometimes it serves only to clarify thinking about a problem by stating it in terms more familiar to the scientist. At other times it is used to provide a more exact description of the phenomena. (Such analogies usually are somewhat more mathematical than those used simply to put a problem in a familiar setting.) At still other times, the analogy provides a means of extrapolating beyond the existing data, and of predicting the behavior of phenomena not already known. Probably the authors of all analogies hope that this latter benefit will result. Time alone can tell whether the predictions are correct.

Ashby has provided psychologists and physiologists with an analogy which attempts primarily to make more understandable some of the known phenomena of the behavior of living organisms. His analogy is basically mathematical, and thus stands the chance of providing a more exact description of behavior even if predictions made are not borne out by experimentation. It is not just mathematical, however, for one of Ashby's goals is to show that complex behavior can be explained with purely mechanistic principles. Throughout the book he uses a physical device (the homeostat) to illustrate how various types of behavior exhibited by living organisms can be shown to exist in an inanimate object.

More specifically, Ashby is concerned with showing that adaptive behavior can be explained with purely mechanistic principles. Since adaptive behavior covers a lot of ground, however, and has often been described as the major distinguishing characteristic of animate objects, Ashby's concern is really quite broad.

It is impossible to do full justice to the system described by Ashby in a short review. Its essential characteristics as seen by the reviewer are as follows, however:

A system is any arbitrarily selected set of variables, and a variable is any measurable quantity which has a value at any instant. The state of the system is simply the numerical values of the variables in the system at a given instant. A line of behavior is the relation between variables. A phase-space shows the relation between two or more variables, neither of which is time. It thus shows the interrelations of these variables, regardless of time. For example, hunger of an animal and speed of running down a straight runway might both be functions of time, and plots of each against time would be lines of behavior. A plot of hunger against speed of running, however, would constitute the phase-space. The field is the phase-space containing all lines of behavior from all possible initial states or starting points. Thus there can be several curves showing the interrelation between hunger and speed of running, if these curves are all started with different initial states.

In an absolute system, all lines of behavior following a given state are identical, regardless of how the system got to that particular state. Thus in an absolute system, two lines of behavior might have started from different states, and eventually ended up in the same state. If this happens, the two lines of behavior are identical once they have been in that given state. This definition of the absolute system simply makes explicit the assumption of complete determinism of the behavior of the organism; for with complete determinism if we know the state at a given instant we can completely predict the future course of action. If the system is not absolute, then we can have a state followed by two or more different lines of behavior, with its consequent lack of complete determinism.

The lines of behavior, or the system, can either be stable or unstable. They are stable if they never leave a region of the phase-space. In this connection systems with feedback

become important, for such systems usually produce stable lines of behavior. Actually, it is only true that systems with negative feedback produce stable lines of behavior. With positive feedback the system becomes very unstable. In a system with negative feedback, all variables are interconnected in such a way that a change in one will produce a change in the others. Thus if the system is in a stable state, if one variable is changed, another variable is affected in such a way that it quickly brings the value of the first variable back to its stable-state value.

Ashby postulates that adaptive behavior leads to physiological stability, or keeps all relevant variables within physiological limits. Now the question becomes: What kind of mechanism will ensure that stable systems will result when the conditions to which the system are subjected are as many and varied as those encountered by animate organisms? The principle of ultrastability is used to take care of this problem. An ultrastable system is one which has the possibilities of stable lines of behavior, and which furthermore seeks those fields which have stable lines of behavior. Such a system can be produced if a step-function variable is introduced which interacts with the main variables of the field. The essential purpose of the step-function is that it provides a constantly changing parameter which determines the phase-space of the main variables. If the step-function interacts with the main variables in such a way that if the stable lines of behavior do not result, the step-function changes value, then the system will continue to change fields until a stable system results. (Actually, the step-function can be thought of as simply another variable in the field, which now has one more dimension. Furthermore, such a variable does not really have to be a step-function. We could simple say that only one field exists, that this field has a stable region, and that all lines of behavior converge to this stable region. However, it probably does make it easier to think of one of these variables as a step-function which acts as a parameter to change the field.)

Such a system will explain adaptive behavior at an elementary level. The problem becomes more difficult, however, when we take into consideration the fact that if a stable state is reached, and then the environment is changed and another stable state is reached, the first stable state can be shown to still be in existence. To take care of such problems, the system is made more complicated, and a multistable system is used. The multistable system is composed of many smaller ultrastable systems which are connected together by main variables. In such a system, one sub-system could reach a stable level, and this fact would then allow another sub-system to reach its stable level without changing the stable field of the first system. Obviously this sort of thing could be built up indefinitely to take care of problems of greater and greater complexity.

In very elementary form, these are the ideas that Ashby presents. They are contained in his first 18 chapters. An appendix contains six more chapters which treat the problem in more rigorous mathematical form.

It is extremely difficult to evaluate such a proposed system. Certainly there are some very interesting concepts here. On the other hand there are some severe deficiencies which will undoubtedly limit the usefulness of the system. For example, this way of thinking does not explain the role of reward in learning very well. Punishment is easily handled, because the effect of punishment is to throw the ultrastable system out of balance, forcing it to seek a new value of the step-function which will produce a stable line of behavior. Reward could probably best be handled with a Guthrian bias. Its role would be to prevent the operation of variables which might throw the system out of balance, thus making it seek values on the step-function which produce unstable fields if a stable one has been found. However, there is too much evidence that reward plays a much broader role than this in increasing the probability that a particular behavior will occur in a given situation again.

Probably a more serious problem than this, though, is the difficulty of handling problems of increasing complexity. For example, if a single ultrastable system is operating,

all learning should be insightful (i.e., sudden). But all learning is not in fact insightful. The use of the multistable system partly takes care of such problems, by providing for a series of sub-learnings each of which could be insightful in turn. Essentially, however, with such a system we would be driven ultimately to a series of step-functions of step-functions. It would not take much regression of this sort to produce a completely statistical description of adaptive behavior, but by this time the value of the particular system used to predict the statistics would be greatly lost because of its cumbersomeness.

A third difficulty, in some ways not really important, is the fairly complete lack of supporting evidence at the neural level. If the system's purpose is only to describe in an analogous way the behavior of the entire organism, this difficulty is of little concern. However, the chance that the system describes the way things really are is decreased by this lack of evidence.

All in all, the reviewer is fairly pessimistic that *Design for a Brain* will have greatly advanced psychology in either a few years or many years. Systems such as this simply do not provide a framework in which to put the known facts of complex adaptive behavior. In many ways one has the feeling that the mathematical model simply describes what we already know to be true, but that the description holds only if we confine ourselves to relatively simple examples. It does not really explain anything. Perhaps this feeling on the part of the reviewer is due to the fact that he already accepts the hypothesis that adaptive behavior is deterministic, and thus needs no proof that it can be, particularly when the proof is for behavior much simpler than that already assumed to be mechanistic. It seems as unnecessary to demonstrate that behavior commonly observed in animate objects can occur in inanimate objects as it does to demonstate the converse. Probably psychology will advance faster by a concerted effort to determine the lawful relations among the variables we find. Unless we already assume that they are lawful, there is little point in searching for them. So what have we gained by demonstrating that they can be lawful—in the deterministic sense?

Regardless of this feeling that this analogy (and other similar analogies) do not really advance the science of psychology, there is much to be gained from a reading of the book. Ashby has been as exact as possible in his developments, and at several places has made quite explicit assumptions which are implicitly made by scientists in their work. His chapter on dynamic systems is very good reading, and provides a set of definitions which would be profitable reading for most psychologists. Also, his chapter on the animal as a machine is good for his clear statement of a way of thinking about organisms in environments.

Perhaps the best way of summarizing the value of such a book is to say that as a particular system which explains how adaptive behavior comes about, it is probably not going to be of lasting interest. If, however, we want a very clear statement of what we mean when we say that adaptive behavior is deterministic or mechanistic, then this book provides a very clear and exact statement of that meaning. Perhaps this was the author's real intent. If so, then he has succeeded. If his intent was really to do more than that, he probably has not.

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