Book Reviews

Human Behavior in Its Natural Setting

Naturalistic Viewpoints in Psychological Research. EDWIN P. WILLEMS and HAROLD L. RAUSH, Eds. Holt, Rinehart and Winston, New York, 1969. x + 294 pp., illus. $7.50.

Ecological Psychology. Concepts and Methods for Studying the Environment of Human Behavior. ROGER G. BARKER. Stanford University Press, Stanford, Calif., 1968. x + 246 pp., illus. $7.50.

Those who judge psychological research to be parochial often maintain that it would become less so if psychologists made more use of naturalistic research methods. These two books provide an opportunity to evaluate this argument.

The intent of the Willems and Raush book was to argue that naturalistic approaches are the only appropriate way to find answers to some questions. In actuality it conveys considerably more. The papers show what naturalistic methods can accomplish when competently used (papers by Menzel, Kelly, Kavanau), how they can lead to cumulative understanding across research programs (Gump read in the context of the Barker book), how an advantageous base of naturalistic data can be destroyed by imposition of rarefied concepts (Guttman), how attempts to detail the uniqueness of this strategy can lead to strained (Willems, Willems and Raush), conventional (Sells), or provocative (Barker, Raush) distinctions, and how durable concepts (such as "attitude") can lose their value when researchers try to posture like naturalists before clarifying the questions they want to answer (Sechrest).

The two most persuasive papers involve infrahuman research:

Menzel, whose work is with primates, has written a frank case study of his gradual emancipation from laboratory-bound techniques and questions which should become the equivalent for naturalists of Skinner's influential essay (*1*) on the realities of laboratory life. He tells how, from an initial concern with primates' reaction to novel objects, his interest broadened to questions of how well primates know their environment, how they classify objects, and how they reach their complex definitions of available space, whence he finally arrived at the important lead that "edges" figure prominently in virtually all their observed behavior. On the way to this resolution some convenient fictions in psychology undergo rough treatment. The way primates shift the definition of an object is troublesome for those who assume that perceptual constancy is ubiquitous and adaptive. When primates react to the same object by avoiding it, threatening it, attacking it, grooming it, ignoring it, or attempting to copulate with it, depending on its context, then those who talk about an object as a "positive reinforcer" obviously are not talking about a characteristic of the object.

This same emphasis on the shifting definition of objects as a function of context pervades much of Kavanau's chapter about captive white-footed mice. Although Kavanau stays in the laboratory, he works on a question that is relevant for naturalistic inquiry: if I bring wild-caught mice and their undomesticated response repertoires into an artificial environment, of what are they capable, in what ways will they try to modify imposed regimes, and what can I infer from this about their adaptation to natural conditions? A glimpse of what he has learned about their capability can be had from a description of the vertical maze system in which he placed some animals. The system is "96 meters long, has 1205 90° turns, 48 meters of vertical passageways, and opens into 445 blind alleys, the latter occupying 53 percent of the total space." Mice learned to traverse the system in as few as two days, a performance that is even more surprising given that there was no extrinsic reward and no prior deprivation, and that the maze had to be learned both forward and backward because there was only one point of entrance and exit. On the question of how captive mice modified imposed regimes, the most striking finding is that mice avoid activities not initiated by themselves, even activities that they engage in repeatedly when they can do so of their own volition, and they similarly resist being forced to stop an activity. Part of the fascination with this phenomenon is that it is virtually identical to one uncovered in human actions by Brehm (*2*) which he labels "psychological reactance." As a clue to what Kavanau finds regarding how mice function in their natural habitats, it is sufficient to mention the lengthy program of illuminance experiments which support the idea that mice use the twilight sun and moon as "navigational" references when they are active at night.

Although I have paid more attention to findings in these two papers than to the insightful methodological points raised by their authors, this is simply because the findings are a more eloquent testimonial to the value of this method than is argumentation. Unfortunately, this eloquence is less apparent in the "human" papers. Some reasons for the difference are easy to see:

Although the adjective "naturalistic" is supposedly used to describe method (p. 273), it frequently becomes attached to phenomena. Something called "common everyday life" figures prominently. (It is even argued that if a study is couched in everyday parlance it has direct relevance to everyday life [p. 280].) Thus the universe is carved up into an odd mixture of natural and unnatural happenings. When you posit "common everyday life" as that for which naturalistic methods are uniquely suited, then you invite skepticism expressed in such questions as common to whom, how frequently must the uncommon occur before it becomes common, why does an inquisitive researcher destroy everyday life yet an inquisitive neighbor preserve it, and so on.

The distinction between molar and molecular behavior is hauled into these discussions, also in unhelpful ways. It is argued that molecular units are not visible enough to be recorded *in situ* (p. 284), a limitation that apparently wasn't evident in the '20's to Szymanski (*3*), who studied body scratching in libraries to see if it followed the principle of least effort, or to Ponder and Kennedy (*4*), who studied eye-blink rate in trolleys and libraries. Even more perplexing are the discussions of the

freedom a subject has to select his own responses. Thus to elucidate the conventional argument that naturalistic methods do not restrict the range of responses used by subjects, one author cites as an example of restrictiveness a study in which hodometers were used to record traffic patterns in a museum (p. 48)—a poor illustration, for while it is true that the record depended on a minute portion of behavior (a footstep) the measuring device in no way restricted what the people could do, nor was the measure arbitrary, since standing and walking is what people do in museums.

Much confusion in the "human" papers seems to arise from romantic and either-or thinking. Romantic thinking in naturalists takes the form: if only we listen, our subjects will tell us about their world (as if they knew or cared to tell). Either-or thinking takes the form of frequent avowals that laboratory and naturalistic strategies are on a single continuum, followed by just as frequent talk as if they weren't.

However, the days of such impediments may be numbered if researchers follow Kavanau's example of using the laboratory as a field situation and Kelly and Menzel's of using the field as a laboratory situation, and rationalize their efforts with Menzel's "naturalist's alternative":

> Manipulate only as much as is necessary to answer your questions clearly, and otherwise leave things alone, for there is order even in what seems to you to be the worst confusion, and you might well introduce worse confusion if you use your hands before you use your eyes [pp. 90–91].

Many of the reservations about naturalistic methods that linger after reading Willems and Raush are swept aside by Barker's scholarly, important, well-written volume. Where authors in Willems and Raush struggle with cryptic treatments of the environment, Barker is clear in his conceptualization of it, sophisticated in his probing of technical issues, thorough in his presentation of data, and imaginative in his theorizing. The contents of the book are equally divided among definition of the central concept, the behavior setting; description of procedures to uncover and categorize behavior settings; empirical data on the settings found in Barker's research site, "Midwest," Kansas, in 1963–64; and development of a theory of behavior settings.

To pinpoint how this volume relates to Barker's previous work, it is useful to compare it with the other major book about the same community, Barker and Wright's *Midwest and Its Children* (1955). The main shift in emphasis is from behavior to the environments in which behavior occurs. The word "ecology" appears on only two pages in 1955, whereas it is discussed on virtually every page of the present book. In 1955 behavior settings were described and analyzed by many of the same procedures reported in the '68 book, with two important differences. First, behavior settings are now combined into genotypes. Second, distributions of these genotypes through the entire behavioral environment of Midwest is traced by means of an Ecological Resources Index. This indicator permits Barker to make meaningful statements about particular behavior settings, the proportion of Midwest's total environment they cover, and the proportion of Midwest's action patterns (for example, religious, professional) and behavior mechanisms (talk, affect) that are generated by particular settings.

Two other items evident in the comparison provide a basis for examining more closely Barker's theory of behavior settings. First, in 1955 there was far more concern with social behavior; 28 percent of the earlier book was devoted to social behavior, whereas now it is merely one of 11 action patterns. Second, in 1955 Heider's theory of thing and medium was used to discuss the interrelation between molar and molecular behavior in a single actor, whereas now the theory is used to discuss the interrelations between behavior settings and their multiple occupants.

The essence of Heider's theory is that things are characterized by high interdependence of elements and an internally constrained pattern, and media are characterized by high independence of elements, the patterning of these elements being externally constrained. Thus when a "thing" is seen to influence a "medium," it means that the thing has acted as an external constraint on several independent media and has combined them into "spurious" entities. A thing is represented less accurately if its components become less interdependent or if those of the media become more interdependent.

Barker has now used these ideas as the basic for his theory of behavior settings. The central tenet is that settings are related to their human inhabitants as thing to medium. This has the advantage that occupants of setting retain individual differences (media are independent, and individual elements are governed by their own dynamics and laws), yet homogeneous behavioral patterns should emerge (as a collective unit, their behaviors are constrained by the higher interdependence of elements in the behavior setting).

But the shift from media as molecular actions in a single actor to media as molar actions among several actors raises new issues. And it is here that Barker's de-emphasis of social factors works to his disadvantage. If the same people repeatedly occupy a behavior setting, then it is probable that they interact with one another and become more familiar, mutual attraction may develop, and interdependence increase. If this happens, then occupants lose some of their efficacy as media, their behavior should be less controlled by behavior setting programs, elements of the behavior setting itself should become less interdependent, and a point should be reached where a thing-medium reversal occurs. Occupants would become the internally determined "thing" which now constrains the externally determined behavior setting (medium). This implication may have eluded Barker because of his concern with the equally compelling possibility that a change in mediation occurs when occupants are removed from the setting, when it is undermanned. Undermanning does change the medium manifold, but it seems unfortunate that Barker has not considered other kinds of changes that are implicit in the theory.

Consider some other possibilities that are implicit in Barker's theory. The more independent a component is, the better it can mediate behavior setting patterns. This suggests that the least interdependent person in a behavior setting should be the best mediator of the setting's pattern. This in turn suggests that he should generate more behavior that is appropriate to the setting, if the program of the setting starts to be disrupted he should be the first to sense this and the first to initiate maintenance activities, and his removal should dissolve the program of a setting more than if a number of interdependent occupants were removed (removal of interdependent actors should improve mediation).

Some of the most useful portions of Barker's theory concern control mechanisms that operate in settings. He argues that when program execution falters maintenance activities are initiated to restore the program. Yet all his

examples of control fit the more general and heuristic idea that control is directed at restoring the independence of media.

The stimulation produced by Barker's theorizing is matched by that of his data and procedures. The data about Midwest's environments have the same unsettling effect on traditional ideas in psychology as do some of the papers in Willems and Raush. Affect, thinking, and instrumental talk are prominent in a relatively small portion of the environments occupied by people in Midwest, yet many theorists argue that these mechanisms are crucial to understanding motivation, attitude formation, social perception, and so on. By comparing the availability of settings with their occupancy in man-hours, Barker is able to pinpoint those settings that overgenerate and undergenerate behavior. It is tempting to equate a setting that undergenerates behavior with one that is undermanned, but it is not clear whether this is warranted and Barker says nothing to inform us one way or the other. This reflects the major disappointment a reader of the book may feel. With all the information and insight Barker presents, there is no attempt to illustrate the theory by means of data presented in the preceding chapters. One can only guess at how the theoretical concepts would be operationalized in terms of the data that were gathered. But there is enough stimulation in this book to insure that other investigators will push these ideas ahead. There is no question—Barker has more to say about behavioral environments than any other person currently writing in psychology.

It is not apparent that many realize this, however. His other major book—*Midwest and Its Children*—is now out of print. Surely a scientist of Barker's stature deserves better than that. Thus it is small wonder that naturalistic researchers feel compelled to argue their case to excess. The *Zeitgeist* in psychology has so far been downright rude toward naturalistic research. Perhaps these two books will induce a long overdue change.

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References

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3. Szymanski, "Untersuchungen über eine einfache natürliche Reaktionstätigkeit," *Psychol. Forsch.* **2**, 298 (1922).
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The Fundamental Concepts of Pharmacology

Principles of Drug Action. The Basis of Pharmacology. AVRAM GOLDSTEIN, LEWIS ARONOW, and SUMNER M. KALMAN. Harper and Row, New York, 1968. xii + 884 pp., illus. $18.50.

Twenty-eight years ago the teaching and practice of pharmacology were given renaissance (perhaps more properly *naissance*) by the appearance of Louis Goodman and Alfred Gilman's *The Pharmacological Basis of Therapeutics.* That book of classic title and content, now in its third edition, has also made possible the definition of this subtle science, a blend of chemistry, magic, molecular and organ physiology, and the life-or-death practice of medicine.

Goodman and Gilman's book is constructed along lines of organ or drug systems, making it possible for the student or physician to find for any subject the essential bridge between basic and applied knowledge. The formula has been successful enough to spawn copies and competitors, none quite matching the original.

But until *Principles of Drug Action* no general text has existed on the fundamental concepts of pharmacology, which cut across all organ systems and therapeutic applications: binding forces of drugs; structure-activity relations and their biochemical basis; principles of drug absorption, distribution, and elimination; drug metabolism; general theories of drug resistance; principles of toxic reactions. These are main topics in medical and graduate school teaching of pharmacology, and now for the first time we have a book for the purpose. And for good measure, the book is literate, critical, and abundantly documented.

Somewhat surprising and most important is the suitability of this book to the medical student. Not that he should or will consume it all, but that the fundamental matter of pharmacology is now available in clear and thoughtful exposition. Perhaps one key to this great success lies in the "476 illustrations" noted on the title page. Thus we are taken, on almost every page, back to original data, which are analyzed and put into context, in a manner eminently suitable for both teaching and research.

There are, certainly, small and large omissions, and some small errors. Most are not worth documenting, but it may be asked if a book that reflects "the breadth of modern pharmacology" can exclude the autonomic and cardiovascular and renal systems. It is not a systems book; yet, there are "principles of drug action" that derive from chemical interactions at the physiological level. Perhaps the next edition. . . .

Then there are some surprising features: chapters on carcinogenesis, teratogenesis, drug development including clinical trials, and (yes!) principles of prescribing. What does it mean when three reasonably avant-garde pharmacologists, who know all about how to misread an RNA codon, devote at least 100 pages to such matters? Are they intimidated by beards talking relevance? Probably not. The truth of the matter is that in pharmacology there is a long but coherent line between the most basic and the most applied; relevance is built into the science, back to Marshall, Abel, and Schmiedeberg.

Are the student and teacher then left with a dilemma sounding like a line out of Beowulf? Goodman and Gilman vs. Goldstein *et al.*? Certainly not! Both are essential; they are complementary. The total cost—0.5 percent of a year's expenses to the medical or graduate student, or about the cost of one football weekend.

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Damage-Induced Growth

Tissue Repair. R. M. H. MCMINN. With a chapter by J. J. Pritchard. Academic Press, New York, 1969. xii + 432 pp., illus. $23.50.

This book is concerned largely with the response of adult mammalian tissues and organs to damage. There is also some discussion of compensatory hypertrophy and hormone-induced growth. Unlike most books on tissue repair, which limit themselves to particular organs such as the skin, this one covers all the principal organs. There is a good author and subject index. The chapter on bone repair is written by J. J. Pritchard.

Tissue Repair is not only the most comprehensive book in recent times on the subject of damage-induced growth, but it also brings order to the data it summarizes, and it tells the reader what is known as well as what is not known. As might be expected in a book of such breadth, the depth