



# Contemporary Scientific Psychology

*Edited by ALBERT R. GILGEN*

DEPARTMENT OF PSYCHOLOGY  
BELOIT COLLEGE  
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## Preface

The integrating theme of this book is *the scientific method in action* in psychology. Each of the contributors, with the exception of Melvin Marx, was asked to write about his own interests, work, and background in such a way as to illustrate the process of psychological inquiry and at the same time provide the reader with an overview of his research area. Marx was asked to discuss the scientific method in a more general way. It was hoped that these papers would highlight the formal aspects of data gathering and theorizing as well as demonstrate the importance of other factors which determine the day-to-day activities of scientific psychologists—for example, the particular graduate training of an investigator, chance opportunities, the stage of development of an area of research, the vulnerability of a subfield to social pressures (the *Zeitgeist*), and the lure of fancy hardware and reliable but narrowly focused techniques.

The essays which resulted surpassed our expectancies. Taken together, they provide the desired overview of scientific psychology and the inquiry process; individually, each paper offers interesting and thought-provoking insights into the work and viewpoints of an accomplished research psychologist. The chapters by Pribram (Physiological Psychology), Staats (Learning), and Rock (Perception) include systematic statements, in considerable detail, of each man's theoretical position, while at the same time touching upon some of the problems characteristic of their respective areas of study. Ratner, on the other hand, selects as his emphasis the need in Comparative Psychology for a more well-defined research schema and a useful cross-species taxonomy of behaviors; what he has to say is of relevance to all psychologists engaged in investigatory activities. Thiessen's long essay presents an intriguing view of the important but until recently rather deemphasized field of Behavior Genetics, and in the process illustrates (as does Pribram's paper) the need for more interdisciplinary training and research. Lipsitt brings us up to date with regard to the exciting and significant work being done to determine the sensory and behavioral capacities

of neonates and young infants. His chapter exemplifies dramatically how progress in one research area (in this case Learning) can open up new vistas in another (Developmental Psychology). Finally, Berkowitz, using some of his own studies to demonstrate his orientation, presents a short history of Social Psychology with particular concern for the roles played by problem-oriented and theory-generated investigations. The papers follow no rigid format. The variety of styles and emphases, in fact, convey a more accurate and interestingly presented picture of the professional activities of research psychologists and the types of problems with which they are confronted than would be possible in a treatise dealing with scientific psychology in a more abstract way.

In the first chapter, I summarize the characteristics and pitfalls of research dealt with in the seven chapters concerned with specific subfields of the discipline. I also found it appropriate to include a brief discussion of the question: Does the impressive work on the learning processes constitute a paradigm (as defined by Thomas Kuhn) for all of scientific psychology? This introductory paper, in conjunction with the chapter by Marx, comprises a sort of general frame of reference for reading subsequent chapters.

The book as a whole, or parts of it, can be used in teaching a variety of courses. It is most appropriate for courses in Experimental Psychology, History and Systems, Psychological Theories, the Philosophy of Science, honors sections in General Psychology, and seminars at both the undergraduate and graduate levels. Several of the chapters (Staats, Lipsitt, Pribram, Ratner, Berkowitz) might also be integrated into courses in Learning because they deal, in part, with the significant contributions made to psychology by investigators of the learning processes.

*A. R. G.*

## **Acknowledgments**

I wish to thank the men who made this book possible for both their enthusiastic support and their cooperation in writing their chapters. I am grateful also for the financial support given by the National Science Foundation for the Summer Institute in Contemporary Psychology for College Teachers held at Beloit College in 1968 which I directed and at which the contributors served as instructors. The book was inspired by the success of that program. My wife, Carol, I thank for planning and serving as hostess at the Institute parties which facilitated the face-to-face interaction among instructors and participants, and for her patience, as I struggled, with her help, to complete this most rewarding project.

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*Part I*

## **SCIENTIFIC PSYCHOLOGY**



# Introduction: Progress, a Paradigm, and Problems in Scientific Psychology

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In spite of the continuing controversy concerning the use and misuse of the scientific method in the social-behavioral sciences (Bugental, 1967; Kagan, 1967; Kessel, 1969; Merleau-Ponty, 1962; Polanyi, 1968; Severin, 1965), there can be little doubt that scientific psychology has evolved into an important and productive discipline. Particularly impressive (and this is reaffirmed by the papers which follow) has been the research on the learning processes as inferred from the observation of behavior change under laboratory conditions. Not only have the findings of investigators in this area found application in such diverse fields as clinical and physiological psychology, but techniques developed in conjunction with this work have opened up new research possibilities in almost all areas.

There have been, and of course still are, significant problems, and so far nothing has come along to unite the entire discipline around a common set of methodologies and principles. As many of the contributors to this volume indicate, psychologists (and probably all scientists) easily get into

ruts; for that reason there is a continual need for frequent reevaluations of the aims and procedural schemes of each area of study.

My purpose in this chapter is twofold: first, to argue that the research on the learning processes, important as it is, does not constitute a model or paradigm for all of scientific psychology; and second, to discuss some of the characteristics and needs of the research areas dealt with in the papers which comprise Part II of this book, and in the process to highlight factors which play a role in psychological inquiry. A list of topics for further study is presented at the end of this chapter.

### I. DOES CONDITIONING RESEARCH CONSTITUTE A PARADIGM FOR PSYCHOLOGY?

Kuhn (1962) in his highly acclaimed book contends that the history of all mature sciences includes a pre-paradigmatic period, a paradigmatic period, and a succession of revolutions which occur when new paradigms replace old. According to Kuhn, the pre-paradigmatic period is characterized by rival schools, little agreement with regard to fundamentals or methodologies, and investigators busy studying casual, easily observed phenomena. Research generally is rather random. A science becomes paradigmatic when an achievement of such importance occurs that practitioners from the various schools accept it both as an integrative scheme and as a guide for future research. Kuhn cites Newton's work on optics and Franklin's theory of electricity as paradigms which appeared in the natural sciences. When a true paradigm emerges, the various schools either fade away or become isolated, and the members of the scientific community concern themselves with investigations suggested by the paradigm. As this "mop-up" work takes place, sophisticated hardware is developed, more precise hypotheses and observations become possible, and there is little squabbling over fundamentals or what constitutes the proper concern of the discipline. A paradigm is embraced until significant discrepancies between expectancies and observations appear; a revolution or radical reorientation, however, does not take place until another achievement provides a new focus for the science.

Keeping Kuhn's analytical scheme in mind, does the systematic study of behavior change, particularly the research on conditioning, constitute a true paradigm for all scientific psychology? Many contemporary psychologists evidently think so because psychology is today frequently defined as *the scientific study of behavior*. I believe, however, that psychology is still in the pre-paradigmatic stage, not only because the observation of behavior change is, in my opinion, a data source and not an end in itself (see also the chapters by Pribram and Rock), but also because psychologists still

spend considerable time arguing over such fundamental questions as: What is psychology? What problems can psychologists legitimately investigate? What constitutes significant research?

A truly paradigmatic psychology, it seems to me, will be based on more than the study of behavior; many of the most important processes involved in organism-environment interactions (perceiving, thinking, emoting, feeling, sensing, etc.) are not highly correlated with gross behavioral changes and therefore require other data sources (biochemical, physiological, introspective) for their investigation. In a sense, physiological psychology has always represented the kind of multilevel approach which, in my view, is necessary for a paradigmatic psychology. Unfortunately, until rather recently, not enough was known about the functional organization of the nervous system to enable us to identify the mechanisms (biochemical and physiological correlates) underlying experiential states and transformation.

During the last fifteen years or so, however, physiological psychologists, neurophysiologists, and biochemists, equipped with new hardware and research techniques, have begun to unravel the mysteries of nervous system functioning, and although the work is far from completed, it is these developments in conjunction with the methods and findings provided by behavioral psychology which may provide psychology with its first real paradigm. Psychology, after passing through a period of premature mentalism (1860-1915), and a period of fruitful, but limited, behaviorism (1915-1970) is, in my analysis is correct, entering a period of mature mentalism characterized by reliable procedures for studying the total functioning (behavioral as well as mentalistic) of the organism. Pribram's research and thinking (Chapter 3), which can be described as a physio-behavioral cognitive psychology, is a good example of this development.

I do not wish to leave the impression that future progress in psychology depends only on the discoveries of biochemists and neurophysiologists, and that we will all have to become physiological psychologists. Far from it, for if psychologists are to identify the physiological correlates of experience, there is much research yet to be done at the purely psychological level of analysis (see Rock, Chapter 8). It may be necessary, in fact, if we are to successfully match experiential events with bodily processes, for us to devise new taxonomies of awareness events; current categories such as feeling, perception, and cognition may not suffice. In this vein, I recently proposed a classification system of awareness events (Gilgen, 1969) consisting of two main categories: "diffuse" and "structured." It is my contention that diffuse changes in awareness are correlates of the neurophysiological activity of systems sensitive only to changes (flow, composition, concentration) in liquids (blood, cochlear fluid, hormones, mucus, etc.); therefore, sounds, tastes, feelings of hot and cold, and the common affects are all classified

as diffuse. Structured events or changes, on the other hand, are considered experiential correlates of the activity of biological systems capable of extracting information about the shape and location of solids (including the bones of the body); these include the visual system, the limb-location detection system, the haptic system generally, the haptic and bone-location systems associated with the hands and mouth, the vestibular mechanism, the system controlling efferent activity, and the memory system. The primary virtue of reexaminations of systems of classification is probably that such inquiries may suggest new ways of organizing observations and data at other levels of analysis. New ways of categorizing our experiences, for example, may lead to new ways in classifying and organizing neurophysiological systems.

In summary, the paradigmatic discipline which I envision will be a truly cognitive psychology (it may even require that we again do some serious introspecting), but it will be a cognitive psychology based on reliable, multilevel data-gathering techniques. Other paradigms will, I am sure, also emerge; and if we expand our scope to the social-behavioral sciences as a whole, I should not be at all surprised if significant reorganizations of the disciplines as now constituted occur before the end of the century. In any event, the future of scientific psychology looks promising—that is, if we put what we learn to good use (see also Murphy, 1969).

## II. CHARACTERISTICS AND NEEDS OF THE RESEARCH AREAS

My purpose in briefly summarizing certain aspects of the perspectives presented by the other contributors is to highlight some of the variables operative in scientific psychological inquiry. This is not an exhaustive analysis or a complete summary; it is, as I stated previously, meant primarily for the reader interested in the interaction between the scientific method and the concerns and needs of the various research areas.

Both psychology and neurophysiology, according to Pribram (Chapter 3 on *physiological psychology*), tend to be technique- rather than problem-oriented. This deficiency, he feels, would be remedied if the investigators in each discipline knew more about each others' work. Researchers become so enamored of their own conceptual frameworks and hardware, that they often fail to see the relevance of their work for problems which are the concern of more than one discipline. Interdisciplinary graduate and post-graduate training, as well as a willingness on the part of more seasoned investigators to read outside of their own areas, is necessary to avoid this type of problem. In short, a narrow perspective, though sometimes required, may stand in the way of discovery (see also Marx, Chapter 2). Pribram's own research, which involves methods and concepts from psychology, neurophysiology, biochemistry, physics, linguistics, the computer sciences,



and mathematics, enabled him to derive a model of brain functioning, which not only has considerable power, but which would never have been forthcoming had he concerned himself only with physiological psychology as narrowly defined.

Thiessen (Chapter 4) calls for a new focus for *behavior genetics*—one which gets away from demonstrating the now well-verified fact that genetic factors and behavior are related, to a concern with the mechanisms developed by organisms as they evolved to cope with the important demands of their environments. Thiessen, in a sense, is suggesting to behavior geneticists (and I am sure that many of his colleagues agree) that they get out of a rut and concern themselves with a new unit of analysis, that unit being the organism (including its physiological mechanisms) and the environment within which it evolved. Such an emphasis requires more collaborative efforts among psychologists, geneticists, ethologists, neurophysiologists, and biochemists, and again reflects the apparent need for interdisciplinary training and research discussed previously with regard to Pribram's paper. Developments in this area, particularly at the biochemical and physiological levels, also illustrate another important characteristic of scientific inquiry, namely that systems of classification, no matter how universally accepted, eventually require modification. It is becoming increasingly clear that the line between purely genetic and strictly environmental factors is getting rather fuzzy. Conceptualizations are emerging which will, I am certain, have important implications for the recently revived nature–nurture controversy (Albee, *et al.*, 1969; Jensen, 1969a; 1969b).

Ratner (Chapter 5) contends that *comparative psychology* has been slow to develop for two reasons: first, because psychologists have not fully understood the relevance of evolutionary theory insofar as the comparative study of behavior is concerned, and second, because no effective general taxonomy of behaviors has been developed. Psychologists, according to Ratner, frequently do animal studies not knowing if the species they select is most appropriate (the best preparation) for the processes they want to investigate, and having very little knowledge of the behavioral capacities of either the animals studied or those animals to which they are interested in generalizing their findings. Such investigations are understandably of little value as comparative research. Furthermore, even when good comparative studies are attempted, researchers are hampered by the lack of a system of classifying behaviors which is applicable across all species. Ratner outlines a “stages” approach to research activity and presents a three-category taxonomy of behaviors which he hopes will help comparative psychologists overcome these difficulties. What he says, however, should be of interest to all scientific psychologists.

As was mentioned previously, the findings, concepts, and methodologies