Psychology of science

Contributions to metascience

Edited by
Barry Gholson
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CAMBRIDGE UNIVERSITY PRESS
Cambridge
New York New Rochelle Melbourne Sydney

This One
KB60-4Y7-GNHT
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Published by the Press Syndicate of the University of Cambridge
The Pitt Building, Trumpington Street, Cambridge CB2 1RP
32 East 57th Street, New York, NY 10022, USA
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

© Cambridge University Press 1989

First published 1989

Printed in Canada

Library of Congress Cataloging-in-Publication Data

Psychology of science.

Includes bibliographies and indexes.

1. Science – Philosophy. 2. Science – Methodology.
3. Science – Psychological aspects. I. Gholson, Barry.
Q175.P8965 1989 501 88–18901

British Library Cataloguing in Publication Data

Psychology of science : contributions
to metascience

1. Science – Psychological aspects
I. Gholson, Barry
501′.9

ISBN 0 521 35410 2

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Preface

The seeds of this book were sown originally in a series of discussions by its four editors, begun in January 1985 in the Psychology Department at Memphis State University. The proximal cause of those discussions was provided by Tom Cook of Northwestern University, who was a visiting scholar with the Center for Applied Psychological Research during the first three months of that year. Tom simply pointed out the obvious to us – something it often takes an outsider to do. He noted that the four of us shared interests in philosophy, history, and sociology of science. Specifically, Neimeyer (1985) had conducted a sociological analysis of the growth of personal construct theory from the perspective of Nicholas Mullins's model of the development of theory groups. Gholson had compared and criticized recent philosophy of science by Kuhn, Lakatos, and Laudan, using the paradigm clashes between competing conditioning and cognitive theories as an example (Barker & Gholson, 1984a, 1984b; Gholson & Barker, 1985). Houts had examined the nature of psychologists' value systems and their influence on scientific orientation (Krasner & Houts, 1984). Shadish had written about salient epistemological problems arising in program evaluation (Cook & Shadish, 1986; Shadish, 1986).

Of course, each of us had been aware of the others' works, and of the possible mutual interests that were implied. Tom Cook's prompt was most helpful because he pointed out that such a concentration of interests in metascience was somewhat unusual in a psychology department, and that we ought to do something constructive to take advantage of the situation. So in January of 1985, we initiated weekly sack lunch discussions to explore how we could develop these mutual interests. (We were fortunate to be joined in the fall of 1985 by two new colleagues in our department – Art Graesser, an expert in cognitive science and artificial intelligence, and Jeff Berman, who specializes in psychotherapy research.)

We spent the first few months of those meetings studying the more recent philosophy of science by Lakatos, Laudan, and Feyerabend, as well as the somewhat older works of Kuhn and of the logical positivists. Gradually our

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scope expanded, and we studied newer work in history and sociology of science, along with the very few works in metascience by authors such as Radnitzky. By the end of spring of 1985, we had achieved the first of our goals, which was to catch up at least generally with the current status and problems in these areas.

In the early summer of 1985, we made the observation that led to this book. In our efforts to gain an overview of science studies, we noted that the philosophy, history, and sociology of science each existed as independently identifiable specialties falling under the general rubric of metascience. It was only a small (if egocentric) step for us to note the absence of a salient psychology of science among these disciplines, at least any of enough visibility and stature to be taken very seriously by those philosophers, historians, and sociologists whose works we had encountered. We decided to begin to address this situation. The first major result of our efforts is the present volume. We hope that by the end of this book the reader will be convinced that the psychology of science, although admittedly underdeveloped compared to its sister disciplines in metascience, deserves increased attention for both its potential and its accomplishments in contributing to the study of science.

Many people and organizations have provided us with assistance and encouragement in completing this book. First and foremost we thank the Center for Applied Psychological Research (CAPR): It provided financial support that contributed critically to our ability to complete this effort. CAPR is funded by a grant from the State of Tennessee Centers for Excellence program to the Psychology Department of Memphis State University. The Centers of Excellence program and its success in Tennessee are topics that are themselves worthy of study by students of science. The program's purpose is to increase the scholarly contribution of selected units in Tennessee higher education. Its success has, we understand, begun to receive some attention and emulation in other states.

Tom Cook provided the focus that eventually led to this book, and we thank him. Our editor at Cambridge University Press, Susan Milmoe, provided helpful support, encouragement, and criticism. Several anonymous reviewers provided useful criticisms that improved the quality of the contents. Several of our students and colleagues, both here and elsewhere, have provided the kind of intellectual or financial support that all academics need as they develop such interests. In particular, we thank Peter Barker, Eric Freedman, Sunil Sen Gupta, Patrick Heelan, Len Krasner, Frank Leeming, Robert Merton, Andy Meyers, David Morgan, and Milt Trapold. Finally, we would like to thank our respective spouses, Cynthia Gholson, Betty Duke Shadish,

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Kathy Story, and Estelle Houts. All academics should be blessed with spouses as patient and supportive.

Memphis, Tennessee

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References

Barker, P., & Gholson, B. (1984a). The history of the psychology of learning as a rationale process: Lakatos versus Kuhn. In H. W. Reese (Ed.), *Advances in child development and behavior* (Vol. 18, pp. 227–244). New York: Academic Press.

(1984b). From Kuhn to Lakatos to Laudan. In H. W. Reese (Ed.), *Advances in child development and behavior* (Vol. 18, pp. 277–284). New York: Academic Press.

Cook, T. D., & Shadish, W. R. (1986). Program evaluation: The worldly science. *Annual Review of Psychology*, *37*, 193–232.

Gholson, B., & Barker, P. (1985). Kuhn, Lakatos, and Laudan: Applications in the history of physics and psychology. *American Psychologist, 40,* 755–69.

Krasner, L., & Houts, A. C. (1984). A study of the "value" systems of behavioral scientists. *American Psychologist, 39*, 840–50.

Neimeyer, R. A. (1985). *The development of personal construct psychology*. Lincoln: University of Nebraska Press.

Shadish, W. R. (1986). Planned critical multiplism: Some elaborations. *Behavioral Assessment, 8*, 75–103.

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1. The psychology of science: An introduction

William R. Shadish, Jr., Arthur C. Houts, Barry Gholson, and Robert A. Neimeyer

What is the psychology of science, and where has it been all these years? Although contributions from philosophers, historians, and sociologists of science have burgeoned over the last half-century or more, the same cannot be said about the contributions of psychologists to our understanding of science. Yet as we will see in this book, both the methods and the theories of psychology have important and unique contributions to make to science studies. In fact, a sizable psychological literature pertinent to science studies already exists; but the contributors to that literature have rarely identified themselves explicitly either as psychologists of science or as members of a coherent specialty called psychology of science.

Today, however, we think that psychology of science is a specialty whose time has now come. Substantively, psychological contributions to science studies are increasing in frequency and quality. Sociologically, psychologists are beginning to identify themselves as interested in the topic. But much work needs to be done if the psychology of science is to achieve its potential. In the present book, we plan to further this agenda – to examine the history of and justification for a psychology of science, to outline its possible content and methods, to document some if its accomplishments and its potential, and most of all, to intrigue and encourage fellow psychologists to bring their expertise to bear on the study of science.

Science studies as a context for psychology of science

The study of science as a topic, sometimes called metascience (Hickey, 1976; Radnitzky, 1968; Rauhala, 1976), encompasses a multidisciplinary array of systematic efforts to examine the operations and consequences of science. Ziman (1984), for example, notes that such contributions have been made by historians, philosophers, sociologists, psychologists, economists, and political scientists. Even that list is incomplete, because science studies arguably include work by such diverse professionals as biographers (Keller, 1983), policy analysts (Brooks, 1977), public opinion pollsters (Yankelovich, 1982),

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and information scientists (Hickey, 1976), to name just a few others. Yet all are not equal in the metascientific arena. Three disciplines currently make the most salient contributions to the field: philosophy, history, and sociology (see Houts, Chapter 3, this volume).

Science studies have traditionally been dominated by philosophers of science, who generally contribute conceptual analyses of such matters as the nature of scientific knowledge and method, the meaning of scientific progress, or the role of values in science. The contributions of philosophers have been both frequent and important. The logical positivist analysis of scientific knowledge, for example, was one of the most influential forces to emerge in the history of metascientific inquiry. The succeeding controversies and alternatives to logical positivism have continued to provide a fertile source of ideas about how science is or should be conducted (Brown, 1977; Lakatos, 1978; Laudan, 1977).

For many reasons, historians of science have also been frequent contributors to the metascientific literature. No doubt this is partly due to the intuitive plausibility of analyzing great scientists or scientific events throughout history as a means of learning about the nature of science. Historians have offered many interesting and compelling examinations of this and other kinds (Holton, 1978; Miller, 1984). Moreover, because philosophers of science often rely heavily on appeals to historical examples in their work, historians and philosophers proved naturally compatible collaborators. In addition to documenting historical data, their theoretical analyses emphasize science as it exists in and is affected by its historical context.

The third firmly established metascientific specialty is the sociology of science (Collins, 1983; Mulkay, 1980). Compared to historians and philosophers, sociologists brought a different theoretical and methodological approach to metascience. Their theories tend to emphasize institutional, societal, and cultural factors that impinge on science, and they often analyze the processes and outcomes of science as a function of these factors. Such analyses have something in common with the emphasis on cultural context in the history of science. But although sociologists have sometimes studied famous scientists of the past or great scientific achievements, they are more likely to study the day-to-day activities of contemporary scientists doing ordinary science. In addition, their methods are more empirical in the sense that we have commonly come to think of social science as empirical, that is, as emphasizing the quantitative and qualitative observation of scientists and their work.

Methodologically, the psychology of science is more likely to resemble the sociology of science than it is to resemble either of the other two dominant specialties. Like sociology, psychology is a social science that emphasizes empirical observation of the operations and consequences of contemporary science. But unlike sociology, psychological theories tend to focus on indi-

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vidual cognition, feelings, and behavior rather than on societal-level variables, although the two disciplines overlap somewhat in the area of social psychology. And psychology has traditionally used a somewhat different set of methods than other disciplines, making more use, for example, of such methodologies as experimentation and psychometrics than other metascientific specialties. We cite these tendencies not as prescriptions for or definitions of a psychology of science, but only as descriptions that in our experience are true of psychology as a whole, and therefore likely to be true of psychology of science as a specialty. However, in the last chapter of this book, we will hazard a few somewhat more specific proposals about what the nature of psychology of science should be in these and other respects.

The growing importance of science studies

Each of these metascientific specialties has matured at a different rate. The philosophy and history of science have been clearly identified subspecialties since at least the beginning of this century. Sociology of science has been recognized widely as a specialty since the 1940s–50s (Collins, 1983; Mulkay, 1980). Psychology of science seems to be, tentatively, just now emerging as a recognized specialty. There is no reason to think that this sequence of new specialties will stop at this point, so that other metascientific specialties will undoubtedly be added to this list over time, such as the political economy of science, science policy, or science evaluation.

The increasing frequency and quality of metascientific studies eventually cannot help but affect the perception and conduct of science itself. Descriptive studies of the operations and consequences of science should probably be given some priority at this point, given the paucity of our empirical knowledge about what actually happens under the name of science. Although some of these analyses of science may confirm our expectations, many of these past studies have shown surprising variations from what some might have expected. Faust (1984), for example, showed that the cognitive abilities of scientists bear little resemblance to common stereotypes about how scientists can or should think. Mahoney (1977) and Peters and Ceci (1982) showed that the peer review system is systematically biased to an unexpected degree. Hedges (1987) showed that the results of experiments in particle physics may not be strikingly more consistent than those of social or behavioral experiments. Such empirical descriptions of science will inevitably change the way we think about it, with ramifications that are as yet unknown.

Eventually, however, empirical science studies may also contribute suggestions about the processes and arrangements that lead to better and worse science. Until now, most such suggestions have been contributed by philosophers of science, who have appropriated the study of "valid" scientific knowl-

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edge as their own purview. Without discussing the details of their arguments, philosophers typically note that descriptive studies can tell only what is, and not what ought to be. But there is a middle ground that some philosophers have overlooked. By relating various scientific processes to a plausible range of outcomes that might count as desirable ends, descriptive studies may tell us what processes predict or cause particular outcomes. In effect, this yields a kind of hybrid descriptive–prescriptive statement, not the sweeping prescription that some particular strategy is best, but the contingent prediction that if outcome A is desired, then the scientist could most profitably do process B, and so forth. Conversely, philosophical prescriptions advocating particular kinds of outcomes or processes are often accompanied by arguments that suggest testable ramifications. Descriptive studies can provide evidence about such ramifications (e.g., Hedges, 1987). For example, empirical demonstrations that falsification does not in practice lead to rejection of theories played a major role in the subsequent debates about Popper's approach to science, and in our willingness to accept this criterion as a singular means of assessing progress in science (Lakatos, 1978; Laudan, 1977).

Psychology has already made a few contributions to this newly developing field of science studies, almost entirely of a descriptive sort. But its role could be more effective with a more concerted, systematic effort. Most past psychological contributions to science studies have been occasional rather than programmatic, socially scattered rather than socially organized and coherent, and isolated rather than cumulative. Psychology of science needs to attract scientists who are willing to conduct long-term, programmatic research, who understand the importance of social organization in fostering the social climate and material support for research, and who will study the research in an area so thoroughly that they can forward integrated reviews or theories that summarize what we know and that indicate where we can go from here. It takes many things to mobilize this degree of support, such as a critical mass of interested scholars who are aware of one another's work, the availability of funding and publication outlets, and the sense of excitement about a field's potential that could be generated by a major achievement or two (Mullins, 1973). We hope that the present book will be one building block in mobilizing this support, at least to the extent that it begins the process of integrating our current understanding of what psychology of science has been, is, and might be.

Psychology of science: Where has it been?

Psychology of science has been present in metascience all along. Although this presence has occasionally been explicitly mentioned (Holton, 1978), it has most often been implicit. Any psychologist who reads metascientific work

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question. What might account for the failure of psychology to achieve a visibility and impact commensurate with that achieved by philosophy, history, and sociology of science? We can think of at least five possible explanations, all of which probably have been contributing factors. First, it may be that psychology of science has made contributions, but has failed to communicate them to other metascientists. Because specialists in other metasciences rarely cite most of the works that Fisch (1977) lists in his bibliography, this explanation has some superficial plausibility. Much of the work in the Fisch (1977) bibliography is published in outlets that sociologists, philosophers, and historians are probably unaware of or have little time to read. Similarly, relatively few psychologists participate actively in organizations that have traditionally been concerned with science studies (e.g., Society for Social Studies of Science, the Philosophy of Science Association, the Society for the History of Technology, the History of Science Society, the European Association for the Study of Science and Technology). To the extent that this explanation holds, it reflects a disciplinary specialization and insulation that is not peculiar to science studies, and that has generally proven difficult to circumvent in the name of interdisciplinary investigation of a problem.

Second, it may be that psychology of science cannot *in principle* make important or unique contributions to science studies generally, or to some particular issues or topics in science studies. Some philosophers and historians of science have endorsed versions of this position. Laudan (1977), for example, holds that psychology and sociology should comment only on irrational beliefs in science, and he reserves the explanation of rational beliefs for philosophy. Hence psychology of science needs to be justified as a field of inquiry. Barker, Heyes, and Mahoney (respectively, Chapters 4, 5, and 6, this volume) all discuss matters pertaining to such justifications, such as the reflexivity problem, or the "psychologism" criticism. While acknowledging that the validity of such objections is a matter of continuing debate, we prefer to defer judgment until psychology of science has had a chance to prove itself. If it makes important and novel contributions to our understanding of science studies, the force of such objections will necessarily be diminished no matter how firmly they are believed. In the meantime, however, it is probable that metascientists who believe such objections may be less likely to take psychological contributions seriously.

Third, it may be that the literature on psychology of science has been too scattered and piecemeal to appear as a coherent body of knowledge. Today, for example, there is no journal called *Psychology of Science*. Rather, psychologists of science have published their work in diverse psychological and nonpsychological books and journals, making it difficult to follow the literature consistently. Moreover, there is to date no comprehensive critical review of available research in the psychology of science, and such reviews are one

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mitted to logical positivism or its variants than they are to any other philosophy of science. If anything, epistemological multiplism is currently the de facto condition of the field. In this intellectual environment, psychology of science may be a more welcomed pursuit.

In all likelihood, all these reasons for the lack of saliency of psychology of science have some merit, and their investigation is itself a topic that might be of considerable interest in science studies. Mullins's (1973) descriptions of how theory groups develop, for example, ought to be relevant to the current status of psychology of science. Of his four stages of theory group development, psychology of science is most likely to be in the first, during which diverse scholars work on the topic in relative isolation from one another without identifying with the emerging specialty. Moving to the second stage requires a more explicit identification with the specialty, an identification that would be facilitated by the production of a scholarly work that is exciting enough to attract colleagues to the specialty. Whether psychology of science is near to entering Mullins's second stage remains to be seen. At any rate, self-critical documentation of the development of psychology of science is timely, is interesting in its own right, and needs to be continued.

But whatever the reason for the lack of saliency of psychology of science, one motive for publishing the present volume is to begin to find remedies. We know from research in the sociology of science that scientific interest groups form partly for sociological and psychological reasons. On the one hand, we hope that by intentionally highlighting psychology of science as an area of inquiry, we will encourage more scholars to study the potentially important contributions that psychology can make to science studies. On the other hand, our intellectual commitments are emphatically to a metascience in which psychology is one of many tools that will foster further understanding of science. A narrow specialization in a psychology of science that is ignorant of this larger context would do a disservice to science studies generally. Therefore, although we think for sociological reasons that a psychology of science needs to emerge as an important force in science studies, for epistemological reasons we advise all who would identify with this specialty to be conversant with and respectful of the contributions of other metascientific specialties (Neimeyer & Shadish, 1987).

The psychology of science: What is it, and what might it be?

We are not about to quibble over a definition of psychology of science, for two reasons. First, a definition would prove difficult to agree upon, since it has proven difficult to get agreement about a definition of the parent discipline, psychology. Most introductory textbooks say that psychology is "the scientific study of behavior," with a few texts adding something like "and

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mental processes." If one were happy with that definition – and we do not have something better to offer – then the psychology of science would be defined as the scientific study of scientific behavior and mental processes. This definition will do for a start, even though it will not save psychology of science from the same kind of diversity and even splintering that the parent discipline has itself experienced practically from its inception.

Second, we will not quibble about a definition because we do not want to encourage obsessive speculations about the precise intellectual boundaries of a field that we have already said should not define its boundaries too rigidly with respect to other metascientific inquiries. Psychology of science will overlap with other metascience specialties in many places. For example, it might overlap with sociology in studying the social psychology of science, with philosophy in naturalistic epistemology, and with history in studying the psychology of great historical figures in science. In fact, the diversity of perspective brought to a given topic by interdisciplinary approaches is, in our opinion, a decided epistemological advantage for ferreting out disciplinary biases that might otherwise go unnoticed. Although we respect the wisdom of having each discipline do what it does best, we have little patience with intellectual turf battles about who should be allotted particular topics. We hope that psychologists will applaud good studies of scientific behavior and thought regardless of the disciplinary specialty of the author, but also that psychologists will not shy away from studying topics just because some other metascience wants to retain that turf.

A final caution about defining psychology of science is worth noting. Practically speaking, psychology of science will be defined in the minds of many by its past accomplishments. The danger of this "operational definition," however, is that it confuses description and evaluation of past accomplishment with a judgment about the potential for future contributions. Psychology of science is a very immature specialty. Compared to the other three metascientific specialties, a fair evaluation would have to acknowledge that psychology's accomplishments may be meager thus far. Nevertheless, we believe that in at least two respects the potential of psychology to contribute to science studies has barely been tapped.

First, the bulk of past studies in psychology of science have focused on three topics: personality, creativity, and cognition. Without minimizing the importance of these topics, even a cursory glance at the table of contents of an introductory psychology textbook reveals a wealth of other topics of potential relevance to science studies. These topics include perception, learning, memory, motivation, life-span development, social psychology, organizational psychology, and even parts of physiological psychology such as hemispheric specialization research. Each of these topics, in turn, holds a plethora of more specialized subjects. Take social psychology, for example. The most

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recent *Handbook of Social Psychology* (Lindzey & Aronson, 1985) includes such potentially relevant areas as role theory, organizational theory, social perceptions and attributions, attitudes and attitude change, social influence and conformity, intergroup relations, and leadership and power. Clearly, psychology of science could be much broader in topical scope than its accomplishments to date suggest.

Secondly, psychology of science is also broader in theoretical and methodological scope than past accomplishments reflect. To judge from some past work, for example, an outsider might conclude that psychoanalytic theory reflected modern work in personality theory, or that psychohistorical case studies are the method of choice among psychologists of science (Eiduson & Beckman, 1973). Fortunately, more recent work is now beginning to rectify those impressions (Jackson & Rushton, 1987). Still, much theoretical and methodological potential remains untapped. For example, few psychologists are applying modern cognitive science theory and method to the study of scientists; and there are few applications of the experimental methodologies that Mahoney (1977) showed could provide such dramatic demonstrations.

To illustrate the range of substantive topics that a psychology of science might include, consider the grid presented in Figure 1.1. The cells in the grid result from the intersection of two factors: domains of psychological theory, and dimensions of scientific work. Given space constraints, we have listed only a few examples of both factors in the grid. A complete list of domains of psychological theory, for example, would have to include all the topics we mentioned two paragraphs ago, and more. The interested reader can no doubt elaborate the two lists.

The simplest use of the grid is to organize the existing psychology of science literature, with each study being categorized into the appropriate cell(s). Armstrong's (1979) study of multiple hypothesis formation and testing is an example of a cognitive study of question generation, project implementation, and interpretation. Adair, Dushenko, and Lindsay's (1985) research on the impact of ethical regulations on research practice is an example of a social psychological study of project implementation and management. Mossholder, Dewhirst, and Arvey's (1981) examination of differences between developmental and research personnel is a personality study of scientists' organizational behavior. Kendall and Ford's (1979) survey of reasons for clinical research is a motivational study of scholarly program planning. Nederhof and Zwier's (1983) study of the crisis in social psychology is a cognitive study of social psychologists' evaluation of their science. Boice and Jones's (1984) examination of why academicians do not write is a motivational and social psychological study of the interpretation and dissemination process. Campbell's (1984) retrospective history of his part in the invention of the regression

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Dimensions of Scientific Work	Domains of Psychology				
	Creativity	Cognition	Personality	Motivation	Social Psychology
Career Choice					
Program Planning					
Problem Selection					
Question Generation					
Project Implementation					
Method Selection					
Project Management					
Data Analysis					
Interpretation					
Dissemination					
Use of Other Work					
Information Processing					
Collaboration					
Organizational Behavior					
Evaluation of Science					
Obtaining Funding					
Training New Scientists					
Social Responsibility					

Figure 1.1. A psychology of science grid.

discontinuity design is a descriptive reconstruction of creativity in methodology.

A comprehensive review of studies that might fill and expand the grid is beyond the scope of this chapter. But supposing such a review had been done, and the resulting studies placed in the context of the grid, several benefits would ensue. First, studies would be organized into logical categories, facilitating the synthesis of what we know, and the construction of new hypotheses and theories to explain the data that these studies provide. For example, the grid would undoubtedly show that many studies now exist about creativity in science. These studies serve as the raw material from which theories of scientific creativity could be constructed, as Simonton (1988; Chapter 7, this volume) has attempted to do. Such integrative efforts seem to be particularly lacking in psychology of science, and are probably essential if the specialty is to take an equal place among other metascientific specialties.

A second benefit is that relatively empty cells in the grid would suggest

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would have no place at all in the grid, suggesting new entries in the basic dimensions that define the grid. In trying to place into the grid the Eiduson and Beckman (1973) work on personality and motivation in science, for example, we discovered that we had no entry for career choice as a dimension of scientific work, and thus added that dimension. Moreover, the grid is not entirely comprehensive, either, because it does not address such things as methodology or whether one is studying contemporary or past figures. The grid is simply one of many possible heuristic devices to illustrate the substantive range and potential of a psychology of science. We encourage other theorists to criticize the grid, and suggest alternatives for describing psychology of science.

Ultimately, the psychology of science is just beginning a process of defining its domain and potential, and the grid is just one way to represent that domain. The topics and methods of psychology of science are likely to resemble those of its parent discipline. But to judge from the potential applications suggested by the grid, and from the contributions to this and other volumes (Jackson & Rushton, 1987), we are optimistic that the contributions of psychology of science will be interesting and constructive.

The present book

This book provides a cross section of some current work in the psychology of science. Our selection of contributors reflects four goals. First, a good deal of controversy exists concerning the *possibility* of a psychology of science. Hence, we tried to select authors who could shed light both on the history of these controversies and on some philosophical problems involving the legitimacy of psychological contributions to science studies. These chapters are contained in the first two sections of the book, respectively. Undoubtedly, these authors do not lay to rest all doubts in this regard. But we do think these chapters will, at a minimum, orient the reader to the history of psychology of science and some of the controversies surrounding it, sensitize the reader to the complexity of the issues involved, and perhaps even convince some readers that the psychology of science is indeed a legitimate field for their inquiry.

The second goal of the book is to begin to outline the scope and content of a psychology of science. The comments earlier in this chapter about the nature of psychology of science, and the grid in Figure 1.1, are relevant to this task. However, the chapters in the third, fourth, and fifth sections of this book also pertain to this goal. We selected as contributors psychologists whose works reflect a blend of various topical, theoretical, and methodological perspectives. Topically, they have studied such diverse issues as the application of Piaget's genetic epistemology to the development of Darwin's theory of

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Brown, H. I. (1977). *Perception, theory, and commitment: The new philosophy of science.* Chicago: University of Chicago Press.
Campbell, D. T. (1984). Foreword. In W. M. K. Trochim, *Research design for program evaluation: The regression–discontinuity approach.* Newbury Park, CA: Sage.
Collins, H. M. (1983). The sociology of scientific knowledge: Studies of contemporary science. *Annual Review of Sociology, 9*, 265–285.
Cook, T. D. (1985). Postpositivist critical multiplism. In L. Shotland & M. M. Mark (Eds.), *Social science and social policy* (pp. 21–62). Newbury Park, CA: Sage.
Cutting, J. E. (1987). Perception and information. *Annual Review of Psychology, 38*, 61–90.
Eiduson, B. T., & Beckman, L. (Eds.). (1973). *Science as a career choice: Theoretical and empirical studies.* New York: Russell Sage Foundation.
Faust, D. (1984). *The limits of scientific reasoning.* Minneapolis: University of Minnesota Press.
Fisch, R. (1977). Psychology of science. In J. Spiegel-Rosing & D. de S. Price, (Eds.), *Science, technology, and society: A cross-disciplinary perspective* (pp. 277–318). Newbury Park, CA: Sage.
Galton, F. (1874). *English men of science: Their nature and nurture.* London: Macmillan.
Hadamard, J. S. (1945). *An essay on the psychology of invention in the mathematical field.* Princeton: Princeton University Press.
Hedges, L. V. (1987). How hard is hard science, how soft is soft science? The empirical cumulativeness of research. *American Psychologist, 42*, 443–455.
Hickey, T. J. (1976). *Introduction to metascience: An information science approach to methodology of scientific research.* P.O. Box 590, Oak Park, IL: Author.
Higgins, E. T., & Bargh, J. A. (1987). Social cognition and social perception. *Annual Review of Psychology, 38*, 369–426.
Holton, G. J. (1973). *Thematic origins of scientific thought.* Cambridge, MA: Harvard University Press.
(1978). *The scientific imagination: Case studies.* Cambridge: Cambridge University Press.
Houts, A. C., Cook, T. D., & Shadish, W. R. (1986). The person–situation debate: A critical multiplist perspective. *Journal of Personality, 54*, 101–154.
Jackson, D. N., & Rushton, J. P. (Eds.), (1987). *Scientific excellence: Origins and assessment.* Newbury Park, CA: Sage.
Janis, I. L. (1972). *Victims of groupthink.* Boston: Houghton Mifflin.
Johnson, M. K., & Hasher, L. (1987). Human learning and memory. *Annual Review of Psychology, 38*, 631–668.
Joreskog, K. G., & Sorbom, D. (1986). *LISREL VI: Analysis of linear structural relationships by maximum likelihood, instrumental variables, and least squares methods.* University of Uppsala, Department of Statistics, P.O. Box 513, S–751, 20 Uppsala, Sweden: Author.
Keller, E. F. (1983). *A feeling for the organism.* San Francisco: Freeman.
Kendall, P. C. & Ford, J. D. (1979). Reasons for clinical research: Characteristics of contributors and their contributions to the *Journal of Consulting and Clinical Psychology. Journal of Consulting and Clinical Psychology, 47*, 99–105.
Lakatos, I. (1978). *The methodology of scientific research programmes: Philosophical papers* (Vol. 1). Cambridge: Cambridge University Press.
Laudan, L. (1977). *Progress and its problems: Towards a theory of scientific growth.* Berkeley: University of California Press.
Lindzey, G., & Aronson, E. (1985). *The Handbook of social psychology*, (3rd ed., 2 vols.). New York: Random House.

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Mahoney, M. J. (1976). *Psychologist as subject: The psychological imperative*. Cambridge, MA: Ballinger.
(1977). Publication prejudices: An experimental study of confirmatory bias in the peer review system. *Cognitive Therapy and Research, 1*, 161–175.
(1979). Psychology of the scientist: An evaluative review. *Social Studies of Science, 9*, 349–375.
Maslow, A. H. (1966). *The psychology of science*. New York: Harper & Row.
Miller, A. I. (1984). *Imagery in scientific thought: Creating 20th century physics*. Boston: Birkhauser.
Mossholder, K. W., Dewhirst, H. D., & Arvey, R. D. (1981). Vocational interest and personality differences between development and research personnel: A field study. *Journal of Vocational Behavior, 19*, 233–243.
Mulkay, M. (1980). Sociology of science in the West. *Current Sociology, 28*, 1–184.
Mullins, N. (1973). *Theories and theory groups in contemporary American sociology*. New York: Harper & Row.
Nederhof, A. J., & Zwier, A. G. (1983). The 'crisis' in social psychology: An empirical approach. *European Journal of Social Psychology, 13*, 255–280.
Neimeyer, R. A., & Shadish, W. R. (1987). Optimizing scientific validity: Toward an interdisciplinary science studies. *Knowledge: Creation, Diffusion, Utilization, 8*, 463–485.
Oden, G. C. (1987). Concept, knowledge, and thought. *Annual Review of Psychology, 38*, 203–228.
Pelz, D. C., & Andrews, F. M. (1976), *Scientists in organizations: Productive climates for research and development*. Ann Arbor, MI: University of Michigan Press.
Peters, D. P., & Ceci, S. J. (1982). Peer-review practices of psychological journals: The fate of published articles, submitted again. *The Behavioral and Brain Sciences, 5*, 187–195.
Pittman, T. S., & Heller, J. F. (1987). Social motivation. *Annual Review of Psychology, 38*, 461–490.
Polanyi, M. (1958). *Personal knowledge: Towards a post-critical philosophy*. London: Routledge & Kegan Paul.
(1966). *The tacit dimension*. London: Routledge & Kegan Paul.
Radnitzky, G. (1968). *Anglo-Saxon schools of metascience*. Akademiforlaget Goteborg, Sweden: Berlingska Boktryekeriet Lund.
Rauhala, L. (1976). Analytic psychology and metascience. *Journal of Analytic Psychology, 21*, 50–63.
Rodin, J. (1985). The application of social psychology. In G. Lindzey & E. Aronson, (Eds.), *Handbook of social psychology* (3rd ed., Vol. 2, pp. 805–881). Chicago: Rand McNally.
Simonton, D. K. (1988). *Scientific genius: A psychology of science*. Cambridge: Cambridge University Press.
Singer, B. F. (1971). Toward a psychology of science. *American Psychologist, 26*, 1010–1016.
Trochim, W. M. K. (Ed.). (1986). *Advances in quasi-experimental design and analysis*. San Francisco: Jossey-Bass.
Yankelovich, D. (1982). Changing public attitudes to science and the quality of life: Excerpts from a seminar. In M. C. LaFollette (Ed.), *Quality in science* (pp. 100–112). Cambridge, MA: MIT Press.
Ziman, J. (1984). *An introduction to science studies: The philosophical and social aspects of science and technology*. Cambridge: Cambridge University Press.

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PART I

Historical issues in the psychology of science

Historical analyses of the psychology of science

William R. Shadish, Jr.

The first part of the book contains two chapters that discuss the history of the psychology of science. It seems odd to be discussing the history of a specialty that we previously suggested may not yet exist in any organized sense. Yet the psychology of science has historical roots not only in the parent discipline of psychology but also in philosophy, history, and sociology of science. We become educated and informed about these roots, and about the important issues to which they give rise for psychologists, by studying that history. History teaches us about important accomplishments and controversies that might involve psychologists, about paths to pursue and blind alleys to avoid, and about the kind of reception psychologists can expect to receive from other scholars of science. Perhaps as important, history teaches us a sense of humility about our place in the larger scheme of science studies by reminding us of the critical contributions made to psychology of science by its early pioneers.

One of those pioneers is Donald T. Campbell (1950, 1959), a psychologist who is particularly well suited to teach us our history. In the first chapter in this section, Campbell avers that he is only alluding to a possible history that someone else may eventually write. But this typical modesty on his part is belied by the breadth of material that he taps. One cannot read Campbell's chapter without immediately sensing the various threads in metascience that give rise to psychology of science, including work by psychological epistemologists among philosophers, and by psychologists who were also epistemologists. But he now writes that this will be his last work on the topic. We are fortunate indeed that he has codified his accumulated experience in this chapter, for our grasp on many of these historical threads might otherwise be lost. If this is indeed to be his last work on the topic, then both psychology and psychology of science will be the worse for it.

The second chapter in this part is by Arthur C. Houts. Houts is a psy-

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chologist, but he received his undergraduate training in philosophy. Hence he is conversant with both literatures. His chapter is entirely devoted to a description of the role of psychology of science in the broader context of metascience. He traces psychological issues and concepts that have been encountered throughout the years in philosophy, history, and sociology of science. We think the reader will find particularly educational Houts's extended analysis of the historical resistance on the part of some philosophers to psychological contributions. Tracing this resistance back to the logical positivist analysis of the Vienna Circle, Houts then explains the objections of succeeding generations of philosophers – objections that include the reflexivity problem, the "psychologism" objection, and the contention that psychology can address only the irrational aspects of knowledge. Although Houts is not pretending to provide a definitive response to such objections, he illustrates plausible responses and shows that such objections, where still entertained, are a matter of intense debate.

The Houts chapter is particularly interesting for another reason, as well. In the five tables in his chapter, he lists possible questions for the psychology of science. Houts argues that these are *psychological* questions that have arisen in other metascience disciplines, and that, in general, psychologists are probably best equipped to address them. He concludes that these questions can form an agenda for the psychology of science. But the reader will find that the agenda that Houts gives us is different in two ways from the agenda that is set forth in the last chapter of this book. The first difference is not controversial: Houts proposes a set of substantive questions for the psychology of science, whereas the last chapter proposes a general theoretical, methodological, and procedural agenda without addressing how to identify a set of important substantive questions at the level of detail to which Houts aspires. The complementarity of the two agendas is illustrated by the fact that all of Houts's questions could be implemented within the agenda set out in the last chapter.

But the second difference does not lend itself to such a simple resolution. It has to do with some implicit assumptions about where and how to construct important substantive questions in the psychology of science. Houts's agenda gives primacy to an interdisciplinary emphasis in question formation, whereas the agenda in the last chapter gives primacy to unique psychological emphasis. Houts clearly believes that the psychology of science will make its most significant contributions when it engages other metascience disciplines to develop an agenda – always with a critical eye, of course. The agenda in the last chapter, on the other hand, accepts Houts's critical approach, but argues that equally important questions for the psychology of science will arise independently also, by application of the fund of psychological theory and methods to the observation of scientists, and that little knowledge of the other metascientific disciplines is required to do so.

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psychology of cognitive bias. Contribution to symposium on The Problem of Experimenter Bias (Robert Rosenthal, chairman), American Psychological Association Annual Convention, Cincinnati, Ohio, September 1959. Unpublished manuscript.

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maze will it be able to master, in how many trials, under what conditions of previous training, etc.? While it is a long way from the orientation of rats in a maze to the intellectual adaptations (if I may be forgiven the irreverent comparison) of the Newtons, Maxwells, and Einsteins in their theoretical constructions of the physical universe, the nature of the problem is the same" (Feigl, 1956, 25–26). Elsewhere he has spoken of the pragmatic approach to scientific induction as in itself an empirical science, "being the psycho-bio-sociology of cognitive behavior" (Feigl, 1950). Bergmann, likewise discussing the differences between the philosopher's and the scientist's tasks, says "the logical analysis of science is one thing, the psychology of discovery is another thing. The former is a philosophical enterprise; the latter, if we only knew more about it, would be a branch of the science of psychology" (Bergmann, 1957, 51).

Thus in purifying their own problem area, these philosophers have pointed to the potential psychology of science. Even though such a psychology is not established in courses, journals, or professorships, many in fact have been practicing it. At its present development, the psychology of science seems to have these problem areas: (a) the psychology of cognitive achievement as applied to the achievements in science – the psychological explanation of scientific creativity, discovery, problem-solving, trial-and-error learning, etc.; (b) the psychology of cognitive bias applied to the biases and blind spots of scientists (Francis Bacon gave this area a good start in his list of the biases or "idols" he found among his fellow philosophers); (c) the motivational psychology of scientists – the role of curiosity, aggressiveness, self-esteem, vanity, power, and other needs in shaping the final scientific product; (d) personality and science – the tendency of certain personality types to be attracted to science, and within science, the tendency for personality differences between those who take various roles and positions; and (e) psychological epistemology – the role of psychological experience in establishing the inductive base for all science, the psychological description of the criteria of evidence and proof used by scientists, psychological aspects to the mind–body problem, and the innumerable other points where psychological problems border epistemological issues. (Campbell, in Boring, 1963, vi–vii)

This essay will concentrate on (e) psychological epistemology, but in a manner that also includes (a) and (b), and which emphasizes the neurological machinery of perceptual knowing, and the sociological machinery of scientific knowing. With this comes an emphasis on mechanical imperfection, waveband limitations, dependence upon useful but nonentailing proxy variables or approximate symptoms, etc. That is to say, the history of our field here proposed is revisionist history done from the point of view of "epistemology naturalized" (Kornblith, 1985; Quine, 1969; and, as extended to a naturalistic sociology of scientific validity, Campbell, 1986a, 1986b; Giere, 1984, 1985). Within philosophy, the naturalistic epistemologists are a growing minority, involving some, such as Quine, Goldman, Dennett, and the Churchlands, who retain central status in philosophy. Whereas the naturalistic epistemology which seems to welcome us psychologists as equal status participants (but see Heyes, Chapter 5, this volume) is still a minority position, the logical positivism or logical empiricism from which Feigl and Bergmann speak so confidently in the above quotes is uniformly rejected by contemporary epistemologists and philosophers of science.

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Locating psychological epistemologists among the philosophers: Descartes, Locke, Berkeley, and Hume as neurophysiological psychologists

To do this history of psychological epistemology properly, we will want to be free to reinterpret some of the classical philosophers as *psychologists in ways central to their epistemology*. For this quartet, this may turn out to be easily done. They all identified mind as a product of brain, and found centrally relevant the mechanical processes mediating sense organ activation. These fallible and arbitrary processes could obviously *not* provide certainty. Indeed, at every mechanical link, opportunities for illusory pseudostimulation occurred. A realist research agenda, extended to taking the machinery of perception as being made up of objects in the world comparable to those being perceived, in the end supported skepticism by making the argument from illusion more plausible, thus undermining the naive, clairvoyant realism which vision is apt to induce.

For Descartes, my primary entry into this view is through Crombie (1967; Clarke, 1982, may also be relevant). (Scattered throughout my chapter are suggestions of articles and books that should be anthologized and reprinted for our new discipline. This essay of Crombie's is certainly one). Descartes, like Kepler, Pascal, da Vinci, and other leading sixteenth- and seventeenth-century intellectuals (see also Alpers, 1983, ch. 2) experimented with the *camera obscura*, that closet with a single pinpoint window, through which came the light rays that projected on the opposite wall an upside-down view of the scene outside. Descartes experimented with glass lenses, and with the lenses of oxen eyes, confirming that they focused an inverted image at the back of the eyeball. He was possibly the most advanced theorist of his day as to central nervous system neurology, going beyond his predecessors by not finding the inverted image a puzzle needing explanation, and by connecting the afferent and efferent nervous systems without a gap. Although he thought of nerves as tubes transmitting a fluid, this mechanical model does as well as a more modern one in making it clear that intermediate stimulation of the nerves (as by his evil demon, or by modern implanted electrodes) could simulate perception in a way that the perceiver could not distinguish from the perception of external objects. He recognized his own dreams to be akin to such percepts, and believed that the insane had such hallucinations. I do not know whether or not he mentions the phantom-limb experiences of amputees, but no doubt they were well known in Descartes's day, and certainly support his perspective.

Let me try to use the skin senses to make more vivid the intellectual affront coming from knowledge of the noniconic, non–truth-entailing, mechanical neural transmission, an affront to any normal naive realist, but especially

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keenly felt by any one sharing Descartes's extreme desire for certainty. Although Descartes does not, to my knowledge, use this example, almost certainly the neuroanatomical knowledge involved was available to him. In terms of Descartes's neural tubes, it might have been assumed that the nerves from the skin receptors for cold transmitted a cold fluid, and those for warm, a warm fluid. In such a model, the neural tubes would be in some sense iconic validity transmitters. But almost certainly it was known to Descartes that this model was wrong, and that the fluids in these neural tubes were all the same. This fact makes it clear that neural transmission is "arbitrary" rather than "iconic" or "apodictic," thus making possible illusory perception in the case of mechanical stimulation anywhere along the nerve, as by a surgeon's scalpel.

Locke was a medical doctor and aware of the neuroanatomy of his day. Berkeley did detailed theorizing about the clues for depth perception in vision that come from the kinesthetic sensing of muscle movements in ocular convergence and accommodation. Both made epistemological use of results of psychological research (later quantified by E. H. Weber of the Weber–Fechner law) in the following quotes:

The same water at the same time, may produce the idea of cold by one hand, and of heat by the other: Whereas it is impossible that the same water, if those ideas were really in it, should at the same time be both hot and cold. (Locke, 1690, book 2, ch. 8, sec. 21/1975, p. 139)

Philonous: "It is not an absurdity to think that the same thing should be at the same time both cold and warm?"

Hylas: "It is."

Philonous: "Suppose now one of your hands hot, the other cold, and that they are both at once put into the same vessel of water in an intermediate state: will not the water seem cold to one hand, and warm to the other?"

Hylas: "It will."

Philonous: "Ought we not therefore by your principles to conclude it is really both cold and warm at the same time? That is, according to your own concession, to believe an absurdity?"

Hylas: "I confess it seems so."

(Berkeley, 1713/1949, pp. 178–179)

Locke argued this antirealist case for only the secondary qualities. For Berkeley, all qualities were like Locke's secondary ones, constructed in the mind. MacCormac (1980) documents Humes's assumption of neural and brain processing underlying mental events. Thus these naturalistic epistemologists denied foundational status to sense perception. Vis-à-vis the naive realism and the seeming experiential directness which vision induces, their message was skeptical: We could never be sure we knew for certain. Perception was not self-validating or foundational in the sense of producing certainty. (From the point of view of a sociology of scientific validity [Campbell, 1986a; Campbell and Paller, in press] and for language learning [Campbell, 1973], visual

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perception may have a pragmatic, if not logically entailing, foundational role.) Although I believe this to be the correct conclusion for *all* psychological epistemology, I should note, however, that it is a conclusion that James Gibson's (1966, 1979) influential work, much attended to in philosophy, has seemed to deny. Elsewhere I have discussed his position at greater length (Paller & Campbell, 1988). Although I strongly disagree with him, Gibson warrants a whole chapter in a volume on the history of psychological epistemology.

Descartes did not end up a skeptic. Without rejecting the illusory possibilities entailed by his mechanistic neurological analysis, he decided that God would not generally deceive us. Rom Harré (1980) argues that a similar providentialism is found in Locke, Berkeley, and even Hume ("Nature's" providence). He finds such a providentialism in a modern form in those evolutionary epistemologies that argue, for example, that natural selection would not have left us with eyes that regularly mislead us. (This point of view is now so widespread that Harré [1980, p. 33] feels no need to provide citations. Quine [1969, 1974, 1975] is briefly illustrative. For expanded attention, see Bradie, 1986; Callebaut & Pinxten, 1987; Campbell, 1974b, 1988; Campbell, Heyes, & Callebaut, 1987; Hahlweg & Hooker, in press; Plotkin, 1982; Radnitzky & Bartley, 1987; Riedl & Wuketits, 1987; Shimony & Nails, 1987; Wuketits, 1984.)

Philosophers as psychologists in the next two centuries

Kant

We should eventually search for psychological epistemologists in the next century as well. The best overall guide I know of is Charles Wallraff's (1961) *Philosophical Theory and Psychological Fact*, a book which we should reprint. Lange (1890/1950), and Sober (1978) also help. One prominent school of Kantians, founded by Jacob Fries around 1800, held that Kant's categories of thought and perception were unavoidable psychological predispositions, essential to human knowing, but of logically unprovable validity. Out of this school came Georg Simmel (1895/1982) and Ernst Cassirer. Lange (1890/1950) takes a similar point of view on Kant. See also A. D. Lindsay's (1934) introduction to Kant's *Critique*, remarkable for its naturalistic, trial-and-error orientation. (Almost all contemporary Kantians reject such views as profound misunderstandings.)

Helmholtz

Wallraff (1961) is also marvelous on the history of misleading uses of "immediate," "unmediated," "unmittelbar," and equivalent terms. What is ex-

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Meyerson

Emile Meyerson (1859–1933) identified his work as psychology, not philosophy or physics, although his analysis was of the history of physical theory, particularly of conservation concepts. Looking at my own copy (1908/1962) of his *Identity & Reality*, I find it much marked up. My personal index in the back papers has some two dozen entries, mostly to "psychology of knowledge." Yet owning to the passage of time and the difficulty of his thought, I do not feel competent to epitomize his position, but merely to advertise his importance. Certainly he believed that physicists' reification of reidentifiable stable entities (molar or molecular) and the positing of conserved constants were expressing deep-seated psychological tendencies in human thought processes, and were *not* summarizing empirical observations, which always belied those "identities." His denial that these idealizations are more useful than knowledge of the imperfect regularities upon which they are based (pp. 41–42) seems to me to be directed against the economic and instrumental evolutionary epistemology of contemporaries such as Mach, Spencer, and Simmel (1895/1982). Fortunately, in Joseph LaLumia (1966), we have a modern interpreter to help us. In his introduction, LaLumia gives us a useful overview of psychological themes in the history of epistemology. (That introduction and his chapter 1 deserve space in our anthology.)

Locke held that all ideas have their source finally in experience, but seems to have believed that to avoid errors in metaphysics and in the theory of knowledge it is necessary to know in advance about the "powers" of the instrument which must make use of experience, namely the mind, or the "understanding," as he called it. Accordingly, he prefaced his theory of knowledge with a psychological theory.

Possibly the main features of Locke's psychological theory are the signs of reaction in it against Descartes. It is a protest against the doctrine of innate ideas and is intended to offer an alternative to it. But the significant thing is that it takes the doctrine of innate ideas to be a psychological theory. The methods of the two men are different. Descartes' method adumbrates and heralds that of Kant. His quest for certainty leads him to ask in a short time how it is possible that he should be in possession of certain ideas, and he is led to the doctrine of innate ideas as the only alternative left when he cannot lay it to imagination or to sensation. He asks, in other words, what is logically presupposed by his possession of certain ideas, but the nature of what is presupposed is a matter of human constitution at birth, and more specifically a matter of the human mind's constitution before any experience whatever. Locke's method is the opposite of this. He has made up his mind that experience is the source of all ideas and that innate ideas are unnecessary, and his object thereafter is to show that the explanation of all the ideas we possess is perfectly feasible on this assumption. Though primarily bent on resolving philosophical problems, both Descartes and Locke thus clearly believed that psychology had some special relevance to metaphysics and epistemology, both felt they needed a psychology for their philosophical theories, and both in some degree or another provided themselves with a psychology.

In various other ways, other thinkers since Locke and Descartes have seemed to

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portantly upon the general question of the relevance of psychology to philosophical investigations. The fact that Meyerson believes himself moreover to have laid bare, as psychologist, the *intellectus ipse* of Leibniz, and to have accomplished in an empirical way essentially the same task as Kant sought to accomplish in the *Critique of Pure Reason*, heightens the interest of his work. Somebody is mistaken, or else Leibniz and Kant were not clear, for it is not at all generally understood that the doctrines of these men were psychological. (LaLumia, 1966, pp. 1–3)

Arne Naess

Arne Naess (1911–), in his early work, provides the contrasting example of a philosopher attempting to base an epistemology upon a behavioristic psychology. Although, once again, I am merely pointing to the importance of, rather than competently reporting on, in this case I have more nearly personal contact. For my University of California at Berkeley Psychology Department (1937–41, 1946–7), Naess was a particular hero. A friend of Brunswik and Tolman, it is my memory that he visited Berkeley both before and after World War II, reputedly hitchhiking there with his sleeping bag. We heard of great skiing exploits in the service of the Norwegian resistance. These visits were supposedly the occasion of working with Brunswik on their joint monograph for the *International Encyclopedia of Unified Science*, announced as soon to appear perhaps as early as 1937 or 1938. (Eventually, in 1952, Brunswik published such a monograph alone.) Soon after Naess founded the journal *Inquiry*, I published my first (and perhaps only) contribution to psychology of science in it (Campbell, 1959a). In this paper I cited his work briefly, and even pleaded guilty to the error of "maze-epistemology" which he warned of, but depended mainly on Brunswik's discussions for knowledge of its contents. Naess and I have never really met. I no doubt attended the two Berkeley colloquia (if they really occurred) and perhaps some of the social events related to them, although only a graduate student.

In 1936 (see Ness, 1936; "Ness" is a German spelling of *Naess*), he published in Oslo, in the German language *Knowledge and Scientific Behavior.* In it he proposed substituting for the introspective psychology of previous epistemologies (including the logical positivists' "sensations," "sense data," "phenomenal givens," etc.) a behaviorist psychology. For the epistemology of science, this would attend to scientists' behavior. This monograph was written in Vienna during 1934–5 while Naess was participating in the Vienna Circle seminars of the logical positivists. In his Foreword, he expresses his indebtedness to the European and American pragmatists as well as to the authors of the Vienna Circle. What we should in particular note is that his behaviorism was of a very atypical sort, namely that of Edward Tolman. Tolman spent a sabbatical in Vienna 1933–4. Naess was in Vienna in 1934–5 at least. L. D. Smith (1986, p. 349, note 51) says: "None of these corre-

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spondents – who include . . . Arne Naess – were actively participating in the meetings of the Circle during the months Tolman spent in Vienna," and did not otherwise meet at that time (Clark, personal communication, June 5, 1987, citing correspondence with Naess), but had already read Tolman's *Purposive Behavior* of 1932 in Norway before going to Vienna. This interest was no doubt reinforced by contact with Egon Brunswik, whose work on perception is cited even more than is Tolman's on learning. L. D. Smith (1986, p. 307) also reports that in 1938 and 1939, Naess did a first-hand study of the rivalry between Hull and Tolman. Naess (1972, pp. 136–137) also briefly describes this research. Naess (1965) gives a brief restatement and reevaluation of his approach in English. A recent Festschrift for Naess (Gullvag & Wetlesen, 1983) devotes three chapters to *Knowledge and Scientific Behavior*, plus a reply to one of them by Naess himself. Gullvag and Wetlesen, L. D. Smith (1986), and Naess (1965) are the important entries in Naess's science of science and science of epistemology, an agenda that he continued to maintain should replace philosophy of science and armchair epistemology, even when later giving up his nonparticipatory observational behavioristic study of scientists. He also introduced the explicit strategy of third-person epistemology ("epistemology of the other one," Campbell, 1959a).

Polanyi

Where to work Michael Polanyi into this list? A physical chemist when he moved to England in the 1930s, he is today best known for his theory of science, and this gets him misclassified as a philosopher. He may well be the greatest of psychologists of science. (I say this even though he rejected Neo-Darwinian evolutionary theory.) On Polanyi, I can offer a personal account.

I first met Michael Polanyi face to face at the Quadrangle Club of the University of Chicago, at a gathering of local alumni of the Center for Advanced Study in the Behavioral Sciences, probably in the spring of 1967 or 1968. I was fresh from a belated reading of his great *Personal Knowledge* (1958), and in that milling social event we somehow got time for a sustained conversation. During my Fulbright year at Oxford, 1968–9, we lived within a few blocks of the Polanyis and saw them regularly. Thereafter, during his almost annual Spring Quarters at the University of Chicago, we got together a time or two (during all of these years I was at Northwestern University, some 12 miles away).

Although he would not fully approve the company, I cite him most frequently (along with Popper, Quine, Hanson, Toulmin, Kuhn, and Feyerabend) as convincingly demonstrating that scientific theories are radically underjustified by the evidence, and thus involve discretionary choices. As he would put it, a scientist's belief in a theory requires a leap of faith, akin to

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that of a religious believer. But, especially in contrast to Kuhn and Feyerabend, this demonstration of the knower's predicament had no nihilistic overtones. He believed in science's progress, and believed that a scientist's moral duty was to choose theories that one believed were real (Polanyi, 1967), and by acting on that belief, to test the theories' credibility. In addition to being a pioneering contribution to philosophy of science, *Personal Knowledge* (Polanyi, 1958) is a great book in the psychology of scientific knowing, a founding document for a field that has yet to mature. So it is also for a sociology of scientific validity.

What he appreciated most in my appreciation of him was my use of several of his other themes. Most important, probably, was my defense of his great antireductionist essay, "Life's Irreductible Structure," in my "Downward Causation" (Campbell, 1974a). This shared emergentism overlay our only fundamental disagreement: my passionate advocacy of Neo-Darwinism evolutionary theory along with blind variation and selective retention as the only paradigm for increasing fit and order, to say nothing of creative thought (Campbell, 1960), versus his deep-seated conviction that such puny, tedious, and precarious processes were utterly inadequate to explaining the marvels of life and the intellectual achievements of science.

A great intellectual and scientist, with participation in no other communities of our complex society (e.g., no organized religious, political, or recreational group memberships), he nonetheless was able to speculate on the tragic role that emancipated secular intellectualism and the scientific world view may have played in recent history. His "Why Did We Destroy Europe?" (1970) is the essay I cite in this regard, in works such as my presidential address to the American Psychological Association (1975), which scolded my fellow psychologists' antitraditionalism and skin-surface hedonism.

While his neighbor in Oxford, I visited Prague in October 1968, two months after the August crushing of the Dubček government. This led to discussions on his "The Lessons of the Hungarian Revolution" (ch. 2 in Polanyi, 1969), and to the great role of the motive of honesty in politics, a revolutionary commitment coming from the desire to be able to affirm publicly what one believes and to be free from obligations to endorse publicly statements one disbelieves (see Campbell, 1988, ch. 11, on "The Experimenting Society").

In addition to the individual psychology of scientific belief, we in the present book are also interested in the social psychology and sociology of science. Most important for this is chapter 7, on "Conviviality" of *Personal Knowledge*, plus his essays on "A Society of Explorers" (Polanyi, 1966), "The Potential Theory of Absorption," "The Growth of Science in Society," and "The Republic of Science" (all three in Polanyi, 1969). These are contributions to the sociology of science that are *epistemologically relevant* in that they provide an explanation of how science (with its overwhelming depend-

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ence upon interpersonal trust) could nonetheless produce belief-change in the direction of increased validity.

Other philosophers as psychological epistemologists

For me, two papers, in press at the same time, founded modern naturalistic epistemology. These were Quine (1969) and Shimony (1970; see also Shimony, 1971; Shimony & Nails, 1987). Naturalistic epistemologies are probably always psychological (even when they are not evolutionary). Shimony (1970, p. 83, and note 7, p. 162) lists among his predecessors Harris (1965), Hirst (1959), Mandelbaum (1964), and Dewey (1929), among others. In my course on "Knowledge Processes," taught at Northwestern University almost every year from 1951 to 1979, I used Hirst and Mandelbaum at least once, and endorsed their relevance and importance. Harris was a colleague and discussion partner. (If I have not ever cited these scholars, the reason is that they made no use of the evolutionary perspective.) I would add Hanson (1958, 1969) and Pasch (1958) to that list. For that course, I duplicated excerpts from a historian of theory of science, A. D. Ritchie (1958, pp. 5–8, 53–57, 115–116, 209–219), giving them the title "Conceptual Errors in Epistemology Resulting from an Overdependence upon Vision." They are worth anthologizing. My Northwestern colleague, Professor E. L. Clark, translated for me Paul Souriau's (1881) *Theory of Invention*, and I still have lots of copies. Paul's son, Ettiene Souriau (personal conversation), was of the opinion that this work had influenced Poincaré's (1913) famous essay on mathematical creativity. (They both use the metaphor of the hooked atoms of Epicurus.)

C. U. M. Smith (1986, 1987) makes it clear that Nietzsche belongs in this list, with a uniquely different and important perspective on evolutionary psychological and sociological epistemology. Bloor (1983) argues that Wittgenstein was a naturalistic epistemologist, of a sociological sort insofar as language is involved. I have grossly underrepresented the French literature in all parts of this chapter, particularly philosophers with a psychological epistemology. Ullmo (1958), a modern philospher who makes some use of Piaget, leads one to Brunschvicg and Bachelard, as well as to Meyerson. Serge Moscovici must have written in our area, or at any rate would be a good guide to the French language history of ideas.

Psychologists as epistemologists: the deceased and aged

James and Baldwin

We are, of course, centrally interested in those who, like us, have combined psychology of science with a professional identity as psychologists. William James was one. His vigorous naturalistic epistemology was done while he was

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temology of 1950 still deserve translation, however much he may have wanted to revise them. They compare histories of specific sciences and mathematics with developmental stages in children. Note the special issue of the *Revue Internationale de Philosophie* (1982, *36*, double issue, nos. 142 and 143) devoted to his epistemology. As an entree into this large literature, Flavell's (1963) chapters still seem best to me. Furth's (1969) title is misleading, as he instead presents the views of Lorenz. Piaget has commented on Lorenz (Piaget, 1971). See also Russell (1978), Mischel (1971), and Kitchener (1986) – the latter two, philosophers.

Others, briefly

Let me rush through scattered "others" whom we should attend to. Wolfgang Koehler's (1938) *The Place of Value in a World of Facts* is really epistemology, in spite of its title; it is a great book that deserves our reprinting. Koehler distinguished his phenomenology from that of Husserl. Merleau-Ponty (1962, 1963) is correct in asserting that Koehler adheres to the physicalist world view. Merleau-Ponty himself deserves our attention. I provide a biased entry into his work (Campbell, 1969b). Michotte (1946/1963) argued that his research on the perception of causality supports the views of the philosopher Maine de Biran rather than those of David Hume. Donald Hebb (1980) had these interests. Roger Sperry's split brain research has stimulated much philosophical discussion, and he has been a psychoneuroepistemologist for a long time (e.g., Sperry, 1952, 1983).

Do not be misled by Laurence Smith's (1986) title, as he covers not only philosophy's impact on psychology but also the contributions of psychologists to the theory of science. Smith's Chapter 5 is on "Tolman's Psychology of Science" (see also Campbell, 1979); Chapter 8 is on "Hull's Psychology of Science" (he misses Ammons, 1962); and Chapter 9 is on "B. F. Skinner's Psychology of Science" (to this, add Johnston & Pennypacker, 1980, ch. 20). Smith also augments Hammond's (1966) entry to Egon Brunswik. I took Brunswik's course on Perception in 1938, and in 1940, I served as his teaching assistant for one semester in Experimental Psychology, which was full of perceptual demonstrations. Because my own epistemology is based on Brunswik's theory of perception rather than on his logical positivism or his later "psychology without the organism," with knowing as "achievement coefficients," I suspect that we should translate his *Wahrnehmung und Gegenstandswelt [Perception and the Object World]* (Brunswik, 1934).

You are not apt to miss Jerome Bruner's contributions, but just in case, do read Bruner (1962, 1973). Nor are you apt to miss Maslow's *The Psychology of Science* (1966/1969). But when we reprint it, we must be sure to add the omitted Maslow (1948), comparing the scientist's knowing with that of the

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gardner (1986) cite further replications, and illustrate the principle in a history of the "sleeper effect." From their title, "Under What Conditions Does Theory Obstruct Research Progress?" it is clear that they are doing epistemologically relevant psychology of science. In my 1958 article, I used Gestalt principles of perception to elucidate the philosophical concept of "entity." My 1956 and 1960 articles are fundamental to my evolutionary epistemology (e.g., Campbell, 1974b). Campbell (1960) has been included in a philosophers' anthology on that topic (Radnitzky & Bartley, 1987), and both articles (1956 and 1960) will be included in my own collection directed to philosophers (Campbell, in press). Another unpublished classic was my (1959b) "Systematic Errors To Be Expected of the Social Scientist on the Basis of a General Psychology of Cognitive Bias." It was, however, heavily derivative of my 1959c article, including the interpretation of Francis Bacon's "idols" as psychology and sociology of science. In my 1961 contributed chapter, I apply such a psychology to anthropological science. I designated my 1964 paper as "applied epistemology." There are other miscellaneous asides that I can point out as illustrating a general preoccupation with an epistemologically relevant psychology and sociology of science (1963, pp. 97–98; 1969a, pp. 365–367; Campbell & Stanley, 1963, pp. 4–6; Cook & Campbell, 1979, pp. 28–30).

Most clearly belonging to our new field is my 1959a article, "Methodological Suggestions from a Comparative Psychology of Knowledge Processes." This was cited (albeit only in passing, in the last paragraph) in Quine's (1969) foundational "Epistemology Naturalized," making me a charter member of that movement. Quine seems, in that essay, to say that all that philosophical epistemologists can do from now on is psychology, without specifying how that psychology could be epistemological. In retrospect, I can claim to have avoided this error by taking a "hypothetically normative" stance, shown by the "methodological recommendations" referred to in the title. That is, if the world and the human knower were to be as we contingently believe them to be, and if one wanted to know, then these are the strategies that one should (contingently) follow. I believe that Quine later makes explicit that he shares this orientation (Quine, 1974, and especially clearly for nonphilosophers, 1975). Focused on Quine and language learning is my obscure but important paper published in 1973. My "Pattern Matching as an Essential in Distal Knowing" (1966) is also pure psychological epistemology. My (1969b, 1979, and 1987b) publications are organizational social psychology of science. In Brewer and Collins (1981), there are frequent citations by the editors and other contributors to my unpublished William James Lectures given at Harvard in 1977, and entitled "Descriptive Epistemology: Psychological, Sociological, and Evolutionary." These are now published (Campbell, 1979, 1987a, 1988, ch. 17).

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Psychologists as epistemologists: current participants

It may be that one reason we are not yet institutionalized is that our field is so extremely large and active and, at the same time, so lacking in a paradigm or dominating exemplar. My additions here to other bibliographic efforts are on this point of no help. Fisch (1977) may provide the most extensive review. Of his 300 or so references, half might be rejected as purely sociology of science, but this still leaves 150 or so. These show little overlap with my references here, although they do contain Polanyi. He misses Boring, James, Baldwin, and Piaget. (He also misses me, quite understandably.) His bibliography is strong on scientific creativity and the personality of scientists. Although he is our best route of entry to European contributors, most of his citations are from American sources. Note in particular that he lists 33 articles from a long series edited by R. B. Ammons and C. H. Ammons on "Psychology of the Scientist," in *Perceptual and Motor Skills*, beginning in 1962.

Joseph Royce has a long investment in establishing our field. His (Royce, Coward, Egan, Kessel, & Moss, 1978) review of the literature on "Psychological Epistemology," lists some 300 items. Again, I would find about half directly related to our agenda. The other half deal with research on perception, thought processes, language, and artificial intelligence, all made cogent through his 13 epistemological issues upon which psychological research is relevant. The Royce and Rozeboom (1972) conference volume on the *Psychology of Knowing* is a neglected classic. I am particularly intrigued by Wolfgang Metzger's contribution. For Gibson's perspective, the discussion exchanges are enlightening. Contributions by Moroz, Gyr, Hammond, Grover, Wilson, Plaus, Weckowicz, and Pribram are also worthy of attention (on Pribram, see also 1965).

Barry Singer's (1972) brief call for a psychology of science is right on target. Without my help, you might have missed Sonja Grover's (1981) book, *Toward a Psychology of the Scientist*. To judge from her citations, she is most dependent upon Polanyi, Kuhn, this volume's Mahoney, Medawar, Neisser (whom I should have done a paragraph on), Nisbett, and Zimbardo. She shares the anger of the younger sociologists of science at the pretentions of certainty for science, but does not cite them. In spite of the title, Biggins (1984) is disappointing, but adds Hudson (1966, 1970) to our list. James Russell's (1984) new book is a survey of psychological epistemology with current philosophers considered, such as Davidson and Quine.

In haste, let me call your attention to some others. Robert Rosenthal (e.g., 1976) may be our most productive epistemologically relevant psychologist of science. See also Adair (1973), Faust (1984), Perry (1966), and those collected by Jackson and Rushton (1987). Mitroff (1974) and Gregory (1981) are both very important. Herbert Simon's (1977) psychology of scientific discovery has

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now been implemented in computer programs that replicate historical advances in science (Langley, Simon, Bradshaw, and Zytkow, 1986). Macnamara (1982, 1986) revises Quine and is otherwise epistemologically relevant in his studies of children learning word meanings. Premack (1986) borrows Quine's famous word as a title in an astute descriptive epistemology. Rock (1983) integrates logic and perception. Jaynes (1977) not only argues the historical relativity of self-conscious knowing, but also persuades me on the ubiquitous role of spatial metaphor in all thought. Humphrey (1984), building upon his research in "blindsight" in monkeys and humans, distinguishes between competent perceptual responding and conscious experience, in a manner that should revise much epistemology.

Let me conclude by recommending that you all join two organizations. The first, the Society for Philosophy and Psychology, is about ten or so years old. This organization is dominated by vigorous young philosophers, with us psychologists poorly represented so far. But these philosophers are paying attention to psychological research, particularly in the areas of cognition, perception, central nervous system neurology, and artificial intelligence. (The current secretary treasurer is Prof. Patricia Kitcher, Philosophy, University of California, San Diego. Dues are $15.00 per year.) The main product is an annual meeting each May or June. If they have a central journal, it is *The Behavioral and Brain Sciences* (Cambridge University Press), although, if it is still going, *Cognition and Brain Science* is equally relevant. (The many publications of Bradford Books of MIT Press are also highly relevant.) Zynon Pylyshn, Richard Nisbett, Lee Ross, Amos Tversky, and the late Hillel Einhorn are perhaps the psychologists who have most regularly participated in the Society for Philosophy and Psychology. They are also among the psychologists most cited by these philosophers. Cherniak (1986) also cites Rosch. Sperry, Shepard, and Kosslyn also rank high in citations. Nisbett and Ross are the only psychologists (other than myself) who have a chapter in Hilary Kornblith's (1985) *Naturalizing Epistemology*. Nisbett (Holland, Holyoak, Nisbett, & Thagard, 1986) is also coauthor with a philosopher and computer scientist of a book on induction. The other psychologists getting repeated citations in Kornblith are Abelson, Fischoff, Gibson, Johnson-Laird, Kahneman, George Miller, Neisser, Piaget, Rosch, Schank, Tolman, Tversky, Uhr, and Wason.

If you are making psychology of science a major specialty, you should also join the Social Psychology Subgroup of the Society for the Social Studies of Science. Their *Newsletter* is edited and published by their permanent secretary, Ron Westrum, Department of Interdisciplinary Technology, Eastern Michigan University, Ypsilanti, Michigan 48197, U.S.A. Subgroup dues are $5.00, and do not require Society for the Social Studies of Science membership. (Make checks payable to Ron Westrum.) However, that society and its

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new journal (*Science & Technology Studies*) are also recommended. Send $30.00 to Academic Services, Inc. (Attention: Paul Henderson), 1040 Turnpike Street, Canton, MA 02021.

References

Adair, J. G. (1973). *The human subject: The social psychology of the psychological experiment*. Boston, MA: Little, Brown.

Alpers, S. (1983). *The art of describing: Dutch art in the seventeenth century*. Chicago, IL: University of Chicago Press.

Ammons, R. B. (1962). Psychology of the scientist II: Clark Hull and his idea books. *Perceptual and Motor Skills, 15*, 800–802.

Asch, S. E. (1952). *Social psychology*. New York: Prentice-Hall.

Baldwin, J. M. (1909, 1910). *Darwin and the humanities*. Baltimore: Review Publishing. London: Allen & Unwin. (Reissued New York: AMS Press, 1980.)

(1906, 1908, 1911). *Thought and things, a study of the development and meaning of thought, or genetic logic, Vol. 1: Functional logic or genetic theory of knowledge; Vol. 2: Experimental logic or genetic theory of thought; Vol. 3: Genetic epistemology*. London: Swan Sonnenschein [in Muirhead's Library of Philosophy]; New York: Macmillan. Reissued in 2 vols. New York: Arno Press, 1975.

Bergmann, G. (1957). *Philosophy of science*. Madison, WI: University of Wisconsin Press.

Berkeley, G. (1713). *Three dialogues between Hylas and Philonous: The first dialogue*. London: Henry Clements. Reprinted in A. A. Luce & T. E. Jessop (Eds.), *The works of George Berkeley, Bishop of Cloyne* (Vol. 2, pp. 171–207). London: Thomas Nelson, 1949.

Berlyne, D. E. (1960). *Conflict, arousal, and curiosity*. New York: McGraw-Hill.

Biggins, D. R. (1984). The psychology of science. *Interdisciplinary Science Reviews, 9*, 172–178.

Bloor, D. (1983). *Wittgenstein: A social theory of knowledge*. London: Macmillan.

Boring, E. G. (1961) *Psychologist at large*. New York: Basic Books.

(1963). *History, psychology, and science: Selected papers* (R. I. Watson & D. T. Campbell, Eds.). New York: Wiley.

(1964). Cognitive dissonance: Its use in science. *Science, 145*, 680–685.

Bradie, M. (1986). Assessing evolutionary epistemology. *Biology & Philosophy, 1*, 401–459.

Brewer, M. B., & Collins, B. E. (Eds.). (1981). *Scientific inquiry and the social sciences: A volume in honor of Donald T. Campbell*. San Francisco: Jossey-Bass.

Broughton, J. M., & Freeman-Moir, D. J. (Eds.). (1982). *The cognitive–developmental psychology of James Mark Baldwin*. Norwood, NJ: Ablex.

Bruner, J. S. (1962). *On knowing: Essays for the left hand*. Cambridge, MA: Harvard University Press.

(1973). *Beyond the information given: Studies in the psychology of knowing*. New York: Norton.

Brunswik, E. (1934). *Wahrnehmung und Gegenstandswelt: Grundlegung einer Psychologie vom Gegenstand her*. Vienna: Deuticke.

(1952). *The conceptual framework of psychology, International Encyclopedia of Unified Science*, (Vol. 1 [10]). Chicago, IL: University of Chicago Press.

Callebaut, W. G., & Pinxten, R. (Eds.). (1987). *Evolutionary epistemology: A multiparadigm program*. Dordrecht: Reidel.

Campbell, D. T. (1950). *On the psychological study of knowledge*. Duplicated draft

Copyrighted material

(1986a). Science's social system of validity-enhancing collective belief change and the problems of the social sciences. In D. W. Fiske & R. A. Shweder (Eds.), *Metatheory in social science: Pluralisms and subjectivities* (pp. 108–135). Chicago, IL: University of Chicago Press.
(1986b). Science policy from a naturalistic sociological epistemology. In P. D. Asquith & P. Kitcher (Eds.), *PSA 1984*, (Vol. 2, pp. 14–29). East Lansing, MI: Philosophy of Science Association.
(1987a). Neurological embodiments of belief and the gaps in the fit of phenomena to noumena. In A. Shimony & D. Nails (Eds.), *Naturalistic epistemology* (pp. 165–192). Dordrecht: Reidel.
(1987b). Guidelines for monitoring the scientific competence of preventive intervention research centers: An exercise in the sociology of scientific validity. *Knowledge, 8*, 389–430.
(1988). *Methodology and epistemology for social sciences: Selected papers*. (E. S. Overman, Ed.). Chicago, IL: University of Chicago Press.
(in press). *Naturalistic theory of knowledge*. Bloomington, IN: Indiana University Press.
Campbell, D. T., Heyes, C. M., & Callebaut, W. G. (1987). Evolutionary epistemology. In W. Callebaut & R. Pinxten (Eds.), *Evolutionary epistemology: A multiparadigm program*. Dordrecht: Reidel.
Campbell, D. T., & Paller, B. T. (in press). Extending evolutionary epistemology to 'justifying' scientific beliefs: A sociological rapprochement with a fallibilist perceptual foundationalism? In K. Hahlweg & C. A. Hooker (Eds.), *Issues in evolutionary epistemology*. Albany: State University of New York Press.
Campbell, D. T., & Stanley, J. C. (1963). Experimental and quasi-experimental designs for research on teaching. In N. L. Gage (Ed.), *Handbook of research on teaching* (pp. 171–246). Chicago, IL: Rand McNally. Reprinted as *Experimental and quasi-experimental designs for research*. Chicago, IL: Rand McNally, 1966.
Capek, M. (1968). Ernst Mach's biological theory of knowledge. *Synthese, 18*, 171–191.
Cherniak, C. (1986). *Minimal rationality*. Cambridge, MA: Bradford Books, MIT Press.
Churchland, P. S. (1986). *Neurophilosophy*. Cambridge, MA: Bradford Books, MIT Press.
Clarke, D. M. (1982). *Descartes' philosophy of science*. University Park, PA: Pennsylvania State University Press.
Cohen, R. S., & Elkana, Y. (Eds.), (1977). *Hermann von Helmholtz Epistemological writings*. Dordrecht: Reidel.
Cook, T. D., & Campbell, D. T. (1979). *Quasi-experimentation: Design and analysis for field settings*. Chicago: Rand McNally.
Craik, K. J. W. (1943). *The nature of explanation*. Cambridge: Cambridge University Press.
Crombie, A. C. (1967). The mechanistic hypothesis and the scientific study of vision. In S. Bradbury & G. L'E. Turner (Eds.), *Historical aspects of microscopy* (pp. 3–112). Cambridge: Heffer.
Dewey, J. (1929). *Experience and nature*. (2nd ed.) LaSalle: Open Court.
Evans, R. I. (1975). *Konrad Lorenz: The man and his ideas*. New York: Harcourt Brace Jovanovich.
Faust, D. (1984). *The limits of scientific reasoning*. University of Minnesota Press.
Feigl, H. (1950). Existential hypotheses: Realistic versus phenomenalistic interpretations. *Philosophy of Science, 17*, 35–62.
(1956). Some major issues and developments in the philosophy of science of logical

Copyrighted material

Hirst, R. J. (1959). *The problems of perception*. New York: Macmillan.

Holland, J. H., Holyoak, K. J., Nisbett, R. E., & Thagard, P. R. (1986). *Induction: Processes of inference, learning, and discovery*. Cambridge, MA: Bradford Books, MIT Press.

Hudson, L. (1966). *Contrary imaginations*. Harmondsworth: Penguin.

(1970). *Frames of mind*. Harmondsworth: Penguin.

Humphrey, N. (1984). *Consciousness regained*. Oxford: Oxford University Press.

Jackson, D. N., & Rushton, J. P. (Eds.). (1987). *Scientific excellence: Origins and assessment*. Newbury Park, CA: Sage.

James, W. (1880). Great men, great thoughts, and the environment. *Atlantic Monthly, 46*, 441–459.

(1980). Necessary truths and the effects of experience. The final chapter of his *Principles of Psychology* (Vol. 2, pp. 617–679). New York: Holt.

Jaynes, J. (1977). *The origin of consciousness in the breakdown of the bicameral mind*. Boston, MA: Houghton Mifflin.

Johnston, J. M., & Pennypacker, H.S. (1980). Behavioral science and scientific behavior. In their *Strategies and tactics of human behavioral research* (ch. 20). Hillsdale, NJ: Erlbaum.

Kantor, J. R. (1945, 1950). *Psychology and logic* (2 vols.). Bloomington, IN: Principia Press.

Kitchener, R. F. (1986). *Piaget's theory of knowledge: Genetic epistemology and scientific reason*. New Haven: Yale University Press.

Koehler, W. (1938). *The place of value in a world of facts*. New York: Liveright.

Kornblith, H. (1985). *Naturalizing epistemology*. Cambridge, MA: Bradford Books, MIT Press.

LaLumia, J. (1966). *The ways of reason: A critical study of the ideas of Emile Meyerson*. London: Allen & Unwin.

Lange, F. A. (1950). *The history of materialism*. New York: Humanities Press. (Original work published 1890).

Langley, P., Simon, H. A., Bradshaw, G. L., & Zytkow, J. M. (1986). *Scientific discovery: Computational explorations of the creative process*. Cambridge, MA: Bradford Books, MIT Press.

Lewin, K. (1922). *Der Begriff der Genese in physik, biologie und entwicklungsgeschichte: Eine untersuchung zur vergleichenden wissenschaftslehre*. Berlin: Springer.

(1926). Idee und Aufgabe der vergleichenden Wissenschaftslehre. *Symposion, 1*, 61–93.

Lindsay, A. D. (1934). Introduction. In I. Kant, *Critique of pure reason*. London: J. M. Dent, Everyman's Library.

Locke, J. (1975). *An essay concerning human understanding*. Oxford: Oxford University Press (Clarendon Press). (Original work published 1690).

MacCormac, E. R. (1980). Hume's embodied impressions. *The Southern Journal of Philosophy, 18*, 447–462.

Mach, E. (1896). On the part played by accident in invention and discovery. *Monist, 6*, 161–175.

(1959). *The analysis of sensations and the relation of the physical to the psychical*. New York: Dover. (Original work published 1902).

(1976). *Knowledge and error: Sketches on the psychology of enquiry*. Dordrecht: Reidel. (Original work published 1905).

Macnamara, J. (1982). *Names for things: A study of human learning*. Cambridge, MA: Bradford Books, MIT Press.

(1986). *A border dispute: The place of logic in psychology*. Cambridge, MA: Bradford Books, MIT Press.

Copyrighted material

Mandelbaum, M. (1964). *Philosophy, science, and sense perception: Historical and critical studies*. Baltimore: The Johns Hopkins University Press.
Maslow, A. H. (1948). Cognition of the particular and of the generic. *Psychological Review, 55*, 22–40.
(1969). *The psychology of science*. New York: Harper & Row. Chicago: Henry Regnery. (Original work published 1966).
McClelland, D. C. (1964) The psychodynamics of creative physical scientists. *The roots of consciousness* (pp. 146–181). Princeton, NJ: Van Nostrand.
Mehra, K. (1954). *Depersonalized identification and professional choice: A study of physicists*. Unpublished doctoral dissertation, University of Chicago.
Merleau-Ponty, M. (1962). *Phenomenology of perception*. London: Routledge & Kegan Paul.
(1963). *The structure of behavior*. Boston: Beacon Press.
Meyerson, E. (1962). *Identity & Reality*. New York: Dover. (Original work published 1908).
Michotte, A. E. (1946). *La perception de la causalité, Etudes Psychol.* (Vol. 6) Louvain: Inst. Sup. de Philosophe. (English translation: London: Methuen, 1963.)
Mischel, T. (Ed.). (1971). *Cognitive development and epistemology*. New York: Academic Press.
Mitroff, I. I. (1974). *The subjective side of science*. Amsterdam: Elsevier.
Naess, A. (1965). Science as behavior: Prospects and limitations of a behavioral metascience. In B. B. Wolman (Ed.), *Scientific psychology: Principles and approaches* (pp. 50–67). New York: Basic Books.
(1972). *The pluralist and possibilist aspect of the scientific enterprise*. Oslo: Universitetsforlaget.
Ness, A. (1936). *Erkenntnis und wissenschaftliches Verhalten*. Oslo: I Kommisjon hos J. Dybwad. (Ness equals Naess.)
Paller, B. T., & Campbell, D. T. (1988). Maxwell and van Fraassen on observability, reality, and justification. In M. L. Maxwell & C. W. Savage (Eds.), *Science, mind and psychology: Essays on Grover Maxwell's world view* (pp. 121–154). Lanham, MD: University Press of America.
Pasch, A. (1958). *Experience and the analytic: A reconsideration of empiricism*. Chicago: The University of Chicago Press.
Perry, S. E. (1966). *The human nature of science: Researchers at work in psychiatry*. New York: Free Press.
Piaget, J. (1950). *Introduction à l'épistémologie génétique, Vol. 1: La pensée mathématique; Vol. 2: La pensée physique; Vol. 3: La pensée biologique, la pensée psychologique, et la pensée sociologique*. Paris: Presses Universitaires de France.
(1971). *Biology and knowledge*. Chicago: The University of Chicago Press.
Plotkin, H. C. (Ed.). (1982). *Learning, development and culture: Essays in evolutionary epistemology*. New York: Wiley.
Poincaré, H. (1913). Mathematical creation. In his *The foundations of science* (pp. 383–394). New York: Science Press.
Polanyi, M. (1958). *Personal knowledge*. London: Routledge & Kegan Paul.
(1966). *The tacit dimension*. Garden City, NY: Doubleday.
(1967). Science and reality. *British Journal of the Philosophy of Science, 18*, 177–196.
(1969). *Knowing and being*. London: Routledge & Kegan Paul.
(1970). Why did we destroy Europe? *Studium Generale, 23*, 909–916.
Premack, D. (1986). *Gavagai: Or the future of the animal language controversy*. Cambridge, MA: Bradford Books, MIT Press.
Pribram, K. H. (1965). Proposal for a structural pragmatism: Some neuropsychol-

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ogical considerations of problems in philosophy. In B. B. Wolman (Ed.), *Scientific psychology: Principles and approaches* (pp. 426–459). New York: Basic Books.

Quine, W. V. (1969). Epistemology naturalized. In his *Ontological relativity and other essays* (pp. 90–99). New York: Columbia University Press. Reprinted in H. Kornblith, 1985.

(1974). *The roots of reference*. LaSalle, IL: Open Court.

(1975). The nature of natural knowledge. In S. Guttenplan (Ed.), *Mind and language* (pp. 67–81). Oxford: Oxford University Press (Clarendon Press).

Radnitzky, G., & Bartley, W. W. (Eds.). (1987). *Evolutionary epistemology, theory of rationality, and the sociology of knowledge*. LaSalle, IL: Open Court.

Ratliff, F. (1970). On Mach's contributions to the analysis of sensations. In R. S. Cohen & R. J. Seeger (Eds.), *Boston Studies in the Philosophy of Science, Vol. 6: Ernst Mach: Physicist and Philosopher* (pp. 23–41). Dordrecht: Reidel.

Riedl, R., & Wuketits, F. M. (Eds.). (1987). *Die Evolutionare Erkenntnistheorie*. Berlin: Paul Parey.

Ritchie, A. D. (1958). *Studies in the history and method of the sciences*. Edinburgh: University Press.

Rock, I. (1983). *The logic of perception*. Cambridge, MA: Bradford Books of MIT Press.

Rosenthal, R. (1976). *The experimenter effects in behavioral research* (enl. ed.). New York: Irvington.

Royce, J. R., Coward, H., Egan, E., Kessel, F., & Moss, L. (1978). Psychological epistemology: A critical review of the empirical literature and the theoretical issues. *Genetic Psychology Monographs, 97*, 265–353.

Royce, J. R., & Rozeboom, W. W. (Eds.). (1972). *The psychology of knowing*. New York: Gordon and Breach.

Russell, J. (1978). *The acquisition of knowledge*. New York: St. Martin's Press.

(1984). *Explaining mental life: Some philosophical issues in psychology*. New York: St. Martin's Press.

Shimony, A. (1970). Scientific inference. In R. Colodny (Ed.), *Pittsburgh Studies in the Philosophy of Science* (Vol. 4, pp. 79–172). Pittsburgh: University of Pittsburgh Press.

(1971). Perception from an evolutionary point of view. *Journal of Philosophy, 68*, 571–583.

Shimony, A., & Nails, D. (Eds.). (1987). *Naturalistic epistemology*. Dordrecht: Reidel.

Simmel, G. (1982). On a relationship between the theory of selection and epistemology. In H. C. Plotkin (Ed.), *Learning, development, and culture: Essays in evolutionary epistemology* (pp. 63–71). New York: Wiley. (Original work published 1895)

Simon, H. A. (1977). Scientific discovery and the psychology of problem solving. In H. A. Simon (Ed.), *Models of discovery* (pp. 22–39). Dordrecht: Reidel.

Singer, B. F. (1972). Towards a psychology of science. *American Psychologist, 26*, 1010–1016.

Smith, C. U. M. (1986). Friedrich Nietzsche's biological epistemics. *Journal of Social and Biological Structures, 9*, 375–388.

(1987). Clever beasts who invented knowing: Nietzche's evolutionary biology of knowledge. *Biology and Philosophy, 2*, 65–91.

Smith, L. D. (1986). *Behaviorism and logical positivism: A reassessment of the alliance*. Stanford, CA: Stanford University Press.

Sober, E. (1978). Psychologism. *Journal for the Theory of Social Behaviour, 8*, 165–191.

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Souriau, P. (1881). *Théories de l'invention*. Paris: Hachette. (Trans. E. L. Clark, privately distributed, duplicated, 1972)
Sperry, R. W. (1952). Neurology and the mind–brain problem. *American Scientist, 40*, 291–312.
(1983). *Science and moral priority: Merging mind, brain, and human values*. New York: Praeger.
Ullmo, J. (1958). *La pensée scientifique moderne*. Paris: Flammarion.
Wallraff, C. F. (1961). *Philosophical theory and psychological fact*. Tucson, AZ: University of Arizona Press.
Warren, R. M., & Warren, R. P. (1968). *Helmholtz on perception: Its physiology and development*. New York: Wiley.
Wertheimer, Max (1959). *Productive thinking*. New York: Harper & Row. (Original work published 1945)
Wertheimer, Michael (1965). Relativity and gestalt: A note on Albert Einstein and Max Wertheimer. *Journal of the History of the Behavioral Sciences, 1*, 86–87.
Whalley, E. A. (1955). *Individual life-philosophies in relation to personality and to systematic philosophy: An experimental study*. Unpublished doctoral dissertation, University of Chicago.
Wolman, B. B. (Ed.). (1965a). *Scientific psychology: Principles and approaches*. New York: Basic Books.
(1965b). Toward a science of psychological science. In B. B. Wolman (Ed.), *Scientific psychology: Principles and approaches*, (pp. 3–23). New York: Basic Books.
Wuketits, F. M. (Ed.). (1984). *Concepts and approaches in evolutionary epistemology: Towards an evolutionary theory of knowledge*. Dordrecht: Reidel.
Wyatt, D. F., & Campbell, D. T. (1951). On the liability of stereotype or hypothesis. *Journal of Abnormal and Social Psychology, 46*, 496–500.

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(1984) on modern physicists. Sociologists of science are now frequently conducting observational studies of scientists in actual laboratories (Knorr-Cetina, 1981; Knorr-Cetina & Mulkay, 1983; Latour & Woolgar, 1986). And despite their rather negative reception within the halls of metascience, psychologists have also acknowledged important contributions from the philosophy, history, and sociology of science to a broader understanding of their own discipline (Bevan, 1982; Gergen, 1985; Gholson & Barker, 1985; Krasner & Houts, 1984; Manicas & Secord, 1983; Scarr, 1985). This growing recognition of the importance of science studies among psychologists indicates that psychologists are expanding their self-understanding as scientists and in the process are participating in the interdisciplinary field of metascience.

Yet, with a few exceptions (Faust, 1984; Grover, 1981; Mahoney, 1976, 1979; Mitroff, 1974; Tweney, Doherty, & Mynatt, 1981), what is striking about these developments among psychologists is that their participation in metascience is unidirectional, describing the importance of metascience for psychology but not vice versa. Systematic efforts to relate concepts and findings from psychology to contemporary metascience are sparse. Notwithstanding that the term "psychology of science" has appeared in several publications over the past half-century (Maslow, 1966; Roe, 1961; Singer, 1971; Stevens, 1939), and that psychologists have published numerous studies of scientists (for reviews, see Eiduson & Beckman, 1973; Fisch, 1977), we still lack even a preliminary exposition of the contributions of psychology of science to philosophy, history, and sociology of science – the three established core disciplines of metascience. The psychology of science is the fourth core discipline and needs to be developed in this interdisciplinary context.

To facilitate that development, this chapter reviews a number of previous arguments offered as objections to psychology of science. The aim is to place psychology of science at a level of intellectual legitimacy and prominence comparable to that now mutually recognized among the other disciplines of metascience. Specifically, I offer some counterclaims to philosophers, historians, and sociologists of science who, because of their often explicit distaste for psychological inquiry, claim that psychological science has little to offer either conceptually or empirically for addressing major questions of metascience. Against those positions I maintain that the development of concepts and questions in metascience, beginning with philosophy of science at the turn of this century and culminating in contemporary history and sociology of science, now points to an important role for the psychology of science. In the material that follows, I will trace developments in philosophy, history, and sociology of science up to the point at which explicit questions pertinent to the psychology of science can be formulated. For each of the disciplines I will then articulate some of the relevant questions, briefly point to existing psychological literature that either has addressed or could address these ques-

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recognized that metascience might make some practical contributions to particular sciences and to policy makers concerned with funding scientific research from limited resources, Radnitzky nevertheless devoted his two volumes almost entirely to a review and discussion of various philosophies of science. Though he recognized the potential contributions of the history and sociology of science to his project, except for passing reference to the appropriation of psychoanalytic concepts among some European philosophers of science, Radnitzky apparently saw no significant role for psychology in contemporary metascience.

Radnitzky's (1968) formulation of metascience without explicit reference to psychology of science is fairly typical (though, see Ziman, 1984, for one recent exception). Similar benign neglect of psychological contributions to the science studies has also characterized previous philosophical, historical, and sociological works (e.g., Campbell, 1921; Gaston, 1978; Sarton, 1927–48). Others, however, have either surreptitiously ridiculed or explicitly dismissed the psychology of science as an undesirable flirtation with subjectivism, irrationality, and relativism – those legendary foes of the Western philosophical tradition. If the psychology of science is to make a major contribution to metascience alongside the three other core disciplines, then such intellectual predilections must be addressed and emended.

The problem then is to clarify some of the reasons why the psychology of science has yet to play any major role within metascience and to show that those reasons are no longer plausible ones for excluding it. Through analyzing some biases of the recent past, we can also trace the development of questions and concepts in contemporary metascience to the present point. I will argue that if some important questions of contemporary science studies are to be answered, a positive theoretical and empirical program for the psychology of science must be developed. The framework for this analysis is historical and hermeneutical (Gadamer, 1974), and what I intend to do is forward a kind of preliminary "geneology" (Foucault, 1977) as well as a future agenda for the psychology of science. The task is to describe the psychology of science in the context of intellectual problems arising from previous philosophy, history, and sociology of science. The problem can be posed in the following way: What are the questions of contemporary metascience that the psychology of science may fruitfully address?

What follows, then, is not a formal argument as to the logical necessity for psychology of science, but rather a selective description of how the various science studies have arrived at questions that may have psychological in addition to other types of answers. Although it may be possible to construct arguments of the former kind, what is striking about recent developments in metascience is that the very possibility of establishing logical necessity and other similar appeals to a solid, authoritative foundation for "objective truth"

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is regularly doubted (Hesse, 1980; Hubner, 1983). In this regard, any attempt to construe metascience according to some suprarationalist foundation is undermined, and what remains is a collection of different disciplines collectively known as science studies. In many respects, this erosion of foundationalist arguments within contemporary metascience marks the occasion for introducing the psychology of science as the fourth core discipline of science studies. Put another way, once we students of science learn to live with our "Cartesian anxiety" about not having indubitable foundations (Bernstein, 1983), then psychology of science is an obvious next development in science studies.

Psychology of science in past metascience

The intellectual impetus for contemporary metascience is the appearance of philosophy of science in the West, specifically the diverse approaches collected under the label of logical positivism. Whereas a thorough appraisal of the logical positivist movement is beyond the scope of this chapter, and is available from other sources (e.g., Ayer, 1959; Brown, 1977; Kockelmans, 1968; Kraft, 1953; Reichenbach, 1951; Suppe, 1974), a selective review is needed because logical positivism is the intellectual ground from which the concepts and questions of metascience have developed as alternative postpositivist philosophies of science have emerged. In turn, these postpositivist philosophies of science have prompted a focus on social history within traditional history of science (Kuhn, 1986) and inspired much of the recent explosion of research in sociology of science (Barnes, 1982). This historical development of the questions and concepts of the established disciplines of metascience provides the context in which the psychology of science has been both eschewed and promised.

Positivist philosophy of science

Although the Vienna Circle of Schlick and the Berlin school of Reichenbach were rife with their own internal controversies and subtle debates, as the chief proponents of logical positivism they zealously pursued a common goal: to rid philosophy of the excesses of metaphysical idealism by clarifying philosophical language. This philosophical project called for strict logical and empirical criteria for assigning meaning to terms and truth value to propositions. The logical criteria were those of deductive logic as supplied in the *Principia Mathematica* (Whitehead & Russell, 1913); the empirical criteria were those assumed, on a particular misreading of Wittgenstein's (1921/1961) *Tractatus*, to be established in the natural sciences. (Toulmin [1969] describes how the members of the Vienna Circle mistook Wittgenstein's quite imprecise

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claims about "atomic facts" as implying that science contained a language of facts independent from theoretical assumptions and presuppositions.)

By adopting these criteria, the positivists sought to subordinate philosophy to science, a goal that required articulation of what science is as well as how science achieves knowledge. The two questions of contemporary metascience were originally raised by the logical positivists in the context of their program for reforming philosophy.

The problem situation of the logical positivists was how to set philosophy straight by making philosophy conform to the propositional calculus of deductive logic and the meaning criteria of naive empiricist epistemology. The vehicle for accomplishing this was a retrospective analysis of developments in the physical sciences. In order to correct philosophy and set it on "The sure path of science," the positivist movement concluded that it was necessary to *justify* scientific practice philosophically. Their justification of science consisted of retrospectively demonstrating that scientific theories had undergone conceptual changes that were structurally consistent with the prescriptions of deductive logic, prescriptions that if followed promised to lead to incontrovertible "truth." In simple syllogistic form, the project could be stated as follows: The operation of logic on "facts" leads to truth; science contains "factual statements" and conforms to logic; therefore, science leads to truth. This project of logically reconstructing the history of science was initially conceived as a proximal goal on the way to the distal goal of making philosophy scientific. Historically speaking, however, that instrumental role for philosophy of science vis-à-vis philosophy was soon abandoned. Instead, the logical reconstruction of science became a terminal goal and comprised the positivist answer to the two basic questions of metascience.

To oversimplify for the moment, the logical positivists' answers to the two questions of metascience may be summarized as follows. What is science? Science is the set of theoretical and empirical propositions devised by physicists, chemists, and biologists to describe the world and explain its physical processes. Science differs from nonscience by adhering both to logical truth as supplied in the propositional calculus and to empirical truth as rendered by the verification criterion of meaning. To the second question, how does science achieve knowledge, the answer was: Science achieves knowledge by making observations of Nature and by applying the rules of deductive logic.

This project of reconstructing science according to some criteria of rationality has remained a dominant theme in some postpositivist philosophies of science and to a lesser extent in subsequent history and sociology of science. Moreover, when objections to a psychology of science have been raised, they have generally been based on the proposition that a rational reconstruction does not depend on a psychological account of scientific development. Thus,

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it is important to examine in some detail the project of rational reconstruction as launched by the positivists.

The positivist dismissal of psychology of science was tied to two major presuppositions required for their logical reconstruction: (1) acceptance of the authority of the *Principia* logic, and (2) endorsement of a naive empiricist epistemology of objectivism. Attempts to base the authority of deductive logic on "natural" habits of mind or psychological processes were rejected as "psychologism," an epithet borrowed from Husserl. Husserl (1911) had maintained that in order for the truths of logic and mathematics to command the high philosophical status of clear and certain truth (also transhistorical and universal), it was necessary that these truths be objectively true. By definition, objective truth meant truth independent of subjective experience. Consequently, any attempt to base the truths of logic and mathematics on a study of cognitive contents and/or processes undermined their privileged status and authority (Murphy, 1986; Notturno, 1985). Because psychology was identified with subjective experience, it was divorced from the pursuit of "objective truth."

Epistemologically speaking, the positivist program assumed that the relationship between human perception and the world was virtually uncomplicated, with "basic facts" being "given" in "direct observation." When taken literally, this account suggested that distortions of observation due to cognitive limitations and biases of the observer did not occur. Psychologically speaking, the scientist or at least the collective community of scientists was conceived as a perfect information processing device capable of isomorphic inputs and outputs. Moreover, the claim was made that the language of science could be neatly bifurcated into distinct and nonoverlapping sets: (1) basic statements about the world or the language of direct observation (e.g., blue, hard, hot) and (2) theoretical terms (e.g., wavelength, density, kinetic energy) which, when introduced, had to be linked to observation terms via various explicit correspondence rules (i.e., operational definition).

Under these assumptions the project of logical reconstruction consisted of demonstrating how new scientific knowledge was achieved through the accumulation of more extensive and accurate observations coupled with rigorous application of deductive logic. Scientific theories were reconstructed *as if* they were axiomatic systems like the postulates of pure geometry, their only difference being that they also had empirical content. The history of science was reconstructed *as if* it followed the rules of logic, where once the postulates are known, logic can be applied to arrive at new theorems. So Nagel (1960), for example, argued that Newton was a logical advance over Galileo because in retrospect an abstracted version of Galileo's theory concerning falling objects could be deduced as a special case of Newton's theory of universal

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reinstated a kind of idealism in the logical reconstruction of science without scientists.

But not only was the psychology of science problematic because evidence from psychological studies of scientists was deemed irrelevant to the task of justification via logic. Much of psychology itself was also judged by Carnap (1932/1959) and others (Bergmann, 1940; Feigl, 1945; Neurath 1931/1959) to be defective and in need of the purification that logical positivism offered. To complicate matters further, the positivist *prescriptions for doing philosophy* were widely taken as *prescriptions for doing science*. Philosophy *of* science became philosophy *for* science. This was nowhere more evident than in the often tacit but nevertheless dogmatic adoption of major tenets of positivist philosophy of empirically oriented sociologists (for reviews, see Bryant, 1985; Giddens, 1978) and psychologists (for reviews, see Koch, 1959–63; Toulmin & Leary, 1985; for a case study, see Morawski, 1986), who apparently overlooked the antidogmatic stance of most members of the Vienna Circle (Ayer, 1984). In this way, psychology of science became reflexively impossible because, on the one hand, psychology needed a philosophy of science to bootstrap itself into a scientific discipline (Hollinger, 1980, 1984), but on the other hand, the philosophy of science that was widely adopted as a guiding methodology had ruled on a priori grounds that psychology had no contribution to make toward answering the important questions of metascience. (Smith's [1986] recent study of leading behaviorists in the 1930s and 1940s raises doubts about the direct connection between their views and those of the logical positivists, but he also notes that by the 1950s logical empiricism was widely accepted as the standard account of science among psychologists in general).

This intellectual gerrymander on the psychology of science was also fostered through wide acceptance of a difference between what Reichenbach (1938) had termed "the context of discovery" and "the context of justification" in science studies. According to this distinction, the primary concern of philosophy of science was the context of justification where one could show via a reconstruction of the history of theory change and development that scientists' products (i.e., their theories) changed and developed in a pattern consistent with logical reasoning. For Reichenbach, this logical reasoning was inductive, and he took great pains to show, for example, that it was possible to tell a story according to which Einstein *could have* arrived at relativity theory via a chain of inductive inferences that started with Newtonian physics. In this way the logic of discovery and the logic of justification were made symmetrical, demonstrating that science is a rational and progressive enterprise steadily marching toward "truth" (Curd, 1980; Nickles, 1980a). In effect, Reichenbach (1938) argued that philosophy of science need concern itself only with the context of justification, because new discoveries could always be given a logical and rational account after the fact.

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wish to imply that these brilliant thinkers whose work led to contemporary metascience were simply narrow-minded or idiosyncratically prejudiced. Nor do I wish to imply that all of the obstacles to the development of psychology of science should be attributed to these philosophers, because with few exceptions (Smith, 1986) psychologists themselves constructed their own discipline according to prescriptions consistent with positivist philosophy for doing science. In this way, psychologists failed to see the relevance of psychology to the metascientific questions as formulated under positivistic hegemony.

In any case, the plausibility for psychology of science to contribute to metascience did not become evident until positivist philosophy of science was challenged by alternative views. Surprisingly, with some notable exceptions to be discussed below, many of the negative views of psychology that were first offered by the logical positivists persisted in postpositivist philosophies of science and to a lesser extent in the history and sociology of science which they inspired.

Postpositivist philosophies of science

Whereas it is reasonable to speak of positivist philosophy of science as if it were a unified point of view, as Putnam (1962) did in referring to "the Received View," such is not the case with postpositivist philosophies of science. These philosophies comprise a plurality of viewpoints, having in common only that they challenge certain and often different core assumptions of the positivist program. With respect to our project of tracing the development of psychology of science in the context of metascience developments, two challenges to the positivist program are important. The first concerns various disputes about the historical accuracy of using deductive logic as a heuristic for rational reconstruction and accounts of progress; that is, a challenge to the positivist answer to how science produces knowledge. The second challenge concerns the acceptability of objectivist epistemology and is a challenge to the positivist answer to what makes science unique from an epistemological standpoint. The former challenge paved the way for alternative sociological and psychological accounts of theory change and development within the history and sociology of science; the latter challenge opened the door for psychological investigation of knowledge acquisition as a process carried out by human knowers.

Challenges to logical reconstruction. Although Kuhn's (1962) *The Structure of Scientific Revolutions* had some intellectually compatible predecessors (Butterfield, 1957; Fleck, 1935/1979; Hanson, 1958; Polanyi, 1958; Toulmin, 1953), there is little doubt that owing to its widespread positive reception in the

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years following publication, this monograph became a watershed for metascience in general and for accounts of theory change and development in particular. In large measure, postpositivist philosophy of science consists of interpretations of and reactions to Kuhn's two basic objections to core assumptions of logical reconstruction. Kuhn (1962) objected to the positivist reconstruction in both its descriptive and prescriptive forms. Kuhn's descriptive counterclaim can be summarized as follows: Major theory changes in science have *not* conformed to the requirements of logic but instead reflect changes of a sociological and psychological nature regarding the guiding assumptions (metaphysical beliefs) of communities of scholars. His objection to the prescriptive form of logical reconstruction was: Progress in science *cannot* be established by application of some objective, universal standard. Instead, theory change and theory development are a function of the changing and evolving social consensus of scientific communities that are guided by sets of values applied in differing degrees and combinations at different periods. As to the uniqueness of science, Kuhn (1962) answered that unlike other cultural practices the sciences are able to sustain relative stability of methods and procedures by adhering to a common set of assumptions and practices over long periods of time. This is the conservative feature of science that Kuhn labeled "normal science." But he also identified periods when these common beliefs and practices were replaced by others, and these he labeled "revolutionary science." Thus, to the second question about how science achieves knowledge, Kuhn (1962) offered the answer that knowledge was achieved by consensus of the scientific community, that theory change and theory development were fundamentally social and psychological processes, and that the matter of truth could not be decided independent of the particular historically situated community which laid claim to it.

Both of Kuhn's (1962) objections and subsequent reactions to them have influenced thinking about psychology of science in postpositivist philosophy of science. Whereas Kuhn was generally receptive to the use of psychological concepts in studying science, his critics often resurrected the old positivist arguments against psychology of science.

Unlike the advocates of logical reconstruction, Kuhn (1962) drew upon psychological research (specifically, the work of Piaget, the Gestalt psychologists, and studies of contextual influences on perception) to find concepts that might account for the logical discontinuities he perceived in the history of physical sciences. In describing the impact on his early work of a year spent among distinguished social scientists at the Center for Advanced Studies in the Behavioral Sciences, he noted the following:

> Particularly, I was struck by the number and extent of overt disagreements between social scientists about the nature of legitimate scientific problems and methods. Both history and acquaintance made me doubt that practitioners of the natural sciences

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Scientific genius

A psychology of science

Dean Keith Simonton

In this book, Dean Keith Simonton develops a theory of scientific genius. His starting point is Donald Campbell's "blind variation and selective retention" model of creativity, which he elaborates into his own "chance-configuration" theory. He then uses this to account for key aspects of path-breaking science.

He considers the mental processes and behaviors behind the creative act, including intuition, incubation, and serendipity. He discusses the cognitive and motivational styles of great scientists in terms of a personality typology. He examines the causes and consequences of exceptional productivity: individual differences in lifetime output, the functional relation between age and achievement, the probabilistic connection between quantity and quality, and such issues as the Ortega hypothesis, the Yuasa phenomenon, and Planck's principle. He reviews the developmental antecedents of distinguished scientific work – family background, education, role models, marginality, and the zeitgeist – with respect to their complex impact on the growth of creative potential. The phenomenon of multiple discovery (where two or more independent investigators chance upon the same finding) is shown to provide some of the best empirical evidence on behalf of the theoretical argument.

A concluding chapter outlines the broader implications of the theory for the measurement and encouragement of genius in science, and places it in the context of the alternative metasciences – the philosophy, sociology, and psychology of science.

Simonton's provocative ideas are a major impetus to the emergence of a true psychology of science and will interest a broad audience.

ISBN 0-521-35410-2

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Cambridge University Press