Constructing the subject

Historical origins of psychological research

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CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge CB2 1RP, United Kingdom

CAMBRIDGE UNIVERSITY PRESS
The Edinburgh Building, Cambridge CB2 2RU, UK http: //www.cup.cam.ac.uk
40 West 20th Street, New York, NY 10011-4211, USA http: //www.cup.org
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

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First published 1990
First paperback edition 1994
Reprinted 1998

Typeset in Times

A catalogue record for this book is available from the British Library

Library of Congress Cataloguing-in-Publication Data is available

ISBN 0-521-46785-3 paperback

Transferred to digital printing 2002

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1

Introduction

What this book is about

What exactly constitutes a field like scientific psychology? Is it constituted by its most innovative and influential contributors; by the scientific findings that it has produced; by the theories it has elaborated; by its concepts, techniques, or professional associations? Obviously, all this and more goes into the making of a field, but most of us would probably see some of these components as playing a more essential role than others. Even if we refuse to commit ourselves explicitly we are likely to imply that certain components define the field more effectively than others by the way we organize our knowledge. For example, in the systematic presentation of information derived from the field of psychology or one of its parts, the material is most commonly organized in terms of prominent contributors, important findings, or influential theories. A perhaps unintended message of such communications is that psychology *is* its theories, *is* its findings, or *is* its individual contributors.

The way in which we organize a field will determine the way we organize its history. If we see the field of psychology as essentially an aggregate of individual contributors, we are likely to treat the history of the subject in terms of a succession of prominent figures. If psychology is its theories or its findings, then its history will become a history of psychological theories or psychological findings. Our organization of the history of the field will also serve as a subtle justification of the way we have characterized the field in the present.

Most psychologists have been taught to characterize their own scientific activity in terms of a framework that is derived from nineteenth-century physical science. They see themselves as individual investigators who seek to accumulate facts about some aspect of nature by the use of appropriate

hypotheses and techniques. When they describe the historical development of their field they are apt to do so in much the same terms, representing it as a succession of individual contributors who accumulated "findings" on the basis of progressively refined hypotheses and increasingly sophisticated instrumentation. Of course, the sad truth sometimes forces a departure from this framework, but the framework continues to operate all the same.

To put it very simply, this book is about some crucial elements that are missing in this framework. What is missing is the recognition of the socially constructed nature of psychological knowledge. The received view is based on a model of science that is reminiscent of the tale of Sleeping Beauty: The objects with which psychological science deals are all present in nature fully formed, and all that the prince-investigator has to do is to find them and awaken them with the magic kiss of his research. But in truth scientific psychology does not deal in natural objects. It deals in test scores, rating scales, response distributions, serial lists, and innumerable other items that the investigator does not just find but constructs with great care. Whatever guesses are made about the natural world are totally constrained by this world of artifacts.[1] The same holds true for the immediate human sources of the psychologist's information. The psychologist's interaction with such sources takes place within a well-regulated social role system, and such roles as that of experimental subject or of client in therapy are the direct result of the psychologist's intervention.

In talking about a field like scientific psychology we are talking about a domain of constructions. The sentences in its textbooks, the tables and figures in its research reports, the patterned activity in its laboratories, these are first of all products of human construction, whatever else they may be as well. Although this seems quite obvious, certain implications are usually evaded. If the world of scientific psychology is a constructed world, then the key to understanding its historical development would seem to lie in those constructive activities that produced it. But this insight has not guided many historical studies. In the past the effects of a naive empiricism may have assigned an essentially passive role to investigators, as though they merely had to observe or register what went on outside them. But this is no longer a popular position.

In more recent times the well-known contrast between "context of discovery" and "context of justification" gave expression to a pervasive tendency to relegate the necessary subjective component in scientific activity to a mysterious underworld that was not susceptible to logical analysis.[2] So there grew up a strange duality in the historiography of fields like psychology, where one kind of historical review would restrict itself to the logical succession of hypotheses and evidence while the second kind would describe the personal lives of those individuals who were the authors of the hypotheses and the producers of the evidence. Whereas the first kind

of review implied that scientific progress was an affair governed by purely rational considerations, the second kind picked up the irrational component and located it in the personal quirks and accidental events that characterized the lives of historically important contributors. The two approaches were the product of a tacit consensus about the fundamental split between two components of scientific activity: a rational transindividual component, of primary importance in the context of justification; and an irrational individual component, important in the context of discovery.

What is missing from this account is any appreciation of the fundamentally social nature of scientific activity.[3] What unites individual contributors is not simply their common possession of the same logical faculties and their common confrontation of the same external nature. Their social bonds are a lot more complex than that. They are related by ties of loyalty, power, and conflict. They share interests as well as logical faculties, and they occupy positions in wider social structures. In this social world of science the neat distinction between rational and irrational components crumbles. The fundamental issue in research is not whether the lone investigator can verify his hypotheses in the privacy of his laboratory but whether he can establish his contribution as part of the canon of scientific knowledge in his field.[4] In other words, the issue is one of consensus, and consensus is not entirely a matter of logic. It involves prior agreements about what is to count as admissible evidence and shared commitments to certain goals. It involves vested interests and unexamined biases.

Once we recognize the essentially social nature of scientific activity, we are compelled to see both the "context of discovery" and the "context of justification" in a different light. The context of discovery is in fact a context of construction, of theories, of instruments, and also of evidence. For the data that appear in the pages of psychological journals are no less a product of the constructive ingenuity of their authors than are the instruments and the theoretical hypotheses; they are not raw facts of nature but elaborately constructed artifacts.[5] However, these artifacts are constructed according to explicit rational schemes accepted within a certain community of investigators.[6] That is why there is a real history of psychological research practice that is neither a series of narratives about famous psychologists nor an enumeration of their successive "findings." This history involves the changes that have taken place in the constructive schemes that psychologists have used in the production of those objects that form the accepted content of their discipline.

But what is meant by these "constructive schemes"? In the first place, such schemes are not just cognitive frameworks for the *interpretations* of empirical data but involve practical rules for the *production* of such data. It is true that general concepts and theories also function as constructive schemes that give a particular meaning to the objects in which the discipline deals. But such interpretive schemes are found in purely speculative psy-

chologies as well as in empirical psychology. Psychology underwent a fundamental change toward the end of the nineteenth century when its practitioners became decisively committed to specific practical methods of data production. The application of these methods became the special characteristic of the field and distinguished it from everyday psychology as well as from its own intellectual predecessors.[7] By the use of these practical methods, modern psychology created a new world of psychological objects that increasingly defined the field and to which any purely theoretical developments were forced to accommodate.

This emphasis on practical constructive schemes does not however lead simply to a history of psychological methods in the conventional sense. The difference lies in the way one conceives of method. Conventionally, empirical methods in psychology are conceived simply as tools for the achievement of certain technical goals. Thus, they need be evaluated only in terms of a logic of means and ends, where the ends are taken as existing independently of and prior to the means. For instance, we may choose the measurement of a particular psychological quality, such as intelligence, as an end, and then trace the effectiveness of various instruments for achieving this end. Or we may take it as an end that we want to assess the simultaneous contribution of a number of factors to psychological effects and then attribute the growing popularity of certain statistical techniques to their superior effectiveness in achieving this preexisting goal. It is possible to trace the history of psychological methodology in terms of such a purely instrumentalist framework, but that is not what is being attempted here.

The concept of investigative practice is wider than the concept of methodology. As conventionally understood, the latter involves an abstraction of certain rational and technical aspects of investigative practice from all the other aspects. The practice of investigators is treated as though it consisted only of logical and technical operations performed by independent individual investigators on bits of the natural world. Left out is the fact that investigative practice is very much a social practice, in the sense that the individual investigator acts within a framework determined by the potential consumers of the products of his or her research and by the traditions of acceptable practice prevailing in the field.[8] Moreover, the goals and knowledge interests that guide this practice depend on the social context within which investigators work. Finally, in psychological research there is the additional important consideration that the investigator is not the only human participant whose actions are necessary for the practice of investigation to proceed. Unless the psychologist works with animals, he or she will also require the collaboration of human sources of data without whose contribution there would of course be nothing to report.

The notion of investigative practice then involves the social dimensions of research activity as much as the logical ones. The latter are recognized

as being embedded in a social matrix that includes such factors as the pattern of social relations among investigators and their subjects, the norms of appropriate practice in the relevant research community, the kinds of knowledge interests that prevail at different times and places, and the relations of the research community with the broader social context that sustains it.

As long as we limit our conception of psychological research practice to its purely rational aspects, we will be inclined to think of the history of that practice solely in terms of technical progress. The norms of good scientific practice will be seen as belonging to an unchangeable transhistorical realm where eternal rational principles rule. All that is left for worldly history is the narrative of how clever investigators came to apply these eternal principles with ever greater concrete effectiveness to larger and larger bodies of knowledge. However, if we refuse to perform this rationalist reduction, we will find that in the history of psychological research practice the most significant changes were changes in the ends rather than improvements in the means.

Although methodological rationalism has adherents outside psychology, the doctrine has long played a particularly important role for this discipline. In the more established fields of natural science, where elaborate deductive procedures are part and parcel of theoretical discourse, theory as much as method was regarded as the repository of scientific reason. But in psychology theoretical constructions have seldom been marvels of logical sophistication, and pessimism about the likelihood of reaching rational consensus on the basis of theory has long been widespread. It is generally accepted that there will be controversy about theoretical fundamentals and that personal, cultural, and historical factors play important roles in the elaboration and acceptance of psychological theory. But this state of affairs is hardly compatible with the claims by the discipline for the objectivity of its insights into human behavior. Therefore, such claims in psychology have depended almost completely on the rational virtues of its methodology. It was only because of the logical–technical features of its investigative practice that psychology could give some plausibility to its claims for scientific status.

Investigative practice therefore constitutes an area of considerable anxiety within the discipline of psychology.[9] Concern with questions of methodological orthodoxy often takes the place of concern about theoretical orthodoxy when research or its results are discussed and evaluated. These preoccupations with the purity of method frequently deteriorate to a kind of method fetishism or "methodolatry."[10] From this point of view there may be something distinctly subversive about the suggestion that the sphere of methodology is not a realm of pure reason but an area of human social activity governed by mundane circumstances like any other social activity.

Nevertheless, the consequences of this suggestion should be explored, for not to do so exposes one to all the risks entailed by a naive and self-deluded style of scientific practice.

Once we restore the abstraction of a purely rational methodology to the broader context of investigative practice, it becomes possible to see it as the primary medium through which social forces have shaped the production of the objects of scientific psychology. But this general pattern can only be fully appreciated when studied in its historical development. Before we can undertake this task, however, we require some clarification of the sociological and historiographic aspects of the topic.

The social generation of scientific knowledge

In recent years references to science as a social activity have become commonplace, and there is a rapidly growing field devoted to the social study of science. Whereas the earlier sociology of science was more a sociology of scientists and exempted the content of science from its purview, more recent developments in this field have pointed in the direction of a genuine sociology of scientific knowledge.[11] In other words, scientific knowledge has increasingly been seen as a product of certain quite specific social processes, and many of its features have been studied in terms of their dependence on these processes. With a few notable exceptions,[12] psychological knowledge has been largely exempt from this process. This may be due to the existence of two large bodies of opinion: For one the social dependence of psychological knowledge is too obvious to be worth studying, whereas for the other the whole topic is too threatening to psychology's hard-won scientific status to be taken seriously. The fact is, however, that psychology's social practices of investigation are too tied up with its claims to scientific status to be ignored in that context. Still, our understanding of the social determination of psychological knowledge must remain rather abstract until we understand the crucial mediating role played by investigative practice.

It may be helpful to think of the social contexts of investigative practice in terms of three concentric circles of varying radius. (Figure 1.1) The innermost circle represents the immediate social situation in which the information that will later be transformed into research data and scientific psychological knowledge is generated. The next circle represents the research community by which the product of the investigation must be accepted if it is to count as scientific psychological knowledge. The outer circle stands for the wider social context within which the research community is embedded – the sources of research support, professional institutions, potential external consumers of knowledge products and skills, representatives of prestigious disciplines, and so forth.

The image of concentric circles is of course meant to represent the idea

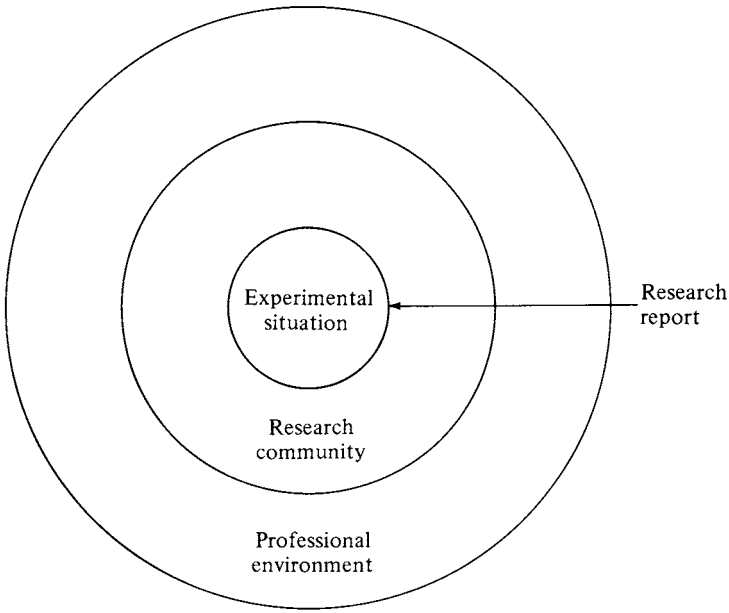

Figure 1.1. Social contexts of investigative practice

that both the immediate investigative situation and the research community are to be regarded as embedded in social relationships that extend beyond them. Similarly, a truly comprehensive account would have to add further circles beyond the three discussed here to represent even broader cultural, political, and economic contexts. But because our concern is specifically with investigative practice, we will limit ourselves to the inner three circles, considering in turn some of the more important questions that arise within each.

At the innermost level we have to deal with what are essentially face-to-face situations. Before the whole process of producing material that counts as scientific psychological knowledge can begin, some individuals have to get together to generate the information that forms the starting point for this process. In coming together for this purpose they set up certain social situations with a distinct character – the situations of mental testing, psychological experimentation, or interviewing, to mention the most obvious ones. In the case of the first two, which are by far the most important ones in psychology, the social interaction of the participants is partly mediated by various kinds of hardware and software, mental-test materials, and laboratory apparatus in particular. This has permitted investigators to overlook the social character of all these investigative situations and to pretend that their subjects reacted only to the dead materials presented to them and not to those who did the presenting. Nevertheless,

it is an elementary truth that the gathering of psychologically relevant information requires the active social participation of individuals who will act as the source of this information. Whatever else it may be, the psychological experiment or test is therefore a social situation and as such it must share the characteristics that are found in all social situations.

Of course, there are marginal cases where the social status of the investigative situation is less straightforward, such as the use of documentary sources or the case where individuals experiment on themselves. But neither of these has accounted for more than a tiny fraction of psychological research over the past century. Their rarity only serves to emphasize the inherently social nature of most psychological investigations.

Until relatively recently the total blindness of psychological investigators to the social features of their investigative situations constituted one of the most characteristic features of their research practice.[13] However, the recognition of so-called experimenter expectancy effects and demand characteristics has now become commonplace, and there is a large empirically oriented literature on "the social psychology of the psychological experiment."[14] Although this undoubtedly represents an advance on psychologists' traditional naiveté in regard to such issues, the limitations of this literature are more remarkable than its achievements from the viewpoint of furthering an understanding of the social process of generating psychological knowledge.

In the first place, much of the literature on social effects in psychological experiments is devoted simply to demonstrating the mere existence of such effects without being directed at furthering an understanding of the social processes involved in psychological investigation.[15] Such conceptualization of social process as does occur in this work is usually confined to social *psychological* rather than sociological categories and therefore does not address the fact that the responses of individuals to experimental situations take place in the context of a miniature social system in which both experimenters and their subjects participate. Moreover, there is a pervasive tendency to relegate the social aspects of psychological experimentation to the status of "artifacts" or, in other words, disturbances of the process of research that do not belong to its essential nature.[16] Thus the rational abstraction of a purely logical, asocial, and ahistorical research process remains inviolate and apparently beyond the reach of empirical correction. Accordingly, in this research program the preferred method for studying the social aspects of experimentation is the experimental method itself, an enterprise whose results are difficult to interpret without getting mired in an infinite regress.[17]

Such problems mostly derive from a single source, namely, the pretense that psychological experiments are not in principle different from experiments in the natural sciences.[18] Because in the latter case experimenters are able to treat whatever they are investigating purely as a natural object,

it is believed that human data sources must be treated in the same way if psychology is to function as a proper experimental science.[19] But human subjects in psychological experiments are in fact unable to behave simply as natural objects. Even if they try to do so, which depends entirely on their appraisal of the social situation they are in, they negate this fictional goal in the very act of trying to reach it, because such efforts represent an exercise of their social agency.[20] Psychological experiments are therefore different in principle from experiments in physics because the experimenter and the human data source must necessarily be engaged in a social relationship. This is no "artifact" but one of the essential preconditions of having a viable experimental situation. Consequently, experimental results in psychology will always be codetermined by the social relations between experimenters and subjects. The precise significance of this factor may be variable, but that it is always present is not a matter open to doubt.

Of course, the old notion that the knowledge produced by experiments depends only on the interaction of individual experimenters with the materials of nature is false even in the case of the true natural sciences. We now understand a great deal about how the kind of knowledge gain that results from an experiment depends quite crucially on the interaction of scientists with each other.[21] But this is a level of social influence that psychological research shares with research in other experimental sciences. Beyond this there is however a level of essential social interaction peculiar to psychology experiments – the interaction of experimenters with the human sources of their data. It is to be hoped that after a century of lusty life, experimental psychology now feels sufficiently secure to face the fact that there may be essential differences between itself and the older experimental sciences. The crude doctrine that relegated everything not cast in the image of experimental physics to the category of the "mystical" is being increasingly treated as a historical curiosity.

Finding out more about the social relationships that exist between experimenters and their human subjects is surely better than pretending they are not there or are of no importance. Now, there are different ways of pursuing such a goal. The experimental way, which has already been alluded to, tends to be limited to investigating certain *social psychological* factors that operate in psychological experiments.[22] But psychological factors like the experimenter's expectancies or the subject's "evaluation apprehension" operate within a certain social framework that has to be taken for granted in such studies. This framework is provided by the traditions and conventions of psychological experimentation that have developed over the years and are now well understood by all experimenters and by most of their subjects. In those societies in which it is practiced on any scale, the psychological experiment has become a social institution recognized by most people with a certain level of education. As in all social institutions the interaction of the participants is constrained by institutional

patterns that prescribe what is expected and permitted for each participant. The successful conduct of psychological experiments depends on the willingness of all participants to abide by these rules, and that of course can only happen if they have the appropriate level of previous knowledge to understand the rules. We know that in cross-cultural research this cannot always be taken for granted.

But before we discuss this as a third-world problem we might remember that hardly more than a century ago the institution of the psychological experiment was as unknown everywhere as it might now be in parts of the third world. It is legitimate to ask how this social construction ever arose in the first place and how it ended up taking the form we are so ready to take for granted today. Like all social institutions, the psychological experiment not only has a certain social structure that can be analyzed but a history that can be traced. Ultimately, this institution is part of the history of those societies that produced it and can be expected to bear the marks of its origins. Thus, if we wish to improve our understanding of the accepted social framework within which individual participants in psychological experiments have to function, we will have to adopt a historical rather than an experimental approach. For neither experimenters nor their subjects enter the investigative situation as social blanks to be programmed in an arbitrary manner. Both are the products of a distinct historical development that has left a heavy sediment of blind faith and unquestioned tradition. It is precisely this historical development that we will attempt to trace in the present volume.

Although studies of the social framework for the interaction of investigators and their subjects are virtually nonexistent, we move onto relatively more familiar ground when we consider the two outer circles that determine the social practice of research. In their interaction with each other and with a wider social context, psychological investigators face problems that are not different in principle from the social problems faced by other groups of scientists. From this perspective we recognize the social practice of scientific psychological investigation as involving a certain organization of work for the purpose of generating a certain kind of product that is identified as psychological knowledge. This identification of the product is accomplished by what amounts to a system of certification on the part of recognized authorities working within an established institutional framework.[23] The kind of knowledge the scientific worker is after is not private but public knowledge.[24] That means it cannot be achieved without acceptance by some kind of public. If the knowledge is to bear the stamp of science, the public by whom it is accepted must be a scientific public.

Psychological research is not something that is carried on by individual investigators working on their own account. Nor are its social aspects limited to the interaction between investigators and the human sources of their data. In designing their experiments and in preparing the cognitive

yield of their investigations for publication, researchers must be constantly mindful of the acceptability of their product to a particular public. That acceptability depends on whether the product measures up to currently prevailing standards of what constitutes scientific psychological knowledge. The fact that these standards have been thoroughly internalized by most investigators in the course of their professional socialization does not in any way affect the public status of these standards. What this means is that the scientific community is intimately involved in the social practice of scientific investigation.

As in the case of experimenter–subject interactions, there are two different levels on which the analysis of this aspect of social practice can be pursued. There is the social psychological level where one would be asking questions about how individual investigators adapt to prevailing standards of scientific knowledge and how they negotiate different interpretations of these standards. But at this level the existence of the standards themselves would have to be taken for granted. The systemic or historical level of analysis would treat precisely this existence of current standards as problematic and would attempt to trace their historical origin and development. This level of analysis will be pursued here.

Some historiographic considerations

The fact that currently dominant standards of what is to count as scientific psychological knowledge have not always existed and were not always dominant is one that is universally recognized. Every well-trained research worker in the field knows that the very idea of a scientific psychology only arose in relatively recent times and that its practical enactment belongs to an even more recent period. It is also well known that subsequent to the founding of experimental psychology there was a major controversy about the standards of what was to constitute scientific psychological knowledge. This controversy is usually represented as one between introspectionists and behaviorists, though, as we will see later, the story is more complex than that. What is worthy of note, however, is that an arch introspectionist like E. B. Titchener always justified his investigative practice in the name of *science* and denigrated the practice of his opponents as being not science but technology.[25] The more naive textbook treatments describe this controversy simply as a conflict between truth and error, the implication being that there can be only one conceivable true version of what constitutes scientific psychological knowledge. But if we rid ourselves of the irrational presumption that truth will necessarily vanquish error, and that therefore whatever achieves historical dominance must be truer,[26] this controversy becomes one between rival conceptions of what was to count as scientific psychological knowledge.

In studying historical changes in prevailing conceptions of psychological

knowledge, the question of the relative truth of these conceptions is not relevant. First of all, we are dealing with standards, criteria, and ideals rather than with matters of fact, and so the attribute of truth is simply an inappropriate one. The introspectionists' choice of a different kind of knowledge was a matter of preference, not a matter of error. Whether what they did was scientific depends on one's definition of science. If their definition turns out to be different from that of the behaviorists, this again is a question of preference, which can only be seen as a matter of being right or wrong if a particular definition of science is accepted as the only true one in some absolute, ahistorical sense.[27]

But such ahistorical standards usually turn out to be the particular standards that happen to be currently popular. Elevating them to the status of final criteria therefore involves the patent illusion that historical development has reached a kind of culmination in the present and will now stop. The conservative implications of this position need no elaboration. When we attempt to trace historical changes in the social practice of investigation, we have to put questions of truth on one side. For here we are not dealing merely with technical means whose logical and practical adequacy can be assessed in terms of some accepted goal but with the goals themselves. Particular scientific communities are characterized by their commitment to certain scientific goals and this commitment determines the nature of their members' investigative practice.

What is certainly relevant in this context are the reasons for existing commitments to certain ideals of scientific practice and the reasons for changes in these commitments. These reasons are to be found in the common historical situation faced by members of a particular scientific community. Such communities never exist in a social vacuum, of course, but find themselves under the necessity of adopting positions in relation to other groups of investigators, to those who control the material resources for research, and to the general lay public. There may be other groups of investigators who are particularly well established and influential and who serve as models of successful practice. Those who ultimately control social resources must be persuaded to divert some of them to particular groups of investigators rather than to other purposes. At all times a newly emerging discipline like scientific psychology had to be careful to distinguish its cognitive product from the everyday knowledge of lay publics and from the rival claims of other disciplines. The requirements of potential consumers of a discipline's cognitive products will also pull their weight. All these and other similar factors help to shape the kinds of knowledge goals that prevail in a given field at a given time and therefore determine the dominant patterns of investigative practice.

This level of social determination can be thought of as constituting the outermost of the three concentric rings that were previously used to represent the multiple layers of the social practice of investigation. The in-