

Psychological Objects, Practice, and History (1993)

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Abstract: The theories of modern psychology always appear as components of complex formations that also have two other components, namely, specific empirical domains and sets of practices employed in the construction of such domains and of the corresponding theories. These formations can only be fully understood through historical analysis, for they are historical products. Their content comprises "psychological objects", which are the things psychologists take themselves to be investigating and theorizing about. Such psychological objects are not to be confused with natural objects, for they are crucially shaped by the theoretical constructive activity and by the practical intervention of psychological communities. The historical situation of these communities influences their construction of psychological objects in that it provides the criteria of legitimacy by which specific constructive activities and their products are judged. This does not mean that psychological objects can be reduced to the status of "nothing but" socio-historical constructions, though it does mean that the categories with which the discipline of Psychology works can never be accepted as "natural kinds" and that its research practices lose their supramundane status.

1. Against Abstractionism

Once upon a time people were very confident that they knew exactly what they meant when they spoke of psychological theory. Psychological theories were sets of theoretical propositions expressed in statements that were quite different from empirical statements and from directions for action. The reduction of theories to sentences made it particularly easy to make rigid distinctions between what was theoretical and what was not (Mandler & Kessen, 1959). It also gave expression to the disembodiment of theory. Theories existed essentially as written statements, and thus could be discussed in isolation from any real life context.

Alas, we have long lost our theoretical innocence and are no longer comfortable with this kind of abstractionism. On the one hand, we have gained some appreciation of the immense difficulties that stand in the way of any hard and fast distinction between the theoretical and the empirical, and on the other hand, we have become much more interested in the pragmatic or use aspect of theories (Hesse, 1980; Manicas, 1987; Putnam, 1981).

Of course, it is still possible to discuss theories in the abstract, as long as we are clear about the distinction between the objects of such discussion and the world in which theories normally are put to use. Abstracted theories are a construction of metatheorists; they exist only in a Popperian "third world" of pure forms (Popper, 1972) whose relationship to other worlds is by no means clear. Now, while there may be psychological theories that, one suspects, were invented primarily so they could be featured in textbooks, most of us would regard this as a perversion.

For the real purpose of theories, surely, is not to provide grist for the metatheoretical mill, but to help in the explanation of events and to guide action.

In the world of psychological research that gives rise to them, psychological theories are coordinated to specific empirical domains which they are supposed to explain in some way. Although psychological theories often make claims to generality beyond specific empirical domains, they would not be taken seriously if they had not first established some plausibility in the context of a particular array of empirical results. The more general claims of Gestalt theory, of neo-behaviorist theories, of psychoanalytic theory, would have counted for little if each had not been able to point to an array of empirical facts that provided specific illustrations of these claims; facts, moreover, that generally would not have existed without the theoretical orientation in question.

There was, after all, a major change in the way in which the game of Psychology was played before and after 1879 (or thereabouts). Earlier, it was quite acceptable to make theoretical claims that relied only on everyday experience to give them a concrete meaning. But that did not help any would-be psychologist very much in the twentieth century. To establish its credentials as a serious candidate, a modern psychological theory must be able to point to some empirical domain in which it seems to work particularly well, or to some practical results which would not have been obtained without it.

When we try to put the relationship between the theoretical and the empirical domain in historical perspective we need to avoid two opposed positions that are both equally mistaken. Among those who devote their lives to empirical research there is a widespread and understandable sentiment that theories formulated before psychology became an experimental science need not be taken seriously. This is often expressed in a categorical distinction between 'theory' and 'speculation', so that theories from the pre-experimental period need not even be recognized as theories but can be dismissed as speculation.

Of course, the naive empiricism on which this point of view relies is very difficult to defend if challenged, but that is no reason to fall into the opposite error of analyzing theoretical positions as though it did not matter fundamentally whether they had empirical and practical correlates or not. Some such belief seems to be implied in attempts to analyze the history of psychology from Aristotle to the present in terms of timeless theoretical "prescriptions" (Watson, 1967), unchanging basic polarities, and so on. Theoretical contests did move to a different arena when the subordination of theoretical discourse to empirical research became the order of the day. But how are we to understand this change? Are we to understand it simply as a transition from mere speculation to a disciplined respect for reality? If we find the monumental assumptions of naive empiricism too hard to swallow, this will not be a viable perspective. The alternative is to look more closely at what can be established historically.

What can be established is that there was a change in the social context within which theoretical claims had to be justified. In the nineteenth century it was still possible for individuals who had no relevant professional or academic affiliations whatever to make theoretical contributions to psychology that were taken every bit as seriously as the contributions of those who had such affiliations. It did not occur even to those who disagreed with the psychological ideas of John

Stuart Mill or Herbert Spencer, for example, to reject them on the grounds that these men were mere amateurs whose competence in research had not been certified by an appropriate institution or professional community. Their theories were discussed in the same breath as those of men such as Alexander Bain who did happen to have academic positions. Such affiliations were simply not relevant in establishing theoretical credibility. In other words, the distinction between experts and laymen lacked the rigid institutional basis that it was soon to acquire.

It is true that for most of the nineteenth century this distinction was more marked in Germany than in Britain or France. But this was only because many of the institutional features that were soon to become the norm everywhere first ripened in nineteenth century Germany. In this respect psychology was simply part of a ubiquitous trend. What is of general interest here is the change in the manner of justifying theoretical claims that these social developments entailed. As long as the differentiation of expert and lay publics is poorly developed it is sufficient to ground one's theoretical claims in the beliefs, assumptions, and experiences that are common to those who share a particular cultural tradition. In the case of psychology this meant relating one's contributions to the conventions and questions of an ongoing philosophical discourse, that of British empiricism, for example. Through this medium psychological ideas could be made directly accessible to a relatively broad public without having to be filtered by any elaborate institutional safeguards erected by an organized group of professionals.

Those days are gone forever. Modern psychology is an affair for experts, and those experts owe their status in large measure to their monopoly over the production of precisely those empirical domains which are considered essential for the proper assessment of psychological theories. Such theories are produced by and for members of the expert group in the first instance and only affect the lay public indirectly. There is a discourse of experts in which psychological ideas must find a place, and that discourse is governed by the norm of empirical relevance. Ideas which lack empirical relevance (as defined by the expert group) will not be permitted to enter expert discourse and will at best survive only in folk psychology.

The relationship between theory and data is reciprocal, a fact that needs some emphasis, because so often only the explanation of data by theory is mentioned. But conversely, it is the empirical co-ordinates which bring the dry bones of theory to life. It is easy to forget this, because we are generally familiar with the empirical co-ordinates of psychological theories and take them for granted. But what sense would we make of stimulus-response theory without some knowledge of animal learning experiments, what sense of field theory without perceptual demonstrations, what sense of Freudian theory without any illustrative dreams or case histories?

Making sense of theory gets us involved in the content of empirical data. But data, as we know only too well, are not raw givens - they are symbolic constructions that are reproduced in the pages of journals and text books. Like all such constructions they have form as well as content; they are arranged in the form of statistical tables, for example, or in the form of graphs, or in the narrative form of a case history. Right away we can see that empirical domains may differ, not only in their content, but also in their form. And that opens up new possibilities for the interrelationship of theory and empirical data in psychology. If theories have to be co-ordinated to empirical domains to be taken seriously, the structure of those domains becomes relevant for the work of theory construction. If my theory has to justify itself by its ability to explain

statistical relationships among measurable variables, for example, its statements will have to be cast in a mould determined by this task. But if, on the other hand, the theoretical task lies in making sense of an unfolding case history, a different type of theoretical formulation would do the job much more effectively.

If empirical domains are never just jumbles of independent atomic facts but are always very carefully arranged structures, that implies that there must be some rules, explicit or implicit, for the erection of such structures. Such rules cover the two stages through which the construction of an array of empirical data typically proceeds. In the first stage investigators procure subjects for their research and then put them in a specially structured situation in order to obtain a product called 'raw data'. In the second stage these raw data are then treated according to various rules in order to produce the data that appear in research publications. Of course, rules operate also at the first stage and regulate the selection of subjects as well as the conduct of all participants in the research situation, investigators included (Danziger, 1985b).

It is necessary to discard the old doctrine of 'epistemic individualism', according to which scientific knowledge is a product of an interaction between investigator and nature. We know quite well that it is not an isolated investigator who confronts nature but some group of investigators. Moreover, in psychological experiments on human subjects, the group involved in the production of knowledge consists not just of investigators but also of those who are the source of raw data. In the research process, knowledge generation is accomplished collectively and not individually. Like all social activity, this collective enterprise is governed by definite rules as well as by the interests of those who participate in it. These social conditions will be reflected in the form of the product (Whitley, 1984). Psychological investigators and subjects whose interests differ, and whose activity in the research situation is governed by different norms and traditions, will generate different kinds of knowledge products. Because of the intimate link between theories and empirical domains investigators with divergent interests and activity norms will also produce different theories.

This embeddedness of theories in certain patterns of collective activity and their empirical products sets limits to what can be achieved by an evaluation of theories against empirical data. Such a procedure may be acceptable in the context of Kuhnian "normal science" (Kuhn, 1970), but it leads to problems as soon as we take a broader perspective since the empirical data that are available have been produced according to certain rules. Among other things, these rules determine which criteria are considered relevant for the selection of subjects, what types of social psychological relationships between experimenters and subjects are regarded as desirable, what constraints shall be imposed on the behavior of subjects and of experimenters, which aspects of the experimental situation are held to be unproblematic and which problematic, what shall be recorded and what not recorded, what format experimental records should take, what transformations of 'raw' data are permissible, what must be and what need not be communicated in published accounts of investigations, what form these published accounts should take, including the form in which 'findings' are communicated, and so on. The conduct of empirical research in modern psychology is hedged around by a myriad of rules and conventions, many of them implicit, many of them never seriously questioned. Among these are rules that affect the conduct of subjects even more drastically than they affect the conduct of experimenters.

In light of this it is necessary to make a clear distinction between the natural order of the world that can be imagined to exist without the psychologist's intervention and the empirical order that psychologists help to create by their intervention. What research produces is an artifactual empirical order whose relationship to the natural order is problematical. We would be begging all the important questions if we were to begin by assuming that the empirical order mirrors the natural order. Possibly it does, but if we want to ground assertions about the relationship between the two orders in anything other than blind faith, we have to, begin by recognizing that the empirical order is first of all a construction, a product of rule governed intervention in some natural process. The question of 'realism' is a perfectly legitimate question, but it is a question that belongs at the end, not the beginning, of any inquiry about the relationship between theory and evidence.

The 'evidence' that provides the necessary empirical context for modern psychological theories does not consist of bits of the natural world, but of the products that result from the highly conventionalized constructive activities of psychologists. What happens in the modern period is that a constructed empirical order interposes itself between psychological theory and the order of natural processes. Any claimed correspondence between theory and natural order is discounted if theory is not able to justify itself in terms of an empirical order constructed by following a specific set of rules. The fate of psychoanalytic theory in mainstream psychology provides a well-known illustration.

Of course, psychology never had just one empirical order, it always had several. Unanimity about the proper way of constructing an empirical order always eluded it. Psychoanalysis, for example, developed elaborate rules and procedures for constructing an empirical order, but its rules differed from those of experimental psychology in many quite fundamental respects. However, it is not necessary to limit oneself to such radical discrepancies. Historically, the empirical order characteristic of experimental psychology was based on rules that differed from those operative in the construction of an empirical order based on the employment of mental tests, a difference that was large enough to lead to talk of "two psychologies" (Cronbach, 1957). Even within experimental psychology there is considerable divergence in the rules of empirical construction that have been favored at different times by different groups of investigators (Danziger, 1990a). The rules governing the construction of an empirical domain in Wundt's Leipzig laboratory were not the same as those operative in most of mid-twentieth century American psychology.

An empirical order produced by the employment of a particular set of rules for the conduct of investigation may be called an *empirical domain*. Thus, one can speak of the empirical domains of psycho-analysis, of group intelligence testing, of 'systematic experimental introspection', and so on. The rules employed in the construction of different empirical domains may overlap to varying degrees, thus rendering comparison between different domains less or more problematical. Clearly, comparing the results of experimental introspection with those of mental testing would be highly problematical, but comparing the results of group and individual testing would be less so.

During the modern period theoretical positions in psychology have had to have a primary link to particular empirical domains, though they often claimed to have validity beyond those domains.

Theories that claimed *only* a general validity without a special empirical domain that was peculiarly theirs were likely to be dismissed as unscientific speculations and, therefore, not to be taken seriously.

This co-ordination of theories with particular empirical domains gives rise to a special set of difficulties when theories are to be compared and judged as to their relative validity. Most of the time, psychological theories do not travel well. Take them out of their appropriate empirical environment and they seem like fish out of water. This is because the rules used in the construction of empirical domains tend to be based on the same fundamental assumptions as the theories devised for the explanation of these domains. If one abstracts theories from their proper empirical context and tries to apply them in an empirical context constructed on fundamentally different, perhaps opposite, principles, one is either engaging in a meaningless or self-contradictory exercise of implicitly changing the theory into one that makes different assumptions about the nature of its object than the original theory. One is then no longer dealing with the original theory but some transformation of it (Danziger, 1985a; 1988).

One consequence of this state of affairs is particularly relevant for the discussion of the history-theory relationship. It becomes apparent that if one treats abstracted theoretical propositions as historical entities one is committing the category mistake of confusing the history of metatheory with the history of theory. For the latter, the appropriate units are not theories abstracted from their special empirical context but complex formations made up of three intimately linked components, a theoretical component, empirical products, and an action component that embodies particular rules of construction. Leaving aside many questions of detail, this is the common denominator implied in the general thrust of philosophies of science which deal in units like "paradigms", "research programmes", and "research traditions" (Kuhn, 1970; Lakatos, 1970; Laudan, 1977).

Such complex units, however, are obviously historical formations, and their analysis and assessment have to take place in a historical framework. It is always possible to abstract any of the *components* of a research tradition, theory, data, or rules of investigation and subject them to a trans-historical treatment, as though they had some kind of universal validity. There is a strong abstractionist tradition which sanctions such a procedure. But once we refuse to take the assumptions of abstractionism for granted it becomes only too obvious that most claims for the universality of theories, empirical results, and rules of investigation have only a minimal initial plausibility.

The alternative is to begin by questioning the initial plausibility of universalistic claims. This means putting the onus of establishing some plausibility for trans-historical claims on those who want to make them. Once we withdraw our commitment to the assumptions on which the abstractionist framework is based the discussion must shift onto historical ground. For the formations of which a body of theory, of data, of research practice, was a part before it was abstracted are historical formations. This historical framework is often ignored or dismissed as irrelevant because many psychologists do not believe that there is any practical alternative to the position of naive universalism that they are accustomed to. The alternative to naive universalism, however, is not necessarily relativism. Even an enlightened universalism has no choice but to

take the historical framework into account if it is to establish some initial plausibility for any of its trans-historical claims.

2. Psychological Objects

Recognizing the need to study historically constituted epistemic domains is one thing, finding the means to do so is another. Here we have to be wary of the conceptual traps left behind by the older traditions of psychological historiography. Those traditions, as is now generally recognized, have a strongly "justificationist" character (Ash, 1983; Samelson, 1974; Stocking, 1965; Young, 1966). In the past, the histories of psychology written by and for psychologists tended to organize the historical material in terms of categories taken from currently popular disciplinary beliefs and practices. This made it particularly easy to present the past in terms of gradual progress towards the present. Certain philosophical commitments that formed part of this 'presentist' bias are particularly relevant to our discussion.

There was, for example, a sharp separation of 'theories' and 'empirical findings'. Such a separation has great practical utility in the context of ongoing psychological research where most of what happens has to be taken for granted, so that attention can be concentrated on that tiny portion which is being questioned. But in a historical context this separation of the theoretical and the empirical only has the effect of removing the most interesting aspects of epistemic domains from view. The category of 'empirical findings' is particularly loaded with ideological freight because of the usual implication that 'empirical' means 'prior to any interpretation', and that 'findings' means the data have simply been 'found' by the investigators rather than carefully constructed by them. Within this framework history becomes a catalogue of what was 'found' plus an account of successive attempts at interpreting what was found. The model here is the 'literature review', commonly encountered in psychological journals.

The counterpart to the category of 'empirical findings' is of course the category of abstracted theory which we have already had occasion to question. A different conceptual framework is required for talking about embedded theory.

In the previous section I referred to theories being co-ordinated to empirical domains because of the familiarity of terms like 'theoretical' and 'empirical'. But this way of putting things can be misleading if it suggests that the separation between the empirical and the theoretical is fundamental, while their relationship is secondary. This is, of course, the reverse of what is intended here. What is intended is to do justice to the fact that before modern psychological discourse can be split into a theoretical and an empirical part there are certain primitives for which this division is irrelevant. I will refer to these primitives, which psychologists usually take as given, as 'psychological objects'. They are simply the things that psychologists take to be their proper objects of investigation or professional practice.

Psychological objects may be certain categories of people, such as experimental subjects, or the 'clients' of counsellors. Such categories of people exist only as the objects of the psychologist's intervention, and they would not exist at all if it were not for that intervention. It is the psychologist's own professional activity that creates these categories in the first place. If there were no psychologists, these categories of people would not exist. In other words, the

psychologist can never investigate any 'natural' human category directly; he/she must constitute an object of investigation in the course of that investigation. The persons who, voluntarily or under duress, play the part assigned to them by psychologists in situations constituted by psychologists, have become members of a category of persons that has no members outside these situations. This does not mean that there are no relationships between psychological objects and other objects in the world, but the nature of these relationships obviously cannot be discovered by psychological investigation because they cannot be concerned with anything but psychological objects.

Categories of people are far from being the only kind of psychological object. At least as important are the categories in terms of which psychology organizes its subject matter. Such categories as 'learning', 'motivation', 'intelligence', 'behavior', 'personality', and so forth are not 'natural kinds' but are posited specifically as the objects of psychological investigation and intervention. Of course, any of these terms can be used in contexts other than that of psychological investigation, but a glance at a good dictionary followed by a brief perusal of some of the relevant psychological research literature would be enough to convince anyone who still needs convincing that the objects addressed by psychological research are not the same as the objects posited by the categories of lay discourse. Indeed, psychologists have often insisted on this. As in the case of the psychological objects that are categories of people, we are dealing with objects that are constituted specifically by and for the purpose of psychological investigation and intervention (Gergen, 1982). Again, this does not mean that these objects have no links with objects outside disciplinary psychological discourse, but the nature of these links cannot be discovered by the methods of psychological investigation that necessarily constitute their own objects and none other.

Does this analysis imply a kind of disciplinary solipsism? Certainly there is an unmistakable tendency in that direction. It is a tendency that is greatly strengthened by the homogenization of psychological methods and by demands for the uniformity of permissible categories of psychological discourse. Two things have, however, counteracted this solipsistic trend. One is the internal disunity of the subject which has entailed the positing of fundamentally different psychological objects by fundamentally different means. The other countervailing factor arises out of the fact that the relationship of disciplinary psychology to the world outside it is not just a cognitive one but also involves an exercise of power and influence by which parts of the world are to some extent changed in psychology's image. Where the categories of psychological investigation are imposed as categories of institutional practice, in educational and treatment facilities for example, the reference of these categories undoubtedly extends beyond disciplinary boundaries, not however, because they reflect some independent state of affairs, but because they have helped to create the reality to which they refer (Braginsky & Braginsky, 1974; Rose, 1988; Walkerdine, 1984). The popular diffusion of psychological categories may produce an analogous effect even where there has been no direct professional intervention (MacIntyre, 1985; Shotter, 1975).

These now widely recognized phenomena still leave open the question of possible links between psychological objects and other kinds of objects, a question which, as has been indicated, cannot be answered by psychological means. To address such questions a different level of discourse has to be adopted. In some sense such discourse would undoubtedly be metatheoretical. But it is

not an analysis of psychological theories that is required here, for reasons that have already been outlined. Such theories commonly refer to psychological objects whose existence and natural status they take for granted. It is not the adequacy of the theories in explaining psychological objects which is at issue here, but the constitution of those objects themselves. That constitution is a social process, which is part of human history. Certainly, psychologists are always recreating the objects of their investigation in the course of investigating them, but they are not free to do so at random. They are constrained by historically constituted structures, both cognitive and practical. Psychological objects are also the products of history and can only be understood as such.

The historicity of psychological objects has three aspects: construction, use, and reference, and in what follows I shall consider each of these in turn.

First of all, psychological objects have to be understood as constructed objects. They are not found lying around in nature. Our experiences and actions do not bear little tags, supplied by nature, that identify them as instances of motivation, a personality trait, or a bit of information. They have to be construed as such. Experiences do not naturally arrange themselves in the form of statistical series; they have to be arranged accordingly. People have to agree to act as experimental subjects and modify their conduct in terms of the structure of that role. To understand psychological objects we need some understanding of the way in which they are constituted. But that is something that has changed historically. Psychological categories, rules for producing acceptable data, and rules for arranging research situations have all been subject to quite drastic changes, and we have little hope of understanding the constitution of psychological objects without some understanding of these changes.

Secondly, the construction of psychological objects is an intentional activity. People are instigated by certain purposes when engaging in this activity or changing it. Of course, the purposes which psychological objects serve need not only be the purposes of those who have produced them. But in any case, these objects have certain uses, and the uses they have depend on historical circumstances. It is only to be expected that different circumstances will favor different objects. To understand why the historical development of psychology has favored certain objects over others, some appreciation of the uses of psychological objects is indispensable.

Finally, psychological objects have a reference to a world outside them. A category like 'learning', for example, is not meant only as a label for what humans and animals do in certain situations set up by psychologists but is meant also to cover important aspects of people's conduct outside these situations. Experimental subjects are not supposed to represent only themselves, but their conduct is expected to be relevant to large groups of people who did not participate in any investigation. Traditionally, questions of reference have been answered on the assumption that psychological objects are natural objects. But if we recognize them as constructed historical objects, the problem of their reference will have to be approached rather differently. We will return to this issue in the last section.

3. Categorical Construction

In considering the constructive activity of which psychological objects are the products, a distinction must be made between two aspects. On the one hand, the construction of psychological objects involves some kind of thought-work, the development of certain categorical frameworks, and the fitting of instances into such frameworks. Categories such as 'motivation' or 'association' have to be invented, and the domain of their applicability has to be worked out. But in the twentieth century it has become obvious that the construction of psychological objects is not just a matter of cognitive reordering but also involves practical intervention. Apparatus has to be built, experimental subjects have to be co-opted and instructed, rating scales and tests have to be assembled, records have to be made, and so on. Of course, in the real world, cognitive reordering and practical intervention are closely linked, but if we do not keep them analytically distinct we will miss many important questions and historically important developments.

Before we can consider this further some remarks on the thought-work involved in the construction of psychological objects are necessary. A collective enterprise like modern psychology can only proceed if the active participants are able to communicate effectively about their subject matter. To do this they must share a common framework for organizing their experience. For example, there has to be an implicit agreement that there is a category of events labelled 'motivational' or 'emotional' or 'perceptual' which can be distinguished from other events and about whose basic features there is a large measure of pre-understanding. Without this, no concrete research problems could be formulated, no particular relevance could be assigned to observations, and no specific theories pertaining to these categories of events could be formulated.

But where does this kind of implicit agreement come from? A first answer can be supplied by referring to the process of professional socialization that members of the discipline are put through before they receive their certification. But that answer only pushes the problem one step back. It tells us nothing about the nature of the cognitive framework in terms of which the discipline organizes the experience of its members, and it tells us nothing about how and why the discipline came to adopt this particular framework and not one of the many conceivable alternatives. Such questions become much more difficult to escape when historical studies oblige us to recognize that the framework in terms of which the discipline has organized its work has changed considerably over time and will undoubtedly continue to change.

The embeddedness of the professional culture in a broader, shared, cultural matrix provides material for a more satisfactory kind of answer. Certainly, this broader matrix provided the basis on which the more specific shared understandings of the profession could develop historically and on which the fate of professional products continues to depend. This perspective also makes us aware of the fact that different cultures have used very different frameworks for constructing domains that we would categorize as psychological (Heelas & Lock, 1981; Lutz & White, 1986; Shweder & Bourne, 1984; White & Kirkpatrick, 1985). Moreover, the cultures within which modern psychology developed have a long history of reconstructing what eventually would become the modern psychological domain.

Part of the historical study of psychological objects is, therefore, concerned with the development of key categories which were eventually appropriated by modern psychology. This type of study stands in sharp contrast to what is still the most common approach to the pre-history of the discipline. That approach is characterized by a thoroughgoing naturalism which assumes that the categories of modern psychology refer to 'natural kinds' that exist as such independently of anything the psychologist might think or do. Accordingly, pre-modern psychology can be divided into two parts; one part which can be dismissed because it manifestly is not concerned with psychological reality as currently defined, and another part, which is salvageable because it can be interpreted as 'anticipating' modern insights. The concern in this type of historiography is to maximize historical continuity in order to furnish modern psychology with an impressive pedigree, and to denigrate anything which is completely resistant to this endeavor.

By contrast, the historiography of psychological objects takes their constructed character as primary and brackets out the question of their possible correspondence with some psychological reality beyond themselves. This point of view renders the historian much more sensitive to the discontinuities in psychology's 'long past' than the traditional approach. So much so, in fact, that the question arises whether the history of psychology has a subject at all (Smith, 1988). This is because, on closer investigation, one finds that most of the basic categories that were to play an important role in twentieth century psychology do not correspond to earlier categories at all. 'Behavior', 'personality', 'intelligence', and so forth, were invented at about the same time as modern psychology, or later. They can only be made to correspond to earlier categories by assuming that they accurately reflect a natural structure of the real world which was also reflected, though imperfectly, by some earlier sets of categories. Once we withdraw our assent to this dubious assumption, we are able to investigate what the old historiography obscured, namely, historical changes in the construction of psychological objects and the reasons for them.

Even where basic categories have a history that is longer than that of modern psychology, it turns out that that history is relatively brief in almost all cases. This is true, for example, of such categories as 'stimulation', 'sensation', 'perception', 'association', 'motivation', and 'emotion'. Historians of psychology are, therefore, faced with a choice. They can assume that these categories accurately represent the 'natural' divisions of their subject matter which must be taken for granted. In that case one's historical account is taken to be about different *theories* of perception, motivation, and so forth, because where the underlying objective reality is taken to be fixed in the form of event types represented in our preferred category system, all that remains is variation in theories *about* these types of events. Quite apart from the cultural imperialism implied by this approach, it blinds one to the possibility of constructing psychological objects themselves in fundamentally different ways. The alternative course is to take the constructed character of psychological objects as primary and to direct one's attention to the historical process of construction.

As soon as one does this one has to take seriously the coherent quality of psychological discourse. By this I do not mean grammatical coherence, of course, but conceptual coherence. This is what was implied by my earlier use of terms like 'framework'. Typically, psychological objects are not constructed one by one, independently of each other, but in a coherent system. By this I do not only mean the 'systems' associated with certain individuals, but, more importantly,

the kind of coherence which makes productive discourse among a group of individuals possible. The elaboration of psychological objects has been a collective enterprise made possible by the fact that these objects are reproduced by many individuals who share what might be called the relevant principles of construction. This process functions best when these principles are not deliberately employed but are simply taken for granted and allowed to do their work spontaneously. Then everyone knows without any argument what is meant when different kinds of psychological objects are referred to. When this shared pre-understanding is not present, psychological discourse typically loses its coherence and misunderstandings abound. The fate of culturally alien psychological systems in North America, like that of Wundt, of the Gestaltists, of phenomenology, of depth psychology, provides many abundantly documented examples of this course of events (Ash, 1985; Blumenthal, 1980; Burnham, 1967; Henle, 1980; Jennings, 1986).

In studying the history of psychological objects we cannot therefore dispense with a search for the 'principles of construction' that give internal coherence to different kinds of psychological discourse. What has to be avoided is the transfer of isolated bits from one coherent framework to a totally different one, while assuming that those pieces retain their identity in the new conceptual context. They do not. The nature of the parts depends on the principles according to which the whole has been constructed.

But talk of 'principles' in this connection can be misleading, because it suggests a far more explicit and deliberate process than one actually encounters. Typically, these 'principles' do their work behind the scenes so that their products are not recognized as constructions at all but are accepted as part of the natural order of the world. Although further historical investigation of the operation of such 'principles' is needed I suspect that metaphorical transfer plays a particularly important role in the way they function. It seems that psychological objects are often constructed by analogy with other objects. This analogy may be quite explicit, as in the various mechanical analogies familiar to historians of psychology; or, more pervasively, the analogy may be implicit, as when the mind is conceived in terms of a population of separate ideas, sensations, or other units which relate to each other much like the independent citizens of a liberal state. In general, principles of psychological organization often seem to have had metaphorical links with principles of social organization, the structure of the one domain functioning as an apparent confirmation of the structure of the other.

In the present context some hints about the thought-work involved in the construction of psychological objects must suffice. I have elaborated on these objects elsewhere (Danziger 1983a, 1990b). It is important, however, to emphasize that this thought-work is accomplished in the context of a discourse that has many participants. Thought-work, therefore, should not be seen simply as the activity of independent thinkers but as the product of a "thought collective" (Fleck, 1979). One consequence of this is that there is generally a practical aspect to what may on the surface appear as purely intellectual constructions. The invitation to think about matters in a certain way is, at least by implication, an invitation to act in a certain way (Schön, 1979). By defining objects of interest in a particular way, attention is focused on certain features rather than others, specific expectations are aroused, and hence priorities for practical action are established. For instance, it is difficult to define psychological objects in terms of the metaphor of psychological energy without putting on the agenda quite practical questions of energy control, as the relevant psychological literature from Bain to Freud seems to indicate.

4. Practical Construction

In twentieth century psychology the practical implications of psychological discourse have not disappeared, but the practical aspect of the discipline now involves much more than a by-product of what was essentially thought-work. The practical aspect has assumed primary importance in the construction of psychological objects. There is a new group of psychological specialists who are in the main defined by their activity of constructing psychological objects through direct practical intervention. They claim, and to a large extent have achieved, a monopoly in the construction of psychological objects, and they have done so on the basis of their practical expertise, not on the basis of their thought-work. One consequence of this development has been a proliferation in the variety of psychological objects. To the product of thought-work there have now been added the products of practical work in the laboratory and in the field. As indicated earlier, these products are human, as in the case of experimental subjects, as well as symbolic, as in the case of arrays of empirical data. In either case, the nature of the products reflects specific features of the constructive activity which has generated them.

Thus, during the history of modern psychology various patterns of practice have been employed to construct the research 'subject', the human source of psychological data (Danziger, 1985b). In the early days of experimental psychology the subject whose reactions and reports supplied the empirical basis for psychological knowledge claims was generally a colleague, a friend, or someone with whom one interacted regularly as teacher or as student. In any case, the research relationship was based on openness and on the trust that had developed in the course of a relationship that also existed outside the laboratory. The atmosphere was collaborative, and experimenters and subjects frequently exchanged roles. One may contrast this pattern of experimental practice with others in which experimenters and subjects are strangers to one another, whose only contact extends over the often very brief, and always heavily circumscribed period of experimental interaction. In other cases the participants in the research situation might know one another, but their relationship would be highly structured in an extremely asymmetrical way, so that insight into the research situation and power to dispose over the arrangements and products would be confined to the experimenters.

Of course, the traditional ideology of the discipline denied that such differences had any relevance for the 'empirical' knowledge generated in experimental situations. This was because psychology refused to define itself as a social science but took itself to be concerned purely with facts of nature. Accordingly, the most basic psychological object of all was the abstract, isolated individual whom it actually attempted to construct in its research situations, though without much practical success. The abstract subject of psychological research was a product of some heavy thought-work which managed to leave in the shadows a crucial part of what the practical activity of a psychologist was daily producing in the laboratory. Even when - several generations later - the discipline finally came to recognize that psychological research situations were social situations it simply assimilated this insight within the framework of its traditional assumptions. The social aspect of these situations was limited to social psychological factors, which meant that they could be categorized as ahistorical, natural events, like other psychological events, and investigated by the natural scientific methods of experimentation (Rosenthal & Rosnow, 1969; Rosenthal & Rubin, 1978). Moreover, the 'effects' of these factors were categorized as 'artifacts'

of research, as though the activity of psychologists could be readily divided into a social part that produced 'artifacts' and another, presumably asocial part, that yielded true facts of nature.

Such a division, however, is itself highly artificial (Farr, 1978). At most, one might make a distinction between those aspects of the social practice of investigators that depend on their interaction with their research subjects, and those aspects which are relatively immune to such effects. But the latter are no less social than the former, for they depend on the interaction of investigators with other investigators and with the whole social world of current disciplinary practices in which any particular investigation is embedded. All empirical products of special investigative procedures are artifactual in that they would not exist but for those procedures and would exist in a different way if those procedures were significantly altered. Those procedures, however, are a product of the history of the discipline and are at all times regulated by prevailing disciplinary norms, institutional structures, control over resources, and so on. Such factors determine not only the social psychology of psychological experiments, but the social structure of the investigative situation. These kinds of relationships, however, cannot usually be studied experimentally but require historical investigation. Even the above distinction between those effects that depend on investigators' interactions with their research subjects and those that depend on their interactions with their colleagues breaks down historically at a time when research subjects and colleagues are the same.

The very late and very partial recognition on the part of psychologists that there is anything at all social about their research activity is not so surprising when one considers the categories in terms of which they think and communicate about their own practice. These categories are part of a scientific rhetoric that expresses their deeply felt claim to the status of natural scientists. What they do as investigators of psychological problems they categorize as 'methodology', 'procedure', or 'technique'; terms which were derived from work with non-human objects. Talk couched in these terms conveys a pervasive suggestion that the practices described by it lack the distinctive social qualities that are commonly associated with human interaction. Instead, the illusion is generated that here we have a sphere of practice which is regulated purely by logical and technical considerations.

It is difficult to extricate oneself from this web of illusion as long as one continues to use the old terms and the categories that they represent. If one aims at an analysis of the discipline that is not constrained by the limitations of the discipline's own ideology it seems preferable to work with categories more suited to the task. That is why I have been speaking of the "practice", or more specifically, the "investigative practice" (Danziger, 1990a) of psychologists, rather than of their 'methods'. These practices are made up of everything that psychologists do as social agents when they construct psychological objects. The category of investigative practice, therefore, includes not only what psychologists do in a research context, but also what they do in their professional work in clinical or educational contexts.

Although there are some differences in the product of their activity that depend on the context, their practices are always social and constructive in nature.

That applies no less to the products which the conventional terminology labels 'empirical' than to those which it labels 'theoretical'. Typically, the construction of empirical objects takes place in two phases. In the first phase, a number of participants work together in defined investigative

situations to produce 'raw data'. The work of the participants proceeds according to strict rules that govern their inter-relationship. In the second phase the investigators manipulate the record that constitutes the raw data so as to produce a form of product that is publishable according to the conventions of the day. This process also is governed by strict rules that have nevertheless seen considerable historical modification. Needless to say, investigators' knowledge of these rules in large measure determines what aspects of the investigative interaction are considered worth recording and, therefore, worth eliciting. For instance, an investigator who knows that lengthy introspective reports are not publishable is not likely to ask for them or to take them seriously as recorded data if they are spontaneously offered.

This example, however, has only a limited relevance to the present analysis, because it operates on the level of the individual investigator. Now, in looking at the practices of investigators it is necessary to introduce a distinction that is analogous to the distinction already made in connection with the behavior of experimental subjects. Just as in the case of the latter we must distinguish between the social psychology of individual subjects and the social structure of the investigative situation within which subjects have to act, so in the case of investigators we have to distinguish between the motives and actions of individuals and the social patterns prevailing in the discipline to which the individual investigator has to react. From the point of view of the individual actor and its social psychological analysis the prevailing social patterns, whether they regulate the structure of the investigative situation or the nature of publishable data, can be taken for granted. But from the point of view of the discipline and its historical development, it is precisely these social patterns that are the major object of interest. This requires a different level of analysis, one which is necessarily historical.

There is a connection between a purely individualistic level of analysis and the tendency to think of investigative practices in terms of purely technical considerations. From the point of view of the individual investigator the choice of procedures may indeed often be reduced to essentially technical, and that is to say, rational, considerations. But this is only possible because the historical development of the discipline has predetermined the nature and the variety of alternatives that are available to the individual investigator at a particular time. Although investigators may be making choices that are rational, given the situation in which they find themselves, there is absolutely no guarantee that these choices will somehow add up to a rational course of development for the discipline as a whole. In fact, the history of twentieth century psychology provides little or no support for such an implicit "Hidden Hand" model of development. Major changes in the favored patterns of investigative practice seem to have depended more on shifts in the goals of investigation than on a rational choice of means with constant fixed goals.

In dealing with the construction of empirical objects one has, therefore, to distinguish between specific instances of such objects, produced at a particular time and place, and the general features of such objects which characterize them over extended historical periods and in numerous locations. The intra-disciplinary rules for producing empirical objects can take on the appearance of purely technical rules as long as the general features of those objects are taken for granted. Thus, rules for producing good introspective reports can appear to be based on purely technical considerations, as long as it is accepted that the desired product will have the form of an introspective report. The same applies to the explicit rules used in the construction of

empirical objects that have the form of a statistical aggregation of individual performance measures. Variations in such rules are governed by technical questions of finding the best means for arriving at a given end. But variations in the ends themselves, that is, in the general type of empirical object desired, are not reducible to technical questions within the discipline. Such variations are only explicable on a level of historical inquiry which takes disciplinary patterns and trends as its subject matter.

Because of the extraordinary hold which a positivist understanding of their own activity has exerted on psychologists, and because of the justificationist commitment of much of the relevant historiography, little attention has been paid to an analysis of the general features of empirical objects in modern psychology. The one major exception is constituted by the historical switch from introspective to behavioral data, which could not be overlooked, because it was accompanied by a great deal of noise. But the amount of noise that accompanies a historical change is not an index of its importance in the long run. There were other profound changes in the general features of desirable empirical objects within psychology which were quite pervasive, although they generated relatively little intra-disciplinary debate. The change from data representing the attributes of individuals to data representing the differences between individuals is one example of a long term trend that is of profound significance for the knowledge base of the discipline (Danziger, 1987a; 1990a), yet few psychologists gave much thought to it. Another example is the imposition of a serial form on the behavior of experimental subjects and on the fundamental psychological objects known as "stimuli" (Danziger, 1987c; 1990a).

Given the categories in which psychologists reflected their own activity, such developments were usually conceptualized as technical changes. But they were quite different from true technical changes because they were not simply an improvement in the means for achieving a constant goal but involved a profound change in the goal itself. In the course of time psychologists changed the nature of the empirical objects they wanted to construct, and at any one time there were usually groups of investigators with varying investments in preferred types of knowledge object. These investments were not based on a rational choice of means but represented commitments of an altogether different sort. To display the origins of these commitments, a broader historical canvas is required.

5. Establishing the Enterprise

The production of psychological objects requires scarce resources which have somehow to be diverted from alternative employment and put to work on some disciplinary task. Although fixed investments in space and apparatus had some importance, the most significant social resource mobilized by psychology was always the time and trained skill of investigators and practitioners. The more these resources are made available for this purpose within a particular society, the more the production of psychological objects will flourish. As investigators and practitioners, members of the discipline have an interest in this mobilization of resources and historically they have taken determined steps to advance this interest (Ash, 1980; Geuter, 1987; Reed, 1987; Samelson, 1979). It should be noted that one is dealing here with a *social* interest, that is, a function of social position and not of individual psychology. So, for this interest to be an

important factor in the behavior of an individual, it is also necessary that the disciplinary affiliation be quite salient for that person, relative to his or her other affiliations.

One can think of examples of individuals in the history of American psychology for whom the affiliation with this particular discipline was not particularly salient. Dewey, Judd, and the older James come to mind. But it is significant that such examples are most likely to be found early in the history of the discipline. On the whole, and increasingly so as the discipline developed, the disciplinary affiliation seems to have been extremely important for American psychologists, so that disciplinary interests dominated their professional lives. One factor which undoubtedly promoted this pattern was the internal organization of American universities in terms of discipline-based departments (Harwood, 1987). A complex of other factors was undoubtedly also involved, such as the relative weakness of more traditional alternative affiliations, and the close link between collective and individual social mobility through the securing of professional advantages (Sarfatti Larson, 1977). These factors also became more important elsewhere in the course of time, but they emerged particularly early and strongly in the history of American psychology. For this reason American psychology takes on the status of a paradigm case for the influence of disciplinary interests on disciplinary practices.

What the discipline required above everything else for establishing and expanding its operations was legitimacy. In order to mobilize the resources on which its life as a discipline depended, it had to show that what it did and what it produced was valuable, by the standards prevailing in its society. In the case of American psychology, there were three criteria which were of constant and overwhelming importance in establishing legitimacy. What the discipline had to show was that it was (a) useful, (b) scientific, and (c) individualistic. (This is not to suggest that in the American context the distinction between these three criteria was always perfectly clear to the participants.)

In a pragmatic civilization the question of utility was unavoidable for an ambitious intellectual enterprise, and American psychologists, beginning with William James (1892), certainly lost no time stressing the potential practical usefulness of their endeavors (Danziger, 1979). Many of their early claims were wildly optimistic in this regard and it was obvious that deeds would have to quickly follow words if the latter were not to sound completely empty. The early investigative practices of the discipline would have to be adapted so as to produce psychological objects that were indeed useful on a significant scale. This led to some quite fundamental transformations which resulted in vast differences between most of American psychology and more traditional European models during the period between World War I and II (Danziger, 1987b).

But what was most significant about this development was the way in which useful knowledge was defined. In the always dominant interpretation, usefulness meant useful to agencies of social control, of management, of institutional administration. Certainly, psychology promised great benefits to individuals, but in the dominant model these benefits accrued to individuals as the objects of agencies of social control, schools, clinics, personnel departments, and so forth. The possibility of a psychology that might be directly useful to individuals was looked at askance, tainted as it was with the label of 'popularization'. There is nothing surprising in this, for while a few individuals might profit privately from an alternative psychology, the advancement of the

discipline as a whole depended on its alliance with existing centers of organized social power (Napoli, 1980).

Such centers, however, were only interested in certain kinds of psychological knowledge objects. They were interested in knowledge that would permit a rationalization of institutional practices - in both senses of 'rationalization'. The contributions of psychologists were acceptable insofar as they permitted defined institutional goals to be achieved more efficiently and insofar as they provided a legitimization for institutional practices that might arouse doubts or opposition. Psychological knowledge objects which depended on the statistical construction of individual differences in performance measures were nicely in accord with the requirements of social institutions for which the grading and sorting of individuals was an important function. As a result, the psychological objects that flourished in these practical contexts were largely of this type (Danziger, 1987a; 1990a).

This undeniable practical success ensured paradigm status for the investigative practices on which it was based. With very few exceptions American psychologists came to take it for granted that the kind of knowledge which would be socially useful was statistically constructed knowledge. Because of the continuing need to legitimize even so-called 'pure' research on grounds of ultimate social usefulness, this conviction was readily translated into norms of investigative practice that became pervasive throughout the discipline.

The consequences of this process were all the more noticeable because they converged with a major effect of the second criterion used to establish the legitimacy of psychology's investigative practices. If the enterprise of modern psychology was to succeed, it was imperative that it be recognized as 'scientific', not only by those in control of relevant resources, but also by potential recruits to the discipline, and by the practitioners themselves, whose belief in the worth of their work was often closely tied up with their faith in 'science'. The reason for the quotation marks around 'science' is that the operative factor in this situation was constituted by certain commonly held beliefs about the nature of science, in fact, an ideology of science. Often, these beliefs seem to have been based on the most superficial appraisal of scientific activity that involved, for instance, an assimilation of the concept of science to the concept of technology, or a non-comprehending imitation of such practices as experimentation and quantification. This kind of thing certainly left its mark on the investigative practices of psychology and, in extreme cases, could reduce them to a ritual that ended up having more in common with magic than with science.

A more sophisticated version of the criterion of scientificity took the form of a belief that, in order to qualify as a science, psychology had to devote itself to the search for universal, and, therefore, ahistorical, 'laws' of human behavior. However, psychological phenomena typically lacked the stability and consistency of the phenomena studied by physical science and, therefore, provided a poor basis for the display of such laws. The most commonly chosen way around this problem involved reducing variations in the conduct of different individuals to quantitative form by constructing appropriate investigative situations, and then treating these variations as 'individual differences' on some supposed underlying dimension or 'variable'. The point is that this procedure was based on the reification of a continuous dimension that remained identical for all individuals (Harré, 1979, p.108; Lamiell, 1987; Valsiner, 1986). With this implicit

assumption, generalization across individuals seemed unproblematic, and the formulation of universal 'laws' became possible. The alternative possibility, that the 'observed' individual differences (which were in fact the products of careful construction) might have been manifestations of an underlying discontinuity, was not a viable option for those who regarded these procedures as a necessary guarantee for psychology's scientific status.

By contrast, with such subtle contrivances, establishing the legitimacy of psychology through its conformity to culturally sanctioned individualism was a relatively simple matter. In fact, it would have required a major effort to escape from the grip of this cultural (and political) imperative (Harré, 1984). Wundt made that effort in his *Völkerpsychologie* (Danziger, 1983b), but that certainly won him no accolades from his erstwhile disciples. Abstract individuals, who contained within themselves all the tendencies that made for good or ill in human social life, were hardly an invention of modern psychology. The ground was well prepared, both in the form of the theoretical objects of pre-modern psychology, and in the form of the social practices of those educational, medical, and military institutions for which psychology later attempted to provide useful supplementary services (Rose, 1985). All that was necessary was that the continuities be preserved, both on the theoretical and on the practical level. So psychological tests continued in the tradition of competitive performance comparison among isolated individuals that had been established by the nineteenth century examination system. The experimental method was used as a means for prying individuals loose from the social formations in which they lived out their lives and treating them as abstract 'subjects'. This made it natural to construct human behavior as the product of the propensities of socially isolated individuals (Lave, 1988).

The purpose of these necessarily highly condensed examples is merely to provide some illustration for my general suggestion that the knowledge constituting activities of psychologists are heavily implicated in their project of legitimating their discipline and expanding its claim on limited social resources. Historically, this project has entailed a profound accommodation to prevailing ideologies and culturally sanctioned prejudices. So, far from being a guarantee of objectivity, or 'scientific neutrality', the investigative practices favored by psychologists have in fact served as a medium through which various social interests and ideological positions have been reflected in the objects that were the products of those practices. Moreover, these interests and ideologies were not just those of the psychologists who were directly involved. For in their efforts at establishing, legitimizing, and expanding their sector of the knowledge industry, psychologists, like others in a similar position (Latour, 1987), were obliged to enter into alliances with established centers of social power, and thus to ensure broad conformity of their own practices with the requirements of their allies. The specific social alliances of psychologists varied from country to country and from one historical period to another, and this is reflected in the variety of psychological objects produced in different places at different times.

6. Objects and Objectivity

At this point, if not much earlier, the question of sociological reductionism obviously arises. Does the analysis of the psychological knowledge generating enterprise which I have presented entail the consequence that psychological knowledge claims are nothing but the reflection of sociological factors? Are such claims ever true with respect to a reality that exists independently of the social conditions that have produced these claims?

A first observation to be made in reply to such questions is that nothing in the approach I have outlined necessarily entails sociological reductionism as a consequence. The fact that psychological objects have a social origin and use does not mean that that is all they 'really' refer to. In principle, there is no reason why the social production of a symbolic structure should prevent it having all kinds of features, including that of objective reference. As Joseph Rouse (1987) puts it:

one need not doubt the existence of the bewildering array of overlapping kinds of particles uncovered by high energy physics (hadrons, leptons, fermions, bosons, baryons, mesons, etc.) to suggest that their existence is intertwined with the interests and practices of physicists (p.223)

When it comes to more mundane elements of social life, like natural language, we do not regard objective reference and social involvement as being incompatible, but for some reason we become jittery where the products of science are concerned. I suspect that this special sensitivity is a consequence of the fact that we are heirs to an essentially magical attitude toward science. It used to be thought that science was the product of a special relationship that individual investigators had established with Nature. Anything social was felt to contaminate this special relationship (Bloor, 1976). One might characterize this view as the 'immaculate conception' theory of scientific production. Its denial does rob science of the sacred quality it has had for many, but that does not mean that it is, therefore, to be equated with illusion.

Two sets of beliefs, which long formed part of mainstream psychology's philosophical underpinnings are, however, to be regarded as illusory. One concerns the naturalistic assumption that the fundamental categories of present-day psychology constitute accurate representations of natural kinds. The other set of beliefs forms part of psychology's still strong positivist heritage and revolves around the notion of 'methodology' as a species of purely rational technique, theoretically and ethically neutral.

It is true that, as Putnam (1981, p. 52) puts it: "We cut up the world into objects when we introduce one or another scheme of description." What this means is that representations of reality do not have an intrinsic objective reference which is entirely independent of their historical origin and use. However, this

... does not deny that there are experiential *inputs* to knowledge;... but it does deny that there are any inputs *which are not themselves shaped by our concepts*, by the vocabulary we use to report and describe them, or any inputs *which admit of only one description, independent of all conceptual choices*. (Putnam, 1981, p.54; italics in the original)

Thus, to say that investigators "cut up the world into objects", including psychological objects, obviously assumes that there is a world to be cut up. However, it is a position that warns against the identification of our constructed categories with the "natural kinds" of the world that exist outside the framework of our descriptions and practices (Lakoff, 1987).

It does not follow from this that one set of categories and practices is as good as another, that 'anything goes'. We do have defensible ways of assessing the value of different frameworks, though simple-minded empiricism is obviously not one of them (Bhaskar, 1979). The comparison of theory and data always involves some conceptual framework and some set of practices that are taken for granted in such comparisons. We can, however, compare different sets of categories and practices with each other in terms of such criteria as the 'depth' of their explanatory schemes (Miller, 1987), and the social consequences of their practices (Harré, 1986). Much of the material for such comparisons will have to be historical, for, as has been argued here, the structures to be compared are historical and not logical structures.

The criterion of practical consequence becomes particularly significant when we appreciate that the relationship between psychological categories, as well as practices, and the reality to which they relate cannot often be a passively reflective one. Psychological objects have at least the potential to function as self-fulfilling prophecies. To the extent that human beings are 'self-defining animals' they are likely to be affected by the knowledge claims and practices of psychologists, and the more successful the discipline is in establishing and expanding its operations, the more pronounced this effect is likely to be. Psychology not only investigates people as other sciences investigate moons and dinosaurs, it also teaches people how to think about themselves and how to act. It does this involuntarily by its socially granted power to cast people in certain roles and by the prestige of expertise which surrounds its knowledge claims. Its relationship to the reality it is trying to represent is more complicated than that of astronomy or paleontology. Ultimately, psychology cannot leave itself out of its account of the reality with which it deals. Reflexivity will always be more important for the work of psychologists than it is for the work of straightforward natural scientists. Unfortunately, psychologists have been more prepared to accept this fact of life on the level of individual investigations than on the level of the discipline or its sub-disciplines.

Because of psychology's peculiar relationship to its subject matter the question of the correspondence of its knowledge claims to a reality that exists independently of them cannot be resolved ahistorically. It is quite possible for a psychological generalization to be true for one historical period, or setting, and not for another. Moreover, a change in this respect might well depend on the social context of psychological knowledge production. Who are the consumers and beneficiaries of psychological knowledge, apart from psychologists themselves? What kind of knowledge do these consumers and beneficiaries require and how do they modify human life with its help (Danziger, 1990a; Kasschau & Kessel, 1980; Miller, 1969)? The question of the relationship of psychology to what it depicts is as much one of impact as one of reflection.

To say, as I have done, that the knowledge producers' interest in legitimation is heavily implicated in the kind of knowledge produced is to suggest that their relationships with powerful social groups and institutions must be taken into account in trying to understand the nature of the product. Psychological knowledge is not only produced *by* but also *for* and *about* people with particular interests and preferences. Epistemological and moral questions, therefore, tend to become linked.

Psychological objects vary greatly in the generality of their reference. For many of them, the claim that they refer to anything outside the world of psychological investigation is based on

pure faith. In other cases, the boundaries of their applicability seem to be set by the extension of specific cultural conventions and institutional structures. It is possible that in some cases these boundaries enclose a very large area, so large in fact, that the limits are of relatively minor practical importance. But we will never be able to establish any of this if we start with the a priori assumption that psychological 'findings' are facts of nature, and that it is the task of psychology to reveal the universal natural 'laws' that underlie these findings. Our only hope of establishing the reach of psychological knowledge is not to take its universality for granted at the outset, but to treat each of its products as a historically embedded achievement. Only when we understand something of this historical embeddedness of specific psychological objects and practices are we in a position to formulate intelligent questions about their possible historical transcendence.

NOTE

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