The Social Context of Methodology

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Many of us will have had experiences similar to those reported some years ago by one teacher of psychology:

> It has often been my experience in the teaching of psychology......that my students had gained not so much as a glimmer of what I considered most important: a sense of the scientific endeavor. Mere exposure to the data and theories of biology or psychology in no way assured them of a concomitant grasp of scientific thinking or discovery (Monte, 1975, xv-xvi).

This he considered "rather a grim discovery", but he tried to improve the situation by writing a textbook that would spell out for students the nature of what he called "Psychology's Scientific Endeavor". In doing so he joined the ranks of contributors to a distinct genre, one that is just about as old as experimental psychology itself. The first methodological text was published by Wilhelm Wundt in 1883, just after he had established his psychological laboratory (Wundt, 1883). Like most of Wundt's important works it was never translated into English.

Since then the genre of methodology texts has divided into one set of texts written essentially for aspiring or active research psychologists and another set directed at undergraduates, who could be regarded as consumers rather than producers of psychological knowledge. This split illustrates the dual function of methodology texts, namely, to explain their research practices to psychologists themselves, and to explain them to others. These explanations are provided in the form of procedural accounts that use a special language. Methodology texts are either written in this language or they seek to introduce students to it. This language, which is familiar to us all, deals in the verbal coinage of independent, dependent, intervening, and confounding variables, of data and hypothetical constructs, of operational definitions and of statistical significance, of experimental and quasi-experimental design, of construct, ecological and other types of validity, of antecedents and consequents, stimuli and responses, hypothesis and evidence, and so on.

Because our professional socialization was so intimately tied up with the learning of this language it has acquired, for many of us, a taken for granted quality that can make us oblivious to the fact that it is in fact a language, a form of discourse, a way of talking about things from a particular perspective, that of the investigator. Now, as Bill Bevan (1991) pointed out quite recently, what we say, what we believe, and what we do as scientists is not at all equivalent. He is in good company here, for, sixty years ago, Einstein said: "If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention to their deeds" (Einstein, 1933, p.1). So let us take this advice and focus on what we do rather than on what we say and believe.

But how would one obtain an account of what scientists actually do? Well, one way would be for trained outsiders to make systematic observations of experimental situations, rather in the way that an anthropologist or ethnographer makes observations of the customs and practices of foreign cultures. Of course, to make sense of what they observe such observers would have to acquire a great deal of information about the practices they are studying, so they would not be naive observers, but they would try to maintain a more distanced perspective than that of the participants who have been socialized to take their own practices for granted.

Such observational studies of scientific experiments have in fact been carried out since the nineteen seventies. They include, among others, studies of scientists working in such areas as neurohormones (Latour and Woolgar, 1979), plant proteins (Knorr-Cetina, 1981), high energy physics (Pickering, 1984; Traweek, 1988), and lasers (Collins, 1985), though not, as yet, behavioral scientists. These kinds of studies have raised some interesting philosophical issues (e.g. Hacking, 1988; Pickering, 1992; McMullin, 1992), but here I merely want to note one rather uncontroversial outcome. What these studies show very clearly is that a lot more happens in the course of scientific investigations than is reported in scientific papers or fully appreciated by the participants themselves. In particular, the official story, if I may use the term, tends to ignore or at least to misrepresent the role of social factors in the process of scientific investigation. Everyone recognizes that science is a collective enterprise, but the implications of that recognition are often avoided. At every step the process of scientific investigation involves choices, and the outcome of those choices depends quite crucially on the social context in which they are made.

To begin with, an investigator has to choose a problem to work on, which means that, explicitly or implicitly, he or she has to choose a particular way of formulating the problem. That choice will depend on the way in which problems of that kind have been formulated by the investigator's scientific predecessors, or, more prominently in the social sciences, by the culture to which the investigator belongs. Having chosen a problem one has to choose a method for studying it. But no-one ever chooses from among the full range of potentially relevant methods. At any particular time the scientific community to which the investigator belongs will have clear preferences for and strong commitments to certain methods rather than others. These preferences and commitments typically change over time, but only some of that change can be accounted for by technological progress, much of it appears to be more a matter of fads and fashions, of economic considerations, of changing value priorities, and so on.

Having got their problem and their methods investigators collect their data. But at this stage too there are choices to be made and a social context which determines the outcome of those choices. In any investigative situation there is always much more potential information than is actually used in the form of data, and the definition of what is and what is not data depends on local scientific traditions which assign a particular meaning to inherently ambiguous observations. Studies of the actual practice of science have shown how the assignment of meaning to experimental observations depends on social processes of consensus formation, and how problematic such a consensus can sometimes be.

When the data have been collected and interpreted the study must be written up in a form that will make it eligible for publication in an appropriate scientific medium. At this stage the influence of social factors is so obvious that it is impossible to overlook. There are rigid norms that scientific publications must conform to, governing both style and content. In practice, these norms have frequently been found to act back on the way data are interpreted, on the choice of methods, and on the formulation of the problem. For instance, one study of APA Publication Manual has suggested that the style it prescribes still favors a broadly behavioristic framework (Bazerman, 1988). All this would not matter too much if publication norms incorporated the distilled essence of scientific rationality. However, these norms, like other human norms, change historically, and while it is relatively easy to relate those changes to prevailing scientific fashions and to extra-scientific influences, it is not at all clear that historically later norms are intrinsically more rational than historically earlier norms.

We need to introduce a common distinction between two senses of the term "methodology" here (e.g. Kaplan, 1964). The term is often used to apply to specific procedures, e.g. analysis of variance. But such procedures are always applied in a broader methodological framework of which they are a part and from which they derive their justification. The social aspects of research that I have been talking about belong to this broader framework.

Both specific procedures, i.e. technique, and methodology in the broader sense change over time. But they change in different ways. In the case of techniques it makes sense to speak of progress. Later procedures generally represent an improvement over earlier procedures. That means that in teaching techniques we can afford to ignore history. Why bother with procedures that we know are not as good as the ones we have now? But when it comes to broader methodological questions we ignore history at our peril. That is because changes in the way such questions are answered do not seem to obey the law of progress in any obvious and uncontroversial way.

So when we are dealing with more general questions of methodology, rather than questions of specific procedures, we had better not ignore history. If we do, we not only run the risk of repeating the mistakes of the past; more seriously, we run the risk of becoming trapped in currently fashionable preconceptions and closing off discussion about the more fundamental methodological issues. In that case, teaching methodology will be a lot like teaching research ethics, we will be imparting norms rather than questioning them, expounding on ideals rather than inquiring into actual practices. Our implicit message will be that the scientific attitude must be abandoned when it comes to the study of science itself.

Rather than run that risk, I would like next to apply a historical perspective to the way in which psychologists have tried to come to terms with the social context of their methodology. But rather than dealing with issues which arise in connection with any socially organized investigative process, whether in the physical, the biological, or the behavioral sciences, I would like to focus on an important special feature of psychological investigation, namely, that human individuals participate in the research process not only as scientists but also as sources of the data on which the science relies. If one conducts research with human subjects one has a social relationship, not only with one's research community, but also with those who provide the material for one's research. This introduces a social component of methodology which the biologist or physicist does not have to be concerned about.

Not that psychologists have embraced this concern with great enthusiasm. Until relatively recently they hardly seemed to notice that there was anything worthy of concern at all, and although that is no longer the case, the tendency to minimize these concerns is still strong. However, as early as 1933 the *Psychological Review* published a paper by a sharp young experimentalist, Saul Rosenzweig, in which he pointed out that in psychological experiments the experimenter formed part of the social environment of the object experimented upon. This made it, not only practically difficult, but also theoretically dangerous to separate the effects of the experimenter from the effects of other experimental variables. In other words, in psychology experimental effects are embedded in the social situation of the experiment and cannot necessarily be generalized to other social situations. Rosenzweig's analysis clearly implied that the effects of manipulating specific stimulus variables did not occur in a social vacuum but were mediated by the effects of an investigative situation of which they formed a part. The silence that greeted this challenge to methodological orthodoxy was deafening. Nobody took up the fundamental issues it raised, and there was not to be any talk of the social psychology of the psychological experiment for another three or four decades.

As far as the discipline was concerned Rosenzweig's theoretical analysis could hardly have come at a worse time. The heyday of neo-behaviorism was just dawning, and a very different conception of psychological investigation was coming into its own. The year before the appearance of Rosenzweig's article had seen the publication of Tolman's *Purposive Behavior in Animals and Men* (1932). There Tolman introduced the language of independent, dependent, and intervening variables to describe psychological investigation, a language which had not been in use before. This was soon taken up by other prominent experimentalists, notably Boring (1933) and Woodworth (1934, 1938). The latter in particular exercized tremendous influence through his widely disseminated textbooks, both at the graduate and the undergraduate level (Winston, 1988, 1990). None of these authorities had the slightest sympathy for Rosenzweig's emphasis on the fundamentally social nature of psychological investigation. They represented the discipline's dominant aspiration to be accepted as a fully fledged natural science rather than merely a social science. The language of independent and dependent variables was sufficiently abstract to obscure the social features of psychological investigation which made it different from investigations in chemistry and biology. If there were such features they could now only be described as one set of variables among others. But that had not been Rosenzweig's point. What he had seen was that empirical relationships of the kind established for example by chemists depended on the possibility of separating the role of the chemist from the role of the chemicals he or she worked with. Chemical laws need be concerned only with the chemicals, not the chemist. But in psychological investigation the role of the psychologist and the role of the treatments or stimulus materials he or she applied were hopelessly confounded, as we would say. So stimulus A applied in social context A was not the same as stimulus A applied in social context B.

Another psychologist who had come to a similar conclusion a few years earlier was Kurt Lewin, though his ideas on this question were not so accessible to an American audience. In the classical series of experiments on such topics as level of aspiration and the psychology of anger, which Lewin directed in Berlin in the late nineteen twenties and early nineteen thirties the experimenter was conceptualized as very much part of the situation to which the subject responded. This was also the case in the well known experiments on "group climates" that Lewin conducted at Iowa a few years later (Lewin, Lippitt and White, 1939). These were certainly among the most influential and provocative experiments ever conducted in social psychology, but let us look at their methodology.

Groups of boys engaged in a task were supervised by adult confederates of Lewin who deliberately adopted different styles of behavior so as to create three distinct kinds of social atmosphere, or "social climate", in the groups. Lewin labelled them "authoritarian", "democratic", and "laissez-faire". In the authoritarian groups the adult leader dictated work assignments and techniques, in the laissez-faire groups the boys were pretty much left to their own devices, and in the democratic groups the adult provided options and discussed them with the boys. We are not concerned here with the striking differences that were observed between the behavior of the boys under different social conditions but with the methodology used to produce these conditions. Like all experiments this study looked at the effect of deliberately varied antecedent conditions on behavior. But what were the variable antecedent conditions? Did they consist of stimuli, or independent variables, in addition to the effect of the experimenter? Obviously not, as the effect of the experimenter *was* the independent variable. Note that it is the social effect created by the experimenter, not the experimenter as an individual, which provides the crucial antecedent conditions. In other words, the social relationship of experimenter and subjects lies at the core of this experimental manipulation. It is not something that can be added to or subtracted from the rest of the antecedent conditions, leaving an unchanged residue behind.

The reaction of American experimenters to Lewinian methodology can only be described as ambivalent. On the one hand, people were greatly impressed by the practical relevance of these studies and by the boldness with which they confronted really significant issues. On the other hand, they were also puzzled and sceptical, because these studies did not conform to the emerging methodological orthodoxy. Leon Festinger, one of Lewin's most prominent admirers, still felt this ambivalence when he looked back four decades later. "Who would have imagined doing a "scientific experiment" in which the independent variable to be manipulated was autocratic versus democratic atmospheres", he recalls (Patnoe, 1980, 239). But he adds, "I still have no conceptual understanding of what all the differences were between these procedures" (ibid.). He carefully says, "conceptual" understanding, because on one level he obviously did have an understanding of the difference between an autocratic and a democratic atmosphere, as we all do. So what was this "conceptual understanding" that proved so elusive, even after forty years? Festinger was an enlightened person of broad interests but as an experimentalist he was thoroughly committed to the new methodological orthodoxy that had crystallized around the time he received his graduate training (Festinger, 1953). This was the orthodoxy expressed in the language of independent and dependent variables to which I have already referred.

Because this is still the prevailing orthodoxy, let us try to understand the source of Festinger's problem, using Kurt Lewin's deviant methodology as a counterfoil. Every description and every choice of a particular methodology involves, explicitly or implicitly, some assumptions about the nature of that to which the methodology is being applied. If we want to be folksy about this we can say that when we choose to eat the dish in front of us with a spoon rather than a fork we do so because we believe that it is soup rather than salad which has been put on the table. Or, if we want to be philosophical about it, we can say that our methods imply ontological assumptions. So if we think we can investigate some phenomenon by dividing it up into distinct independent and dependent variables, it must mean we believe the nature of this phenomenon to be such that we can safely go ahead without running into the kinds of problems we would run into if we tried to eat our soup with a fork. Here I think is one source of the misunderstanding between Festinger and Lewin. Festinger believed that to be scientific you had to isolate distinct variables, and you had to assume that you could do this without doing violence to the nature of reality, because if you didn't, you would have to give up your faith in the effectiveness of the scientific method. Lewin's experiments resisted an analysis in terms of relationships among distinct independent and dependent variables, and so, in spite of his admiration for their originator, Festinger puts "scientific experiments" in inverted commas when he refers to them. They are interesting, but they are not the genuine article.

Lewin, of course, came out of a different methodological tradition, one which did not equate the scientific method with the search for functional relationships between isolable and ontologically distinct variables. He shared the conviction of the Gestalt psychologists that reality was not a bundle of elements, and therefore it wasn't very smart to investigate it as though it were. He called his approach "field theory", and though conceptually people generally followed what he meant by that, the methodological implications were often ignored. But as Lewin's own work demonstrated, field theory legitimated, and ultimately required, working with holistic units, like group climates, that represented complexly patterned experimental situations. These were of course social situations organized by the nature of the relationship between experimenters and subjects.

Lewin was not the only psychologist whose approach was difficult to reconcile with the methodological orthodoxy that became established between the mid thirties and the mid fifties of this century. Analogous tendencies can be seen in the work of social psychologists like Sherif (1953) and Ash (1952). But social psychologists had a low rank in the internal status hierarchy of the discipline (Sherif, (1979), and so far from exerting any wider influence would themselves have to adapt to the dominant methodological paradigm, if they wanted to be taken seriously. So when the question of experimenter-subject relationships was reopened in the nineteen sixties after a long absence from the literature, the discussion was conducted within a methodological framework developed by experimentalists strongly committed to the image of psychology as a natural rather than a social science. This gave the discussion a particular direction.

The new phase in psychology's concern with the social aspects of its methodology received its initial impetus from Martin Orne's realization that there was something similar about the hypnotic situation and the experimental situation (Orne, 1962). He discussed this in terms of the concept of "demand characteristics", a Lewinian concept that indicated the existence of some historical continuity in this area. Demand characteristics are features of perceived *situations*, not isolated stimulus elements. Orne also pointed out that applied psychologists had long had to recognize the social effects of investigative situations, most explicitly so in the well known Hawthorne studies (Roethlisberger and Dickson, 1939). Pure experimenters had been much less ready to do so, because "our model for experiments comes from the physical sciences" (Orne, 1970, 220).

The years that followed Orne's work saw an explosion of empirical studies on the social psychology of the psychological experiment, so that by 1978 Rosenthal and Rubin were able to entitle their position paper on the topic: "Interpersonal expectancy effects: The first 345 studies". There is no question that this represented a great advance over the previous period. It certainly made it more difficult to think of experimental situations as utterly unique among human situations in having no social character, or at least none that mattered. To achieve that result most of the effort in the new research area had been devoted simply to the demonstration of the existence of interpersonal expectancy effects or of relevant intrapersonal factors, like evaluation apprehension. But, as one of the commentators on the Rosenthal and Rubin position paper pointed out, this empirical advance had not been accompanied by a commensurate theoretical advance (Adair, 1978).

Among other things, this meant that the new research interest in social factors in experimentation was held to have only limited methodological implications. The rules of the game were still those that had been developed in the thirties and forties by the hybridization of elements from stimulus-response psychology, positivist philosophy and statistical procedures from applied biology. By the nineteen sixties and seventies these rules governed what was virtually the only game in town for American research psychologists, and one played by these rules if one wanted to achieve plausibility within the discipline and maintain one's own scientific self-respect. So experiments of fairly conventional design became the favorite context for discussions of the social aspects of experimentation. This had its limitations. Methodological orthodoxy depended on offering the same abstract account of experimental situations, whether in physics, biology or psychology. Experiments were situations in which human investigators manipulated some external material in order to verify or falsify their predictions. Valid conclusions could only be reached if it was assumed that investigators and the objects of their investigation had no effect on each other apart from what could be manipulated or controlled in the experimental situation. From this point of view the unavoidable social features of human experimentation had the status of a nuisance. They were categorized as "artifacts", with an implied distinction between experimental facts and artifacts.

But how do you distinguish between facts and artifacts? The answer one finds in the literature seems to come down to this, that artifacts are unintended effects. In other words, we end up with a completely subjective criterion for distinguishing fact and artifact. We become involved in this paradox when we try to talk about experimental situations in a language that already presupposes a particular version of what an experiment is, a point made quite a number of years ago by Gadlin and Ingle (1975). The concept of "experimental artifact" may be useful in the technical context of a particular experiment, but it becomes seriously misleading if it is used to imply some distinction in principle between the factual and the artifactual components of experiments in general. All experimental situations are artifactual, and so are their products. The whole point of conducting experiments is to produce artifacts, to construct situations and to make observations that would not exist without our deliberate efforts. Our goals and intentions are deeply implicated in the structure of experiments, and as our goals change so does the structure of our experiments, including of course the social structure.

Let me illustrate this with some historical examples that I have been looking at during the last few years (Danziger, 1985; 1990). Everyone knows about the world's first major psychological laboratory where a large proportion of the first generation of experimental psychologists was trained. That was Wilhelm Wundt's laboratory at Leipzig. But if you compare the social arrangements under which experiments were conducted at Leipzig with either current practice or with some of the prescriptions of current textbooks you are in for a shock. First of all, the participants in those Leipzig experiments frequently exchanged the roles of subject and experimenter, even in the same experiment. We, of course, tend to think of these roles as quite distinct. In fact, considering the frequency with which the subject role is reserved for undergraduate college students, the idea of having to exchange experimenter and subject roles is a bit disturbing. But then our notion of experimenter and subject roles is rather different from what it was in those far off days. Both then and now subjects are the sources of data, but whereas we proceed as though this function could not possibly be combined with functions like administering experimental stimuli, conceptualizing the experimental hypothesis, or writing up the experimental report for publication, this belief was not shared by the pioneers of psychological experimentation. Members of those early laboratories, not only at Leipzig but also at Cornell, Chicago and elsewhere, frequently alternated with one another as stimulus administrators and as sources of data within the same experiment. Moreover, the person under whose name the published account of the experiment appeared was not necessarily the one who had played the role of experimenter in the modern sense. Sometimes a paper on an experiment was published by the person who had functioned solely as the experimental subject, while others had functioned as experimenters. We might also note that the participants in those early experiments were not strangers to one another. They interacted outside the laboratory as professor and student, as fellow students, and often as friends.

Maybe you feel that the difference between then and now simply demonstrates scientific progress, that people have become more sophisticated about likely sources of error in psychological experiments and about experimental design in general. But that interpretation is difficult to sustain in the face of three sets of evidence. First of all, when one actually reads the early experimental literature one finds that its authors were extremely sensitive to possible sources of error in their procedures - in some ways they are more sensitive than a modern critic, unfamiliar with those procedures, is likely to be. For instance, there is hardly a better example of meticulous attention to laboratory procedures and problems than Titchener's "Manual of Laboratory Practice" (1901-1905). Secondly, the old pattern never died out completely but rather survived in a small way in the sensation/perception area, which is not generally associated with scientific backwardness. Thirdly, when one makes a systematic survey of the empirical reports of early modern psychology one finds instances where the relationship of experimenters and subjects was much more like our own conception of what it should be. This was particularly the case where the subjects were children or were drawn from clinical populations. In those cases experimenters and subjects did not exchange roles, and the roles remained quite separate. Subjects provided the raw data and experimenters analysed them, theorized about them and published the results. In this respect these experiments were much more like modern mainstream experiments, but in other respects they tended to be methodologically naive compared to the experiments conducted at Leipzig, Cornell or Chicago. So one can hardly say that they represented a higher level of scientific progress.

Recall my earlier distinction between technique and methodology. The notion of progress can be unproblematically applied to technique, but not to methodology. Now, any differences in technique between the early experimentalists and ourselves clearly depend on more fundamental differences in methodology. We do not have the same conception of what we are trying to do when we engage in psychological research. We have different knowledge goals.

This concept of knowledge goals brings us back to the investigator. In order to understand investigators' adoption of a certain methodology we have to understand their goals, but to do that we first have to distinguish between specific and more general goals. Investigators commonly look for specific information relevant to a particular hypothesis. But they do not accept just any piece of information indiscriminately. They make certain demands on the data they are willing to accept. We have concepts like reliability and validity to express such demands. When we do research we are only interested in certain kinds of information, information that satisfies certain criteria. Our behavior implies that we have general knowledge goals in addition to the specific goals formulated in research reports. Normally, we do not need to formulate these general goals explicitly; we can take them for granted, because they are shared by all members of our scientific community. In fact, such shared goals are part of what constitutes a scientific community.

Day to day research activity depends on never questioning the general knowledge goals of your scientific community. That is part of what Thomas Kuhn (1970) meant by "normal science". The price paid is the equation of methodology with technique and the resulting conservatism with regard to fundamental methodological change. If, however, we want to understand methodology in the more general sense, we need to adopt a broader perspective so that we can compare different scientific communities with each other without prejudging the issue of which is on the right track and which is not. At this level of analysis the concept of general knowledge goals is indispensible.

For example, the arrangements of the early psychological laboratories become understandable as perfectly rational when one keeps in mind the general knowledge goal pursued by these investigators. They had a very definite knowledge object, namely, the universal features of the individual adult consciousness. To obtain information on this object you needed sophisticated and reliable observers of the individual consciousness. However, it was not the individuality of each consciousness which was of interest but their common features. There was therefore no reason why experimenters and subjects should not exchange roles, especially as an understanding of the purpose of the experiment was held to establish the best conditions for careful introspective observation (Wundt, 1906; Danziger, 1980). The knowledge goals of these investigators required sophisticated, well informed and dedicated experimental subjects, and their methodology was designed around that requirement.

Those who did research on children or on clinically stigmatized subjects, on the other hand, had different knowledge goals. They were ultimately interested in deviance from some supposed adult (and usually male) norm and so had to work with subjects they could not exchange places with, subjects who were by definition excluded from filling the shoes of the scientific investigators. During the period between World War I and II American psychology increasingly adopted a kind of research practice for which the aggregation of observations across groups of individuals was fundamental. In this style of investigative practice, pioneered by Francis Galton, knowledge goals had shifted away from the individual consciousness, or the individual organism for that matter. The new goal focused on the production and analysis of inter-individual variance. For that you needed data from a relatively large number of subjects and this imposed new constraints on the social context of methodology. As resources were limited, investigators and subjects would have to meet for relatively brief periods and would almost certainly be strangers to one another.

The social situations in which psychological data were now produced differed quite considerably from the situation in earlier investigations, whether of the laboratory or the clinical type. It seems likely that a different set of social psychological problems would characterize each of these investigative situations. Studies of the social psychology of psychological experiments have tended to concentrate on one kind of experimental situation, that of the contemporary mainstream experiment. It would be interesting to broaden this perspective and to extend social psychological analysis to other types of investigative situations.

This is particularly desirable to-day, because in recent years there have been a number of innovative developments in investigative practice. After several decades of methodological gridlock there appears to be a growing realization that the range of social contexts which lend themselves to the systematic collection and analysis of psychological information is much larger than the very limited context to which most psychological investigation had been reduced. To mention only a few, we now have discourse analysis (Potter and Wetherell, 1987) and estrogenic analysis (Marsh, Rosser and Harre, 1978) which are proliferating in Britain; we have various kinds of collaborative research in institutional settings (e.g. Argyrols, 1985; Torbert, 1981); we have research in cultural psychology which has adopted some of the methods of modern ethnographic research (Stigler, Shweder and Herdt, 1990). All these methodologies are based on a different construction of the relationship between investigators and their subjects than the one that has characterized psychology over much of this century. In fact, it is inappropriate to speak of "subjects" in the context of "new paradigm" research. What we can say is that all forms of investigative practice have participants, and that in one particularly widespread form of practice the relationship among some of the participants takes the form of the familiar division into asymmetrical experimenter and subject roles. But we should stop treating this form as though it provided the only conceivable social framework for the achievement of scientifically significant psychological knowledge.

Because of the close link between the kind of knowledge we achieve and the social conditions under which it is produced, we always impose limitations on our knowledge when we accept restrictions on how it is produced. Often, these limitations are accepted quite deliberately, in fact, they are seen as positive knowledge goals, but if a certain methodology becomes simply taken for granted, thought about the kind of knowledge it yields tends to stop. Then we may end up with a kind of knowledge we did not really want, but we accept it anyway, because we have been taught that if we abandon a specific version of scientific methodology we are abandoning all hope of any kind of valid knowledge.

The kind of knowledge that the conventional methodological context is very good at producing is knowledge about the unidirectional effect of unilateral interventions. It is also knowledge that targets statistical effects rather than individuals. There is certainly room for such knowledge, for example, when you are interested in what Philip Runkel (1990) calls "casting nets", that is, finding the proportion of some population with a particular attribute, as in public opinion or advertising research. What must be resisted, however, is the insinuation that this kind of methodology provides the only basis for any kind of psychological knowledge that deserves to be called scientific. Like any methodology, net casting makes strong assumptions about the nature of the objects to which it is applied, and these assumptions cannot be adequately tested within the framework of the methodology (Danziger, 1988). Insofar as these assumptions are fundamentally incorrect, blind persistence in the indiscriminate use of such a methodology can hardly be regarded as an example of true scientific spirit. Genuine science would seem to have more to do with care in choosing methods appropriate to the subject matter.

If departures from methodological orthodoxy are becoming more and more frequent, it is because traditional assumptions about our subject matter are increasingly being questioned and traditional knowledge goals are increasingly being replaced by new goals. Developmental psychologists like Winegar and Valsiner (1992) have indicated that the current psychogenetic reconceptualization of the developmental process requires "a rethinking of some of our most cherished methodological tools: independent and dependent variables and analysis of statistical variance" (p. 258). Others have elaborated on a kind of knowledge required by practitioners which is fundamentally different from the kind of knowledge supplied by the application of traditional methodology (Polkinghorne, 1992; Kvale, 1992; Shotter, 1993). Gergen (1989) and others see the goal of psychological inquiry as lying in the direction of an enlargement and enrichment of psychological intelligibilities and therefore needing to draw on hermeneutic methods. Moreover, a considerable body of feminist scholarship has not only called attention to the historical and systematic links between the social context of research and a certain kind of knowledge (Sherif, 1979; Wallston and Grady, 1985; Morawski, 1988; Bayer and Morawski 1992), but has gone on to suggest alternatives (Hollway, 1989; Lykes, 1989; Morawski, 1990; Morawski and Steele, 1991).

In the face of these rapidly growing developments the task of the teacher of psychological methodology needs rethinking. Surely it is no longer adequate to define this task in terms of training in a prescribed set of technical skills associated with one methodological approach. That is not to say that one set of skills is simply to be replaced with another. Rather, we need to pay more attention to questions of methodology as distinct from questions of technique. That means abandoning the unfortunate tradition of pretending that one narrow set of techniques exhausts the range of methodological options at our disposal. It means raising questions about our knowledge goals and about the ontological assumptions that are implied by different methodologies. And that means going beyond the idea of methodology as the embodiment of contextless general prescriptions and recognizing that methodology in use always involves the implementation of some social scenario.

The methodological norms of our discipline crystallized about half a century ago. Around that time and before many prominent psychologists were definitely interested in current developments in the philosophy of science, and their methodological notions reflect that interest. Unfortunately, the period of crystallization, which the discipline probably needed, gradually developed into a period of fossilization, which the discipline probably did not need. A methodological consensus achieved at a particular historical moment was accepted as valid for all future time. No longer did it appear necessary to keep up with developments outside the discipline which might be relevant to questions of methodology. Not only did it become unfashionable to take an interest in what was happening in the philosophy of science, but other related fields were treated with the same disdain. More recent developments in the history and sociology of science were widely ignored, although some of these developments were of potentially enormous significance for our conception of psychological research. Thomas Kuhn's widely misinterpreted work (Peterson, 1981) was a dubious exception. But in any case, since it first appeared, thirty years ago (Kuhn, 1962), there have been many further investigations of the social context of science, and this has led to a number of more recent formulations regarding the historical contingency of scientific methodology. These range from Dudley Shapere's (1984) demonstration of the historical interplay of method and content in science to Ian Hacking's (1992) notion of "styles of reasoning" of which the statistical style is a prime example. As Thomas Nickles (1989, 318, 321) put it: "methodology is a human social-scientific subject and not a purely logical subject....the methodological order and the social order are inseparable".

Perhaps the most radical, but, at least in Europe, also the most influential, version of the new account of scientific method is to be found in the writings of the French philosopher, Michel Foucault, who is known for his concept of a "regime of truth". "Each society", he says, "has its regime of truth: that is, the types of discourse which it accepts and makes function as true; the mechanisms and instances which enable one to distinguish true and false statements....the techniques and procedures accorded value in the acquisition of truth; the status of those who are charged with saying what counts as true" (Foucault, 1980, 131).

When one makes the jump from these kinds of conclusion to American textbook discussions of psychological methodology one not only knows one has landed on a different continent, one also feels one is in a different century. That simple faith in the timeless effectiveness of certain techniques, mistakenly identified with *the* scientific method in the singular, may be quite touching, but it certainly seems to have more in common with the world of the late nineteenth than that of the late twentieth century.

The question is whether we can afford to go on educating our students in this way. If we do, are we not perhaps consigning them to the role of low level technicians who are unable to demonstrate much understanding of the wider implications of what they are doing, and hence are increasingly less likely to be consulted about those implications? Of course, no-one is suggesting that technical questions are no longer important, or that "anything goes". What I am suggesting is that in this day and age a purely technical training is not enough. If they are to apply them wisely and creatively students need to be able to put their technical skills in perspective, and for that they need something more than technical training, they need a broad, interdisciplinary, education in methodology.

NOTE

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