Psychology for Whom? (1987)

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The question I would like to address concerns the uses of psychological knowledge. Let us grant that psychological research and practice generates something we are prepared to accept as knowledge. Then we are left with the question of what all this knowledge is for. Does it do anyone any good, and if so, how?

Would it serve as an answer to say that any knowledge is a good thing in itself? Yes, if we were talking about a culturally isolated community entering the modern world, but that is hardly our own case. Our problem is one of a surfeit, at least of factual knowledge. None of us can keep up with the torrent of new knowledge except in relatively tiny areas. Even among specialists the utilization of new knowledge is limited. We know that most research papers are never referred to again after publication, except occasionally by their authors. And knowledge that never affects anyone or anything but its author's career chances does not seem to be a matter of great public value.

Moreover, there is the troubling issue that is so entertainingly illustrated by the game of Trivial Pursuit. Undoubtedly, this game is about knowledge. But for most of us it is knowledge with very little, if any, intrinsic value. One could, without the slightest difficulty, construct a vast game of Trivial Pursuit from the information to be found in the pages of psychological journals. Perhaps it has even been done. Perhaps it is done every time a multiple choice examination is constructed. But of course that is not how the production of psychological knowledge is justified. The existence of knowledge that is fit only for games and entertainment illustrates something quite profound, namely, that not all knowledge is on the same level. Some of it is indeed trivial, and when we assign some essential value to the generation of knowledge we would presumably want to limit ourselves to non-trivial knowledge.

But then we come up against the fact that triviality is obviously relative. For most of us the information in a game of Trivial Pursuit may indeed be trivial, but not for all of us all the time. For some members of some professions, e.g. journalists or people active in the entertainment industry, some of this knowledge may be quite significant at times. So the value of knowledge depends on the people who can do something with it and on what it enables them to do. Therefore, if we want to answer the question of what a particular body of knowledge is good for, we must look for the people who use that knowledge and then inquire what they use it for.

What applies to existing knowledge applies equally well to its production. Knowledge is produced, not to be scattered at random by the breeze, but to be circulated among and applied by a specific audience. In our society the channels for the dissemination of psychological knowledge are highly structured and institutionalized. There are journals with a limited and defined readership, textbooks targeted at specific groups of students, and so on. This means that the producer of such knowledge has a very good idea of who the audience is going to be and what they will expect of the product. That is bound to affect the nature of the product. Producers of scientific knowledge never work as independent individuals but are enmeshed in a

network of social relationships. What they initially produce is not so much knowledge as knowledge claims. Such claims are only transformed into knowledge by an acceptance process that involves a number of individuals, reviewers, readers, textbook writers etc., that share certain norms and interests. Naturally, the anticipation of this acceptance process affects the production of knowledge from the beginning.

This social system for the production of a certain kind of knowledge is always in process of change. There are fashions in research as in so many other areas. What was acceptable yesterday is no longer acceptable today and vice versa. Beneath the ripples of fashion, however, there are deeper currents that extend over much longer periods and have much more lasting effects on a field. The contemporary researcher, practitioner, or consumer of psychological knowledge is aware of the system only as it affects his or her current work. Typically, the demands of the system of knowledge production and distribution are simply taken for granted, or perhaps are seen as requiring reform in certain specific respects. If, however, we want to understand why psychological knowledge production has taken on the shape that it has, we will not be able to dispense with a historical perspective. It is only when we inquire into how our system of psychological knowledge production originated that we will be able to understand where the forms come from that are taken for granted today.

Let me therefore go back briefly to the early days of modern psychology. Any new group of knowledge producers, like scientific psychologists at the beginning of this century, must come to terms with a world that already contains knowledge producers that make related claims, like medical professionals, educationists, experimental physiologists, and so on. These established groups wield a certain amount of social power, and that power is based on their institutionalized monopoly over certain types of knowledge product. How can a new group of knowledge producers thrive under these conditions?

It can thrive only if it manages to form effective alliances. This it can do by enlisting the interest of established groups in its knowledge products and avoiding their censure, a process that has many facets. First of all, the new knowledge products had better reflect well established preconceptions about the forms of valuable knowledge. If quantitative knowledge is particularly valued, then it helps to establish new claims if one can give them a quantitative form. But established preconceptions about the form of knowledge products automatically extend to the methods used to generate them, for the form of the product depends directly on the nature of the method of production. So one must be seen to be engaging in practices that produce the right type of knowledge, even though such rituals may have more in common with magic than with science. More specifically, the borrowing of techniques from better established fields provides a basis for limited alliances which link the new field and its products to the existing network of recognized scientific knowledge.

But this kind of alliance would provide only a limited basis for the development of a new discipline if its knowledge products were not seen as having a significant social value of their own. They must become marketable, and that means that there must be categories of persons to whose interests the new product is able to appeal. The more powerful and better organized these consumers of knowledge products are, the more successful the producers will be in consolidating their own position. American psychologists scored some real successes in this direction by

providing knowledge products that mobilized the interests of educational and military administrators as well as the administrators of private foundations. Even if the alliances so formed were often temporary, and based more on promissory notes than on real goods, they served an important function in establishing the credentials of the new discipline at a critical stage in its development.

The successful establishment of a new discipline is very much a "political" process in which alliances have to be formed, competitors have to be defeated, programmes have to be formulated, recruits have to be won, power bases have to be captured, organizations have to be formed, and so on. These political exigencies necessarily leave their mark on the discipline itself because they largely determine what types of knowledge product can be successfully marketed at a particular time and place.

Almost from the beginning of the twentieth century psychology ceased to be a purely academic discipline and began to market its products in the outside world. That meant that the requirements of its potential market were able to influence the direction in which psychology's practices were likely to develop. Practices which were useful in the construction of specific marketable products were likely to receive a boost, while practices which lacked this capacity were henceforth placed under a handicap. Many American psychologists wanted to see their discipline transformed into a socially useful science. But as soon as they tried to convert this ideal into practice they had to accommodate themselves to the specific opportunities offered by a particular historical context. In principle the possibilities of applying psychological knowledge might be unlimited, but the actual possibilities available here and now are always sharply circumscribed. They depend on existing institutional forms and on the requirements of those who can command the social resources for putting psychological knowledge to work. For example, the first important market that opened up for the products of psychological research was in the field of education. However, the people who were the major consumers of these products were not parents, and not even classroom teachers but educational administrators.

To these clients research meant something that was rather different from academic psychologists' traditional laboratory practice. Research, to the administrators, was an activity whose results had to be relevant to managerial concerns. It had to provide data that were useful in making immediate decisions in restricted bureaucratic contexts. This meant research which produced essentially statistical information on relatively large numbers of individuals. What was definitely excluded was research which went beyond the given human and social parameters within which the administrators had to make their decisions. It was, in other words, technological research that would help in dealing with circumscribed problems defined by currently unquestioned social goals. Not infrequently, administrators simply needed research for public relations purposes, to justify practices and decisions which they judged to be expedient. What they needed were scales that measured intellectual performance and experiments that assessed the relative effectiveness of such environmental conditions as were potentially under bureaucratic control. The primary consumers of the results of such research were those whose controlling position made the results potentially useful to them, least of all the children who participated in the research as subjects.

A significant number of psychologists quickly responded to the requirements of this type of research. What they did not anticipate was the fact that the attempt to apply psychology to the requirements of educational administration would have consequences for psychology that were far more profound than any contribution psychology was likely to make to education. In the first place, it meant a decisive break with the kind of educational psychology that had been envisaged by the pioneering giants of American psychology, by James, Baldwin, Hall and Dewey. Their broader vision was now replaced by a much narrower and purely instrumental conception of what psychology could accomplish. The institutional constraints which the new educational psychologists took for granted required them to emphasize the passivity of the child and to restrict themselves to measured performance. Precious resources were not to be wasted on an exploration of mental processes that had no obvious utility in terms of bureaucratic goals.

The new pattern of psychological research practice which originally crystallized in the work of educational psychologists proved to be increasingly attractive to other psychologists in the years that followed. It was the experience gained in research for educational administration that made it possible for psychologists to exploit the professional opportunities presented by the large scale requirements of military bureaucracy in both World Wars. Without the methods of mental measurement already tried out within an educational context American psychologists would have had nothing immediately useful to offer the military authorities. Virtually the entire field of applied psychology now came to be defined in terms of psychological knowledge that would be useful in administrative contexts. Moreover, the conviction that psychological research had to produce the kind of knowledge that would be potentially applicable in certain practical contexts characterized an important and growing section of academic psychologists. What this concern with useful knowledge generally came down to in practice was the desire for knowledge that was marketable in bureaucratic administrative contexts.

In the broadest terms, the kind of knowledge that was most obviously useful in such contexts was statistical knowledge. Information about individuals was generally of interest only insofar as it pertained to the categorization of individuals in terms of group characteristics. Dealing with individuals by categories constitutes the essence of bureaucratic practice. In fact, the historical origin of statistics (including the term itself) is very intimately bound up with the practice and rationalization of public administration, and psychological statistics are no exception. Mindful of the need to market the products of their research, applied psychologists avoided the exploration of individual mental life and limited themselves to assigning individual performance a place in an aggregate of performances.

It became fashionable to refer to this new, administratively relevant, psychology as the psychology of individual differences, and its major practical application was the field of mental testing. But the reference to individual differences hides more than it reveals. The term "individual differences" taken in isolation from a specific context is exceedingly vague and could just as easily apply to the work of the novelist as to that of the psychologist. Clearly, when mental testing is derived from an interest in individual differences it is not this very general meaning that is relevant but a very specific meaning which is conveyed by the context. It was an interest in looking at individual differences in a particular way that found expression in the development of mental tests.

Indeed, the investigation of individual differences preceded the use of large scale mental testing by many years. This earlier interest, however, approached the topic of individual differences in terms of questions of individuality and typology. What the development of mental testing did was to redefine the problem of individual differences. It was no longer conceived as a problem of describing individuality or of analysing typological patterns but as a matter of specifying the individual's position with respect to an aggregate of individuals. This meant that the individual was now defined on the basis of the properties of an aggregate. The characterization of the individual depended just as much on the performance of a set of others as it did on anything she did herself. Moreover, the whole exercise depended on the assumption that the salient qualities for characterizing an individual were qualities which she shared with others rather than qualities unique to herself. These common qualities had to be thought of as constant elements whose nature was unaffected by their co-habitation with other such elements in the same individual. Carried to its logical conclusion this methodology for assessing "individual differences" actually eliminated the individual by reducing him to the abstraction of a collection of points in a set of aggregates.

While the statistics of individual differences constituted the very antithesis of an interest in psychological individuality, they were able to speak quite directly to another kind of concern. This was the problem of conformity. The new psychological practice was based on the setting up of "norms" in terms of which individuals could be assessed. In most cases these norms were psychological only by inference; in the first place they were norms of social performance. The categories in terms of which individuals were graded were not generally socially neutral categories but carried a powerful evaluative component. Because categories like "intelligence" embodied very specific social definitions of what was desirable the normative study of individual performance became a matter of establishing who would most effectively conform to certain socially established criteria. These criteria ranged all the way from the unidimensional "general intelligence" of the eugenicists to the qualities needed in a good salesman. But they were always criteria that only made sense in the context of particular social interests, be they grand and ideological or practical and mundane.

For the new style of psychological research the individual was of interest only in terms of his or her standing in an aggregate. Research objectives largely shifted to the comparison of such aggregates and the statistical relationships between them. In certain practical settings ("psychological clinics") a more individualized employment of mental tests, closer to the original vision of Binet, did continue. But insofar as such practice claimed to have a scientific basis, that basis was also statistical. Even though individual patterns might be considered, they were still patterns of performance defined in terms of common group norms.

At this point we have to pause to remind ourselves that the activity of psychologists was not only constrained by the available possibilities for marketing their knowledge products but also by their concern to establish and then improve their claim to the status of scientists. What the individual investigator did had potential social consequences for others who shared his or her professional identity. The kinds of research practices with which psychologists were associated served to distinguish psychologists from certain neighbouring disciplines and professions with which there was actual or potential competition, and they served to draw a sharp line between

experts and laymen. Above all, however, research practices were crucial for legitimating the scientific credentials of the discipline.

It must be emphasized that the criteria for being recognized as scientific had relatively little to do with how the established sciences actually achieved their successes. That involved a set of very complex issues which remain controversial to this day. What was socially important, however, was the widespread acceptance of a set of firm convictions about the nature of science. To be socially effective it was not necessary that these convictions actually reflected the essence of successful scientific practice. In fact, the most popular beliefs in this area were based on external and unanalysed features of certain practices in the most prestigious parts of science. Such beliefs belonged to the rhetoric of science rather than to its substance. They clustered around certain unquestioned emblems of scientificity like quantification, experimentation, and the search for universal (i.e. ahistorical) laws. A discipline that demonstrated its devotion to such emblems could at least establish a serious claim to be counted among the august ranks of the sciences.

The success of psychology as a discipline therefore involved two sets of problems with often diverging implications. On the one hand, there was the need to develop practices whose products would answer to the immediate needs of socially important markets. But on the other hand, there was the need to establish, maintain and strengthen the claim that what psychologists practiced was indeed to be counted as science. These two requirements could not always be easily reconciled, and so it was inevitable that there was conflict within the discipline with some of its members placing relatively more emphasis on one or other of these directions. But in the long run the two factions depended on one another, rather like two bickering partners in a basically satisfactory marriage.

We can regard the products of psychological research as grounded in two sets of knowledge interests. On the one hand, there is an interest in producing the kind of knowledge that appears to be practically useful to certain potential consumers of the knowledge outside the discipline. But on the other hand, there are interests which aim at the advancement of the discipline, both in a cognitive and a social sense. What is desired here is knowledge which will further the cognitive and technical control of the discipline over its subject matter and which will improve its status among the sciences.

Although an emphasis on practically useful knowledge is more salient for applied psychology, while concern with scientific standards is primary in pure research, the two parts of the discipline have always depended on each other. Applied psychology thrives on professional alliances and the creation of markets for its products. But to survive in this competitive environment it has to maintain at least a modicum of scientific credibility. Psychology as a whole must have earned sufficient respect as a knowledge producing enterprise to be a serious candidate for competition or for alliance with other professionalized fields. That means its products must have become clearly distinguished from the everyday or common knowledge and belief of the lay public and achieved the status of "expert" knowledge. There is nothing more inimical to a field's success as a source of valued knowledge than the suspicion that it is able to supply no more than a duplication of what "everyone" knows anyway, or worse, a reinforcement of popular superstitions. The "soft" areas of psychology and parapsychology have always had to contend with this problem and have usually had to ride along on the back of the core discipline.

However, there would have been no core discipline to be carried along by if there had not been constant vigilance about maintaining the sharpest possible differentiation from folk knowledge. This was not a problem limited to some marginal areas but affected the discipline as a whole. For does not everyone have to rely on psychological knowledge in making his or her way through the world? How could anything offered by experts compete with a lifetime of experience in human affairs? The effects of this ever present background challenge on the investigative practices of psychologists should not be underestimated. Whatever else they may have done these practices also served to demonstrate a crucial "distance" from those mundane situations in which everyday psychological knowledge was acquired. This was achieved largely by drawing on the mystique of the laboratory and the mystique of numbers, both of which had been well established prior to the appearance of modern psychology. The very artificiality of laboratory situations became a plus in establishing the credentials of knowledge claims emanating from this source, and the imposition of a numerical form on otherwise trivial knowledge gave it an apparent significance with which lay knowledge could not compete. Replacing ordinary language with jargon helped too.

Nevertheless, the relationship between expert and everyday knowledge had other aspects. For one thing, research practices involving human subjects were unavoidably social practices and as such were not as clearly distinct from extra-laboratory practices as the experimental mystique made out. In order to generate psychological knowledge, if one was not to rely solely on self-observation or on animal analogies, one had to set up social situations involving clearly patterned relationships among the human participants. These situations resembled certain more familiar social situations, not only because they were born in already established institutional environments, but also because the type of knowledge product desired had affinities with other knowledge products gathered in those environments. Clinical experiments emerged in a clinical environment that had preformed the social relationships among investigators and their "subjects" as well as the shape of the knowledge product that resulted from their interaction. Similarly, mental testing mimicked the social situation of a school examination in quite a recognizable way, and its product, the objective grading of individual performances, represented the optimal result of an idealized examination. In the last analysis psychological investigative situations constituted a development of already existing social practices.

Applications of psychological research products tended to fall along a scale from specific applications in mundane situations to grand claims about human nature that had profound implications for social policy. Among the latter one would find claims about the existence and heritability of "general intelligence" or claims about the origins and social role of race prejudice. At the other end of the scale there would be local decisions about individuals on the basis of test results, or improvements in human engineering. Now, it is easy to see that the two kinds of application differ in terms of the distance between the investigative situation and the situation to which the research products are to be applied. In the case of grand applications to social policy there is a vast distance between the situations in which the research products were generated and the kinds of situation to which they are to be applied. This distance has to be bridged by a host of unproven and often unspoken assumptions, and thus the whole enterprise is essentially based on leaps of faith. But in the case of narrow, technical applications of psychological knowledge there is typically a considerable degree of continuity between the investigative situation and the

situation in which the application takes place. When one applies intelligence or aptitude test results to the prediction of future performance in the appropriate settings, academic or otherwise, one is essentially using a "simulation" technique. The more effectively the investigative context simulates the context of application the better the prediction will be.

However, we should note that there are two ways in which the distance between the context of investigation and the context of application can be narrowed. Letting the context of investigation simulate the context of application is one way, and certainly the more easily recognized way. But in many cases of successful application of psychological knowledge one can also detect a reciprocal process which involves a change in the context of application so that it comes to resemble the context of investigation. The conventional model of the application of scientific knowledge involves a fixed context of application, but of course this is not true in the real world. Rather, the application involves an actual change of practices which are transferred from the artificial laboratory setting to the world outside. In other words, the application of knowledge is possible only insofar as an artificial construction, derived from the investigative situation, is imposed on the natural world. This is what happens in the construction of industrial plants or in the adoption of sterile conditions in medicine, for example.

An analogous process operates in the case of psychological knowledge. Applying mental tests often meant modifying some of the practices of schools so that they resembled testing practices more closely. Having spawned intelligence tests in the first place some school examinations subsequently imitated their offspring in terms of such matters as timing, question format, use of statistical norms, and so on. Or, to take another example from a later period, the adoption of behaviour modification programmes in institutions involved the restructuring of the context of application so that it more closely resembled a particular research context. Practices have always wandered from non-research to research contexts and back again. Genuine application of psychological knowledge depends on this, for the bond between knowledge and the practices with which it is associated is an extremely intimate one. Abstract knowledge only exists abstractly; its application requires a transfer of corresponding practices. So if the purely ideological application of psychological knowledge in support of particular social policies were ever to be converted into a real application that affected its objects directly it would require an appropriate reconstruction of society, as B. F. Skinner so clearly demonstrated in "Walden Two".

The distance that separates research situations from the situations in which their knowledge products are to be applied leads to a serious problem. On the one hand, the rhetoric of science requires that this distance be emphasized and magnified. Because the yield from investigative situations was supposed to consist of universally valid generalizations (so-called nomothetic laws) these situations were endowed with a mystique that rendered them so remote from ordinary life that they were not even seen as social situations. Even the idea that there might be a social psychology of psychological experiments only arose at a late stage in the development of the discipline. However, there remained the rather indigestible fact that the discipline's ability to make fairly reliable predictions about human beings outside the psychological laboratory depended to a large extent on the closeness of the context of investigation to the context of application. More often than not, psychological knowledge had some technical utility only insofar as its investigative practices were continuous with relevant social practices outside the investigative situation.

This contradiction between scientistic rhetoric and the facts of life in applied psychology tended to maintain the separation of "pure" and "applied" research. Politically, both the rhetoric of science and specific technical utility were however indispensable for the rapid development of the discipline and both continued to flourish side by side. In this way each partner could profit from the value attributed to the activities of the other. "Pure" research could claim support on the basis of its ultimate practical usefulness and "applied" research could speak more authoritatively by clothing itself in the mantle of science. In actual fact, "applied" research usually relied on its own practices with little or no help from "pure" research, and "pure" research showed a distinct tendency to adopt some of the crucial practices of "applied" research, distinguishing itself mainly by technical sophistication and a more abstract terminology.

Here we have another illustration of the major point that has formed the basis of these reflections: The building of a discipline like psychology is not something that takes place in a realm of disembodied ideas but involves the social activity of specific groups of people who have particular interests and who have to operate in a political environment, like everyone else. The peculiarities of their historical situation are reflected in the kinds of practices they adopt and in the kind of knowledge which results from these practices. Knowledge, including psychological knowledge, is produced by and for people with particular social identities and hence particular social interests.

In taking this position with regard to psychological knowledge I have merely been extending some perspectives that have been opened up by recent work in the sociology, history, and philosophy of science. If psychology is to be counted among the sciences it cannot escape this kind of critical scrutiny. Now, what has tended to happen in the field of science studies is a kind of demystification of science. There was a time when the activity of the scientist was seen only in highly idealized terms, but the more it has itself become the subject of systematic examination by sociologists, historians etc., the less distinguishable it has seemed from more mundane social activities.

Inevitably, this has affected the way in which we think about the source of scientific achievements. No longer are we convinced by the old heroic image of the individual investigator who unlocks nature's secrets in single-handed pursuit of the truth. More sceptically - and surely more realistically - we are likely to see models of reality emerging out of the collective interaction of groups of investigators with each other and with other groups in society.

If we apply this perspective to the development of modern psychology we are led to a recognition of the fundamental role played by the two factors I have emphasized here: The need to produce administratively useful knowledge, and the need to imitate the practices of the more prestigious sciences. These were not the only factors involved in the construction of modern psychological knowledge, but their crucial importance cannot be overlooked. They have given much contemporary psychological knowledge its characteristic shape and are responsible for its characteristic limitations.

At the present time it is not uncommon to hear proposals for the reconstruction of psychology along lines that are meant to avoid the limitations of more traditional models of psychological

knowledge. But from the perspective developed here it would be a mistake to regard such a reconstruction as a purely intellectual matter. Psychology will change only insofar as its social alliances change. For example, institutions that are interested in psychology as a basis for people management have not been the only consumers of psychological knowledge. Individual consumers, who are interested in psychological knowledge as a means to achieving self-understanding, constitute another group. They tend to demand and to get a very different kind of psychology, one that is likely to rely much more on psychoanalytic or phenomenological sources. The shape of the discipline as a whole is likely to reflect the relative importance of different types of consumer interest in its products.

Each variety of psychological knowledge will have its own appropriate field of application and its own limitations. There is nothing strange about that. What is strange is the notion of a single body of abstract psychological knowledge that is valid in all contexts and for all purposes. It is not by chasing after this chimera that the discipline will make a real contribution to matters of major human or intellectual significance. That kind of achievement is more likely to come its way if it extends its alliances and thereby transcends the limitations inherent in everyone of its specific incarnations.

It is possible to identify a number of possibilities in that direction which have opened up in recent years. I will briefly mention three. Firstly, some feminists have introduced novel perspectives into the field which have already enriched it. Secondly, the field of community psychology has demonstrated that the consumers of applied psychological research need not be limited to traditional bureaucratic elites. Thirdly, the spread of psychological research to societies other than those in which it originated has sometimes linked it to a new range of concerns which have revealed the cultural bias that underlies much traditional work. All these cases have this in common, that the content of psychology has become influenced by new groups whose social interests and presuppositions are very different from those which dominated the first century of the discipline's modern existence. There are certainly sections of the discipline, like physiological psychology, which are relatively impervious to this sort of effect. But the discipline as a whole, including the place accorded to areas like physiological psychology, remains firmly dependent on the preferences generated by specific social interests.

If the history of psychology teaches us anything, it is that psychological knowledge can take different forms. Which of these forms predominates at a particular time and place depends on the interests, concerns and assumptions of those who have sufficient social influence to leave their mark on the generation of psychological knowledge. In other words, there is a political, and hence also a moral, dimension to the process of knowledge production. Pretending it isn't so will not alter the historical reality but will certainly prevent us from developing an appropriate response to it. We can discover a great deal about psychology by studying its past - but only if we ask the right questions.

NOTE

Michael Kiernan Lecture given at the University of Saskatchewan, 1987.

SUGGESTIONS FOR FURTHER READING

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