

What I Wish I Knew in 1950 (1997)

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Abstract: In 1950 I was a graduate student doing research in the field of motivation. My work led me to doubt the existence of a separate category of motivational processes clearly distinct from other psychological processes. However, I did not suspect that this doubt might be appropriate for all psychological categories until I encountered an indigenous psychology in Indonesia that operated with an entirely different set of categories. In the course of my more recent historical research it became clear to me that psychological categories could not be regarded as *natural kinds*, reflecting immutable psychological distinctions, but that they were culturally bound. Psychological language is an important part of a *context of construction* within which psychological phenomena are constituted. Had I understood this in 1950 I would have had a better grasp of what my research meant and been more circumspect in the choice of my empirical questions and in the interpretation of my results.

Perhaps I should begin by explaining the title of this talk. The phrase "I wish I knew that before..." is one that most of us have uttered at one time or another "I wish I knew that this house had a leaking basement before I bought it"; or, "I wish I knew I'd be landed on top of an all-night disco when I booked a room at this hotel"; or, "I wish I knew that someone else had already finished a book just like the one I was planning to write". The occasions for such regrets are legion, some of them trivial and some rather more serious. Certainly, not many weeks have passed when I have not said to myself, if not to others, "I wish I knew that before".

So why do I pick on what I wish I knew in 1950? Why 1950? Well, at a time when millennial Angst is everywhere (even the world of computers has its own version of it) it does not seem altogether inappropriate to mark the year of the half century as a base line for assessing the direction of current change. Any such assessment is of course going to be a subjectively slanted affair, and that leads me to a more significant reason for picking 1950. In that year I was a graduate student at the Institute for Experimental Psychology in Oxford, and I was writing my first scientific paper, published the following year under the title of "The operation of an acquired drive in satiated rats" (Danziger, 1951). So I was then on the threshold of an academic career that was to coincide, more or less, with the second half of the twentieth century.

But this threshold had more than a purely personal significance. In 1950 the discipline of psychology was beginning to embark on a period of extraordinary expansion and professionalization. A decade earlier psychology was still a relatively small and predominantly academic discipline characterized by profound ideological and national divisions. By 1950 the life of the discipline had just taken on the form that was to characterize it for most of the remaining half century: A global enterprise dominated by American models of professional practice, theorizing and research.

Although I did not realize it at the time, I was doing my bit to spread the new way of doing psychology, at least in terms of a certain style of theorizing and research. My research was conducted within what was then still a novel framework of experimental design and analysis of

variance. My theorizing was constricted by formalistic rules regarding the so-called hypothetico-deductive method. Time has not been kind to these features. Although they are still quite popular in some of the old-fashioned backwaters of our discipline, they have become, in my opinion, rationally indefensible. But these are not the developments I want to talk about today. There is a large critical literature in this area, and it would be going over old ground to add to it.

I would like rather to concentrate on some other features of my work in 1950 in order to draw attention to certain problematic aspects that, I believe, have not been worked through in the subsequent critical literature of the discipline. The methodological rules and strictures I mentioned a moment ago were *explicit* features of a certain style of psychological research. They were clearly spelt out in programmatic articles and texts and were therefore relatively straightforward targets of criticism. But, beyond these rather obvious features there were more subtle assumptions *implicit* in the kind of work I was committed to in 1950. What I want to concentrate on today are one or two of these taken for granted, implicit, assumptions that I shared with most of my psychological cohorts.

As you may have gathered from the title of the paper I quoted, my research in those days was dedicated to providing knowledge in the area of motivation. My doctoral dissertation had the word "motivation" in its title, my first paper, the word "drive". At the time I began this work I did not for a moment question that these words each referred to a distinct aspect of an objective psychological reality. "Motivation" referred to an area of psychological phenomena that could be clearly distinguished from other such areas, for example learning or emotion, and "drive" referred to a specific mechanism or set of mechanisms that was different from other psychological mechanisms, for example, the formation of associations.

However, quite early in my work this view received a jolt with the publication, in 1949, of Donald Hebb's *The Organization of Behaviour*. In that book Hebb explicitly rejected the distinctness of such a process as "motivation". He wrote: "When the experimenter takes (further) steps to limit the variety of conceptual activity that will occur in an animal he sets up a *motivation*. The term motivation then refers (1) to the existence of an organized phase sequence, (2) to its direction or content, and (3) to its persistence in a given direction, or stability of content. This definition means that "motivation" is not a distinctive process, but a reference in another context to the same processes to which "insight" refers" (p.181). On Hebb's view then, the term "motivation" no longer describes some basic and distinct aspect of psychological reality but merely provides a convenient, though superficial, way of referring to certain practical experimental manipulations.

Hebb's view was considered quite controversial at the time. Most American psychologists ignored it and continued to treat motivation as a fundamental sub-division of psychological reality, doing research on motivational mechanisms, writing texts on so-called "principles" of motivation, and so on. My own reaction to Hebb's approach was however quite positive. I thought that his argument for the non-distinctness of motivational processes made a lot of sense, and the outcome of a series of experiments I conducted at the time reinforced this judgement. By the time I had completed my doctoral research in 1951 I was convinced that "motivation" was one category that a system of basic psychological principles could do without.

I also knew something else, though its significance escaped me at the time. I knew that "motivation" was actually quite a novel term in scientific psychology. In 1950 we were still close enough in time

to the first appearance of the term to realize that it had a very brief history. The first general text featuring the word "motivation" in the main title, was published in 1928 by the Harvard psychologist, L. T. Troland, (*The Fundamentals of Human Motivation*). In the same year the editors of *Psychological Abstracts* apparently felt that it was time to give "motivation" its own entry in their index and introductory texts began to add a chapter on this topic to their survey of the discipline (e.g. Dashiell, 1928; Hollingworth, 1928; Perrin, 1932). By 1936, the author of a new authoritative volume on motivation was able to mention its use as a text in a college course devoted to this topic (Young, *Motivation of Behavior: The Fundamental Determinants of Human and Animal Activity*).

All this was not hard to discover if one conducted a normal literature search in 1950, though as the critical year 1928 receded ever further into the dim past the recent arrival of the category of "motivation" tended to be forgotten. Yet, before the second quarter of the twentieth century, psychology managed without this category. You will not find an entry for "motivation" in Baldwin's turn of the century multi-volume dictionary of philosophy and psychology (Baldwin, 1901). In other words, whatever else it is, the category identified as "motivation" is a twentieth century category - it arose in a particular historical context.

This much I was already aware of in 1950. But my awareness had its limits. For instance, I do not think I realized then that the term "motivation" was a newcomer, not only within psychology, but for all users of the English language. If you look it up in a good dictionary you will find that its general use hardly antedates the twentieth century. The term "motive" is older, but the verbal form, "to motivate", and the abstract form "motivation", are not documented as having occurred before the late nineteenth century. Even then, these forms are quite rare and the context of their use is literary rather than psychological; there is reference to the motivation of events in a novel, for example. It is only in the twentieth century that there occurs a veritable explosion in the use of the verbal form "to motivate" and of abstract derivatives like "motivation".

When one explores the early twentieth century history of "motivation" one finds that both the term and the concept were around for a number of years before scientific psychologists adopted them. They were not in general use, but they had currency in a very special area, namely, the growing literature on the improvement of advertising and salesmanship, industrial efficiency, teaching practice, and personal advancement. In that literature it was increasingly recognized that the goals of advertisers, salesmen, teachers, and efficiency experts could only be achieved if one knew how to play upon what individuals wanted, what they were interested in, what they privately wished for. It was precisely in this context that the previously obscure verbal form "to motivate" was given its specifically modern meaning and began to be employed widely. So we find, for example, that as early as 1917, about one half of a book directed at ambitious managers and salesmen, entitled *The Executive and his Control of Men* (Gowin, 1917), was devoted to what was called "motivating the group".

Initially, everyday terms like desire, want, interest, and also motive, were used to represent what it was that salesmen, teachers, managers etc. had to influence. But as the discourse of social influence grew and became more generalized it required a general term to refer to the entire variety of personal direction as a potential object of external influence. By the nineteen twenties "motivation" had come to play that role. The situation was quite clear to those who began arguing for a

psychology of motivation at that time. An early contribution to this argument, published in the *Psychological Review*, begins as follows:

A rather insistent demand for an adequate psychology of motivation has always been made by those who are interested in the control of human nature. It has come from economists, sociologists, educators, advertisers, scout masters, and investigators of crime; more recently it has been voiced by certain psychologists, particularly those interested in personality and character, and in the various applied phases of the science. (Perrin, 1923).

Early textbooks of motivation continued to show awareness of these roots in introducing their topic. Troland (1928: 1) begins his pioneering treatise by referring to the businessman who "wishes to know how to play on the motives of other men so that they will purchase his goods and services". Young (1936: 2) says disarmingly: "We all desire to influence and control human behavior - our own and that of others", and follows this with a tale about a student who applied "scientific motivational principles" to his work as a salesman "and before the semester was over had won a national prize in salesmanship".

Of course, there had always been words referring to different facets of human intentionality: wish, desire, want, will, motive, and so on. These were usually invoked when it was a matter of accounting for one's own, or others', deviation from the automatic, habitual patterns of action that characterize everyday life. "Motivation", however, departed from this usage in setting up an abstract category that grouped all the older referents together, implying that they were all expressions of a common psychological reality which transcended all differences among social situations and even among biological species. There were now general "principles" or even "laws" of motivation to be discovered and taught.

Certainly, that is what I believed when I set out on doing research for my doctoral dissertation. By the time I had finished, as I have indicated, I was no longer so sure. Another way of putting this is to say that I began with the belief that "motivation" was what philosophers of science call a "natural kind", and that I ended up doubting this. A natural kind term reflects natural divisions among objective features of the world that exist independently of the efforts of scientists. This is certainly how most psychologists think of the categories they employ in their work, motivation among others.

If psychological categories are natural kinds this reinforces the status of psychology as a natural science. If they are not, then psychology must be regarded as a discipline whose categories are not reflections of an independent natural world but products of social life. In 1950 I was beginning to suspect that at least one psychological category, that of motivation, was not a natural kind, but it had not yet occurred to me that this might have wider implications than the field of motivation itself.

I am sure there were all kinds of reasons for this. But today I want to concentrate on just two of them. Both involve something I did not know in 1950. The first I have already mentioned. Though I knew that "motivation" had entered psychology relatively recently I did not know - and never asked - where it came from. Nor did I inquire into the circumstances of its adoption. My knowledge of these matters is a product of much more recent historical investigation. In 1950 I would have considered this kind of information to be irrelevant to my psychological research, just as many of my colleagues still do today. Now I do consider it relevant. For it is the categories we necessarily

employ in our psychological work that enable us to identify what it is we are investigating. It makes a crucial difference to the meaning of research results whether they pertain to natural phenomena or to cultural artefacts.

Compared to what I know now, there was a second missing piece in the way I saw things in 1950. I had become sceptical about the scientific status of "motivation"; what did not occur to me was the possibility that there were fundamental problems not pertaining specifically to motivation but to psychological categorization in general. Right at the beginning of my dissertation I had observed: "When they find themselves using the word 'motivation' some psychologists are inclined to forget that they are only using a convenient abstraction and begin to imply a separate and distinct mechanism called 'motivation' which exists side by side with other such mechanisms, called thinking, perception or learning." When I read over this statement now, I am amazed that it took me so long to see that it could be read in more than one direction, that the basic question was not just one of the separate reality of motivation but of the separate reality of any psychological category.

For the first inkling of this possibility, however, I had to wait the proverbial seven years. I had reached my conclusions about motivation by 1951. In 1958 I arrived in Indonesia for a stint as a visiting professor at a large university in Java. When I arrived to take up my duties, I discovered that a course on Psychology was already being taught by one of my Indonesian colleagues. This was not Western psychology, but something based on an extensive local literature that had roots in Hindu philosophy with Javanese additions and reinterpretations. So the students had a choice of two psychologies, one Western and one Eastern. I thought it would be a good idea if my Indonesian colleague and I organized some joint seminars in which each of us would explain our approach to the same set of psychological topics, followed by an analysis of our differences. However, I soon discovered that there were virtually no topics that were identified as such both in his and in my psychology.

I tried various topics: motivation, intelligence, learning, and so on. But the result was always the same. My colleague would not recognize any of them as domains clearly marked off from other domains. He granted that each of them had some common features, but he regarded these features as trivial or as artificial and arbitrary. Grouping psychological phenomena in this way seemed to him to be, not only unnatural, but a sure way of avoiding all the interesting questions. Similarly, I could do nothing with the topics he proposed. So we reached an impasse and the seminar series never happened.

What this taught me was that it was clearly possible to classify psychological phenomena in very different ways and still end up with a set of concepts that seemed quite natural, given the appropriate cultural context. Moreover, these different sets of concepts could each make perfect practical sense, if one was allowed to choose one's practices. My colleague and I could both point to certain practical results, but they were results we had produced on the basis of the preconceptions we were committed to. We knew how to identify whatever presented itself in experience because we each had a conceptual apparatus in place that enabled us to do this. The apparatus itself, however, seemed to be empirically incorrigible.

More recently, a whole field of investigation, known as "ethnopsychology", has grown up, which attempts to explore the way in which members of other cultures conceptualize the realm that we

categorize as "psychological". These studies have produced a mass of converging evidence on the non-universality of some basic distinctions that form the conceptual skeleton for our own conventions of psychological classification. Contrary to common belief, psychological categories do not occupy some rarefied place *above* culture but are embedded in a particular cultural context.

My experience in Indonesia had left me curious about how modern western psychology ended up with the categories that characterize it. I thought that one day I would like to devote the kind of time to this question that it deserved. I had to wait a long time for an opportunity to do that, but in recent years, having nothing better to do, I did get around to it. The results of my efforts have now appeared in a book I have called *Naming the Mind: How Psychology Found its Language*, published by Sage. This text is devoted to an examination of how and why modern psychology ended up with some of its fundamental categories, categories like sensation, intelligence, behaviour, learning, motivation, emotion, personality, attitude, and so on.

I think I was able to show how, at different times and in different places, psychologically significant categories have been constructed and reconstructed in attempts to deal with different problems and to answer a variety of questions, many of them not essentially psychological at all. Psychological categories were always relevant to the lives of those who used them, whether they were ordinary people or experts. Changes in these lives were accompanied by changes in psychological categories. Motivation, which I have been using as an example, is not peculiar in this respect. Although it is difficult to say that these categories represent *natural* kinds, what one can say is that they represent *relevant* kinds (Hacking, 1999). They are relevant to the people who use them, relevant to their concerns, their interrelationships with each other, their possibilities of action. There are factors in their lives which lead them to make and to emphasize certain distinctions and to ignore any number of others. Because people's lives change psychological categories have a history.

Let me return now to my earlier incarnation as an experimenter. When all is said and done, would it have made any difference if I had known in 1950 what I know now? Would it have made any difference if I had known that, not only the category of motivation, but all psychological categories are historical formations, the products of changing socio-cultural circumstances? One thing I am pretty sure of: It would not have stopped me from doing any empirical work. I think I would still have had an interest in experimentation, but my sense of what those experiments meant would have been different. I would have discarded the idea that my results reflected some essential truth about the nature of organisms. I would have recognized these results as being meaningful only within a particular discursive framework. Instead of beginning by taking this framework for granted I would have been aware of its historical roots, and this might well have made me reject it in favour of some other framework. Had this kind of knowledge been available to me it might well have made a difference to my choice of research problem, to the way I formulated it, to my identification of the theoretical issues for which my experiments were relevant, to the kinds of generalization I would have been prepared to make on the basis of my experimental results. At the very least, this additional knowledge would have increased my range of choice. Even if I had ended up doing exactly what I would have done in any case, I would have been doing it as a result of an intellectually informed decision and not as a result of an automatic acceptance of some received discursive framework. That seems to me more in accord with what I would call a scientific approach to problems.

I think one can generalize these observations by revising an old distinction in the philosophy of science, that between a context of discovery and a context of justification. That distinction was introduced in the nineteen thirties by the philosopher Hans Reichenbach as part of the conceptual equipment of logical positivism. Reichenbach (1938) recognized that although science was essentially a rational activity the progress of its work did depend partly on irrational factors. However, as these latter were not amenable to logical philosophical analysis they should be clearly separated from the truly scientific aspects of science in a kind of waste paper basket called the context of discovery. That way one could preserve the rational purity of science within what was called the context of justification.

These days we know that any context of justification depends on a framework of beliefs, traditions, choices, cognitive styles, cultural preferences, and so on, which cannot itself be rationally justified. It is this framework which makes the process of discovery possible. I refer to it as the *context of construction*. The constraints that govern the choice and formulation of problems, the conduct of investigations, and the interpretation of their outcomes form an indispensable context for the production of scientific theories and empirical data. Justification of the tenability of hypotheses and the validity of results can only proceed by operating within such a context of construction.

Half a century ago contexts of construction were largely invisible. But that has changed and modern studies of science are very much concerned with making such contexts the objects of scientific scrutiny. Contexts of construction have two aspects which seem to me to be crucial. One is the practical aspect, the highly regulated social activity of intervention which we know as the investigative practices of science. In the eighties my own studies of the context of construction were largely concerned with this aspect; an account of this work appeared in 1990 in the text *Constructing the Subject: Historical Origins of Psychological Research*. In the nineties I have concentrated on the second crucial aspect of the context of construction, the discursive aspect. Let me explain what I mean by this.

Among the instruments of psychological investigation the most basic one is often overlooked. It is language. Without language the other instruments could not be constructed, the results of investigations could not be described, hypotheses could not be formulated, and investigators could not arrive at a common understanding of what they were doing. Even a graph or a table of figures must be verbally labelled, that is interpreted, to convey its proper meaning. A scientific fact is always a fact under some description. The discursive framework within which factual description takes place is as much part of science as its hardware and its techniques of measurement. Any reference to the "facts of the world" has to rely on some discursive framework in use among a particular group of people at a particular time. Facts are there to be displayed, but they can only be displayed within a certain discursive structure. These structures provide the framework for labelling and categorizing both real and hypothetical objects under investigation. This categorization gives objects their identity and enables investigators to have a particular understanding of what they are doing.

Investigators, like everyone else, live in a world that has already been classified. What gives a particular sense to a term is its cultural and discursive context. But such contexts are not static entities. Being the product of human activity and interaction, they are always in flux; they change historically. And as the cultural and discursive context changes the categories that are embedded in

this context change too. This applies to the quasi-scientific categories of psychology as much as to the categories of lay discourse. All psychological categories have changed their meaning through history, as has the discourse of which they are a part. Any effect which empirical findings have on the conceptualization of psychological categories can only manifest itself through the medium of psychological discourse. Therefore, to gain an understanding of the categories in common use at present, we need to see them in their discursive context. And that means adopting a historical perspective.

The relevance of historical studies for the discipline of psychology seems to me to lie primarily in their potential for contributing to an understanding of the context of construction. As members of the discipline we have all been socialized to adopt certain prescribed practices and to communicate about our subject in terms of a specific received vocabulary. The nature and meaning of what we achieve depends on these practices and this vocabulary. We can certainly go on producing effects without ever reflecting on the context of construction that enables us to do so. But our understanding of what we are doing will be profoundly defective. For that to be remedied an appreciation of the historicity of our practices and our language seems to be indispensable.

So in the last analysis, what I wish I knew in 1950 is a bit more about the historical status of the context of construction within which I was operating. Of course, on one level this is just the old story of old age regretting the follies of youth. But I think that my sentiments have more than a purely autobiographical significance. As a graduate student my approach was not altogether untypical of what one might call the nineteen fifties spirit in psychology - a spirit of gung ho empiricism that was altogether unconscious of the historicity of its own experimental and linguistic practices. That spirit is still widespread, of course, and, just as in the nineteen fifties, it leads to the pursuit of psychological ghosts.

But there have also been many positive developments, particularly in fields that have relevance for psychology. Progress in our discipline has always been peculiarly dependent on developments in other fields, physiology, statistics, and computer science, to mention only the most popular examples. I think we would benefit if we paid more attention to developments in some other fields, particularly the philosophy, history and sociology of science. In that respect we were not well served in 1950. Today, the knowledge that could free us from the shackles of psychological essentialism is there for the asking. The more we avail ourselves of it the less likely we are to repeat the mistakes of the past.

NOTE

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