Problematic Encounter:

Talks on Psychology and History

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Introduction (2010)

When I was still a teacher of undergraduates, the beginning of each academic year was always a time for advising students on their course selections. I have long forgotten the details of these consultations, but there was one that remains in my mind to this day. It involved a young student who wanted to combine a heavy concentration of courses in psychology with an equally heavy concentration of courses on historical topics. However, she was worried that this would be regarded as a weird combination. Indeed, she said that her friends had told her it was weird and that casual comments by one or two faculty members had implied much the same thing. What did I think? Well, I thought it was a combination that made a lot of sense and advised her to go right ahead. But after she left it occurred to me that her friends had not been altogether off the mark. Simply on the basis of popularity they had actually been right. An intentional focus on psychology and history at the same time was quite unusual among our undergraduates – going back over the years I could hardly find a similar case. So yes, the combination was "weird" in the sense of being statistically pretty rare, but of course this left open the question of whether it was weird in some other sense, as being somehow irrational, or as trying to speak two different languages at the same time.

My student was clearly worried about whether her admittedly idiosyncratic choice was defensible on other than purely personal grounds. A lucky chance had brought her to the right place for reassurance on that point. For by the time she saw me I had made a career switch from empirical psychology to research and teaching that brought historical perspectives to bear on topics within the discipline of psychology. This work culminated in the publication of three books: *Constructing the Subject: Historical Origins of Psychological Research* (1990), *Naming the Mind: How Psychology found its Language* (1997), and *Marking the Mind: A History of Memory* (2008).

In the years between the first and the last of these books I also spoke about interrelationships between psychology and history at a number of scholarly meetings, usually because I had been invited to do so. Some of these talks hewed closely to what I was saying in my books. But on other occasions I used the opportunity to expand on what was being published there or to address additional and related topics that had not found their way into any of the books. Some of these talks were never published, several others were published in obscure places that are now very difficult to access. I have brought them together here so as to make them more accessible in a digital age and added introductory comments to provide some background from prior and from subsequent discussions relevant to the topics of the talks.

Where talks were published I have generally used the text of the published versions with a little editing and in one case (no. 12) considerable expansion. I have also included two texts (nos. 1 and 6) that were never delivered orally but formed part of discussions in written form. Think of them as virtual talks.

Chronologically, the first of these talks was delivered in 1990, the last in 2008. But their chronological order is much less significant than their thematic interrelationship. The fact that the last three talks here were also the last to be delivered does reflect the late rise to prominence of "historical psychology" in my own work, but apart from that the exact chronology of the presentations was mainly determined by extraneous and accidental factors. Hence the order in which they appear here is not of great significance.

In order to facilitate access to this material I have arranged the individual items into four sections, each with a different focus. The first section, labelled "Background", should make the reader aware of various themes that always formed part of the framework within which I was working, though they were not always addressed quite so explicitly. The second section, on "Psychological Objects", forms the core of this collection in that its components (chapters 4 to 7) deal with topics of fundamental importance for my approach to this field. In the third section the concern shifts from general issues to matters arising out of the specific historical situation faced by those with a professional interest in the history of psychology. The fourth section, as I have indicated, brings together some more recent contributions relevant to what, for want of a better term, I am inclined to call "Historical Psychology". Introductions to each of these sections follow.

A: Background

During the fourth quarter of the last century there were signs of an emerging scholarly interest in the special problems of the historiography of psychology. Within the discipline of psychology that development was a natural consequence of the growing institutionalization of the history of psychology as a recognized specialization.

Elsewhere I have described my own move into this sub-field in the 1970's (Danziger, 2009) but I was not the only one. Some years before, a new journal, *Journal of the History of the Behavioral Sciences*, had begun publication, and in 1968 *Cheiron*, the International Society for History of the Social and Behavioral Sciences, was founded. A few years later a similar development took place in Europe, forming an association that subsequently changed its name to the European Society for the History of the Human Sciences (ESHHS). Psychologists were the most prominent group in both these associations. The American Psychological Association had a new Division (26) for the history of the discipline. In due course the Canadian Psychological Association followed suit, and there were a few analogous developments in Europe. Certainly, this was a period when more psychologists were developing a more professional interest in the history of their discipline than ever before (for an overview, see Samelson, 1999).

In many cases this amounted to little more than invigorated antiquarianism, but for others the new historical sensibility had a marked *reflexive* aspect, that is to say, there was interest, not only in the past as such, but in the way this interest ought to be pursued and in how it might be justified by members of a discipline generally dedicated to ahistorical practices. Much of what had previously passed for scholarship in the history of the discipline had recently been subjected to extensive and withering criticism by historian J.M. Young (1966). This prod and the growing professionalization of the history of science in the ensuing years encouraged a somewhat higher level of sophistication about historiographic issues among a new generation of disciplinary historians. Terms such as "Whig history", "presentism", and "internalism" began to be bandied about.

Self-awareness as historians meant taking on board several issues that professional historians had had to confront for many years. During the last part of the twentieth century awareness of those issues formed a background to the work of a significant number of disciplinary historians though this was seldom reflected in public debate. When discussion did take place it was likely to be unreported or archived in obscure publications, as in the case of the first of the papers reproduced here.

My argument in this paper essentially addresses the "objectivity question" that had long divided historians. In the case of psychologist-historians consideration of this question was often clouded by the taking for granted of an empiricist epistemology that had long underpinned any training in natural-science psychology. This epistemology, with its strong fact-value and subject-object distinctions, had dominated the disciplinary history of modern psychology and was hard to shake off (Smith, 1998). Although not mentioned by name, it was the empiricist philosophy of history that formed the underlying target of my paper.

The second paper of section A provides an example of how a sensitivity to another historiographic issue, that of "justificationism", could lead to a questioning of unexamined implications in received historical accounts. Earlier twentieth century histories of psychology had usually taken the form of surface accounts of the historical succession of psychological theories and “findings”, generally linked to the efforts of notable predecessors and their success or failure at anticipating current knowledge. Such accounts seemed designed to avoid uncomfortable questions about the remarkable evanescence of theoretical advances in modern psychology and the problematic nature of cumulative progress. By avoiding such questions historical narratives could convey an implicit justification of claims for the modern discipline's status as a natural science.

It seemed to me, however, that these claims were themselves in need of historical contextualization. As the claims were based on modern psychology's adoption of scientific methods, it was this process that deserved further historical scrutiny. Accordingly, in work that preceded the present collection of papers, I explored some of the historical divergences and changes that marked the adoption of scientific methods by modern psychologists.

Just as psychologists’ professional practices had historical affinities with the practices of other professions (see Richards, 2002), so the variety of situations considered appropriate for the gathering of psychological knowledge had affinities with situations already familiar in other contexts, such as school examinations and medical consulting rooms. The variety of knowledge gathering practices was not evenly spread over different periods of time and in different social locations. On the contrary, I had found that there were very marked preferences for one or other type of investigative situation at different times and among different communities (Danziger, 1987, 1990). Because different investigative situations yielded different kinds of psychological knowledge it appeared likely that preferences for one model of investigation or another might well be linked to locally and historically varying interests in particular forms of psychological knowledge.

Any kind of method designed to obtain new knowledge about people would necessarily involve working with people, and there were many different ways of doing this. Which way was best?

That would depend on the kind of information you were looking for. In other words, human problems were not something that only existed *outside* the scientific enterprise, they were intrinsic to that enterprise itself. Investigators' use of certain methods rather than others, the way they structured their relationship with those who supplied the data to be analysed, and the effect of this relationship on the data, all involved human choices and expectations dependent on a wider social context. When information generated under these circumstances turned out to be useful under somewhat different circumstances this could be traced to structural similarities between what I called the context of investigation and the context of application.

The relevance to ordinary human experience of psychological knowledge gathered under special "scientific" conditions would depend on bridging the gap between these special conditions and the messy circumstances of ordinary human life. Solutions to this bridging problem were sought almost as soon as laboratories, experiments and methods labelled "psychological" appeared on the scene. Typically, these solutions were conceptualized in terms of "applied psychology", the choice of the term "applied" immediately implying a model for what was supposed to be happening, namely, a two-step process in which knowledge generated in the course of "pure" research was subsequently "applied" to real world problems.

However, when I undertook an analysis of the journal literature during modern psychology's first half century I encountered little or no evidence that the two-step model reflected what had actually happened in the relationship between the two parts of the new discipline. When experts identified with the discipline of psychology applied themselves to practical problems in fields like advertising, personnel selection and legal testimony they found the limited results of academic psychological research of little help and developed their own approach to these problems. In the course of time, their innovations affected the way academic research was conducted, exactly the reverse of what the term "applied psychology" implied. This term and its underlying two-step model of scientifically based technology seemed to serve essentially rhetorical functions, insinuating that the new psychology would be useful in human affairs in much the same way as physical science had proved its usefulness for problems of industrial chemistry and engineering.

In 1990 I used the opportunity presented by the 22nd International Congress of Applied Psychology to present this argument in the way it appears in the second paper of this section. As far as it goes, I believe the argument still holds, certainly for the first half of the 20th century. My argument should not be read as implying that *nothing* was transferred from the science of psychology to its "applications". What was *not* transferred were scientific "laws" of human behaviour and experience that were in any way analogous to the principles of physics and chemistry relied on in industrial applications. There was no such transfer because there were no such "laws".

The complex relationship between the practices of psychological laboratories and the practices employed by psychological experts in non-academic settings has lately begun to open up for more detailed historical study (Ash & Sturm, 2004), a development that should finally lay to rest the unexamined model of psychological "applications" that still lingers in textbooks. As I pointed out in my paper, the migration of specific information and particular techniques was not unidirectional but also proceeded from contexts of application to contexts of investigation.

Psychology's transformation from an academic pursuit into a discipline deeply implicated in institutionalized agencies of social action not only brought about profound changes in its methods, the topic addressed in my 1990 book *Constructing the Subject*, it also led to a reconstruction of its basic categories. John Carson (2007) has provided a detailed account of this process for the case of "intelligence", in my own recent work I have explored transformations in the meaning of "memory" (Danziger, 2008). Other recent studies have thrown light on the difficulties encountered when categories, such as "attention", migrated from the laboratory to industrial contexts (Lüders, 2007). Transfer in either direction was beset with difficulties (van Strien, 1998). The relationship between psychology as science and psychology as "application", which was only beginning to be questioned at the time of the present paper, has now become an area of active historical scholarship.

Though early modern psychology had little to offer by way of scientific laws, it did offer a certain approach to human problems that was different from other approaches available at that time. On the surface at least, psychology promised a naturalistic, secular approach as opposed to a moralistic or faith based approach. When people relied on psychological expertise this is what they expected, and this is what they got. But that hardly distinguished psychology from other naturalistic advice. What distinguished the psychological approach from the insights into human problems offered by the nascent social sciences in particular was the presupposition that the *causes* of these problems were to be looked for within the natural constitution of the separate *individuals* that made up humanity. Applying psychology meant taking on board this fundamental presupposition.

When it came to describing the natural constitution of human individuals psychologists were often forced to fall back on a set of categories, such as sensation, memory or intelligence, that were not the products of empirical research but were part of the pre-scientific heritage of modern psychology. Scientific psychology converted these categories into arenas for the interplay of specific causal relationships that it set out to investigate. Insofar as the entire burden of explaining human conduct was then shifted onto these relationships one ended up with a general model that was sometimes referred to as *psychologism* (Kusch, 1999), though *psychological essentialism* would provide a better descriptive label. Early attempts at applying psychology often amounted to little more than the transfer of this working model to new fields.

This is not something I would have been able to express quite so explicitly when I first became engaged in experimental psychological research in the middle of the twentieth century. At that time I was simply following current orthodoxy, running white rats in an attempt to discover bits of what were called "laws of behaviour". I soon began to doubt the existence of such laws, mainly because my understanding of what a scientific law was had been formed by my previous training in chemistry. The laws I had encountered there described dependable and precise relationships among significant and unambiguous observables. In psychology, however, all four characteristics, dependability, precision, significance and non-ambiguity were problematic. This resulted in a rather rickety empirical foundation. Theories resting on this foundation only added another level of uncertainty.

It took me a long time to get beyond these negative conclusions. An important reason for this delay was the lingering effect of an empiricist philosophy that recognized only two kinds of things, observables and speculative non-observables that accounted for the regularity of relationships among observables. My first intimation that there was something missing in this account occurred about a decade after my rat running days when I became acquainted with Georges Canguilhem's (1955) study on the history of the concept of the reflex. To this day, many English language textbooks on the history of psychology repeat the myth of a Cartesian origin for this concept, yet more than half a century ago Canguilhem had shown that this concept arose much later and was subsequently attributed to Descartes in a typical Whig search for "anticipations". In later years I came to appreciate the general implication of what Canguilhem had grasped so early, namely, that conceptual categories had their own history which was not reducible to the history of either data collection or theorizing. This insight guided some of my early historical studies on the emergence of the stimulus-response concept (Danziger, 1983).

By then my transformation from a young rat runner, steeped in the philosophical pieties of neo-behaviourism, to a sceptical historian of psychology was complete. Having recognized the role of psychological categories in giving a specifically *psychological* meaning to empirical data, I decided to explore the history of some of the more widely used of these categories. That would include the category of "motivation" which had provided the topic for my experimental work as a graduate student. In the years between my two encounters with motivation I had pursued other interests, but when I explored the history of the category in the relevant psychological literature I came across some old acquaintances, books and papers that I remembered engaging with when I was writing my doctoral dissertation. In a way, it felt as though, while quietly pursuing my historical studies as an aged scholar in the library, I had unexpectedly met up with a figure that was both familiar and distant, my much younger self.

A meeting of the Canadian Psychological Association provided an occasion for some public reflections on this encounter. I have included this address as the third paper of this section because it provides some of the background for the views developed in later sections. In many respects the scientific attitudes of my younger self were not dissimilar to those of most experimental psychologists in the second half of the twentieth century so that my personal encounter has some general implications for the relationship between empirical psychology and the history of psychology, implications which I explore in the latter part of the paper. Empirical work is not conducted in a social and cultural vacuum. It is constrained by what I refer to as a *context of construction*, and historical analysis provides an indispensible means for understanding this context.

B: Psychological Objects

I began to think in terms of a context of construction in the 1980's when I became acquainted with then current inquiries in the sociology of science that had shifted from normative studies of science to observation of how scientific work was actually accomplished in the real world. (Logino, 1990 and Golinski, 1998 provide useful pointers to this rather extensive literature). In the light of these developments it was no longer possible to regard empirical data as the starting point of science. On the contrary, the empirical yield that science provided had to be seen as the result of an elaborate process of production. Insofar as aspects of the natural world become *objects for science* they exist within a discursive world supported by particular conceptual frameworks and specific technologies.

But these frameworks and technologies have undergone much change over time. They are historical products. Therefore, it has to be accepted that the objects of science, insofar as they are defined by underlying concepts and techniques, are also historical products. As such, they cannot be fully understood without appropriate historical studies. In the early 1980's I began such a study with a research project that brought an old interest in the sociology of knowledge to bear on the analysis of social relationships in the earliest experimental psychological investigations (Danziger, 1985).

These relationships were very different from those that became normal later on, a difference clearly linked to changes in the kind of knowledge sought after and duly generated in these investigations (Danziger, 1990).

The idea of a context of construction was also a reaction against a certain interpretation of the well known distinction between a context of discovery and a context of justification that had become common in psychology. That distinction had its origins in the philosophy of logical empiricism which provided the explicit or implicit meta-scientific grounding for most psychological research during the second half of the twentieth century. Taken abstractly, a distinction between the conditions under which scientific knowledge claims are generated and the conditions under which they are evaluated was hardly controversial. Historically however, and especially in the case of psychology, the distinction had acquired specific connotations that arose out of its role in the legitimation of a particular kind of research practice. It was common to invoke “logic” as the essential attribute of the context of justification, an attribute that was usually lacking and certainly not required in the context of discovery. The contrast between two sets of conditions was therefore replaced by a contrast between the logical and the alogical (see also Hoyningen-Huene, 1993). Textbooks could teach students of psychology a logic of justification but the context of discovery remained a rather chaotic field, at best reduced to certain non-rational factors at work in “the social psychology of psychological experiments”.

Though belated, the eventual emergence of this topic at least implied a recognition of the fact that the collection of psychological data on human individuals required a social context, one characterized by the relationship between investigators and their subjects. However, this context was only considered in its *psychological* aspects, in terms of individual expectations and readiness to comply, for example. As long as discussion was bounded by an essentially asocial disciplinary language the *social structural* aspects of investigative situations, the normative distribution of roles and power relationships, remained largely invisible. Still, the recognition that social psychological factors might play a role in the production of psychological knowledge represented a slight lifting of the veil that had long hidden the social processes of knowledge production from those most directly involved. For a further lifting of the veil one must cross the disciplinary boundaries of psychology and make use of analyses offered by sociology, history and philosophy. What emerges then is a context, not so much of discovery, as of construction.

But what of the context of justification? Does the move from individual discovery to social construction mean abandoning the distinction between the *normative* requirement for the logical evaluation of hypotheses and the *description* of the conditions under which these hypotheses are generated? (Sturm & Gigerenzer, 2006). No, but it has to be recognized that norms for establishing the veridicality of knowledge claims are themselves subject to historical change, even within scientific practice itself. These changes can be described and a connection may well be established between historical changes in the conditions of knowledge generation and knowledge justification. The priority therefore belongs, as I argue in the first paper of this section, to history, to the existence of historically variable "styles of reasoning" (Hacking, 1990), or “regimes of truth”, in Foucault’s felicitous phrase. Instead of cutting off further inquiry by appeals to a timeless logic of justification and a chimerical logic of discovery we should be asking questions about the historical succession of these regimes in different branches of knowledge. As far as psychology is concerned, does the order glimpsed in that succession resemble what we see in the physical sciences, or is it much more reminiscent of patterns familiar in the arts, as I suggest at the end of this paper?

Research psychologists usually regard science as being concerned with only two kinds of things, data and theories. Data are based on observations of one kind or another, theories are constructed to explain regularities in the data. This framework is adequate for everyday empirical work with its relatively limited time horizon but below the highly visible surface of theories and data there are layers of long term historical change that repay closer attention. One of these involves the history of investigative practices, alluded to in the first paper of this section and discussed more fully in *Constructing the Subject: Historical Origins of Psychological Research* (Danziger, 1990). However, the content of the discipline was shaped, not only by experimental and statistical practices, but also by linguistic practices. Psychologists could produce shared knowledge only by using language to describe what they did and what they found. To serve as a vehicle for shared knowledge disciplinary discourse had to rely on certain intuitively understood categories that structured psychologically relevant thought and experience in a particular way. When data are reported they have to be given some kind of psychological reference, as pertaining to sensations or behaviour for example, and theories must be taken as applying to these kinds of referents. That requires the use of certain descriptive categories recognized as "psychological" by a particular linguistic community.

In the second paper of this section I explore the distinction between such categories and theories. Their discursive function is quite different. Theories have an *explanatory* function – they attempt to provide reasons why certain patterns of empirical observation were to be expected. Categories, on the other hand, have a *constitutive* function – they establish that an empirical or a theoretical statement pertains to a particular segment of the world understood in a certain way. This presupposes some accepted way of dividing up human experience so that different *kinds* of real things can be referred to. The constitutive function of categories derives from the fact that the world allows human cognition considerable leeway in dividing it up. Some distinctions, such as that between up and down, may be inevitable, but huge areas of experience, especially in the area of human interaction, are wide open to differences in classification and interpretation. Cultural differences attest to that. In adopting a particular set of categories people constitute their world, especially their social world, in a particular way.

Categories of understanding not only vary between cultural communities, they also change over the course of human history. Even the notion of a distinct kind of knowledge, pertaining to a part of reality understood as being "psychological" in the modern sense, did not always exist but emerged hesitantly over a particular span of historical time (Smith, 2009). Many of the subcategories of "the psychological" emerged much more recently and more are being added every year. Once they became an accepted part of discourse these categories seldom stayed fixed but changed their meaning, sometimes very slowly, sometimes quite abruptly, sometimes very subtly, sometimes completely.

Evidence for these changes is most readily provided by historical variations in category names. In the simplest cases a new name is invented to designate a new category not previously recognized as such. This is what typically happens in the case of "clinical" categories, such as multiple personality or sociopath. Conversely, a word that once designated a distinct aspect of human nature may lose that function because the basic features of human nature come to be divided up and understood differently. *Passion* would be a good example. At one time human passions were universal traits that characterized all of us to a greater or lesser extent. Nowadays, passion is merely an unscientific way of describing certain features of (usually temporary) psychological states. On the other hand, *emotion*, a relatively young word, has come to designate a category of psychological events and processes that are by no means equivalent to the passions of former times (Dixon, 2003).

Although changes in categorization are often signalled by changes in vocabulary this is not always the case. A category may still be known by the same name although its boundaries and prototypical content have undergone profound changes. *Memory* provides copious examples of this (Danziger, 2008). The term has been in continuous use for millennia but it did not always refer to the same set of phenomena. Some older categorical distinctions, such as that between remembering and reminding, lost their importance, newer distinctions, such as that between memory and imagination, became crucial. At times, memory has been understood primarily as a matter of cognition, at other times as a matter of character change.

Unlike theories, which are often value neutral, inherently evaluative categories are generally used to designate the kinds of objects that theories need to explain. Whatever the prevailing conception of memory, the distinction between good memory and bad, between successful and unsuccessful remembering, was always present. However, the criteria for evaluating acts of memory changed over time. The central position accorded to accuracy of reproduction is highly characteristic of the modern period. At other times, the success of memory depended on different achievements, for example its role in producing vivid imagery or in supporting morally desirable actions. Categories of memory incorporate particular *mnemonic values* that change historically as a result of changes in the material technology of memory (writing, printing, audio-visual devices, etc.) and because of changes in the social and economic life of human communities.

Regarded purely as a cognitive achievement, the divisions that carve up human nature into its parts would be only of intellectual interest. However, the evaluative quality that is inherent in so many of the categories produced by these divisions gives them an added ethical dimension. Incorporating certain values, they are subject to moral judgment deriving from other current values. Unlike the categories of a value-free science, these categories of human understanding are enmeshed in a network of moral relationships, not only in a purely intellectual network. This has rather profound implications for any human science that employs categories of this kind (Brinkman, 2005). It becomes a legitimate target of social critique on moral grounds.

Such conclusions are at variance with a common implicit assumption that psychological categories are “natural”, that they refer to real classes of phenomena clearly distinguishable by their essential properties, that they are in fact analogous to chemical elements. From this point of view, historical changes in these categories would simply reflect the gradual discovery of the actual structure of psychological reality. That structure is assumed to have remained fixed, independently of any attempts at capturing it in the categories of psychological knowledge. Human motivation, for example, would always have existed as a distinct kind of entity that never changed; psychologists would merely be discovering more about its essential properties and about its causal interaction with other permanently fixed entities.

Contrary to this view, I would count myself among those who believe that the easily demonstrable changes of psychologically relevant categories over the course of recorded history indicate a change in the reality to which those categories refer. In other words, psychological kinds have what Ian Hacking (2002; also Sugarman, 2009) calls an *historical ontology*: they exist only within human history and are shaped by that history. By contrast natural kinds, such as different minerals, exist independently of human history. This does not mean that psychological (or more generally *human)* kinds are any less real than natural kinds (Martin & Sugarman, 2009). However, theirs is an inherently self-referential reality which the reality of natural kinds is not. Humans construct categories classifying both humans and rocks but rocks do neither. When humans speak in psychological terms they are saying things about their own kind, so that there is a coincidence of subject and object. In the case of natural kinds, however, the subject performing the distinction and the object being constituted by that distinction always remain quite separate.

In chapter 6 the reality constituted by human kinds is examined more closely. The objects populating this reality are human objects, a sub-set of which are the *psychological objects* defined by psychological categories. These can be certain kinds of people, the psychologically traumatized or the intellectually under-endowed for example, or they can be certain kinds of experiences, actions and states that are commonly attributed to human individuals: sensations, drives and personality, for example.

Human kinds and the objects they define have always been affected by variable human interests, institutions, social practices, and technical possibilities. Certain specialists, such as philosophers, theologians and legal authorities, have long had an enhanced influence on the historical fate of human kinds. But in more recent history the sub-set of *psychological* kinds has been particularly linked to the emergence and rapid growth of a new class of psychological specialists, a class with its own professional interests, traditions, institutions and beliefs. These provide an important part of the social context for changes in psychological objects during the modern period. Moreover, as indicated in chapter 6, modern psychological specialists shape their objects, not only by virtue of the categories they employ, but also by means of the practices they use to investigate the human subjects of their research and to intervene in their lives.

Psychological kinds and psychological objects are profoundly *relevant* to the experts whose professions would not exist without them. Of course, they are hardly irrelevant to the people whose characteristics they describe, especially when psychological categorization can have life changing significance. This state of affairs opens up possibilities for endless interactions between psychological categories and the reality to which they refer. Psychological objects are also human subjects, or at least predicates of those subjects. That brings us back to a crucial difference between human kinds and natural kinds, a difference that can also be described in terms of a distinction between *interactive* and *indifferent* kinds (Hacking, 1999); the important point is the distinction, not the terminology.

Philosophers are in some disagreement about the significance of the distinction. Does it derive from a fundamental difference between the *objects* of the human sciences and those of the natural sciences? Does this difference come down to the presence of self-consciousness in the one case but not the other (Martinez, 2009)? There are problems with this view because the entire project of natural science psychology shows that it is quite possible to build a vast edifice of knowledge about human beings by means of methods that treat human self-consciousness as irrelevant. Either this knowledge is all an illusion or there is something lacking in the criterion of self-consciousness.

It is hard to believe that knowledge in say the field of psychophysics is illusional, yet it is based on work with self-conscious human sources of data. But that is not the end of the story. When we turn from experiments in psychophysics to experiments in social psychology there is indeed a problem. In these experiments deception is a common practice. If the subjects in many of these experiments knew what was really going on, if the purpose and procedures of the experiment were to be correctly described to them, their reactions to the procedures would be seriously affected. Therefore they have to be deceived, especially about the experimenters' goals. Were they to understand those goals their reactions could no longer be interpreted as those of a natural object.

But what is the difference between a deceived person and one who has been dealt with honestly, surely not self-consciousness which exists in both cases? The difference lies, not *within* the person but in the *relationship* established between people occupying different social positions. Psychological experiments attempt to duplicate the gap that separates subject and object in the natural sciences, an artifice based on the fiction that individual experimenters and subjects are isolated beings and not members of human discursive communities. Sometimes this fiction may be justifiable in terms of the very limited information the experiment is designed to produce. In social psychology this is difficult to bring off, and so deceptive measures have to be taken to partially destroy the human bond that would normally tie experimenter and subject together in a community of understanding. That this is also a moral community is demonstrated by the fact that the use of deception is commonly recognized as entailing problems of research ethics.

I use social psychological experiments purely for purposes of illustration. They are representative of a more general class of epistemic situations that establish subject-object relationships designed to illuminate human kinds. These relationships can only be established among individuals that belong to discursive and moral communities, a feature that is not characteristic of subject-object relationships in the natural sciences. Needless to say, there is no claim here that the human sciences should *always* restrict themselves to epistemic situations of this type.

In the final paper of this section, presented a few years after the first three, the approach is retrospective and also more idiosyncratic. Requested to look back at the last two decades of the century that had just passed, I presented a summary framework that incorporated some of the most significant trends of the time. What really distinguished some of the newer studies from the older disciplinary history was their critical stance towards questions of historicity. By and large, the older history had been constructed in a historiographically naive spirit. Its narratives had been cast in the framework of folk legends: famous authors attempting to extract the truth about the part of nature that was human psychology. Abandoning that framework essentially involved subverting the status of its major components: 'authors' and 'nature'. These concepts had been the unexamined foundations of the old historiography; both needed critical reflection.

Those of us who were not sunk in disciplinary isolation received much help in pursuing this path from two intellectual developments of the time. "The death of the author" had been proclaimed in French intellectual circles for some time, but most outsiders became familiar with that idea in the form given to it by the work of Michel Foucault on the history of the human sciences. My remarks on Foucault's ideas in my 2001 Amsterdam paper were a little flippant, but this does not mean that aspects of his work had not played a significant role in my attempts at sketching an alternative historiographic framework. Some of his earlier studies (especially Foucault, 1970, 1972, 1973) provided provocative examples of how to approach the analysis of intellectual products as discursive realities rather than as authorial achievements.

When the death of the author leads to “history without a subject” it evokes echoes of an older debate, namely that about the division between so-called internal and external factors in the development of disciplines and knowledge domains. Internal factors were intrinsic to the domain in question and external factors were such things as power structures, social interests, and institutional pressures that might impinge on practitioners in the field. The trouble was that the distinction between internal and external factors was to some extent arbitrary, and in any case was itself subject to historical change. In the Foucauldian approach, however, questions about separating internal and external factors do not even arise because the intimate link between power and knowledge and between social practices and ideas becomes basic to any understanding of historical developments. This was more appealing, yet it was an accommodation achieved at some cost. It seemed that mere human beings had entirely disappeared from this model. They and their interests would have to be put back.

More recently I summarized my thinking about "psychological objects" as follows: "In speaking of historically constituted psychological objects I was trying to get away from an implicit metaphysics of timeless psychological phenomena that existed out there, waiting to be discovered and explained by professional psychologists. Instead, it seemed to me that no phenomenon could be transformed into an object-for-psychology without passing through the mill of psychological categorization and practical intervention. The subject matter of psychology was not constituted by “phenomena”, which strictly means things that *appear*, but by objects, things posited by subjects as the target of their activity. There was a layer of constituting action interposed between observers and the phenomena that appeared to them. This layer was itself a historical product that the older historiography had rendered invisible. What now needed to be done was to make it visible" (Danziger, 2009, p. 118).

C: Consequences

When I was a student it was common to ground the history of psychology in the original contributions of certain individuals, most of them identified with the discipline. However, as no one could pretend that the discipline existed in a social vacuum, the consequences of being part of an ever changing wider world deserved at least a nod. One paid one's respects to something E.G. Boring (1929) had called the *Zeitgeist*, the spirit of the times, apparently oblivious to the irony of yoking a fierce advocacy of rigorous experimentalism to one of German idealist philosophy's more nebulous concepts.

This was hardly surprising because, at the time, alternative ways of thinking about the way science and society interacted were hard to come by. That changed during the second half of the 20th century. By the time I wrote the two papers in this section there had been much new work in the history, philosophy and sociology of science and, as indicated in the earlier parts of this book, the influence of this work had begun to penetrate the cloistered walls of the historiography of psychology.

However, a heightened historical sensibility was not necessarily appreciated in departments of psychology. In fact, it could create problems for teachers of the customary disciplinary history course in these departments. The function of these courses was purely pedagogical: they were to provide an account of disciplinary origins and development that was in accord with the perspectives of currently active members of the discipline. Scholarly historical work that might lead to questioning of these perspectives was not really in demand. Academic psychologists with genuine historical interests were often in the uncomfortable position of trying to balance the expectations of their departmental colleagues against the scholarly standards of an alien discipline; alien, because the ethos of modern psychology was uncompromisingly ahistorical.

In this respect psychology was hardly unique. For very good reasons, the natural sciences did not encourage deep historical interests among their students (Brush, 1974). They left the history of their fields to professional historians. Psychology was peculiar in that courses on the history of the subject remained on departmental curricula at many institutions. In this respect they resembled the humanities or social sciences, an anomaly in view of the discipline's general aspiration to natural science status.

In 1992 I used the opportunity afforded by an address to my Canadian colleagues to confront the dilemma faced by those affected by this anomaly. Surely, we were in an unstable even precarious situation expressed in the title of my talk: "Does the history of psychology have a future?" My approach was double edged. First, I emphasized that the deep roots of the conflict between the moral community of science and the obligations of historical scholarship could not be denied. But, secondly, I suggested that the fractionation of psychology offered some hope for a continuing place for critical historical studies within the discipline and offered some specific examples. The publication of this argument evoked some discussion (Rappard, 1997, 1998; Dehue, 1998; Danziger, 1997, 1998). It was felt that the terms in which I had posed the opposition between science and history were too stark, that an accommodation was possible if disciplinary historians were sensitive to the requirements and preconceptions of their colleagues. I had however emphasized the fragmented nature of the discipline and its corollary: that support for historical studies that questioned current pieties could only be expected from some of these fragments. This was simply a matter of sociology, there was no implication that one community was intrinsically more right than another.

In the intervening years the position of disciplinary historians within psychology has certainly not improved – institutionally it has apparently become more precarious and anxieties about the future persist (see e.g. Bhatt & Tonks, 2002; Chamberlin, 2010). Fifteen years later, it seems that my attempt to assess the future of disciplinary history in psychology was overoptimistic. I had attempted to contextualize reasons for optimism by reference to certain trends within the discipline. But I had failed to take into account the influence that much broader socio-political trends were bound to exert on the discipline. In the meantime, it has become obvious that ever increasing pressures to make the discipline conform to the norms of technoscience and to the demands of visible practical utility will not be favourable to the survival, let alone the growth, of critical historical scholarship within psychology's conventional disciplinary boundaries.

The second talk of this section addresses a special feature of standard disciplinary history whose significance only became apparent towards the end of the century. In the form established by E.G. Boring and copied innumerable times the history of the discipline was presented as the unfolding of an empiricist project from its inception in 18th century Britain through its materialization by 19th century German experimenters to its culmination in 20th century American psychology. The problem with this narrative was not that it was intrinsically mistaken but that, by privileging one historical strand among many alternative constructions, it turned the history of one tendency into *the* history of the discipline. From the vantage point of a mid-twentieth century American experimental psychologist this was not an inappropriate substitution but for other members of the discipline, especially those outside North America, it was hardly *their* history. Yet, because of the dominant global position of American psychology during much of the 20th century, this version became *standard* disciplinary history.

Towards the end of the century some problematic aspects of the standard model began to become more visible. In Europe psychology had recovered from its virtual eclipse during and after World War II and, insofar as that recovery was not simply based on an importation of American psychology, there was renewed interest in alternative historical narratives more appropriate to local circumstances. When I was invited to edit an issue of the journal *History of the Human Sciences* in 1990 I made sure to include articles that reflected the new European historiography. By making some of this work available in English I hoped to reinforce the general proposition that a global history of psychology would have to adopt a multicentric perspective rather than one presented from one privileged point of view (Danziger, 1991). The older historioriography had given a historical dimension to a global image of the discipline in which there was one geographical centre and a scattering of less significant peripheral locations. That image no longer seemed appropriate. I repeated this argument five years later at the International Congress of Psychology in Montreal in a paper that is reproduced here in an expanded form, including three paragraphs taken from the original 1991 version.

When one historical narrative, geared to the requirements of psychologists with a particular geographical and disciplinary location, becomes the standard history for the discipline as such we are encouraged to embrace an implicit model of global psychology reminiscent of the solar system: a central sun with revolving planets that reflect its light. The distinction between centre and periphery is not simply geographical, it can also involve the conceptualization of psychological processes. Research at the centre is usually presented as investigating universal psychological phenomena, whereas at the periphery the dependence of these phenomena on local conditions is much more likely to be made explicit. This aspect is discussed at greater length in the last of these papers, "The Holy Grail of Universality".

Shifting from one master narrative of psychology's history to a polycentric perspective became more acceptable as the relative dominance of the centre declined and local quasi-psychological traditions asserted themselves in the form of "indigenous psychology", especially in Asia (Allwood, 2002; Allwood & Berry, 2006). By the beginning of the new millennium the "internationalization" of the history of psychology was well on its way (Brock, 2006). This tiny section of historical scholarship had joined a general trend towards "world history" that had been discernible for some time (Stuchtey & Fuchs, 2003). But the replacement of a canonical master narrative, suited to one privileged location, by a more decentred account entails a recognition of the crucial importance of local, "indigenous", factors - from the polycentric beginnings of modern psychology in certain European countries through its profound Americanization to its global presence in a variety of forms (Danziger, 2006).

D: Historical Psychology

In the closing years of the last century the main focus of my historical studies shifted from the discipline of psychology to take in a much broader time frame. My decision to explore the history of the concept of memory, work that led to the book *Naming the Mind: A History of Memory* in 2008, entailed some engagement with the topic of historical psychology. Discourse on the nature of memory is of course far older than the modern discipline of psychology and is deeply entangled with many non-psychological issues. I was only too well aware of the mischief that could be wrought by hasty psychological forays into these distant and unbounded regions, so in the first place I had to define the conditions under which my project might be justifiable.

I was hardly the first to confront this problem. For a century, it had been common to make a distinction between the history of psychology and something older, related to it yet different, in terms of a contrast between psychology's "short history" and "long past". For Hermann Ebbinghaus, an early experimentalist who is usually credited with introducing this contrast, psychology had recently acquired a history, instead of a mere past, because it had finally embarked on a path of cumulative progress by means of empirical investigations. This view was widely shared among modern psychologists and the long past - short history distinction became popular.

It so happened that when I presented the paper which begins this section exactly a hundred years had elapsed since Ebbinghaus had first introduced the distinction and so I used the occasion to draw attention to its significance. Chronologically, this paper is later than the others, but its basic and relatively simple theme makes it an appropriate introduction to this section. It turns out that Ebbinghaus was not in fact the first to distinguish history and past with respect to psychology. What he did was to change the meaning of the distinction to suit the empiricist ethos of his fellow experimentalists. In doing so he managed to obscure a more profound distinction, that between historical continuities and discontinuities.

It is clear that the past/history distinction points to the existence of a historical discontinuity. But what is the nature of this discontinuity? Should it be traced to the advent of cumulative progress after a long period without progress? For a contemporary historian that looks like a highly problematical suggestion. What seems more certain is the advent of something definitely new, a disciplinary formation involving a complex interplay of intellectual norms, institutional structures and investigative technologies that is characteristic of the modern period. The history of this formation cannot be traced very far back in time. At best, one can explore the intellectual and social conditions that made possible the eventual emergence of a genuine discipline. But a discipline is essentially a matter of shared forms: shared categories of understanding, shared norms regarding effective technologies, shared institutional practices. The history of these forms should not be confused with the history of the content which they shape. That latter history may extend much further back than the history of disciplinary forms, as the history of memory illustrates. There was speculation about memory long before the modern discipline of psychology made its mark on memory discourse. The fact that this earlier history is not part of the history of psychology does not mean that it cannot be investigated. The question is how.

Certain limitations on what can be achieved by such investigations have to be accepted right at the beginning. First, there is the question of boundaries, alluded to in the second paper in this section. A distinction has to be made between the history of human subjectivity, which is a vast, probably limitless field providing subject matter for a variety of disciplines, and what I would call "historical psychology", a field whose boundaries are set by the concepts and practices of modern psychology. Historical psychology takes these concepts and practices as a starting point and inquires into their background before and after they became part of the discipline. It is indeed a Foucauldian "history of the present". It has nothing to do with historical biography, nor should it be expected to provide access to the inner life of people who lived long before us.

Interpreting past lives in terms of the concepts and categories of modern psychology is not what I mean by historical psychology. Such interpretations presuppose a supra-historical status, a trans-historical validity, for currently popular perspectives that are themselves part of a history that needs to be explored. It is not a matter of bringing the light of the present to the dark recesses of the past but of questioning the past because its otherness may help us to see some aspects of the present that are usually transparent to our gaze.

Approached in this spirit, historical psychology cannot be teleological, that is, it cannot treat history as a narrative of progress towards an end fulfilled in the present. The concepts and categories of modern psychology have historical links that are enormously dispersed, both intellectually and chronologically. A master narrative that attempted to weave these links into one strand would be pitifully artificial. This does not mean that there are no visible historical continuities. In the case of memory, the long-term presence of certain metaphors, particularly those of storage and inscription, can hardly be overlooked (Draaisma, 2000), nor can the recurrence of certain questions, for instance, whether memory is one or many. The challenge is to give such continuities their due while remaining sensitive to the innumerable sharp turns and breaks to which the historical record attests.

At this point a fundamental question arises: does the more narrowly defined historical psychology envisaged here have any implications for "human psychology" in some objective sense? "Psychology" is a polysemous term that refers both to a modern discipline and to its object of study. Past usage therefore implied a distinction between two histories, that of the discipline, i.e. history of psychology, and that of its object, the psychology of humankind, which would constitute historical psychology. By restricting the ambit of historical psychology in the way I have suggested, have we forfeited the possibility of making any contribution to historical psychology in the traditional sense, of having anything to say about the historicity of human subjectivity?

As indicated in the second of the papers in this section, I do not think so. The distinction between psychology and its subject matter, although important analytically, does not entail the existence of two entirely separate domains without influence on each other. It has to be assumed that in psychology the subject matter has some influence on the science - if it did not, the entire project of such a science would make no sense. But many people who accept this proposition without question are made uncomfortable by its counterpart, the claim that in psychology the discipline has an influence on its subject matter. Of course, it is clear that what forms the subject matter of the discipline at any time is a function of what members of the discipline find interesting and significant at that time. It is the stronger claim that is disturbing, namely, that disciplinary concepts and practices can have an effect on human individuals who constitute the very reality the discipline attempts to investigate.

On a micro-level the reality of such effects is widely recognized. In experimental settings the expectations of experimenters can influence the outcome and one can take precautions, such as double-blind procedures, to eliminate or minimize these "experimenter effects". In the present context, however, it is a matter of recognizing analogous effects on the macro-level, that is, on the level of history. Yet, one only needs to look at areas of human conduct judged to be deviant to find spectacular examples of such effects (Gergen, 2007). The rise of a new class of experts in human deviance was accompanied by the appearance of newly defined patterns of action, such as child abuse, and newly distinguished human types, such as multiple personalities (Hacking, 1995). This sort of classification had strong implications for the way certain individuals were treated and for their own sense of identity and self worth. When new ways of self-understanding become widely disseminated and supported by institutional authority they can have significant effects on peoples' lives. Individuals' understanding of who they were and what they wanted to do might well change. They might even deploy the new categories in ways not intended by the professionals who had introduced them.

But this sort of interaction need not be limited to the highly visible area of human deviance. Historical changes in the way people categorize their everyday action and experience are unlikely to leave the quality of those actions and experiences unaffected. As I suggest in the third paper of this section, this kind of effect can now be detected across a broad spectrum of human conduct in communities across the world. Early in the history of modern psychology there were expectations that the 20th century would be "the century of psychology". These expectations were often linked to hopes for psychology as a profession but they were also correct if read as forecasting the relentless growth in psychological ways of interpreting human life among nonprofessionals. Because human action, as distinct from the movement of bodies in space, is only conceivable under some description, whether it be simply "walking", or "writing a letter", or "being appalled", the replacement of one set of descriptions by another set changes the meaning of actions. When psychological categories replace other categories for describing human actions, those actions are no longer what they were before. On a historical scale, psychology can act as a self-fulfilling prophecy: as more people more often understand themselves and each other in psychological terms, more domains of psychology come closer to achieving a universality that previously had only been a hopeful assumption. That is the argument developed towards the end of the third paper in this section.

Time will tell whether this argument holds up in the light of future developments. But in the same paper I also present what amounts to another argument for a historical psychology, and this time the evidence has much greater historical depth than the rise of modern psychology could ever supply. Here I invoke the extraordinary dependence of human life on artefacts of human invention and construction. Beginning with stone implements, changes in these artefacts must have been accompanied by changes in their makers and users. Insofar as we can establish reasonable hypotheses about these changes we already have the beginnings of what is surely a kind of historical psychology. When we enter the period of recorded history the artefactual remains of symbolic technology, writing above all, provide a rich source for studies on the historical development of human skills, attitudes and abilities. "Historical psychology" would seem to be the most appropriate collective name for such studies.

Because of the established traditions of their discipline, psychologists are not likely to be major contributors to this field in the foreseeable future. But precisely because of these traditions psychologists have good reason to take an interest in the field. Experimentation in psychology is historically rooted in experimental physiology, the discipline that provided the template for how causal studies on the reactions of living organisms should be set up. That template has proved useful for the study of a range of human responses that is limited by the limitations of the template itself. In the present context two of these limitations are of particular relevance. First, this template was designed for the investigation of causal effects operating within a relatively brief time frame, events spanning minutes, seconds, fractions of seconds, or at most hours. Experimental psychology successfully extended this time frame by repeating experimental episodes in the form of "trials", but rarely beyond time spans of days or weeks. As the period between experimental intervention and effect lengthens the difficulty of maintaining adequate control increases very quickly. For the investigation of long term phenomena, other approaches such as longitudinal studies, have to be used. Beyond that, one either ignores really long term developments or accepts the need to turn to historical studies.

The experimental template inherited from physiology was designed to investigate effects that were not only short term but also unidirectional. It was meant to reveal what happened when an organism, or one of its parts, was exposed to specific experimental interventions. The arrow of causality was always from experimental conditions to organismic response. This became the model for experimentation in the young science of psychology and dominated the experimental tradition of that science for a long time to come, a development that was strongly reinforced by theoretical preconceptions based on the reflex concept and by a persistent tendency to look to animal behaviour as a source for models of human behaviour. Although this model of experimentation is quite adequate for investigating certain aspects of human behaviour, for example reaction times and sensory mechanisms, it cannot be used to investigate the interlocking processes that are so characteristic of human behaviour in interaction with human artefacts. In this case, the most interesting effects are not one-way because the environmental factors to which humans adapt are themselves products of constructive human adaptation. Where the time span of such processes is relatively short, as in certain feedback loops involving humans and machines, the model of experimentation can be modified to suit this reality. But where the time span is long, as in the interaction of humans with symbolic forms that are human constructions, the limits of experimental method are soon reached and historical modes of inquiry must take over.

A third limitation of traditional experimentation derives from the fact that its physiological origins entailed a focus on individual organisms. The experimental investigation of physiological processes, such as digestion, circulation and respiration, depended on drawing a sharp line between what went on inside the organism and what happened outside. Only when solids, liquids or gases crossed this line did their fate become relevant to physiological investigation. Traditional psychological experimentation maintained this sharp separation between intra-individual processes and the big world outside, leaving the latter to be studied by a variety of other disciplines.

This division of labour led to an unfortunate knowledge gap. The relationship between what goes on in any one individual mind and the human setting within which it occurs is rather different from the relationship between food in an experimental animal's stomach and food in the laboratory storeroom. What happens in the storeroom does not depend in any way on what happens in the stomach, and what happens in the stomach is at most contingently related to what happens in the storeroom. Investigation of the two processes can safely be left to different sciences without significant loss of information. But in the case of human subjectivity there is a long term interdependence of intra-individual and social-contextual processes that becomes invisible if the boundaries confining the disciplines of psychology and history are too tightly drawn. For most psychologists, because they work within strictly intra-individual boundaries, this interdependence is indeed invisible. Whatever specific knowledge we have of these matters is largely due to the work of historians.

For psychology, this disciplinary isolationism can be maintained only at the cost of a profoundly distorted perspective on human experience and conduct, a perspective in which attention is focused on what happens inside the mind/brain while the context for those inside-the-head events is only dimly perceived or totally eclipsed. It is true that scientific studies require specialization but, when specialties develop in directions that leave large holes in what they attempt to cover, some repair work seems to be in order. In the case of the hole that has opened up between psychology and history this repair seems to require an initial recognition that mind/brain processes function within larger systems that extend beyond any individual specimen's skin and sometimes beyond its limited lifespan. In the human case, those larger systems depend on symbolic activity and on the transformation of the lived-in environment by instrumental activity involving artefacts of human construction. The importance of symbolic activity has been more generally recognized than that of the second, technological, factor. Because of this, and because these factors are not independent of each other, I have concentrated on the second factor here.

Most human activity is not simply responsive but instrumental, and the instruments available at a particular time are the products of past instrumental activity. As the available instruments change, the demands they make on human capacities change too. To say that humans are tool making animals is to describe only half of the human condition, the other half being the remaking of humans by their own tools. "It is not only the subjects that do something with the things; the things also do something with the subjects" (Schraube, 2009, p.300). Sometimes the effects are relatively minor but sometimes they are profound and take many years to become manifest, as in the case of constructed urban environments, techniques of literacy, or mechanization of production. These examples make it unnecessary to belabour the point that talk of "things" and "tools" here is not based on a stripped down concept of technology but is meant to include the network of social relations in which effective deployment of tools is always embedded.

Human tool making has been studied by archaeologists and historians of technology for quite some time. Their studies have also led to scattered observations and hypotheses regarding the way altered environments acted back on the tool makers over time. It now seems feasible to study this side of the relationship between tools and their makers in a more systematic way. We might then begin to fill some gaps in our knowledge of those long term psychological changes that are beyond the reach of experimentation. Now that would be a historical psychology the discipline could use!

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(A) Background

1. The Moral Basis of Historiography (1997)

Abstract: For historians of psychology it is important to recognize that any intelligible historical account must necessarily be made from a position within history. Thus there can be no historical account without a particular perspective or point of view from which aspects of historical reality are seen or not seen, emphasized or played down, accepted or rejected. The pretence of being able to step outside history and describe it as it were from nowhere puts the historian in a godlike position and repesents a major obstacle to good scholarship. One can land in this position if one fails to distinguish between morally tinged historical biases and emotional commitments, and also if one does not recognize the constitutive role of interpretation in historical accounts.

Last fall's special issue of this Bulletin was devoted to the topic of *Convergence Across History, Theory and Philosophy*. I would like to pick up one thread of that discussion by addressing a question on which there appears to be some confusion, namely, the role of moral judgements in historiography.

As one might expect, this question has long been a matter of concern to professional historians and they have produced an extensive literature on it. Historians of psychology can no more afford to ignore this literature than physiological psychologists can afford to ignore relevant physiological literature. In a contribution that takes issue with views he attributes to Adrian Brock and myself Chris Green (1996) quite appropriately draws support for his position from the writings of historians, Butterfield and Carr in particular. That seems to me to be a step along the right road. Let us see where a few more steps along that road will take us.

I'll begin with Carr, whose views on the role of moral judgements are very explicit. Taking his cue from Max Weber, he writes: "The historian does not sit in judgement on an individual oriental despot. But he is not required to remain indifferent and impartial between, say, oriental despotism and the institutions of Periclean Athens. He will not pass judgement on the individual slave-owner. But this does not prevent him from condemning slave-owning society. Historical facts, as we saw, presuppose some measure of interpretation; and historical interpretations always involve moral judgements - or, if you prefer a more neutral sounding term, value judgements." (Carr, 1964, p.79). Clear enough, one would think.

It is not *moral judgement* that Carr objects to as a historian but what he calls "*emotional and unhistorical reaction*" (ibid. p.98). For him, as for many of us, the two are not at all the same thing. Even on the level of everyday understanding one can surely recognize that moral judgements need not be accompanied by emotional reactions and that emotional reactions need not involve moral judgements. So I think we have to be clear about whether we are talking about the role of moral judgements in historical studies or the role of emotional reactions. Our concern should focus on the former, not the latter. The merits of judgements do not depend, directly or inversely, on the degree of emotion invested in them.

There are two postures which commonly lead to a blurring of the distinction between moral judgement and emotional response. The first is the posture of the rhetoritician who equates the *other's* judgements with emotional reactions, but typically not his own. Though this is an old trick, it is surprising how well it still works. The second, much more interesting, posture is that of the *emotivist* philosopher for whom all value judgements are nothing but emotional expressions. This is not the place for a discussion of such a philosophy - anyone wishing to pursue the topic might want to look at Alasdair MacIntyre's *After Virtue* (1984) - but it is clear that from a perspective of philosophical emotivism there would be a necessary equation of moral judgement and emotional reaction.

Carr was obviously not an emotivist and neither am I. In fact, I believe that the adoption of such a perspective would prevent us from pursuing the question of the moral basis of historiography in any useful direction. The role of emotion in the moral choices of particular *individuals* may be an interesting *psychological* topic, but it only becomes relevant to the principles of historiography if one takes some rather dubious philosophy on board.

The case of the historian Herbert Butterfield illustrates a different, though related, problem. Everyone is familiar with his 1931 critique of "whiggish history" that provided the context for his strictures on moral judgements by the historian. We can surely agree that these strictures still have some point. Indeed, some of us have explicitly criticized the older whiggish history of psychology on similar grounds. But Butterfield's days as an authority on the role of the historian are long past. More recent discussions of that topic by historians have drawn attention to the inadequacies of the position he represented. These discussions are, I think, quite instructive in terms of our own concerns.

To illustrate: After criticizing the whig historians' bad habit of allowing value judgements to colour their history Butterfield (1957) proceeded to write a history of science that struck a later generation of science historians as distinctly whiggish in character (Hall, 1983). This happened in spite of the fact that he sought to avoid mixing history and value judgements in the infamous Whig manner. How was that possible? The answer probably lies in a rather naive conception of historical method which was not uncommon among British historians of his generation. Briefly, that conception was based on a division between collecting historical facts and reading the significance of those facts (Wilson & Ashplant, 1988). The former activity was basic and essentially unproblematical, the latter was constituted by acts of inductive generalization from the collected facts. Such an understanding of what he was doing as a historian effectively shielded Butterfield from recognizing the effect of his own moral commitments on his work. He saw himself as one whose business was with facts, not with values. When he encountered values that were not his own he could easily separate them from what he regarded as facts, but his own values remained embedded in the process of favouring one set of facts over another and in everything that he took for granted when generalizing from these facts. The point is that historical accounts do not emerge inductively out of fact collection; they depend, as Carr and many others have emphasized, on interpretive frameworks that embody value judgements and moral commitments.

Needless to say, Butterfield was only a prominent, but hardly a unique, victim of what one might call "the Butterfield effect". Appeals to an interrelated set of polarities - fact vs.value, scholarship vs. propaganda, dispassionate judgement vs. emotion, distance vs. commitment, etc., have a long history in the annals of rhetoric. I have no doubt that there were times when such appeals played a role that most of us would applaud. But that should not prevent us from entertaining doubts about the grounds on which such appeals were based.

All such appeals rely on a radical asymmetry between a point of view that is epistemically privileged and another that is not. The privileged viewpoint is one that rises above all social interests, local prejudices, individual weaknesses, and partial perspectives; it is a godlike viewpoint. On the other side there is the all too human viewpoint which remains trapped in grubby local entanglements. There have been historians whose claims implied a godlike perspective on their subject matter. Peter Novick's (1988) magisterial account of debates on "the objectivity question" among American historians provides an overview of such claims on this side of the Atlantic.

But these days there is a lot of scepticism about the notion that there is one true story to be told about the past. Such a story could only be told by those without their own place in history, those who could see it all without being anywhere, in other words, by gods. That has not stopped humans from pretending to godlike knowledge by employing what Donna Haraway (1991) very appropriately calls "god tricks". Human knowledge, however, is always *situated* knowledge, that is, knowledge obtained from a particular position and hence partial. And, in the recent words of Harvard historian Mario Biagioli (1996, p.193), "being partial is no sin". We cannot avoid being partial, we can only avoid owning up to it.

How does this affect the notion of "scholarship"? Do standards of good scholarship go out of the window when "partiality", in Biagioli's sense, is admitted? Quite the opposite, I think. As long as no-one has found a way of eliminating interpretation, eliminating a situated point of view, from the practice of historiography the first, essential, requirement of good scholarship is surely the recognition of the location from which it is practiced. The greatest obstacles to good scholarship are to be found in the "god tricks" that serve to hide and obscure the necessary partiality of historical accounts. Being unaware of one's biases is no guarantee of good scholarship. Conversely, there is no inherent opposition between scholarship and commitment. Both self-deception and enthusiasm can cloud historical judgement.

Beyond certain elementary norms, like accuracy of citation and attribution, consultation of primary sources, attention to the relevant literature, and so on, it may well be difficult to achieve agreement on what constitutes "good scholarship" in this area. But one does not have to search very far for examples that illustrate how, even on this elementary level, the pretence of moral neutrality offers no protection at all against the infringement of scholarly norms.

In parenthesis, it is worth noting that the exclusion of "god tricks" excludes relativism, for the judgement that one situated knowledge is as good (or bad) as any other could only be made from a position that has no partiality itself, that is, no situation. We might also note that the exclusion of god tricks does not exclude the possibility of all privileged knowledge but only that based on the pretensions of the supposedly unsituated subject. It is quite possible that certain situations sensitize one to aspects of reality that remain invisible or blurred when regarded from other points of view.

Applying these general considerations to the historiography of psychology, it is clear that the issues we have to be concerned about are issues that arise out of its situatedness. There are quite a few such issues. For example, we have to be concerned with a set of problems that arise out of our situation in a present time that is different from the time we are studying. So we have to give thought to what is possible and desirable in bridging this gap. Then there are the problems that arise out of the existence of different social locations from which one could regard the history of psychology. This was the kind of problem I addressed in my article on whether the history of psychology had a future (Danziger, 1994). I suggested we look at the implications and consequences of situating the historiography of psychology in different locations with respect to the discipline and with respect to various human groups. Depending on where we situate ourselves, different aspects of our subject matter will come into focus, different priorities will operate, and different contours will emerge.

I hope it will be clear that when I speak of location here it is to social, and not theoretical, situatedness that I refer. Pursuing the historical study of psychology in order to gain support for a specific theoretical position within the discipline is of course likely to result in a form of Whig history and is not to be encouraged. But this should not be confused with the problem of social location and its attendant moral perspective that the historian of psychology shares with all historians. From *this* problem there is no escape, not by resorting to god tricks, and certainly not by resolving to be a good historian on weekdays and a good citizen on Sundays. We have a choice, yes, but it is a choice among different moral locations, not a choice between making judgements from somewhere and making them from nowhere.

NOTE

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2. The Autonomy of Applied Psychology (1990)

Abstract: The claim that progress in applied psychology depended on previous advances in basic psychological research became part of the discipline's scientific rhetoric in the early years of the twentieth century. In reality, however, one finds very few instances where this was indeed the case. Far more commonly, psychologists engaged in finding solutions to practical problems, for example in advertising, the reliability of testimony, or personnel selection, developed their own approaches and methods that owed little or nothing to the basic research of the time. In due course, the major direction of influence was actually the reverse of that claimed by the standard rhetoric: basic research adopted many methodological innovations pioneered by "applied" psychology, including techniques for the analysis of individual differences and control group methodology. Applied psychology merited its name more by its use of concepts, such as association, that antedated the emergence of scientific psychology than by its reliance on an existing basic science.

We are all familiar with what has sometimes been called the two-step model of the relation between science and society (Bakan, 1980). According to this model the practice of science occurs in two phases. In the first phase, that of "pure" or "basic" science, scientists are concerned with the discovery of general laws of nature that have universal validity. Then, in the second phase of so-called "applied" science the task becomes one of applying the previously established insights of basic science to particular practical problems in the world outside the laboratory.

This vision of science became extremely popular during the second half of the nineteenth century and was successfully exploited by research oriented scientists to mobilize support from social institutions whose priorities were dictated by strictly utilitarian rather than intellectual considerations (Kevles, 1977; Weingart, 1976). The prize exemplars for the two-step model were provided by the chemical and electrical industries, but it was quickly generalized as a general norm of scientific progress. What the model insisted on was the necessary priority of basic research. The application of science to industry was supposed to depend on the prior discovery of universal laws of nature under controlled laboratory conditions. Thus, investment in basic research was presented rather like investment in capital goods - it could be expected to pay handsome dividends in due course. By the beginning of the twentieth century it had become part of the generally accepted image of scientific research that it had two levels, a basic level, and an applied level which used the products of the basic level in practical contexts.

At this point it began to become apparent to a few psychologists in the Old World, and rather more in the New World, that something more than the use of laboratory methods would be needed to ensure psychology's acceptance as a fully-fledged science. In the long run laboratory work might not be more effective than pure speculation in mobilizing social support, unless the results of such work could be shown to have practical applications. Successful scientific disciplines had their basic and their applied sectors. So far, scientific psychology only had a laboratory sector whose products lacked any apparent practical utility. The example of the more established sciences suggested that a layer of "applied" research would have to be added if modern psychology were to prosper and to be able to draw on a broader level of social support.

The use of the two-step model of science was widespread among psychologists from the early years of the twentieth century onwards. Already in 1904 we find J. McKeen Cattell stating in a public lecture:

> I see no reason why the application of systematized knowledge to the control of human nature may not in the course of the present century accomplish results commensurate with the nineteenth-century applications of physical science to the material world (Cattell, 1947; p.207).

A rhetorical reference to the relationship between pure and applied studies in the natural sciences became virtually obligatory whenever some new venture in applied psychology had to be justified. Thus, when the *Journal of Educational Psychology* - the first major American journal in the applied field – was launched in 1910, E. L. Thorndike states in his lead article:

> Just as the science and art of agriculture depend upon chemistry and botany, so the art of education depends upon physiology and psychology (p.6).

Such analogies were not limited to the New World. When the Journal, *Industrielle Psychotechnik* was launched in Germany in 1924, the first paragraph of the introductory editorial announces:

> It is the task of psychotechnics to make the methods and results of psychology serve the demands of practical life, just as in a similar way broad practical areas of application of the theory of electricity were opened up by electrotechnics (Moede, 1924).

Hugo Münsterberg, a major pioneer of applied psychology, was a particularly eloquent advocate of the two-step model for the relationship between pure and applied science:

> The history of mankind shows that the greatest technical triumphs were always won through the work of scientists who did not think of the practical achievements but exclusively of theoretical truth. The work of the engineer has always followed where the physical truth seeker has blazed the path.

It cannot be otherwise with applied psychology (Münsterberg, 1914, p.342-3).

During the early years of the twentieth century the volume of applied psychological research gradually grew to the point where it justified the founding of special publication outlets, the *Zeitschrift für angewandte Psychologie* in Germany, and the *Journal of Applied Psychology* in the U.S.A. being the major examples. A major sub-field of applied psychology, educational psychology, had its own journals, the *Journal of Educational Psychology* in America, and various publications in Germany (*Die experimentelle Pädagogik*, *Pädagogisch-Psychologische Arbeiten*, *Zeitschrift für pädagogische Psychologie*). Thus, by the end of World War I, psychological research in its main centres, Germany and the U.S.A., was clearly characterized by a two-tier structure of research, traditional laboratory research on the one hand, and the new applied research on the other. Each of these types of research had its own distinct publication outlets which differed in their editorial policies.

Now the question arises in what sense the research published in the "applied" journals really constituted an application of the results of "basic" psychological research. A more general way of putting this question is to ask what exactly was being applied in "applied" psychology. The notion of "application" implies two constituents: Something, call it X, is brought to bear on something else, call it Y. There is no mystery about Y. These were practical, real-life problems on which psychologists felt they could throw some light: the reliability of testimony in courts of law, the effectiveness of advertising, fatigue among school children, the comparison of instructional methods, and above all, the selection of individuals in terms of a variety of institutional requirements in educational, industrial and military contexts.

But what exactly was it that was being applied ”to” these practical problems? According to the popular two-tier model of science what ”should” have been applied were scientific laws previously established in the basic research laboratories. However, in a systematic review of the relevant journal literature (Danziger, 1990) I have been able to find virtually no cases where this in fact happened.

On the contrary, traditional laboratory work and psychological investigations prompted by practical problems usually took entirely different directions. Take the psychology of memory, for example. Laboratory work in the tradition of Ebbinghaus and G. E. Müller had been in existence for almost two decades when "the psychology of testimony" became a major topic for early applied psychology (Stern, 1904). Was this new line of work an "application" of the methodology and generalizations developed as a result of work with nonsense syllables? Not at all. Those interested in the so-called "applied" problems of memory under real life conditions had to develop their own very different methodology, using meaningful material, and they had to answer theoretical questions for which the generalizations based on classical laboratory work were essentially irrelevant.

Or take another example, the psychology of advertising, which had begun to develop in America by about 1910. Empirical studies in this area involved the psychology of judgment (Strong, 1911). Subjects had to make comparative judgments about the merits of advertising material. Now, a psychology of judgment, based largely on laboratory studies in experimental aesthetics, had been in existence for some time before this. But you will not find those who studied judgment in an advertising context gratefully applying the achievements of the "pure" psychology of judgment. Aesthetic issues were irrelevant in obtaining answers to questions of practical effectiveness. Moreover, the information of interest to the advertising psychologists and those who commissioned their work had (for economic reasons) to be based on the overt responses of large numbers of naïve subjects, not on the intensive analysis of mental processes of judgment in a few sophisticated individuals. So the psychology of advertising developed into a relatively autonomous field that owed little or nothing to earlier laboratory science.

Another example that illustrates the same point involves the psychology of individual differences. Before the emergence of an applied psychology that served the social task of selecting individuals in institutional contexts there had existed a so-called individual psychology

(Binet and Henri, 1895). This psychology was based on the intensive comparative study of certain individuals in an attempt to arrive at a complex qualitative characterization of individual style. Binet's study of his two daughters is a good example of this approach. But the development of mass psychological testing in educational, military and industrial settings actually owed very little to this earlier work which had been conducted without any practical ends in view. Not only did the new applied psychology of selection have to develop its own statistical methodology, but it even had to develop new definitions for fundamental psychological concepts like intelligence, aptitude and personality.

Typically, when psychologists turned their attention to practical, real-life, problems during the first four decades of the twentieth century they developed methods of investigation and modes of conceptualization that were developed *sui generis* and that diverged sharply from the then existing laboratory practice of experimental science. This was nowhere more apparent than in the vast new field of mental testing, but it was also the case in other areas. Where genuine attempts were made to apply some of the methods and concepts of laboratory psychology to practical problems, disillusionment often followed. Sometimes this disillusionment set in quite soon, as in the field of experimental psychopathology, where brass instruments were quickly abandoned for paper and pencil methods (Popplestone and McPherson, 1984). Sometimes the disillusionment with methods taken over *holus bolus* from traditional experimental psychology took a little longer to develop, as in the field of experimental educational psychology, or experimental pedagogics, but develop it did (Travers, 1983). By and large, the areas where applied work successfully based itself on previously existing pure research (e.g. work on sensory acuity or sensorimotor coordination) constituted only a small part of the entire spectrum of psychological work in practical contexts. One could try to rescue the two-step model of "pure" and "applied" research by arbitrarily limiting the label "applied psychology" to these atypical areas, but this would leave most psychological research carried out in practical contexts outside the field of applied psychology.

What we find in such research during the first four decades of the twentieth century is some persistence of old psychological notions, like associative memory, that antedated the coming of experimental psychology and owed nothing to it. What we do *not* find is the application of specific empirical or theoretical generalizations based on pure research to practical problems outside the laboratory.

Most of the time the relation of "applied" psychology to its "pure" counterpart did not in the least correspond to the two-step model that had been popularized by examples from the physical sciences. The actual relation of "basic" and "applied" psychology was not grounded in the generalization of natural laws from the laboratory to practical real life problems. It would have been far closer to the truth to speak of "practical" rather than "applied" psychology. But that would have meant dispensing with the very considerable rhetorical effect conveyed by the term "applied psychology." For such a term implied (largely without foundation) that psychology conformed to the two-step model of basic science and its applications that had been so successfully deployed by representatives of the established and relatively well funded physical sciences (Potter & Mulkay, 1982). The term "applied psychology" must be regarded as part of the rather extensive armamentarium of rhetorical devices (Leary, 1987) that the new discipline of psychology employed to legitimize its claim to scientific status.

But in the long run talk of "applied psychology" had the effect of hiding much more than the existence of two parallel and factually independent disciplines of psychology, one devoted to the pursuit of abstract truth under laboratory conditions, the other devoted to the solution of practical problems. If one follows the relationship between these two disciplines into the period between the two World Wars one finds that it is often the *reverse* of that implied by the traditional two-tier model. Increasingly, one finds that quite fundamental methodological innovations that originated in the area of so-called applied psychology are imported into the research practice of laboratory psychologists.

A notable example is the use of control groups which was completely unknown in experimental psychology prior to the nineteen twenties, and which only became common in laboratory practice during the next two decades (Boring, 1954). However, the use of control groups in psychological research originated in the practical context of research in schools during the early years of this century. Parallel grades of school children began to be used at the time to compare the effects of different conditions of instruction on such variables as mental fatigue and memory transfer (Winch, 1908; 1911). Only after the use of control groups had become accepted in the "applied" field of educational psychology did their advantages begin to be appreciated in the context of "pure" laboratory research Danziger, 1990).

Another example of a fundamental methodological innovation that was pioneered in practical contexts was the use of individual differences in performance measures for the investigation of intelligence and personality. The methods employed in theoretically motivated research on cognitive processes were clearly irrelevant for the solution of practical problems. They were the introspective methods pioneered by the Würzburg School in the early years of this century, or methods based on similar approaches to the study of cognition (Humphrey, 1950). Such methods were of no help in dealing with practical problems of selection in educational, military or economic institutions. Therefore, psychologists working for such institutions had to develop their own methodology, i.e. the methodology of mental testing. Although this technology had been initially developed for use in practical contexts, it soon gave rise to a considerable theoretical literature based on the factor analysis of the intercorrelations among scores on tests of intellectual functioning. In fact, the entire field of psychological theory devoted to the structure of intelligence owed its existence to methodological developments originating in "applied" psychology.

A similar state of affairs prevailed in the field of personality research. At the end of World War I theoretically motivated personality research was virtually non-existent. This entire area of "pure" research only emerged gradually during the nineteen twenties and thirties as a result of the increasing availability of techniques of investigation that had been initially developed for the solution of practical tasks. More specifically, these were the techniques of projective testing, developed in the context of clinical diagnosis, and personality ratings, initially developed for purposes of personnel selection in both civilian and military contexts (Parker, 1986).

Expressive methods had a similar practical background (Geuter, 1984). The entire area of "personality" as a recognized field of psychological investigation owes its existence to the need to provide a theoretical foundation for work that had flourished as a result of widespread attempts to give practical answers to practical problems (Cohen, 1983).

What conclusions can be drawn from this brief review of early historical trends? Undoubtedly, one can detect lines of influence between "basic" and "applied" psychology in both directions. However, these influences were not symmetrical. Generally, the influences emanating from the established "basic" psychology were relatively specific in character and of limited application. They included such things as specific techniques of measurement and the utilization of specific findings in limited areas like sensory discrimination. By contrast, the influences in the reverse direction often involved a fundamental methodological re-orientation that resulted in widespread changes in "basic" psychology. In other words, the relationship between "applied" and "basic" psychology came close to being the reverse of that suggested by the two-step model borrowed from late nineteenth century natural science.

Having arrived at this conclusion, it must be emphasized that it is not applicable to the whole of psychology at all times and in all countries. First of all, there were always large areas of both "basic" and "applied" psychology where there was no detectable influence in either direction. To a significant degree, both divisions of psychology remained autonomous. Secondly, the susceptibility of "basic" psychology to methodological innovations originating in "applied" psychology was more marked in the USA than in most of Europe (Danziger, 1987). Conversely, the autonomy of the two divisions was, on the whole, greater in Europe. Thirdly, it must be emphasized that the present review, and therefore all the above generalizations, are limited to the early period in the development of "applied" psychology, the period before World War II. After World War II the pattern changes. Differences between North America and Europe gradually become less marked, and "basic" psychology has more to offer to "applied" psychology. However, it must be remembered that the "basic" psychology of this later period is very different from the "basic" psychology that the pioneers of "applied" psychology had available to them. Large areas of "basic" research during the post-World War II period already bear the imprint of influences that originated in the "applied" research of the first half of the century.

NOTE

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3. What I Wish I Knew in 1950 (1997)

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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(B) Psychological Objects

4. The Social Context of Research Practice and the Priority of History (1993)

Abstract: During the period when positivism was dominant scientific methodology and its products were believed to rest on ahistorical principles. More recent work in the sociology, philosophy and history of science has undermined this belief. Modern science studies are based on the realization that the nature of any actually existing science will be profoundly determined by the historical conditions of its existence. Hence the objects of scientific research must be regarded, not as purely natural, but as historical facts. This even applies to empirical data because they are heavily dependent on theoretical preconceptions and methodological constructions. The historical understanding of a science like Psychology is therefore no mere garnish but is concerned with the essential nature of the discipline.

Half a century ago virtually everyone regarded the products of natural science, its discoveries and findings, as being different from other products of human activity in that they transcended the mundane circumstances of their production. Although scientific results were of course produced by people of flesh and blood, working under specific historical conditions, the results themselves were generally seen as independent of these origins once they were received into the canon of genuine science (see however Fleck, 1935).

Such beliefs were reflected in the philosophy of science which predominated in the Anglo-Saxon countries at that time. It had been a primary concern of the logical positivists to set down criteria by which one could distinguish science from non-science. A standard philosophical rationale for the purity of science was based on an explication of the purity of its language. It was said that in the language of science every statement could be either derived by necessary deduction from first principles, and/or it could be demonstrated to be true by reference to direct empirical observations. By contrast, the mundane language of everyday life was sloppy and ambiguous; most of the statements it contains were neither derivable by any process of strict logical deduction, nor were they unambiguously verifiable by empirical observation.

American psychologists were understandably fascinated by a philosophy that promised to end the persistent doubts about the scientific status of their field. By spelling out the criteria of scientificity in a very explicit way logical positivism gave psychology a clear set of conditions whose fulfillment should result in broad recognition of the psychologists' claims to the status of scientists (Mandler and Kessen, 1959). This philosophy seemed to provide the discipline with a prescription for how to become a genuine science, and in the next few decades this prescription was quite thoroughly internalized by large sections of the discipline (Toulmin and Leary, 1985).

In the meantime the world has changed. Attitudes to science have become, on the whole, more ambivalent than they were half a century ago. (When I say "science," throughout this presentation, I am of course using the term in its usual Anglo-Saxon sense of "natural science"). This ambivalence is a consequence of the way in which certain negative aspects of the application of science intruded on people's consciousness, whether in the form of weapons of mass destruction or in the form of environmental damage or threats to health as everyday facts of life. Inevitably, such developments have tarnished the once unblemished image of science in people's minds.

As might have been expected, these developments have been accompanied by a change of fashions in the philosophy of science, a change that had already been placed on the agenda by the internal difficulties of logical positivism.

In the post-positivist phase that has prevailed during the last quarter century the differences between science and non-science have not seemed nearly so great and so clear cut as they seemed before, and the need to make this distinction has lost some of its urgency. A new field of social studies of science has flourished, and this field takes scientific activity as an object of sociological examination in exactly the same way as any other human activity. [Knorr-Cetina (1984) provides one well known example for such studies, but the English language literature in this field is by now enormous]. Doing science is now seen as being as much a matter of social organization, competition for scarce resources, social interests, rhetorical persuasion and consensus building as many more mundane forms of human activity. Thus is science robbed of its moral exclusiveness. Perhaps we should see this disenchantment of science as the last stage in the "disenchantment of the world" that Max Weber diagnosed as being characteristic of the modern period.

Within Anglo-Saxon philosophy of science the process of disenchantment manifested itself in a turning away from the relentless formalism that had been so characteristic of the period dominated by logical positivism. The old philosophy had tried to establish the purity of science on the basis of the purity of its language. This was largely based on a strict separation of an empirically grounded data language and a formal theoretical language. Unfortunately, this distinction could never be rigorously justified and soon became a casualty of powerful philosophical attacks (Achinstein, 1968; Hesse, 1970; Hanson, 1959; Quine, 1953). There is no need for me to cover this well known ground here. Suffice it to mention that the empirical statements of experimental science are never formulated in a language that simply refers to sensory impressions but in a theory loaded language. When they are talking as scientists, physicists do not talk about displacements of light points in their visual field, they talk about measuring the length of light waves. But reporting on the length of light waves already presupposes a wave theory of light as well as some theoretical understanding of the measurement process and the instruments it relies on. Psychologists do not empirically report marks on paper but scores on an intelligence or personality test. That presupposes a massive framework of theoretical presuppositions that make possible the identification of certain pieces of printed paper as intelligence tests and other pieces as personality tests, not to speak of the battery of assumptions and decisions that lies behind the concept of a "score" (Danziger, 1990a).

Thus, the so-called empirical statements that occur in scientific reports and texts depend as much on particular theoretical frameworks as they do on the sensory experience of individual scientists. But it is not only that some kind of theoretical framework is necessary for formulating an empirical result, it is also the case that the appearance of the empirical result depends on prior methodological decisions. Experimental facts are not usually discovered just lying around the floor of the laboratory or blown in through the window by the wind. They have to be painstakingly constructed with the help of complex instruments and carefully thought out procedures. But the use of one set of instruments and procedures rather than another depends on decisions and assumptions that are certainly underdetermined by any list of scientific facts known at the time. In a very real sense, empirical data are not the starting point of science, rather they are the yield that science ends up with as a result of an elaborate process of production (Bhaskar, 1978).

It is clear that if we want to understand this process of production we will have to pay a great deal of attention to the underlying theoretical assumptions and the methodological choices that play such an important role in the constitution of empirical data. No longer is it permissible to relegate questions about the genesis of theoretical assumptions to some limbo of irrationality, called the "context of discovery," that was created with the express purpose of preserving the purity of scientific rationality in the form of a "context of justification." Real science does not conform to this hopelessly idealized image. The rational and the irrational are thoroughly entangled, or, to put it another way, the rational side is not nearly so rational and the irrational side not nearly so irrational as the older view would have had us believe.

What then is the origin of the implicit and explicit theoretical assumptions and methodological choices that co-constitute the empirical yield of science? Part of the appeal of the idealized view of science was due to its heroic image of the individual scientist. Scientific activity was the arena of a tournament in which the ingenious man of science (generally it was always a man) pitted his wits against nature (feminine, of course) who was reluctant to yield up her secrets. The products of science therefore depended on three sources: Nature herself, specific psychological qualities of the individual scientist, and certain principles of scientific rationality and morality that regulated the scientist's practice. These last were conceived ahistorically - they were basic rules of logic, like the rule of non-contradiction, and basic rules of ethics, like the rule of honesty, for example, that had not changed since the time of the Ancient Greeks (Ben-David, 1971).

Such an account could be regarded as useful only as long as its function was regarded as prescriptive rather than descriptive. Of course people knew that the actual practice of science included features not covered by this account, but those features were relegated to the history of science, a field of messy detail that was quite separate from the lofty world of principle inhabited by the philosophy of science. However, this separation has not lasted. As the interest in science shifted from a prescriptive interest in an idealized science to an interest in the actual practice of science as a human activity so the study of the history of science has become crucial for an understanding of the nature of science.

Once we see science, not as an idealized abstraction, not as a set of disembodied propositions, but as an activity engaged in by real flesh and blood people, then the heroic image of the individual scientist confronting nature collapses. The timeless canons of scientific method turn out to be not timeless at all but subject to massive historical change as well as profound variation that depend on local conditions and traditions (Danziger, 1990b; van Strien, 1990a). The epistemic access to nature that science provides is always a collective access, and the arena within which the ingenuity of the individual scientist is allowed to operate is an arena constituted by social groups whose life and whose struggles are subject to the same mundane constraints as are those of other social groups.

Before the raw givens of nature can become data for science, can become the kinds of things that science can actually work with, they have to be transformed by the collective activity of human investigators. This transformation is not only material - chemical substances have to be purified, for example - but also conceptual. I have to think of the white powder in front of me as a chemical compound with a certain molecular structure before I can set to work on it as a chemist. As P. van Strien has reminded us: "Facts are always interpreted facts. The development of science depends on the success of competing traditions in constructing a plausible account in which these "facts" are interpreted in the light of theory" (van Strien, 1990a, p.39).

In other words, insofar as objects are objects <u>for</u> science they exist within a certain conceptual framework. But the conceptual frameworks of science, as we know very well, have undergone much change in the course of history. They are historical products. It follows that the objects of science which only exist within these frameworks (and take their meaning from them) are also historical objects that change in the course of human history (Danziger, 1992).

To quote the German philosopher of science, Kurt Hübner:

> most of the objects that science has dealt with in the course of its history, objects which appear ostensibly to be the same, really bear only a family resemblance to one another. Whether it be space, time, the starry heavens, the forces which move bodies, or some other object of science, we would look in vain for some shared or common meaning which might apply to any of these objects throughout their respective histories and which as such . . . might serve as the common and continual ground for all the scientific theories devoted to any such object. It was hard enough for mankind to grasp that the same time does not tick off in all parts of the world. It may be even more difficult to grasp that when we investigate some scientific object, both today and as it existed in the past, we are not necessarily speaking about one and the <u>same</u> thing (Hübner, 1979, p.218).

Hübner was speaking of the objects of physics. But if the objects of physics must be regarded as embedded in human history, how much more obvious is this in the case of the objects of psychology. The memory that a contemporary student of the area investigates is not the same object as that which Ebbinghaus tried to study by means of nonsense syllables, and neither of them has more than a tenuous connection with memory as understood by Aristotle (Danziger, 1990c). The individual differences that an Eysenck, for example, believes to constitute objective features of the world have nothing in common with the individual differences pondered by a Carl Jung. The "behaviour" studied by the "behavioural science" of the recent past is a very different object from that which inspired John B. Watson or Lloyd Morgan.

Such historical changes are due to changes in the framework within which different generations of scholars and scientists have operated. But such changes of framework are embedded in a general historical situation that includes the values, the implicit assumptions and the social interests of groups of investigators as well as their placement in a broader sociocultural context whose influence they cannot escape. Thus, if the objects of science necessarily exist within some theoretical framework, and if such a framework is always part of a broader historical context, it follows that the objects of science are historical objects. But in order to arrive at an adequate understanding of historical objects we must engage in historical studies (Jüttemann, 1986). That is why history can legitimately lay claim to a certain priority when we try to understand what it means to study some topic scientifically.

The history of science has, as philosophers like Hübner have pointed out, a propadeutic function. Among other things, it provides us with "a standard against which to judge the scope, validity, and applicability of the methods, principles, postulates, etc., that have been worked out by scientific theoreticians." It also has an important critical function that counteracts the "degeneration" that often sets in when a particular scientific position becomes generally accepted. The position is soon considered "self-evident" and eventually becomes something that can no longer be seriously questioned (Hübner, 1979, p.94). In psychology one can most readily detect evidence for such a degenerative process on the level of methodology (Gigerenzer and Murray, 1987).

Contemporary philosophers who stress the propadeutic function of history of science had some notable predecessors, for example, Dijksterhuis (1961) and Duhem (1954). The latter believed that "the legitimate, sure and fruitful method of preparing a student to receive a physical hypothesis is the historical method," because, as he put it, "to give the history of a physical principle is at the same time to make a logical analysis of it" (Duhem, 1954, p.268-269). Duhem advocated the historical approach even though he believed that physical concepts became "perfected" in the course of history, in other words, that later versions were in some real sense better than earlier versions. This is a position that is easier to maintain in physics than in psychology. Duhem showed that even though the more recent position might be more satisfactory scientifically one could not really understand scientific development without introducing the historical dimension.

In psychology the superiority of more recent positions is often not as obvious as it is in physics. How much more reason then to turn to historical considerations when trying to assess the status of recent "advances" in psychological theory. Of course, this means running the risk of discovering for oneself the truth of the old dictum that those who ignore history are condemned to repeat it. But that seems preferable to a state in which one lacks the means to distinguish real progress from the illusion of progress.

But is it still possible to make such a distinction if one historicizes science to the extent that I have? Let me try to answer by invoking an analogy. In the traditional view the process of science was seen rather like the painting of a picture, a landscape perhaps, that was gradually being represented on canvas. Both the landscape and its pictorial representation were vast and complicated, but over a long period of time more and more details would be filled in, until one day the picture would be finished, and we would be able to enjoy the perfection of the final product.

To-day we know that we have to make some changes in this analogy. The picture we are confronted with rather resembles one of those canvases that has been painted over many times by different painters. After the first one each of them has reacted both to the work of his predecessor and to the landscape as he saw it. Of course, they didn't all see it in the same way, and in painting it they pursued different purposes. They learned from the techniques of their predecessors, but they also reacted against their apparent inadequacies. Each of them thought of his predecessor's style as old-fashioned and of his own work as advanced. This process need never end. But from the fact that there may always be a fresh artistic style, it does not follow that there exists no landscape to be painted. It also does not follow that one painting is as good as another. Some are better. But that is a judgment which is only possible on the basis of certain criteria which change historically. Does this change constitute progress? The problem is one of definition. There is no such thing as absolute progress, only progress in this or that sense. And that means that progress is often ambiguous - which is exactly the conclusion many people have arrived at with regard to scientific progress, or economic progress for that matter.

In concluding my argument I cannot improve on the words of P.J. van Strien: "It cannot be denied," he writes, "that awareness of the history of current theories also leads to a sense of the relativity and temporality of all theories. Of course, this will only lead to resignation if we are still hoping for timeless Truth. Problem situations are always historically relative situations. Can we then expect the answers to be context free?" (van Strien, 1990b, p.313).

NOTE

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5. Natural Kinds, Human Kinds, and Historicity (1997)

Abstract: Common psychological categories, such as personality, motivation, attitude, emotion, do not correspond to inherent divisions within a timeless human nature. They do not represent natural kinds, such as gold, which exist independently of how we depict them. The categories in terms of which humans understand their individual conduct and experience are part of human social life and change as that life changes. They are "human kinds" (Hacking) also in the sense that humans are affected by the terms in which they understand themselves. The culturally embedded and historically changing meaning of specific psychological categories forms a layer of implicit knowledge usually taken for granted by more explicit psychological theorizing.

I want to begin by making a distinction between two kinds of theorizing that go on in psychology - and any other science, for that matter. Mostly, when we talk of theorizing, we are referring to an activity that involves *explicitly* formulated propositions, explicitly articulated assumptions, and often clearly described models. However, there is another kind of theorizing that goes on out of view and usually remains behind the scenes. It is this second kind of theorizing that I want to talk about. In particular, I want to focus on certain presuppositions that are built into the network of *categories* that psychologists use to define the subject matter of their scientific and professional practice.

One cannot formulate psychologically relevant theories without the use of psychological categories. Nor can one communicate one's empirical observations without falling back on a network of pre-existing psychological categories which define what it is that is being observed. To be psychologically interesting both theories and observations have to be couched in terms of psychological categories. Learning, motivation, sensation, intelligence, personality, attitude, constitute examples of such categories.

Psychologists have devoted a great deal of care to making their theoretical concepts clear and explicit. But much of this effort has been undermined by their complaisance about the way in which psychological phenomena are categorized. The meaning of these categories carries an enormous load of unexamined and unquestioned assumptions and preconceptions. By the time explicit psychological theories are formulated, most of the theoretical work has already happened - it is embedded in the categories used to describe and classify psychological phenomena.

A century of specialized usage has not sufficed to eliminate the dependence of basic psychological categories on shared understandings in the general culture. Psychology may have developed certain theories *about* drive, *about* intelligence, *about* attitudes, and so on, but the network of categories that assigns a distinct reality to drive, to intelligence, to attitudes etc. has been adopted from the broader language community to which psychologists belong.

One consequence of this is a disjunction between the way scientific psychological discourse handles explicit theoretical concepts and taken for granted psychological categories. Conventionalism characterizes the deployment of explicit theoretical concepts. It is generally accepted that such concepts are human inventions subject to continuous revision in the light of new research. However, when it comes to the domains that their theories are meant to explain psychologists are inclined to adopt a stance of unreflecting naturalism. They tend to proceed as though everyday psychological categories, like intelligence, emotion or learning, represented natural kinds, as though the distinctions expressed in such categories accurately reflected the *natural* divisions among psychological phenomena. Psychological discussions typically assume that there really is a distinct kind of entity out there that corresponds exactly to what we refer to as an attitude say, and it is naturally different in kind from other sorts of entities out there for which we have different category names, like motives or emotions.

The belief that scientific psychology adds to our knowledge of attitudes, drives, intelligence, etc., involves the implicit assumption that there is a fixed human nature whose natural divisions are reflected in this received network of categories. A sensation is not an emotion and an attitude is not a memory, though relationships between them are conceivable. While psychological theory addresses at length such topics as the structure of intelligence or the laws of motivation, it quietly assumes that the terms "intelligence" and "motivation" refer to distinct kinds that require explanation by means of separate sets of theoretical constructs. What is certain, however, is that psychological theory requires some pre-understanding of that which it is a theory of.

That pre-understanding has generally involved the unspoken conviction that psychological categories constitute historically invariant phenomena of nature, rather than historically determined social constructions. Therefore, the most appropriate way of investigating them would be by means of the experimental method of natural science rather than by means of historical analysis.

The traditional historiography of psychology reflected these commitments. It did not question the currently entrenched divisions among psychological domains, assuming that those divisions truly reflected the actual structure of a timeless human nature. Though categories like "intelligence", "personality" and "learning", may only have become *psychological* categories at the end of the 19th century earlier texts were reinterpreted as though they contained psychological theories about such topics. The timelessly true shape of such categories was assumed to be defined by present day usage (Danziger, 1990). Older work was appreciated only insofar as it "anticipated" what we now know to be true.

The older historiography considered only two kinds of factors in the development of a science, the discovery of empirical phenomena and the construction of explicit theories that would account for them. It tended to overlook the existence and historical change of categories that incorporated basic assumptions and provided the framework which gave a particular structure to both theories and phenomena.

One historian of science whose work ran counter to the prevailing trend was the French historian of biology, Georges Canguilhem. Among the topics whose history Canguilhem (1955, 1979, 1989) investigated was that of the reflex, biological regulation and normality. These are clearly not theories, as that term is ordinarily used. One can have theories *about* reflexes, about biological regulation, about normality, but these notions themselves are not theories. Nor are they phenomena. They are categories that provide a framework for identifying phenomena, giving them a particular meaning. Such frameworks are historical constructions, and it is the job of the historian of science to trace their development.

The topics whose historicity Canguilhem investigated were biological categories. In due course, some of these biological categories provided the basis for current psychological categories. Examples of such categories are those of stimulation, intelligence, behaviour, and learning. These provide a framework for describing and identifying psychological phenomenona in a certain way. The possibility of describing phenomena in terms of such a framework did not always exist because these categories only became part of the history of psychology relatively recently.

In a recently published book (Danziger, 1997), I traced historical changes in the meaning and use of such biologically derived categories, as well as several other common categories of psychological discourse, including personality, motivation, emotion and attitude. In each case I explored the historical context in which modern psychological categories emerged and the way in which they gradually acquired their current meaning.

When one conducts such an analysis it soon becomes apparent that 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. Although, in the light of their historicity, it is difficult to say that these categories represent *natural* kinds, what one can say is that they represent *relevant* kinds. 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. That is reflected in historical changes in psychological categories.

Nevertheless, psychologists have always tended to think of the categories they employed as "natural kinds", groups of naturally occurring phenomena that inherently resemble each other and differ crucially from other phenomena. Psychological categories were assumed to represent natural divisions among objective features of the world that existed independently of the efforts of psychologists.

However, there are good reasons for rejecting natural kinds as an appropriate conceptual basis for psychology. Natural objects, as defined by natural kinds, are indifferent to the descriptions applied to them. If we change our identification of a chemical compound as a result of advances in techniques of analysis, this changes our knowledge of the compound but the compound itself remains the same compound it always was. But psychological objects behave in a very different manner. A person who learns not to think of his or her actions as greedy or avaricious but as motivated by a need for achievement or self-realization has changed as a person. Students who learn to classify things they see under a microscope no longer have the same perceptual experience they had during their initial encounter with microscopic preparations. The sorts of things that psychology takes as its objects, people's actions, experiences and dispositions, are not independent of their categorization.

This is hardly surprising because the individuals who are the carriers of psychological objects are able to represent these objects to themselves in a self-referential fashion. Radical behaviourists believe that such representations have a purely epiphenomenal status, but more generally it is believed that their existence introduces a profound distinction between psychological objects and natural objects that have no capacity for self-reference. The manner of their articulation in language becomes a constituent part of psychological objects so that their identity changes with changes in psychological language (Taylor, 1985).

Another reason why psychological objects are not independent of their categorization is that they are intelligible only by virtue of their display within a discursive context. Whatever forms they assume are due to their embeddedness in particular discursive practices (Semin & Gergen, 1990). The conception of psychological entities as natural objects is often grounded in a naive belief in the existence of a private world of psychological essences. However, distinctions that constitute emotions as emotions, motives as motives, cognitions as cognitions, and so on, do not exist in some sealed private box *before* they are so labelled in public. Identifying experiences, actions and dispositions is not like sticking labels on fully formed specimens in a museum. Psychological objects assume their identity in the course of discursive interaction among individuals.

Distinguishing among kinds of actions and kinds of people is part of human interaction everywhere. Psychology attempts to offer causal explanations of the domains created by these distinctions, using empirical investigation and theoretical hypotheses. The extent to which these attempts act back on the distinctions themselves depends on the authority commanded by psychological expertise in a particular culture. It also depends on the way in which psychological work relates to existing needs and interests. If the work is truly innovative and threatens established preconceptions and relationships it will meet a great deal of resistance. But the great bulk of psychological work has never been in any danger of this fate. Both in its inspiration and in its effects it has been profoundly conservative. Except on a very superficial level, it has shared the prevailing preconceptions of its culture and arranged its investigations in such a way that no knowledge with revolutionary implications could possibly emerge from them. In assessing the effect of psychological science on psychological kinds it is easy to overlook the biggest effect of all, namely, the reinforcement of existing culturally embedded preconceptions and distinctions.

This cultural embeddedness accounts for the taken for granted quality that so many psychological categories possess. It is a quality that makes them appear "natural" to the members of a particular speech community sharing a certain tradition of language usage. However, this sense of "natural" is not to be confused with the concept of natural kinds that has featured in the present discussion. Natural kinds have nothing to do with culture, whereas the natural appearing kinds of psychology have everything to do with it. We need a term for the latter that will recognize this distinction. The term "human kind", introduced by Ian Hacking (1992), is useful here. Hacking's main interest is in categories that define kinds of people, like homosexual or multiple personality disorder, but, in principle, kinds of human activity are covered too. The difference between natural and human kinds rests on the distinctions I have already mentioned, that is, whether the kind is self-referring and whether it is intrinsically part of social practice.

One consequence of the distinguishing features of human kinds is that their relationship to the reality they refer to is different from that of natural kinds. The latter refer to something that would be the case, whether any particular act of reference had occurred or not. Human kinds, on the other hand, affect that which they refer to. Historically, "the category and the people in it emerged hand in hand" (Hacking, 1986: 229). The way humans categorize themselves and their activities is not independent of their actual conduct, because, as we have noted, such categorization is part of human conduct and therefore not a matter of indifference to the people concerned. This leads to what

Hacking (1994) has described as "looping effects", the reaction of people to the classes to which they and their activities are consigned. This reaction may range all the way from passive acceptance to militant refusal. In other words, the meaning of human kinds develops and changes in the course of interactions among those affected. (This interaction has sometimes been described as a process of "negotiation", though that implies a more deliberate and more articulate process than is often the case). Human kinds of the sort I have analysed (Danziger, 1997) are not natural kinds, but neither are they mere legends. They do refer to features that are real. But it is a reality in which they are themselves heavily implicated, a reality of which they are a part.

The reality to which human kinds refer is a cultural reality, and that in several senses: First, because the phenomena depicted are ones which exist only in some cultural context; second, because these phenomena commonly depend on a certain social technology for their visibility and their production; third, - and this is the aspect that has been the focus here - because the categories used in their representation are culturally grounded. Psychology has acquired its categories from the culture that gave rise to it and in which it remains embedded.

Consequently, all psychological categories have been historically variable constructions. To gain an understanding of the categories in common use at the moment, we need to see them in historical perspective. When we go back to the origin of these categories we usually find that what later became hidden and taken for granted is still out in the open and questionable. We also discover some of the reasons why a new category was introduced and by whom. Because psychological categories are heavy with historically formed pre-understanding one hopes that a better understanding of their historicity will promote their more insightful deployment in everyday practice.

NOTE

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6. Psychological Objects, Practice, and History (1993)

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

1. Against Abstractionism

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

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

Of course, it is still possible to discuss theories in the abstract, as long as we are clear about the distinction between the objects of such discussion and the world in which theories normally are put to use. Abstracted theories are a construction of metatheorists; they exist only in a Popperian "third world" of pure forms (Popper, 1972) whose relationship to other worlds is by no means clear. Now, while there may be psychological theories that, one suspects, were invented primarily so they could be featured in textbooks, most of us would regard this as a perversion. For the real purpose of theories, surely, is not to provide grist for the metatheoretical mill, but to help in the explanation of events and to guide action.

In the world of psychological research that gives rise to them, psychological theories are co-ordinated to specific empirical domains which they are supposed to explain in some way.

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

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

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

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

What can be established is that there was a change in the social context within which theoretical claims had to be justified. In the nineteenth century it was still possible for individuals who had no relevant professional or academic affiliations whatever to make theoretical contributions to psychology that were taken every bit as seriously as the contributions of those who had such affiliations. It did not occur even to those who disagreed with the psychological ideas of John Stuart Mill or Herbert Spencer, for example, to reject them on the grounds that these men were mere amateurs whose competence in research had not been certified by an appropriate institution or professional community. Their theories were discussed in the same breath as those of men such as Alexander Bain who did happen to have academic positions. Such affiliations were simply not relevant in establishing theoretical credibility. In other words, the distinction between experts and laymen lacked the rigid institutional basis that it was soon to acquire.

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

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

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

Making sense of theory gets us involved in the content of empirical data. But data, as we know only too well, are not raw givens - they are symbolic constructions that are reproduced in the pages of journals and text books. Like all such constructions they have form as well as content; they are arranged in the form of statistical tables, for example, or in the form of graphs, or in the narrative form of a case history. Right away we can see that empirical domains may differ, not only in their content, but also in their form. And that opens up new possibilities for the interrelationship of theory and empirical data in psychology. If theories have to be co-ordinated to empirical domains to be taken seriously, the structure of those domains becomes relevant for the work of theory construction. If my theory has to justify itself by its ability to explain statistical relationships among measurable variables, for example, its statements will have to be cast in a mould determined by this task. But if, on the other hand, the theoretical task lies in making sense of an unfolding case history, a different type of theoretical formulation would do the job much more effectively.

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

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

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

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

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

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

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

During the modern period theoretical positions in psychology have had to have a primary link to particular empirical domains, though they often claimed to have validity beyond those domains. Theories that claimed *only* a general validity without a special empirical domain that was peculiarly theirs were likely to be dismissed as unscientific speculations and, therefore, not to be taken seriously.

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

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

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

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

<u>2. Psychological Objects</u>

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

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

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

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

Psychological objects may be certain categories of people, such as experimental subjects, or the 'clients' of counsellors. Such categories of people exist only as the objects of the psychologist's intervention, and they would not exist at all if it were not for that intervention. It is the psychologist's own professional activity that creates these categories in the first place. If there were no psychologists, these categories of people would not exist. In other words, the psychologist can never investigate any 'natural' human category directly; he/she must constitute an object of investigation in the course of that investigation. The persons who, voluntarily or under duress, play the part assigned to them by psychologists in situations constituted by psychologists, have become members of a category of persons that has no members outside these situations. This does not mean that there are no relationships between psychological objects and other objects in the world, but the nature of these relationships obviously cannot be discovered by psychological investigation because they cannot be concerned with anything but psychological objects.

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

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

These now widely recognized phenomena still leave open the question of possible links between psychological objects and other kinds of objects, a question which, as has been indicated, cannot be answered by psychological means. To address such questions a different level of discourse has to be adopted. In some sense such discourse would undoubtedly be metatheoretical. But it is not an analysis of psychological theories that is required here, for reasons that have already been outlined. Such theories commonly refer to psychological objects whose existence and natural status they take for granted. It is not the adequacy of the theories in explaining psychological objects which is at issue here, but the constitution of those objects themselves. That constitution is a social process, which is part of human history. Certainly, psychologists are always recreating the objects of their investigation in the course of investigating them, but they are not free to do so at random. They are constrained by historically constituted structures, both cognitive and practical. Psychological objects are also the products of history and can only be understood as such.

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

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

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

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

3. Categorical Construction

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

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

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

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

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

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

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

As soon as one does this one has to take seriously the coherent quality of psychological discourse. By this I do not mean grammatical coherence, of course, but conceptual coherence. This is what was implied by my earlier use of terms like 'framework'. Typically, psychological objects are not constructed one by one, independently of each other, but in a coherent system. By this I do not only mean the 'systems' associated with certain individuals, but, more importantly, the kind of coherence which makes productive discourse among a group of individuals possible. The elaboration of psychological objects has been a collective enterprise made possible by the fact that these objects are reproduced by many individuals who share what .might be called the relevant principles of construction. This process functions best when these principles are not deliberately employed but are simply taken for granted and allowed to do their work spontaneously. Then everyone knows without any argument what is meant when different kinds of psychological objects are referred to. When this shared pre-understanding is not present, psychological discourse typically loses its coherence and misunderstandings abound. The fate of culturally alien psychological systems in North America, like that of Wundt, of the Gestaltists, of phenomenology, of depth psychology, provides many abundantly documented examples of this course of events (Ash, 1985; Blumenthal, 1980; Burnham, 1967; Henle, 1980; Jennings, 1986).

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

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

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

4. Practical Construction

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

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

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

Such a division, however, is itself highly artificial (Farr, 1978). At most, one might make a distinction between those aspects of the social practice of investigators that depend on their

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

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

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

That applies no less to the products which the conventional terminology labels 'empirical' than to those which it labels 'theoretical'. Typically, the construction of empirical objects takes place in two phases. In the first phase, a number of participants work together in defined investigative situations to produce 'raw data'. The work of the participants proceeds according to strict rules that govern their inter-relationship. In the second phase the investigators manipulate the record that constitutes the raw data so as to produce a form of product that is publishable according to the conventions of the day. This process also is governed by strict rules that have nevertheless seen considerable historical modification. Needless to say, investigators' knowledge of these rules in large measure determines what aspects of the investigative interaction are considered worth recording and, therefore, worth eliciting. For instance, an investigator who knows that lengthy introspective reports are not publishable is not likely to ask for them or to take them seriously as recorded data if they are spontaneously offered.

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

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

In dealing with the construction of empirical objects one has, therefore, to distinguish between specific instances of such objects, produced at a particular time and place, and the general features of such objects which characterize them over extended historical periods and in numerous locations. The intra-disciplinary rules for producing empirical objects can take on the appearance of purely technical rules as long as the general features of those objects are taken for granted. Thus, rules for producing good introspective reports can appear to be based on purely technical considerations, as long as it is accepted that the desired product will have the form of an introspective report. The same applies to the explicit rules used in the construction of empirical objects that have the form of a statistical aggregation of individual performance measures. Variations in such rules are governed by technical questions of finding the best means for arriving at a given end. But variations in the ends themselves, that is, in the general type of empirical object desired, are not reducible to technical questions within the discipline. Such variations are only explicable on a level of historical inquiry which takes disciplinary patterns and trends as its subject matter.

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

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

5. Establishing the Enterprise

The production of psychological objects requires scarce resources which have somehow to be diverted from alternative employment and put to work on some disciplinary task. Although fixed investments in space and apparatus had some importance, the most significant social resource mobilized by psychology was always the time and trained skill of investigators and practitioners. The more these resources are made available for this purpose within a particular society, the more the production of psychological objects will flourish. As investigators and practitioners, members of the discipline have an interest in this mobilization of resources and historically they have taken determined steps to advance this interest (Ash, 1980; Geuter, 1987; Reed, 1987; Samelson, 1979). It should be noted that one is dealing here with a *social* interest, that is, a function of social position and not of individual psychology. So, for this interest to be an important factor in the behavior of an individual, it is also necessary that the disciplinary affiliation be quite salient for that person, relative to his or her other affl1iations.

One can think of examples of individuals in the history of American psychology for whom the affiliation with this particular discipline was not particularly salient. Dewey, Judd, and the older

James come to mind. But it is significant that such examples are most likely to be found early in the history of the discipline. On the whole, and increasingly so as the discipline developed, the disciplinary affiliation seems to have been extremely important for American psychologists, so that disciplinary interests dominated their professional lives. One factor which undoubtedly promoted this pattern was the internal organization of American universities in terms of discipline-based departments (Harwood, 1987). A complex of other factors was undoubtedly also involved, such as the relative weakness of more traditional alternative affiliations, and the close link between collective and individual social mobility through the securing of professional advantages (Sarfatti Larson, 1977). These factors also became more important elsewhere in the course of time, but they emerged particularly early and strongly in the history of American psychology. For this reason American psychology takes on the status of a paradigm case for the influence of disciplinary interests on disciplinary practices.

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

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

But what was most significant about this development was the way in which useful knowledge was defined. In the always dominant interpretation, usefulness meant useful to agencies of social control, of management, of institutional administration. Certainly, psychology promised great benefits to individuals, but in the dominant model these benefits accrued to individuals as the objects of agencies of social control, schools, clinics, personnel departments, and so forth. The possibility of a psychology that might be directly useful to individuals was looked at askance, tainted as it was with the label of 'popularization'. There is nothing surprising in this, for while a few individuals might profit privately from an alternative psychology, the advancement of the discipline as a whole depended on its alliance with existing centers of organized social power (Napoli, 1980).

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

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

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

A more sophisticated version of the criterion of scientificity took the form of a belief that, in order to qualify as a science, psychology had to devote itself to the search for universal, and, therefore, ahistorical, 'laws' of human behavior. However, psychological phenomena typically lacked the stability and consistency of the phenomena studied by physical science and, therefore, provided a poor basis for the display of such laws. The most commonly chosen way around this problem involved reducing variations in the conduct of different individuals to quantitative form by constructing appropriate investigative situations, and then treating these variations as 'individual differences' on some supposed underlying dimension or 'variable'. The point is that this procedure was based on the reification of a continuous dimension that remained identical for all individuals (Harré, 1979, p.l08; Lamiell, 1987; Valsiner, 1986). With this implicit assumption, generalization across individuals seemed unproblematical, and the formulation of universal 'laws' became possible. The alternative possibility, that the 'observed' individual differences (which were in fact the products of careful construction) might have been manifestations of an underlying discontinuity, was not a viable option for those who regarded these procedures as a necessary guarantee for psychology's scientific status.

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

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

6. Objects and Objectivity

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

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

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

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

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

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

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

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

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

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

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

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

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

NOTE

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7. The Historiography of Psychological Objects (2001)

Kurt Danziger

Abstract: An interest in the analysis of theories and concepts, implicitly accepted as discursive products, was already apparent at the early meetings of the European Society for the History of the Human Sciences. Becoming more explicit about this approach leads to an examination of the notion of discursive objects and the problematic notion of history without a subject. If a kind of discourse idealism is to be avoided an analytic distinction between discursive objects, human interests and social practices must be preserved. It is suggested that in the future more attention should be paid to diachronic studies of investigative practices and to the "epistemic objects" that result from these practices. The recent metaphorical use of "biography" in connection with diachronic studies of scientific objects has already proved fruitful. This is illustrated with some examples from the history of the concept of memory.

Two decades have elapsed since the first meeting of this Society. What kinds of topic were addressed at its early meetings? A glance at the *Proceedings* of the first three meetings (Bem et al, 1983-85) shows that questions of historiography were important for a significant number of contributors. This was only to be expected.

Even more frequently, however, the papers presented at our meetings focused on a particular theoretical system, or more often, on a specific concept to be found in the writings of an identified historical figure, or occasionally, a number of figures. Examples from the first Amsterdam meeting would be a talk by Sandie Lovie on "Images of Man in Early Factor Analysis", a contribution by Sybe Terwee on William James' conception of emotion, or Willem van Hoorn on "Freud's two Definitions of Instinct". By my count this kind of contribution accounted for nearly 30% of all contributions during the first three years. It was the clear favourite and perhaps has remained so. Let us call this content category "conceptual analysis".

This is of course a very broad category. At least two variants of this approach can be distinguished. In the one case the focus of the analysis is on a set of interrelated concepts, a theoretical system, in the other case the focus is on a particular concept, for instance emotion, instinct, violence, youth, social influence, or subjectivity. I would like to explore some ways in which this second kind of conceptual analysis might be developed in the future.

But before I do that I want to make an observation that is so obvious that it is usually treated as something that goes without saying. Yet it is precisely the things that go without saying which often hide truths that are crucial for understanding human practices. For the practice of conceptual analysis one crucial observation that must be made is that it invariably deals with texts, that is, a discursive reality. Most of the texts that formed the basis for conference contributions of the type I am talking about were published texts, that is to say, representations in the public domain. Whatever the private thoughts of the authors of these texts, the content we encounter and analyse is not the content of these private thoughts but the content of public documents. Some of us may want to speculate about authors' private thoughts on the basis of what these authors have put on paper, but that is always a further step whose riskiness stands in sharp contrast to the certainty we have about the textual basis of our evidence.

Unpublished manuscripts, and even private documents like notes and letters, are of course subject to the same observation. Whatever we find there is based on discursive forms that preceded any intervention by a particular author. The very notion of a novel contribution implies the existence of a prior discursive reality in terms of which such novelty is defined.

All this is hardly news to people who attend ESHHS meetings. But I think it is sometimes useful to restate the obvious in order to draw attention to the common ground on which most of us stand even while looking out in different directions. I also think it is helpful when explicit recognition of this common ground is reflected in one's terminology. Traditionally, the things to which conceptual analysis has been applied have been given various names. They have been called theories, concepts, categories, ideas, to mention only a few popular examples. The differences between these kinds of things are seldom addressed, and when they are addressed, the answers tend to be idiosyncratic. This does not mean that there are no distinctions to be made; there certainly are. But for a consideration of historiographic issues such distinctions are secondary to a primary recognition of something that all these things have in common. They are all discursive objects. They are objects that have an independent existence in a discursive domain shared by numerous subjects. As such – and this is a crucial point – they have a history of their own which is quite different from the history of any individual author who may have played a part in their history. A particular author's text on instinct, on emotion, on youth, on violence, and so on, may be part of that author's intellectual biography, but it is also part of the history of a certain discursive object which began before this author's intervention and continued after it. This second history is a history of objects, not a history of subjects.

As far as I can see, this kind of history was not yet on the agenda during our first meeting in 1982. Concepts were generally analysed as authorial achievements, not as stations in the history of discursive objects. But by the second meeting, a year later, the new history seems to have made its entry. Roger Smith's talk on `Inhibition' in the nineteenth century provided a splendid example of the new approach (see Smith, 1992) and some other contributions showed a distinct tendency in this direction. There were probably a number of reasons for this relatively late appearance of a history of discursive objects, but I suspect one of these reasons involves a reaction against the positivist historiography characterising much of the older disciplinary history of psychology. That history had been concerned with objects, though these were never understood as discursive objects but as natural objects. It was taken for granted that the constructed categories within which psychologists conducted their research, categories like motivation, intelligence, and personality, corresponded to objective divisions in the natural world. The historically contingent character of such divisions was not recognized. Therefore, the history of psychology could never include the history of its objects; these were timeless, though their appearance was covered by a veil. This meant that history became an account of the discoveries and errors made by individuals as they sought to unveil the true essence of the natural objects that were the focus of their investigations.

By the time of our first meeting in 1982 this model had already been largely discredited, at least among those who were interested in the prospects of a Society that was not hampered by any disciplinary ties, be they institutional or ideological. So among the contributions at our early meetings there were virtually none that followed the old model. Instead, we got several contributions whose analysis of specific concepts in the human sciences clearly indicated their constructed and historically contingent nature. No one could have been left in any doubt that at these meetings historical analysis would focus on discursive realities rather than on discoveries about the natural world.

To recapitulate, the two topics whose early prominence gave a certain character to our early meetings were historiography and conceptual analysis. I can think of no better way to mark the anniversary of the first of those meetings than by bringing these topics into relation to each other and considering some of the historiographic issues that conceptual analysis has had to face in the intervening years. To do that, I will have to widen the scope of my references beyond the early contributions to these meetings and pick out some of the more recent broader discussions and trends in the history of the human sciences, especially the history of psychology. I will also try to indicate a particular direction for this work which appears to me to be particularly promising.

I have already indicated that there are advantages in recognizing the targets of our ever popular conceptual analyses as discursive, rather than natural, objects. Talk about discursive objects is part of an anti-Cartesian trend whose influence in the history of the human sciences has been quite noticeable in recent years. This trend rejects the traditional dualism that insisted on maintaining a strict separation between the subjective and the objective. For this tradition the notion of a discursive object would be a contradiction in terms. Discourse was something that subjects engaged in and objects were things out there that were independent of the discourse of subjects. But nowadays we are more inclined to recognize that one cannot talk of objects without talking of their relation to subjects. Historically, the categories of the objective and the subjective arose together. One cannot have one without the other; they define the points of a polarity. How objects present themselves depends on the way people act in regard to them. In other words, there is an intimate relationship between human practices and the way the world of objects presents itself.

Where opinions differ is in the interpretation of this intimacy. At its most radical the insistence on the convergence of subject and object leads to their assumed identity in the overarching construct of “discourse”. There are dangers in this approach that have worried some historians of the human sciences. In particular, there is the danger of falling into the trap of so-called “discourse idealism”. If all is discourse we are not so far from a Hegelian view of history.

And here I have to introduce a name that, rightly or wrongly, has rather dominated discussion of these issues during the eighties and nineties. As some of you may have guessed, the name is that of Michel Foucault. Twenty years ago it might have been possible to have a discussion about the historiography of the human sciences without mentioning Foucault, but not to-day. Some new perspectives in the history of the human sciences show the invigorating effect of his influence, for example, the idea of a `history of the present’. In other respects his influence has been less felicitous. In Foucauldian history there are lots of wonderful things, like epistemes, historical a priori, regimes of truth, power/knowledge structures, problematizations, discursive strategies, and many more, but where are the people? True, this scheme of things makes room for bodies, but even bodies are not people. It is not that most of Foucault’s more specific concepts lack heuristic value for historical studies, it is that taken together they amount to something less than history. To resurrect history one needs to put people back into the picture and to recognize that people forge social ties and develop interests that are not reducible to purely discursive phenomena. Without taking these into account the most beautiful analytic machinery will not get us from point a to point b in the historical succession of things.

Undoubtedly, the development of a subjectless history provided a much needed corrective to a romanticizing tradition that thrived on stories about great men and disembodied ideas. But – and maybe this will sound a bit like a death bed conversion – I have to say that this correction can be taken too far. To point out that many of the phenomena we look at in the history of the human sciences are discursive in nature is one thing, to claim that all is discourse is another. Such a claim may be all we need if our historical account is to be purely descriptive, but once we so much as imply causal interconnections we have to go outside discourse if we are not to relapse into a new kind of idealism, discourse idealism. Whether this represents an advance over more traditional kinds of historical idealism is a moot point.

In particular, social practices cannot be absorbed into discourse. There is little justification for doing so, except for the argument that practices can only be known in terms of some discursive description, such as “measuring”, or “testing”, and this anchors them firmly in the realm of discourse. But this ignores the fact that social practices are also known by their extra-discursive effects, something that Foucault made more explicit later in terms of the effects of power on the body. Whether in this or some other form, an extra-discursive status for crucial aspects of social practices has to be recognized.

It seems then that we need to make a clear analytic distinction between the three kinds of things we need to recognize in order to construct an adequate history of the human sciences. We need to recognize people, which means recognizing that they have interests, projects, preferences, resistances, and so on; we need to recognize social practices, including antagonistic and institutionalized ones; and of course we need to recognize discursive objects which include classifications, concepts, generative metaphors, and much more. These three things are in constant two-way interaction with each other. For instance, people construct discursive objects, but discursive objects also shape people. A good example of the latter effect is the “looping effect of human kinds” that Ian Hacking (1995) has discussed extensively. When people find themselves classified in a particular way it can greatly affect their self-perception and their actions. The history of any domain is largely the story of the interactions among people, practices and objects, discursive and otherwise.

In the second half of my talk I want to develop some implications of this position for the history of the human sciences. In principle, particular studies in our field may well focus on either people, social practices, or discursive objects. But when one looks at any list of contributions to our meetings one finds a heavy preponderance of studies focusing on the first and last items and not many studies that focus on social practices. My own contributions have mostly been among this minority, so it will come as no surprise when I express the hope that there will be more studies with this focus in the future. One service which historians can perform for the social science disciplines is to help in the demystification of what is known as “methodology” in those disciplines, psychology in particular. The historical analysis of the social practices by which methodology is constituted can accomplish that, but more are needed.

Studies that focus on people, on the other hand, are not in need of any boost from me. They have always been popular and probably will continue to be. In the time that remains I would rather turn, or rather return, to the third topic focus I mentioned, i.e. discursive objects. First of all, a question of terminology. Although I freely used the term "discursive objects" earlier on in this talk, I now have to express some doubts about its suitability as an overall term for all kinds of objects that owe their existence to human constructions. As I have tried to indicate, discourse is not the only constructive activity that humans engage in; there are also social practices that cannot simply be assimilated to the category of "discourse". But if one used "discursive objects" as a generic term one would be implying just such an assimilation, i.e. that all is discourse. I therefore prefer to borrow a term from a historian of science, Hans-Jörg Rheinberger (1997), and speak of "epistemic objects". That doesn't commit one to a prejudgment regarding the relationship between discourse and practice.

Epistemic objects come in various kinds. One way of differentiating them is in terms of the kind of construction to which they owe their existence: some might be the product of purely discursive practices, others of material practices that impinge on bodies, human and non-human. Some are scientific objects that owe their existence to the investigative practices of scientific groups. Another way of differentiating objects is by their reference, by the kind of reality they imply. Then one can distinguish psychological objects, biological objects, physical objects, and so on. These distinctions could of course be discussed further, but what is more relevant for to-day's occasion is the question of studying the historical trajectory of these objects.

We are all quite familiar with the genre of biography, which traces the historical trajectory of human individuals. This is probably the commonest form of diachronic studies in our field. But there is the possibility of another kind of diachronic study, to which I believe we ought to pay more attention, and that concerns the historical trajectory of epistemic objects. Foucault's genealogical studies of the self, of sexuality, and so on, are still the best known, though controversial, examples of this genre, but more recently there have been other examples that owe little or nothing to Foucault and offer perhaps a better indication of the way things are moving in this field.

Last year, for example, there appeared a volume of studies edited by Lorraine Daston (2000) under the provocative title "Biographies of Scientific Objects". The historians of science contributing to that volume accepted that the concept of biography could be metaphorically extended from people to scientific objects. These follow a certain course from the time of their first emergence to the time they have ceased to have any significant historical presence. In some cases, of course, that time has not yet arrived, and one is describing the past of an object that is still very much with us. That applies, for example, to two of the psychological objects studied in this collection, dreams and the self. These are still with us, but that does not mean we are prevented from studying episodes from their past.

One reason why it seems odd to speak of objects, scientific or otherwise, having biographies is that our histories have been so preoccupied with the acts of individual persons that the material at which these acts were directed has been degraded to the status of mere manipulanda. Individual historical actors may well see them as such and it is quite proper for their biographers to follow them. But from the broader perspective of the historian it is clear that the objects at which individual persons direct their efforts are more than just manipulanda. They may be that, as far as the individual working on them is concerned, but they also exist independently of any individual's efforts. Moreover, they exist historically, that is, they change over time; the scientific object I encounter to-day is not the same object I would have encountered fifty years ago. The history of these changes is something quite different from the history of any one individual's contribution to these changes, no matter how significant they were.

What sorts of questions arise within the framework of a biography of epistemic objects? One set of questions that is typical of the historiography of epistemic objects addresses the *emergence* of such objects. We can trace the birth of the object from a time when it did not exist, or existed in a completely different form, or as something without any significance, to a time when it has become highly salient, broadly recognized and targeted in discourse and practice. The psychological object, "behaviour", provides a good example of such emergence. Right up to the late 19th century it did not exist at all; the word "behaviour" was part of a moral discourse that was the exact antithesis of the morally neutral discourse of which 20th century "behaviour" was such a crucial component. One can easily follow the course of this birth which took place over just a few years at the very end of the 19th and the beginning of the 20th century (Danziger, 1997). One can then inquire into the circumstances of this birth, what projects and interests propelled it onward, what practices endowed certain interpretations of phenomena with the status of objective truths.

A second and related set of questions pertains to the critical transformations that epistemic objects sometimes undergo in the course of their historical existence. To revert to the biographical analogy: Like William James, we can distinguish between the once born and the twice born. Some people go through a crisis at some point in their lives from which they emerge a changed person. Occasionally, this might even happen more than once in a lifetime. Similarly, there are psychological objects which seem to have been born more than once. They can even do something people can't do, they can have a rebirth after death. Dreams might be a good example. They were always known, of course, and endowed with all sorts of meaning, but their emergence as specifically psychological objects cannot be definitely established until the second half of the 18th century. Then they virtually died as psychological objects until they reemerged in the discourse and practice of psycho-analysis.

But this kind of rebirth is not so common. More common is the case where an object undergoes a critical transformation without an interim period of complete oblivion. The exceptionally long biography of the object "memory" could provide several examples, but I will just mention one, very briefly. For centuries, memory was defined as an object of the inner life, intimately tied to the conscious experience of recollection. (The fact that there was also a long tradition of speculating about the physical basis of memory did not affect this definition any more than speculation about the physical basis of perception affected the status of perception as a psychological object). Then, during roughly the last quarter of the 19th century, memory was reinvented as a biological object. Various developments converged to produce this result. First of all, the rapid recognition of phylogenetic evolution meant that there was now a third kind of history to add to the histories of human collectivities and human individuals, and this was a biological history. What is more, this third kind of history seemed to many to be the most important, the most fundamental of all. But where there was history there was surely also memory. At least that was the conclusion invited by a number of prominent Darwinians, such as Samuel Butler in England and Ewald Hering on the Continent. What they propagated was an enormous expansion in the meaning of "memory" so that it could cover everything from visual recall to the inheritance of acquired characteristics, instinct, habit, and even the effects of exercising a muscle. Gradually, memory became a different kind of object, a transformation that was simultaneously being fostered by early medical studies of memory defects associated with brain lesions. In the late 19th century memory as a biological object had its own name, it was often referred to as “organic memory”. Quite soon, however, the implied distinction between memory as a psychological and a biological object was dropped and the biological object captured the unqualified term “memory” for itself.

These were significant developments for creating the “conditions of possibility” (as Foucault would say) for the emergence and increasing acceptance of the Ebbinghaus approach to the study of memory. His famous technique of testing for the reproduction of memorized lists could only be regarded as a technique for investigating “memory” if memory was given a very particular meaning (Danziger, 2001). Ebbinghaus explicitly considered the traditional phenomena of memory, conscious remembering, unsuitable for scientific study. But if one redefined memory as simple retention one had an entity that *was* susceptible to objective testing without any reference to conscious experience. Ebbinghaus’ redefinition of memory was eminently compatible with the new conception of memory as a biological object. Without this background, it is doubtful that it would have been accepted as capturing the essence of memory. Indeed, someone like Wundt, for whom memory was not a biological object, never did accept it. However, Ebbinghaus, and G.E. Müller following in his footsteps, established a new set of investigative practices that were highly routinized and therefore eminently suitable for institutionalization. Thank to these practices memory as a scientific object could be produced over and over again in psychological laboratories. "The persistence of scientific objects depends on the institutionalization of practices" (Daston, 2000).

Approaching the history of psychology in terms of the biography of psychological objects has significant implications for the relationship between the discipline and its history. Traditionally, practitioners of the discipline have too often made use of a historical perspective to create two essentially false impressions, namely, that the field of psychology represents some kind of unity, and that, in spite of some ups and downs, history is a story of progress. The very title of many texts used for pedagogical purposes conveys the impression that there is indeed a relatively coherent and unified topic known as the history of psychology, and by implication, that the field whose history this is manifests a similar coherence and unity. But how does one decide what properly belongs in a history of psychology and what does not? Potentially, the history of psychology is as broad as a history of human subjectivity in general (Richards, 1987; Smith, 1988). It might include large parts of the history of art, literature and religion, as well as much else. If history of psychology texts tried to do justice to this potential richness they would either lack coherence or else convey a kind of coherence that is foreign to the kind of coherence projected by a science of psychology. So the content of texts is selected in accordance with implicit criteria that enhance the appearance of coherence and historical continuity. Assumptions that currently enjoy widespread acceptance in the discipline and issues that are currently salient shape these criteria, and this easily generates an overall sense of progressive development towards the present.

There are various ways of presenting the history of psychology which help to avoid these dubious effects. One way is to embed this history in the much broader history of the human sciences (Smith, 1997), but more often, constriction rather than expansion of subject matter has been the preferred route. This can be accomplished in different ways, for example, by restricting oneself to one limited period and maintaining a relatively narrow cultural focus (Reed, 1997). The use of the biographical method opens up other ways of avoiding the mirage of coherence and progress (Fancher, 1996). Yet another approach is the one suggested here. If one treats the history of psychology in terms of the history of psychological objects one need claim no more coherence for the field than is implied by an assembly of such objects (Danziger, 2002). Although, for the psychologist historian, the choice of objects is likely to be determined by their recent salience within the discipline, the emphasis on their fundamental historicity works against any unjustified narrative of progress.

Although the same objects, memory or motivation for example, are targets for the investigations of both scientists and historians there is a division of labour between them. The latter investigates scientific objects as historical objects whereas the former treats them as natural objects. But because of a culturally reinforced tradition of taking for granted the status of psychological objects as natural objects their history as discursive objects has been relatively neglected. It is too easily assumed that psychological objects have essential qualities forever fixed by nature. Moreover, it is unfortunately the case that there are strong professional interests bound up with the belief in the rock solid permanence of certain psychological objects. The political implications of different constructions of the object “memory”, for example, have been painfully evident during the last two decades (Pezdek & Banks, 1996).

That leaves historians with a twofold critical task. On the one hand, they need to investigate what lies behind the historical persistence of some psychological objects, the contribution of institutionalized structures or discursive practices for instance. On the other hand, they need to question the tendency to credit psychological objects with much greater historical persistence than they in fact possess and to make visible the extraordinary historical mutability of these objects. Inevitably, that will not make their work popular among those with vested interests in the status quo. But significant sections of the discipline will not be threatened by critical historical investigations and may even be encouraged by them (Danziger, 1994). Ultimately, historical studies are about historicity. The demand for a priori limits on historicity would subject historical investigation to a kind of censorship, producing a muzzled history that threatens no one. I believe that the historiography of psychology can make a more significant contribution to the discipline than that.

NOTE

Revised version of the opening address, European Society for the History of the Human Sciences meeting in Amsterdam, August 2001.

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(C) Consequences

8. Does the History of Psychology Have a Future? (1994)

Abstract: History of psychology tends to be accorded a purely pedagogical role within the discipline rather than being seen as a possible source of substantive contributions. This reflects a type of mobilization of tradition that is characteristic of the natural rather than the human sciences. The shallow history of the scientific review helps to organize consensus while critical history represents a threat to the moral community of researchers. However, there are developments which provide a more favourable context for critical historical scholarship. These developments include the emergence of a somewhat disenchanted view of science, feminist scholarship, and the international diversification of psychology. The potential effects of critical historical studies on conceptions of the subject matter of psychology, on the understanding of its practices, and on the nature of its social contribution are briefly discussed.

Departments of physics or chemistry are not in the habit of offering courses in the history of their subjects, yet the history of psychology continues to be taught in departments of psychology. This seems to point to the existence of at least a lingering belief that the history of psychology has a role within the discipline of psychology which the history of physics no longer has within the discipline of physics. But what is the nature of that role?

To answer that question, let us apply some further institutional tests. How many university departments of psychology would accept a doctoral thesis in the history of psychology as grounds for certifying a candidate as qualified in the discipline of psychology? Or let us ask how many historical studies are accepted for publication in the standard research journals of the discipline. Such questions only need to be formulated to illustrate the point that tolerance for historical studies diminishes sharply as we enter the serious business of the discipline, its scientific practice. The role that is conventionally conceded to the history of psychology appears to be largely limited to a pedagogical context, the introduction of undergraduates to the discipline's view of itself. From that point of view the teaching of the history of psychology may well be considered to be too sensitive to be left to the historians, but for most psychologists this does not imply that historical studies have any significant contribution to make to the science of psychology. In that respect their position does not differ essentially from that of most physicists. The other side of this coin is to be found in autonomous history of science departments with their traditionally heavy emphasis on the history of the physical sciences. The advantage of this institutional separation of the discipline and its history is to be found in the highly professionalized standards that prevail in historical studies of the physical sciences. The downside is that practicing physical scientists are probably the last people to take any notice of the work done by historians of their disciplines.

For an altogether different model of institutionalizing disciplinary history we have to turn to the social sciences. The history of economics probably represents the extreme case. Historians of science ignore this discipline altogether, but that does not mean that no work is done in it. On the contrary, there is a venerable tradition of economists themselves, sometimes very eminent economists, engaging in studies on the history of their discipline. This is not an insignificant engagement. In recent years publications in the history of economics are said to have averaged about two hundred papers and thirty books per year, and the North American History of Economics Society has almost six hundred members (Schabas, 1992). Courses in the history of economics are regularly offered by departments of economics. The situation in other social sciences is broadly similar, though in their case historical studies may not be as well established as in the case of economics. But the prevailing pattern is one where the history of the subject tends to be studied by persons whose professional affiliations are with that subject rather than with history.

Between the polar opposite models represented by physics and economics there are mixed models, to be found, for example, in biology, and of course also in our own field, the history of psychology. In the history of biology the very strong presence of professional historians of science has not eliminated historical work by a few biologists, including very prominent ones, like Ernst Mayr and Stephen Jay Gould. In psychology there is a certain tradition of intradisciplinary historical studies, but, increasingly, professional historians are also making contributions in this area. The time has come to ask whether the model represented by physics or that represented by economics is the appropriate one for psychology.

Two sensibilities

The question clearly points beyond the level of institutional arrangements. It would be unwise to pretend that there is no fundamental divergence of interests between the historian of science and the practicing scientist. On the contrary, we can only get a grip on this problem by confronting the reality of a basic division that cannot be wished away. Scientists and historians may both be struggling with the truth, but, to adapt a metaphor due to the historian Paul Forman, they each conduct their struggle in a different arena. History is not the arena in which natural scientists look for the truth; quite the contrary, they believe it cannot be found there but rather in the laboratory. From their point of view history will at best yield up stale truths that have been superseded.

Although this outlook is common among experimentalists, it usually remains implicit in their practice rather than being a topic that is felt to require much discussion. For the most explicit and articulate statements of this outlook one has to turn, not to scientists, but to certain philosophers of science. Since John Stuart Mills' *Logic* there has existed an ideology of science which absolutizes a particular version of scientific method and removes it from its human, and therefore historical, context. The principles of scientific method (as interpreted by a particular group of methodologists, of course) are regarded as being beyond history. Their application ensures the progressive emergence of the truth about nature. Once we adopt this ideology we must consider ourselves as being "in a historically privileged position that permits us to dispense with history, for we now have a correct logic of investigation" (Nickles, 1991, p.354). However, in the traditional sciences it was always recognized that scientific method was simply a necessary but hardly the sufficient condition for successful research. It is only in twentieth century pseudo-science we get a more extreme version of this ideology that elevates methodology to a sufficient condition of scientific progress.

For those who adopt this position history can have at best only an ornamental role. It can retrace the steps by which the pinnacle of the present was reached; it can describe the errors along the way. But in any case it will take the conventional wisdom of the present as its standard and judge the past by that. In other words, this will be Whig history, and whatever it discovers about the past will be implicitly a celebration of the present and of the steps by which it was achieved. This is feel-good history which will never have any impact on current scientific practices. Its place in the life of the discipline is not in the area of research or knowledge generation but in the area of public relations through undergraduate education or the area of professional socialization through graduate training. These are the services which disciplinary history renders to the discipline and which keep it alive in spite of its ultimate irrelevance to the central scientific tasks of the discipline.

The professional historian, whose institutional base lies outside the discipline, has the good fortune of not being bound by these disciplinary constraints. Such a historian is quite likely to turn the tables on the scientists by treating their current preoccupations as irrelevant. Professional historians of science will have their own criteria of historical significance, and they are likely to be very different from those of currently practicing scientists (Forman, 1991). Being free of the corsets of Whiggism they often produce intrinsically more valuable history, but they do so at a price. The price is isolation from the community of scientists. The audience reached by historians of science is likely to consist of other historians of science, not of working scientists. So the professionalization and increasing autonomy of the history of science actually strengthens the ideology of science according to which history and the methodology of science mutually exclude each other. The historian of science and the scientist each work in their own corner without the one ever interacting with the other.

According to some historians this state of affairs is hardly avoidable. Paul Forman, for example, has given a very sharp formulation to the division that separates the scientist and the historian of science. There are two "basic moral judgements" we can bring to bear on history, he says, and he calls them "celebration" and "criticism". In contradistinction to the celebratory historian, "the critical historian - understanding that scientific knowledge is socially constructed, partly within and partly outside the scientific discipline - must (instead) focus either on social problems of science or on science as a social problem." (Forman, 1991, p.83). This means a parting of the ways between the scientist and the critical historian of science; for, says Forman: "The one takes science as primary referent and source of value, the other gives priority to society." (ibid.) Taking science as one's primary referent means accepting the moral authority of the scientific community and writing history in celebration of that authority. Critical historians refuse to do this and thereby place themselves morally outside the pale as far as the disciplinary community is concerned. They cannot expect to be listened to, or to be taken seriously, by members of that community.

Although Forman's analysis appears to describe the problem of the disciplinary historian quite accurately, it is limited by its failure to contextualize the moral and ideological aspects of the clash between science and history. Forman has correctly identified the source of the moral authority of the scientific community in its claim to "transcendence", its claim to have the key to objective truth. But we also have to recognize that this claim is grounded in the special way in which scientific communities organize their internal life. They have perfected patterns of collective technical practice and internal communication that transform individual agency and authorial responsibility into the passive observation of "objective" event sequences (Pickering, 1992). A special way of handling history forms an integral part of these patterns.

The life of scientific communities is of course grounded in their history just like the life of other human communities. But scientific communities have developed a way of representing this grounding in a way that seems to deny it. Research publications that follow the pattern of natural science recognize the historical past out of which they grew in the form of references to the recent relevant research literature. The emphasis is on recency and relevance. Some kind of historical tradition is in fact recognized in every research paper, but, with few exceptions, the tradition is a shallow one, both in terms of time - what happened more than a decade ago is hardly worth mentioning, and in terms of domain - what is relevant is what falls within a narrowly defined research area. That way of handling history carries the twin implications that, firstly, anything worth saving from the past has already been incorporated in recent research practice, or in other words, that progress in science is inevitable, and secondly, that the definition of the relevant research area is dictated by objective factors and hence not a matter for debate.

As Gyorgy Markus (1987) has pointed out, this way of relating to its own historical tradition, so characteristic of the literature of natural science makes it possible for science to continue as a largely consensual enterprise. The replacement of genuine history by a brief account of the recent relevant research literature serves to demarcate, within predefined research areas, a sphere of knowledge from a sphere of uncertainty and ignorance. "In this manner the past is construed as objectively posing some questions, to which the paper then addresses itself" (Markus, p.38). "Natural science", says Markus, "can afford a lack of reflective historical consciousness, because each literary objectivation immediately participates in the articulation and interpretation of that (shallow) past which is relevant from the viewpoint of their present activities." (Markus, p.37).

The way in which a scholarly (or any other) community relates to its own history depends on the way in which tradition is mobilized to support an ongoing pattern of community life. One such pattern, most successfully developed in the natural sciences, involves the maximization of consensus around the formulation of what is already known and what is still uncertain. The shallow history of the research paper helps the achievement of this kind of consensus.

But when we turn from the natural to the human sciences we commonly find a very different kind of pattern. Here we are more likely to come across fields that are structured in an agonistic manner, fields which are characterized by deep divisions between alternative schools of thought rather than by the achievement of a general working consensus. Typically, such fields have a very different way of mobilizing tradition. They do so in a manner which supports their agonistic structure. They tend to cultivate a critical historiography of considerable chronological depth. In this way they give maximum visibility to fundamental differences among alternative schools of thought and highlight the availability of conceptual alternatives. For such fields deep historical studies can have considerable contemporary relevance and hence fall within the boundaries of the field itself. Weber and Durkheim are still studied by sociologists, just as Adam Smith and Ricardo are still studied by economists, whereas Galilean and Newtonian studies are not part of physics but of an altogether different discipline, the history of science.

The great majority of experimental psychologists relate to the tradition of their field in much the same way as physicists. Their look at the past might take the form of a review of the literature in a specific research area, and perhaps they would go so far as to take time off for celebrating a few icons on appropriate ceremonial occasions, but there is no room in their world for a reflective or critical history. They would gladly leave anything like that to the professional historians without any sense of having surrendered something that might have the slightest relevance to their own research interests. In the U.S. this attitude may be more widespread than elsewhere, and it is certainly accompanied by a growing tendency for the history of psychology to be taken up by historians rather than psychologists, but of course, the same attitudes are to be found wherever there are psychological laboratories.

The past and the future

Traditional work in the historiography of the discipline did little to counteract the disjunction of science and history. Initially, the engagement of modern psychologists with their own history took the form of producing textbooks for didactic purposes such as those of Klemm (1914)) in Germany and Gardner Murphy (1929), Pillsbury (1929), and, most successfully, Boring (1929) in North America. There followed four decades of sterility during which numerous derivative text books appeared, a little antiquarianism was indulged in, and great psychologists "from Aristotle to Freud" were celebrated. The crass excesses of this period were given their due in R.M. Young's (1966) definitive critique, "Scholarship and the History of the Behavioural Sciences". It was hardly accidental that this period was also one in which the natural science model for psychology was at its most pervasive. The quality of historical work was hardly improved by the tendency, among some American psychologists, to extend the ingrained ahistoricism of their discipline to the study of history itself, thus replacing the study of historical change by the study of "the persistent (read timeless) problems of psychology" (MacLeod, 1975; Watson, 1967).

However, by the mid-1970's signs of a change were beginning to appear (Woodward, 1980). European psychology was recovering from its mid-century depression, "behavioral science" was no longer the only game in town, and a few critical and reflective historical studies saw the light of day. Since then, the growth of critical scholarship has become more vigorous. Textbook and ceremonial history have not disappeared, but the field is now a contested one (Hilgard, Leary and McGuire, 1991). Many psychologists still find it difficult to conceive of any way of relating to their discipline's past in any way other than that which is characteristic of the physical sciences. But there is also a growing body of historical studies within psychology that follow a pattern more usually associated with the human sciences. Conflict about the way in which the discipline is to relate to its past is very much connected with perennial ambiguities surrounding the status of Psychology as a natural or a human science (Morawski, 1987).

Fluctuations in the interpretation of those ambiguities must be seen against a background of broad trends that extend far beyond the boundaries of the discipline. One such trend involves a process that we might call the disenchantment of science. Max Weber referred to the disenchantment of the world, a historical process in which science played a major role. In this process the world ceased to be an arena for miracles and spirits and for divinely inspired moral dramas and became an arena for human calculation and rational prediction. But while science was a major agent of this process it was itself largely exempt from it. At a time when all other human activities began to be looked at critically and sceptically, when all gods were found to have clay feet, the production of scientific knowledge somehow remained morally pure and its results untainted by their mundane origins. It was not only the scientists for whom the moral authority of the scientific community was unassailable, it was a whole civilization.

There is little doubt that cracks have begun to appear in this picture. Among the general population attitudes towards scientific progress have become more ambivalent, partly because of certain undeniably negative by-products of scientific advance, like the possibility of nuclear war and massive environmental pollution. Although such problems may not be directly related to the work of science, they still serve to undermine the old belief that only good things are to be expected from the onward march of science.

Accompanying these more general shifts of attitude, there have been corresponding changes on the intellectual level. The emergence of a critical history of science was itself part of this change. The historian Charles Rosenberg has noted "the development of a critical, and even antagonistic attitude toward the past and present role of science in the United States", a development strongly implicated in "the growth of a more critical, and self-consciously political, spirit" among American historians of science (Rosenberg, 1983, p.356/7). In Europe, particularly in Britain, there has been a vigorous growth of sociological studies of science which have radically undermined the moral authority of science and propagated the once shocking idea that the practice of science is a mundane human activity governed by essentially the same principles as other forms of human work. Doing science is seen as being as much a matter of social organization, competition for scarce resources, social interests, rhetorical persuasion and consensus building as many morally less respectable activities.

Another area profoundly affected by this sea change was the philosophy of science. During the heyday of faith in the moral authority of science the philosophy of science was largely dominated by different varieties of positivism, the final variety being logical positivism which grounded the purity of science in the logical purity of its language and the sensory purity of its observations. Virtually everything that was human about science was relegated to a so-called "context of discovery", leaving the so-called "context of justification" as a suprahuman residue of idealized science. Some three decades ago, this conceptual structure, which was already beginning to totter because of its internal problems, was struck a near fatal blow by the publication of Thomas Kuhn's book on "scientific revolutions" (1962). In the wake of Kuhn's analysis, and the flood of literature to which it gave rise, it became increasingly difficult to maintain the strict separation between the timeless rationality of science and the historically changing scientific communities that embodied and practiced this rationality. Accordingly, the new philosophy of science began to look to the history of science for tests and illustrations of its generalizations.

All these developments tended to open up possibilities for the history of science that had previously been marginalized. During the heyday of scientism, when the supramundane authority of science was beyond question, there was little for the history of science to do, except engage in antiquarianism or celebration. But with the new scepticism and its recognition of science as one social enterprise among others a space had opened up that could be filled by a critical history of science. Scientific objects came to be seen as objects with an essentially historical existence:

> Most of the objects that science has dealt with in the course of its history, objects which appear ostensibly to be the same, really bear only a family resemblance to one another. Whether it be space, time, the starry heavens, the forces which move bodies, or some other object of science, we would look in vain for some shared or common meaning which might apply to any of these objects throughout their respective histories and which as such . . . might serve as the common and continual ground for all the scientific theories devoted to any such object. It was hard enough for mankind to grasp that the same time does not tick off in all parts of the world. It may be even more difficult to grasp that when we investigate some scientific object, both today and as it existed in the past, we are not necessarily speaking about one and the same thing (Hübner, 1983, p.123).

Hübner was speaking of the objects of physics. But if the objects of physics must be regarded as embedded in human history, how much more obvious is this in the case of the objects of psychology. The memory that a contemporary student of the area investigates is not the same object as that which Ebbinghaus tried to study by means of nonsense syllables, and neither of them has more than a tenuous connection with memory as understood by Aristotle (Danziger, 1990b). The individual differences that Eysenck, for example, regards as objective features of the world have virtually nothing in common with the individual differences pondered by someone like Carl Jung. The "behavior" studied by the "behavioral science" of the recent past is a very different object from that which inspired John B. Watson or Lloyd Morgan.

In the case of psychology, of course, it is not only the concepts and methods of the discipline that undergo constant historical change, but the very subject matter itself. Human subjectivity, the reality behind the objects of psychological investigation, is itself strongly implicated in the historical process, both as agent and as product. Moreover, the history of psychology and the history of human subjectivity are not independent of one another. Changes in the one have effects on the other. So the grounds for claiming a certain priority for history are much stronger in the case of psychology than in the case of the natural sciences. That means that historical studies are potentially of much greater significance within psychology than they are within physics.

The challenge lies in converting this potentiality into reality. But that depends on a change in the traditional metaphysical commitments shared among psychologists and their historians. Those commitments, as I have indicated, revolved around a naive naturalism that assumed an essential correspondence between the latest set of psychological categories and an unchanging human nature. Because of the foundational role which positivism and scientism played in the constitution of modern American psychology, a historicist conception of science will not be easily assimilated. Nevertheless, questions can now be asked that would previously have been out of bounds. This loosening has made it possible for a critical historical trend to develop within a generally unpromising disciplinary framework.

Decline of insider history

Until relatively recently the historiography of psychology was essentially a history of "insiders", that is to say, individuals identified with the group whose history was in question. In other words, histories of psychology were written by psychologists. But the notion of "insider history" involves more than that, for "insiders" and "outsiders" can be distinguished on a number of relevant dimensions. Disciplinary affiliation represents one such dimension, but members of the discipline do not form a homogeneous community. For example, there is a traditional hierarchy within the discipline that places so-called hardcore experimentalists at the top and applied psychologists somewhere near the bottom (Sherif, 1979). From this perspective, a history like Boring's, for example, was insider history in the sense that it was written from the point of view of an elite within the discipline, an elite of experimentalists for whom child or social psychologists constituted lower forms of psychological life that were tolerated only at the margins of the discipline and of its history.

For a long time, those who were marginalized tended to accept the criteria that legitimized their inferior status. In fact, they tried to emulate their betters by striving to become more like them, more "rigorous", more experimental, and so on. Therefore, the traditional historiography of the discipline was not seriously challenged from this quarter. However, in recent years there have been numerous indications that the old disciplinary hierarchy is beginning to crumble. The increasing autonomy and confidence of previously marginalized sections of the discipline, the organizational splitting off on the part of disaffected experimentalists, the proliferation of radical alternatives to traditional scientism, all these are sure signs of the ongoing corrosion of old certainties and old hierarchies. Among these trends some provide a more favourable existential basis for the further development of a critical historiography than others. Two developments are particularly significant in the present context.

The first of these developments concerns what one might call the human geography of the discipline. The period when scientism and positivism reigned supreme in regulating the life of the discipline was also the period when psychology had become to all intents and purposes an American science. For at least a generation after the Nazi takeover in Germany psychology outside the United States was of little account and increasingly took its lead from North America. The historical work that bears the stamp of this period quite naturally equated the celebration of a certain conception of science with the celebration of psychology as an American science.

More recently, however, American hegemony in psychology, as in many other areas of life, has come to an end. The discipline has been expanding rapidly in a number of European countries and elsewhere, and on an international scale the proportion of psychological research emanating from the United States has been shrinking steadily for quite a number of years (Rosenzweig, 1984; Sexton and Hogan, 1992). This development is now leading to a renewed interest in their own psychological tradition among an increasing number of psychologists outside the United States. In most cases, of course, that tradition is very different from the course that psychology took in the United States. Major themes in the American context, like behaviourism, are relegated to minor footnotes, and other themes, unknown to most American psychologists, become highly significant. Important developments for American psychology, like the cognitive revolution, turn out to be non-events from a European perspective, because of the existence of a local cognitivist tradition that never managed to cross the Atlantic. Many other examples of such differences could be cited. Some of them raise rather profound issues. For instance, the history of the relationship between psychology and society, both on the institutional and on the cultural level, shows a variety of patterns in different European countries, and none of them conform to American patterns (e.g. Dehue, 1991; Geuter, 1992; Joravsky, 1989; van Strien, 1991).

But it is not only in the first world that groups of psychologists with a different historical agenda have been finding their voice. More slowly perhaps, but in the long run inevitably, psychologists in East and South Asia, in Africa and Latin America, are raising questions about their own traditions and their relationship to the theory and practice of psychology (Moghaddam, 1987). The more they do this the more dissatisfied they become with the parochialism of a historiography of psychology anchored in North American and European perspectives (Ardila, 1982). This leads to questions that are alien to traditional histories of the discipline, including questions about psychology and cultural imperialism, for example, or about the link between psychology and the historical project of modernism.(Bulhan, 1985; Moghaddam, 1990; Sampson, 1991; Sloan, 1990). These developments have also led to the emergence of new concepts that are of great interest to the disciplinary historian. The concept of "indigenization", for example, refers to the process by which imported psychological notions and practices become assimilated and changed by the local social context (Adair, 1992; Church, 1987; Lagmay, 1984; Sinha, 1986). But this is not a process limited to countries currently classified as "developing". To a significant extent the first half century of the history of modern American psychology involved the Americanization, i.e. indigenization, of psychological concepts and practices originating in the very different social and intellectual climate of Europe. The fate of the key contributions of Wundt and the Gestaltists as well as those of Kurt Lewin and Fritz Heider illustrates this very clearly (Antaki and Leudar, 1992; Ash, 1985, 1992; Blumenthal, 1977; Brock, 1993; Danziger, 1992; Henle, 1980; Rieber, 1980).

In a sense, modern psychology is returning to the position from which it began: a polycentric position in which there are diverse but intercommunicating centres of psychological work that reflect a diversity of local conditions and traditions (Danziger, 1991). As these centres are emerging against a recent historical background of domination by one centre, they first of all feel the need to define their own historical identity. But this quickly leads to more general questions that are also relevant to the history of the discipline in its more established centres. In particular, the broadening of historical perspective that is the result of the more recent globalization of psychology leads to questions about the conditions that affect the transcultural migration of psychological categories. Studies in this area also have great relevance for the question of the relationship between the categories of scientific psychology and culturally embedded beliefs as well as local forms of institutionalized practice.

Insofar as psychology resembles the natural sciences in being independent of local culture its history will be perceived as being irrelevant to its current practice and therefore appropriately relegated to professional historians. But time and again this independence has turned out to be far more fragile than in the case of the natural sciences, a circumstance that has enhanced the link between historical reflection and current practice and created a role for the disciplinary historian that is critical in more senses than one.

As long as the moral authority of the scientific community remains unchallenged from within, history will be seen either as irrelevant, or as an occasion for celebration. It is when that authority becomes questionable, when the professional community is divided in some profound way that a critical disciplinary history has a significant contribution to make. I have pointed to the transformation of psychology from an essentially national science to an international and intercultural enterprise as having a particularly important corrosive effect on the monolithic nature of intra-disciplinary authority. But of course there are other developments which are having similar effects. Among these there is one that exceeds the others in its potential importance, and that is the emergence of feminist critique of science.

The notions of scientific authority which legitimate the moral claims of the disciplinary community are not only grounded in a specific cultural tradition, they also depend on patriarchal power relationships. With the rise of contemporary feminism these relationships have come under criticism, and in due course this criticism was extended to the kind of science culture that they have supported in the past (Harding, 1986; Nelson, 1990). Like other groups who have found their own voice after being excluded from the commanding heights of disciplinary authority, women have initiated critical historical studies that make an important contribution to the self-understanding of the discipline (e.g. Furomoto, 1989; Morawski,1988, 1990, 1992). Their ability to do this depends in no small measure on their success in transcending the limitations of an earlier "feminist empiricism" that remained unquestioningly committed to traditional assumptions about the nature of science and its practices. With the emergence of a more critical feminist historiography of psychology we may look to analogous developments in the historiography of biology (e.g. Bleier, 1984; Fox Keller, 1985; Haraway, 1989; Jordanova, 1980) and related areas (e.g. Daston, 1992) as providing some indication of what may be expected from such contributions in the future.

The emergence of a critical historiography within the discipline of psychology suggests a modification of the sharp contrast between the perspective of scientific insiders and historian outsiders that was discussed earlier. Where the moral cohesion of the scientific community remains tight and effective scientists and historians may well represent two professional solitudes unable to communicate. But for the reasons I have indicated psychology is unable to maintain that kind of cohesion. This has meant the appearance of voices that are the voices of outsiders from the point of view of the scientific insider but that lay claim to the position of insider by virtue of their disciplinary affiliation with psychology. The increasingly polycentric structure of the field, the growing awareness of agonistic relationships within it, and the resulting loss of moral cohesion, create a more complex situation than the one allowed for by the stark opposition between scientific and historical sensibilities. It is a situation that provides a context for the development of what has been described as "the creative tension between distance and commitment" (van Strien 1993). Where the insider's engagement with the discipline's concepts and practices is combined with the moral distance maintained by the outsider one has reason to look for the emergence of a historiography that is both critical and effective.

Impact on Psychology

There are at least three ways in which a critical historiography might have an effect on psychology. It could affect conceptions of the subject matter of psychology, the understanding of its practices, and the nature of its social contribution.

Traditionally, the discipline of Psychology, as we know it, has defined its subject matter in completely ahistorical terms. Human nature was part of unchanging nature, not part of history, and was therefore to be studied in essentially the same way as the rest of nature, by methods analogous to those employed in the natural sciences. But as cracks begin to appear in this image of psychological investigation, so the question of exploring the historicity of human functions finds a place on the disciplinary agenda (Gergen and Gergen, 1984; Staeuble 1993). Though surrounded by strong taboos for most psychologists the study of the historicity of human subjectivity has a considerable body of scholarship to draw on (Staeuble, 1991).

But for psychology there is a particularly intimate connection between the historicity of the subject matter and the history of conceptions about that subject matter. Human beings, as has often been noted, are self-defining. What we are is expressed in the categories of psychological discourse, so that as we change the categories we use to describe ourselves to ourselves also change. This means that two fields of study, the history of psychological functions and the history of conceptions about those functions, have considerable relevance for each other. That provides the history of psychology with a potentially significant role in the development of new fields of study, like a historical social psychology or a historicized abnormal psychology, for example. Thus, in Germany, the same journal, *Psychologie und Geschichte*, publishes studies in the history of psychology and studies in historical psychology.

In view of the close relationship between subject matter and disciplinary practices it is difficult to historicize the one without historicizing the other. Traditionally, Psychology has constructed its ahistorical subject matter by means of ahistorical investigative and conceptual practices. Its investigative practices were understood, not as social practices, but as applications of timeless logical and mathematical principles. Its conceptual practices relied heavily on the reification of recently constructed psychological categories that were assumed to reflect the categories of an unchanging human nature. However, it becomes increasingly difficult to resist calls for a revision of these practices in the face of critical historical scholarship. The demonstration that Psychology's investigative practices are historically contingent products reflecting a limited set of knowledge interests (Danziger, 1990) may contribute to the break-up of the discipline's methodological gridlock. Historical studies can also provide access to alternative ways of conceptualizing the procedures and the subject matter of psychology. If nothing else, historical inquiry can serve to "challenge the taken for granted and objectified realities of the present" (Gergen, 1991, p.27).

Nowhere is this more apparent than on the level of conceptual practices. Most of the general categories used to identify the subject matter of modern Psychology, categories like personality, motivation, depression, behaviour, emotion, and many, many others, are in fact of recent origin, often being younger than the discipline itself. It cannot be irrelevant to current theoretical discussion to gain some understanding of the circumstances under which the subject matter under discussion came to have the meaning currently assigned to it and what alternatives this current meaning replaced (Danziger, 1993). Different historical periods have been marked by what Gergen (1991) calls different "psychological intelligibilities". We can hardly hope to understand the character of our own intelligibilities without the relevant historical knowledge.

As theoretical discussion gathers historical depth we might also expect a change in the social contribution of Psychology. It has sometimes been observed that the contribution which the discipline of Psychology has made to the major currents of intellectual discourse in the twentieth century has been rather disappointing. Near the beginning of the century there were high hopes that this new discipline would have a decisive impact on intellectual life and there was talk of the "psychological century". But as time went on psychologists, came to see themselves more and more as technicians offering solutions to specific problems but leaving the big questions to others. So even when there were obvious psychological aspects to major debates about such matters as the nature of power in human affairs, the decline of modernism, or the scope of scientific rationality, the contributions of psychologists tended to be conspicuous by their absence. One suspects that a measure of historical sophistication about their field would work wonders for the ability of psychologists to enrich the cultural life of their own as well as other societies. And because they would be less dependent on current fads the quality of their more technical contributions might be expected to improve as well.

NOTE

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9. Towards a Polycentric History of Psychology (1996)

Abstract: The most influential histories of psychology tend to adopt the perspective of a particular centre of psychological development, most often the USA, with developments elsewhere forming a kind of periphery. Conceptualizations and practices favoured by social conditions at the centre are treated as universally valid core principles of the discipline while knowledge emerging at the periphery is often awarded only local significance. More recently, this model has become difficult to maintain and the history of the field is more readily seen in terms of an interaction among several focal centres. Such a perspective leads to an analysis of the way in which the generation, transmission and application of psychological knowledge has been shaped by power relationships as well as by cultural biases and barriers. A polycentric history has considerable relevance for current developments within the discipline.

Is there such a thing as <u>the</u> history of psychology? Is there a definitive linear narrative to be told about the origins of psychological speculation in Ancient Greece, its long imprisonment in the philosophical discourse of mediaeval and post-mediaeval Europe, its liberation by nineteenth century laboratory methods, and its flowering in twentieth century America? Many American textbooks have followed such a historical path. Yet, there are several reasons why this path is misleading. For one thing, psychology, in the modern disciplinary sense, has a relatively brief historical presence - about one century. Even the use of the term "psychology" to distinguish a particular subject matter is not very much older than that - in English, only half a century older. It is therefore up to the historians to choose what is to be included and what excluded from the history of a subject that was not recognized as such during most of its history. Their choices will of course be affected by their present day agenda, by their intellectual horizon, by their prejudices. The linear narrative of psychology's history is their creation - it is not a mirror held up to an unambiguous course of events. For this reason a prominent British historian of the subject has suggested that "<u>the</u> history of psychology should be abandoned" (Smith, 1988).

There is another reason why there can be no such thing as <u>the</u> history of psychology, even if we restrict ourselves to modern psychology. This discipline did not develop from a single seed sprouting in one specific location, certainly not from Wundt's Leipzig laboratory. 20th century American psychology represents a virtually complete break with everything that Wundt stood for, starting with his insistence that psychology remain affiliated with philosophy. One has to go Galton's anthropometric laboratory in London, to Charcot's clinic in Paris, to the Bureau of Salesmanship Research at the Carnegie Institute of Technology in Pittsburgh, and to many other places if one really wants to trace the roots of modern psychology. Different versions of modern psychology appeared at more or less the same time in a number of countries. Nor did these versions undergo a progressive fusion. On the contrary, during the three decades between 1915 and 1945 the gap between different national psychologies did not narrow, it widened. Therefore, it is only by privileging certain local developments over others that it is possible to construct anything like <u>the</u> history of psychology, even for the most recent century.

But which local developments will be singled out in this way? What criteria will determine this choice? In the past, two criteria have played a prominent role. First, there was the historian's own affiliation with a particular part of the discipline, an affiliation which could easily lead him to assign a central, unifying, role to that part, even substituting the history of that part for the history of the field as a whole. A well known example is provided by E.G. Boring's *A History of Experimental Psychology* (1950), where the traditional experimental parts of the discipline are at the centre of attention and everything else becomes a matter of merely peripheral interest. It has been suggested that this bias was connected to the author's involvement in intra-disciplinary politics where he represented the interests of the experimentalists (O'Donnell, 1979).

But emphasis on a part of the discipline at the expense of the rest has not supplied the only, or even the most important, criterion for privileging certain historical developments. A second criterion has its source in the national diversity of psychology, which provides scope for historical accounts organized around one particular national tradition. As long as this bias is made explicit, there is nothing objectionable about it.

However, a serious complication is introduced by the extremely unequal national development of modern psychology. In most countries the discipline had a difficult time in getting established. But there was one massive exception, the USA. Throughout the twentieth century, psychology has flourished there as it has nowhere else. Coupled with the emergence of American economic and military predominance, as well as the ravages of wars and political disasters in Europe and elsewhere, this resulted in a dominant position for American psychology on a world-wide scale. The fact that the discipline seemed to have a recognizable geographical centre imposed a particular structure on its historiography (Danziger, 1991). American textbooks on the history of psychology could ignore virtually everything outside the USA and still claim, with some degree of plausibility, that they were presenting, not a history of American psychology, but a history of modern psychology as such. Other nationally based histories would have to accept the status of merely *local* histories. Yet, a history of psychology that takes its perspective from American developments is as much a local history as a history that takes its perspective from developments in, for example, Japan or India.

The time when this could go unrecognized is now past. That is due to the gradual decline of American predominance and the relatively greater prominence of several other centres of significant disciplinary activity. As a result, the image of a discipline that has one geographically defined centre is fading fast. The model of centre and periphery is being replaced by a polycentric one (see e.g. Moghaddam, 1987).

In a sense, modern psychology is returning to the position from which it began: a polycentric position in which there are diverse but intercommunicating centres of psychological work that reflect a diversity of local conditions and traditions (Danziger, 1991). As these centres emerge against a recent historical background of domination by one centre, they first of all feel the need to define their own historical identity. This often takes the form of "contributionist" history in which local figures are given their due. In due course, however, there occurs something analogous to what Woodward (1994: 203), speaking of feminist historiography, has referred to as "a Gestalt switch from seeking predecessors and role models to constructing science differently". What does this mean?

First of all, it has to be recognized that the metaphor of centre and periphery applied not only on the level of geography, but also on the level of conceptual content and methodology. It implied a particular model of the internal structure of the discipline. As long as this model prevailed developments at the periphery could be seen as subject to local social influences, while the centre represented universal values or even rationality as such.

Certain areas of the discipline, usually involving particular methodological commitments, were designated as "basic" or "core" areas and others as areas of "application". In the core areas experimental research was to discover universal principles of psychological functioning, while in the peripheral areas less rigorous procedures might suffice to study local manifestations of these principles. The basic principles were always conceived of as asocial and ahistorical, and their investigation was typically pursued in a decontextualized manner. Examples of such principles are the so-called laws of learning or the principles of cognition. There is supposed to be nothing intrinsically social about these laws and principles; they are thought to apply to individual organisms and individual minds, irrespective of the social content of either learning or cognition. It is assumed at the outset that the laws of learning and the principles of cognition are the same everywhere and at all times. They have the same kind of universality as the laws and principles of chemistry. However, just as in chemistry, local conditions can affect the results of their operation. In psychology, these local conditions are often social in nature. So we get a dualistic model: On the one hand, basic processes that are regarded as inherent features of individual organisms and individual minds, and on the other hand, local social conditions that affect the specific manifestations of these processes. The core of psychological science is constituted by the investigation of universally valid basic processes; the study of human psychology in social and historical context, however, is regarded as peripheral to this core endeavour, less important because its results are not universally generalizable.

There was always a very marked parallelism between core and periphery on the level of geography and on the level of disciplinary content. Those at the geographical periphery usually did not have the resources to mount major investigations of basic processes. That kind of thing generally remained the prerogative of those at the geographical centre. Those at the geographical periphery typically had to content themselves with being at the scientific periphery as well. If they claimed universal validity for their findings, they could expect these claims to be ignored. But more often they did not make such claims; accepting the leadership of a far away centre, they accorded their own work a purely peripheral significance in terms of the discipline as a whole. They would take over the conceptual categories and the methodological imperatives of the centre and try their best to apply them under local conditions that differed profoundly from those that prevailed at the centre. They were subject to the limitations imposed by what has sometimes been called a "borrowed consciousness" (Easton, 1991).

In more recent years this situation has changed to the point where the structure of the discipline, on the conceptual as much as on the geographical level, no longer conforms to the model of one preeminent centre and its periphery. I have already discussed geographical decentralization. Conceptually, the discipline has fractionated into numerous sub-fields that, for the most part, have very few ideas in common. The age of "grand theory", when a single set of principles was to unify the discipline, is long past. Even on the level of methodology, which always united the discipline much more effectively than any theory, there are strong signs of an advancing eclecticism and an increasing receptivity to procedures that were once considered beyond the pale.

I believe that changes in the historiography of psychology must be seen in the light of these developments. It is said that every age has to write its own kind of history. If that is the case, the polycentric historiography that is now emerging certainly seems appropriate for our era of increasing decentralization on both the geographical and the conceptual level. This shift to a polycentric understanding of the history of the discipline has favoured a shift towards a more contextualist historiography. As long as there was an equation of one locally generated truth with the truth as such, the question of the social roots of that truth was not likely to be asked. But with the end of privilege, both on the geographical and the conceptual level, the intelligibility of alternative accounts rests on seeing them in terms of their social context.

For a polycentric historiography the question of how to characterize social context therefore becomes crucial. Here there are two temptations, that are actually two sides of the same coin, which I think must be resisted. One temptation is to adopt the popular discourse of modernization and to write the history of world psychology in terms of the onward march of scientific, that is to say modernistic, psychology. What such an account overlooks is that modernism does not come in only one model, that it is always someone's version of modernism, whether American, Japanese, German, Russian, or whatever. In each case, local cultural features have been incorporated in a particular version of modernism. There is no such thing as modernism-in-the abstract that floats above all local cultures.

The other side of this coin is formed by a romanticizing of local traditions that ignores the very real interlinking of local influences that has always been such a significant feature of the history of modern psychology. There is a vast difference between a polycentric historiography of the discipline and the mere addition, in disconnected chapters, of one local history after another. What is needed now is not a string of parochial visions but a focus on the changing interrelationships among centres that have constituted the world history of the subject in the modern period.

I must stress again that "interrelationship among centres" is to be understood in both the geographical sense and in the sense of particular contents. When students from many countries flocked to Leipzig and to other German centres in the late nineteenth and early twentieth century and then returned home with new ideas they established a pattern that was to be repeated throughout the modern history of the discipline, though the direction of travel changed. Of course, the pursuit of formal studies abroad was only one avenue through which international links were established. Books were translated and marketed, money was invested in scholarship funds, instruments were exported and imported, innumerable conferences were held, and so on. In the long run, no local tradition could be unaffected by this, but neither was the result a complete homogenization of psychological discourse (Sloan, 1990).

A polycentric historiography must attempt to do justice to the complexity of such phenomena. To do this it must work with categories that seek to capture the interrelations among centres, rather than the characteristics of centres considered in isolation. Intellectual migration is perhaps the most obvious of these categories, not only in reference to persons, but, more significantly, in reference to concepts and practices. What happened to psychological concepts, theories, procedures when attempts were made to transplant them? Why did some of these prove to be much better travellers than others? How did travelling change them, sometimes beyond recognition? Who found them useful and why? There are stories of successful transfer to be told here, but also stories of misunderstanding, mistranslation, total incomprehension and downright hostility that are often more illuminating.

The link between knowledge and power cannot be ignored when considering such issues. Questions arise about the circumstances under which the spread of modernistic psychological knowledge and practices can be seen as a manifestation of cultural imperialism. It has been observed that the proliferation of the discipline of psychology all over the world owes a great deal to "North American advertising of its value in society" (Valsiner, 1996, p.129). At the same time, the conceptualization of the contrast between "us", meaning the scientifically enlightened modernizers, and "them", meaning the backward traditionalists, was historically linked with the role played by western social science, including psychology, in imperialist projects (Staeuble, 1992). The way in which such contrasts converged with certain methodological patterns in the social sciences is now beginning to become apparent.

Other questions relate to the extent to which resistance to imported ideas, a kind of intellectual protectionism, has shaped the history of modern psychology. Such resistance certainly played an important role in the history of modern American psychology. Its first half century was, after all, a period of "indigenization" that resulted in the Americanization of concepts and practices originating in the very different intellectual and social climate of Europe.

From this perspective the history of modern psychology cannot be regarded as a unilinear movement from the pre-modern to the modern (Mitchell & Abu-Lughod, 1993), where the modern is identified as being scientific, the pre-modern as pre-scientific. Conceptions of what it means to be scientific in psychology have varied at different times and in different places (see e.g. Danziger, 1990; Dehue, 1995), and each conception of the scientific has entailed a corresponding conception of the non-scientific. It is of course possible to write a historical account in terms of linear developments leading up to any favoured variant of scientificity. But all such accounts suffer from a fatal arbitrariness. A polycentric historiography would replace them with studies of developing co-constructions of different versions of modernity and pre-modernity.

Such studies would have to break with another powerful convention of the traditional historiography of psychology, its marked disciplinary focus. The history of modern psychology is commonly identified with the history of the discipline of psychology, where the boundaries of the discipline are defined by academic and professional organizational structures, not by the subject matter. Whether some topic is regarded as forming part of the history of modern psychology depends on its reception by academic departments and professional associations. But this too is subject to local and temporal variation. Common examples of topics with a variable status are psychoanalysis, graphology, parapsychology, and much of social psychology. However, instead of being taken for granted, organizationally and administratively enforced boundaries become a major focus of inquiry for a polycentric historiography. The locally variable reasons for the erection of such boundaries and their historical effects constitute important features of variant developments in different parts of the world. Clearly, when the historical construction of disciplinary boundaries becomes an object of inquiry, the perspective of a purely intra-disciplinary history has to be abandoned. Historical studies will then be able to contribute to an outcome that is long overdue, namely, the "de-parochialization of the disciplines" (Prewitt, 1996).

That raises the question of the relationship of the discipline and its history. It is obvious that the new historiography will change this relationship. The older linear historiography took the natural sciences as the model for the relationship between history and disciplinary content. In the natural sciences theoretical achievements and investigative practices are generally regarded as being independent of local culture. Therefore the history of a science, especially its social history, is seen as irrelevant to current issues in that science. A physicist does not need to be enlightened about the history of physics to be a good physicist. In the social sciences, however, the claim for the independence of scientific categories from specific cultural traditions is rather less plausible. Subject and object of study are generally linked by common cultural understandings which are the products of a certain historical experience. The relationship between current social science and historical studies is therefore potentially more intimate than in the case of the natural sciences (Danziger, 1994).

A polycentric historiography of psychology would have to explore the historical dependence of the categories and procedures of scientific psychology on culturally embedded beliefs and on local forms of institutionalized practice (Danziger, 1997). This is likely to reinforce existing trends in the direction of a less autocratic, more self-reflective, form of disciplinary practice. But localization is only one side of the historical process. The other side involves the interaction of centres and the consequent emergence of common understandings as well as renewed differentiation. In the past, certain locally generated categories of psychological discourse were often regarded as the only true descriptions of the universal attributes of a timeless "human nature". Insofar as they were built into the ahistorical methodology of so-called "cross-cultural psychology" they were immune to empirical refutation. A different approach to the history of psychology, however, offers the possibility of another perspective on the question of the universality of psychological phenomena. Though it is wrong to begin by taking such universality for granted, one could treat it as one possible outcome of specific historical conditions that are open to investigation. "Trans-social meanings emerge not as a result of methodological tricks, but of a real historical process" (Stompka, 1990). Modern psychology, with its universalizing categories and methods, might well be accorded a significant role in this process. The new historiography is well placed to investigate such questions.

Thus, the turn away from a unifocal linear history to a socially contextualized polycentric history is not a matter of interest only to antiquarians. It entails an enhanced link between historical reflection and current practice and will move the self-definition of the discipline much closer to the socio-historical rather than the natural sciences. Ultimately, this involves a long overdue historicizing of psychological knowledge.

NOTE

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(D) Historical Psychology

10. Long Past, Short History: The Case of Memory (2008)

Abstract: For Hermann Ebbinghaus, an early experimentalist usually credited with introducing the popular contrast between psychology's long past and short history, the distinction had to be made because psychologists had finally embarked on a path of cumulative progress by means of empirical investigations. However, Ebbinghaus did not invent the distinction so much as change its emphasis in a way that hid continuities between long past and short history. To illustrate: the history of memory reveals significant links between the experimental period and its predecessor, not only on the conceptual level, where ancient metaphors survive, but also on the level of practice, where there was a long tradition of systematic intervention in the operations of memory that provided material for modern approaches. Exclusive reliance on the particular discontinuity emphasized by Ebbinghaus also tends to obscure other peculiarities of the modern period that may be equally important historically.

I thought today might be a good opportunity for reflecting on a centenary, a centenary that I am sure will be news to many of you. Not that I think we really need another centenary – we've got too many of them already - but this one happens to be of rather special interest to historians of psychology.

So what centenary do I have in mind? Well, admittedly it's a rather modest centenary. It doesn't mark the birth of a great personality or of a laboratory but rather the birth of a slogan. It so happens that a catchphrase you all know made its first public appearance in 1908. Here it is:

> Psychology has a long past but only a short history (Ebbinghaus, 1908).

These were the words with which Ebbinghaus began his popular general textbook of psychology. As we know, they were the words of a slogan that quickly acquired a life of its own, often repeated, most famously by E.G. Boring two decades later. The words functioned as a declaration of independence for the new scientific psychology intent on breaking any links with the preceding era of mere speculation.

Clearly, for Ebbinghaus, and his followers, the new autonomy of experimental psychology had profound implications for the historiography of the discipline. On the first page of his 1908 text he goes on to explain what he means: Before the advent of modern psychology, he says, there was no “lasting progression”, no “progressive development” in the subject. A basic structure, laid out long ago by Aristotle, had lasted right into the 19th century without real change. That was psychology's long past – a time without progress. But now that psychology had become scientific it had finally acquired a history, by which Ebbinghaus meant a story of change and cumulative development.

In the early years of the 20th century, the distinction between psychology's long past and short history had a provocative aspect because 19th century books on psychology's past had had no hesitation in presenting this past as a history. But once experimental psychology had established an irreversible institutional presence historians of psychology faced a problem:

What exactly forms part of psychology's "long past"? What defines the boundaries of such a field? Before the 18th century, at the earliest, there was no generally recognized conception of psychology as a distinct subject area in the way we understand it. How then do we decide what belongs to psychology's past and what does not? As one professional historian put it:

> Histories of psychology…possess no rational criteria of inclusion or exclusion (Smith, 1988).

As long as we restrict ourselves to the more recent history of empirical psychology we are on relatively firm ground: we can use professional and institutional criteria to decide what is part of our topic and what is not. But once we go further back, those criteria become useless. In practice, what gets included in or excluded from psychology's long past becomes essentially a matter of convention. Of course, we can always give an anachronistic psychological meaning to any and all reflections about human experience. But this leaves so vast a field that we might well ask:

> Then of what would History of psychology **not** be a History? (Richards, 1987).

Does this mean we should just forget about the long past that is claimed for psychology and concentrate on its short history? Certainly, this is justified for many specific investigations. But if we never took a broader perspective modern psychology would begin to look like the result of some immaculate conception, which it wasn't. So what do we do about that long past?

One good reason why psychology's past is so hard to pin down derives from the fact that psychology itself is not so much a unified subject as a loose assembly of topics and approaches that is constantly changing. The very notion of A history of psychology implies an internal coherence that isn't there. One cannot expect history to supply the unity which the subject itself lacks. So we have to adopt an approach to the history of psychology that is based on the recognition of multiplicity rather than the myth of unity.

For the more recent history, this approach is evident in studies that trace the development of various content areas and of diverse professional and investigative practices. But psychological concepts also have their multiple, though interlinked, histories. The meaning of psychological research and practice is framed in terms of specific categories, such as motivation, intelligence, behaviour, attitude, personality, each of which has its own identifiable history. Some years ago I tried to trace some of those histories in a book I called *Naming the Mind* (1997). It turned out that virtually all of these categories only acquired their current psychological meaning in relatively recent times. Their history was co-extensive with the history of modern psychology. They might have had a past, but it certainly was not a psychological past. The break between their pre-psychological meaning, often moral or theological, and their psychological meaning was quite sharp. So apparently Ebbinghaus had been vindicated.

But then I asked myself whether there might be other categories for which this break was less pronounced. Very quickly, memory presented itself as a likely candidate. Unlike most of the terms in the modern psychologist's vocabulary, memory has a truly ancient lineage. Plato and Aristotle engaged in speculations about memory that attracted comment and discussion right up to the present day. Ancient Roman writers addressed the subject of memory as part of their discourse on rhetoric, a topic they took very seriously. Monastic authorities of the Middle Ages added their own interpretation of the nature and uses of memory. During the Renaissance there was an outburst of writings devoted to memory, and over the centuries there was also speculation about a physical basis for memory. In the late nineteenth century, memory becomes an object of investigation for modern science.

I won't pretend that it was only this remarkable history that aroused my interest in memory. Such an interest can safely be regarded as one of the symptoms of old age. The day comes when you have to admit to yourself that your memory isn't what it used to be. For me, that day came around the time I was becoming intrigued by the issue of psychology's long past and short history. So academic and personal motives converged nicely to send me off on what became a decade-long search for memory and its history. The outcome of that search was a book, *Marking the Mind: A History of Memory* (Cambridge, 2008), that has just gone off to the printer.

I would like to use my remaining time to share with you some of the things I found out about memory that seem to have a bearing on the relationship between psychology's long past and short history. As one would expect, there is both continuity and discontinuity in this relationship. Much of the discontinuity is fairly obvious, and there is no need for me to go over familiar territory: the features that make the modern scientific approach to memory different from what went before. Let me rather mention some of the historical continuities that show the break between scientific and pre-scientific to be not quite as sharp as it is often made out to be. After that, I will briefly mention one of the less obvious aspects of discontinuity.

One widely recognized link between ancient and modern ideas about memory is their common dependence on metaphor. Whenever people have tried to come to grips with the nature of memory they have found it virtually impossible to avoid the use of metaphors, especially metaphors of storage, of course.

> We make search in our memory for a forgotten idea, just as we rummage our house for a lost object…We turn over the things under which, or within which, or alongside of which, it may possibly be (James, 1890).

But this is just one example among an uncountable number stretching over many centuries. At the end of the seventeenth century, John Locke referred to memory as 'the storehouse of our ideas'. Well over a thousand years earlier, the storehouse metaphor had been celebrated by St Augustine who wrote about the 'wonderful storerooms` of memory, its 'huge cavern, with its mysterious secret and indescribable nooks and crannies'. Some medieval variants of the storage metaphor referred to boxes and chests for storing valuables, and some writers liked purses or money bags that could be used to convey the idea of memory as a compartmentalized store. In twentieth century literature on the psychology of memory the image of the purse was not unknown and was joined by that of a bottle, a junk box and even a garbage can.

Doing memory experiments did not mean escaping from an age-old metaphorical tradition, because it was difficult to describe what experiments told us about memory without having recourse to metaphor. When trying to explain the way memory worked, 20th century psychologists regularly invoked storage metaphors, as people had been doing for about two thousand years. This historical continuity was no secret (Roediger, 1980), and metaphors of memory were sufficiently topical to be discussed in journals such as *Behavioral and Brain Sciences* (Koriat and Goldsmith, 1996).

What accounts for the longevity of storage metaphors? Why do they seem so natural? Is there something that supports these metaphors' persistence? I think we can find a clue to the answer in the fact that physical storage was only an extension of earlier and persistent references to another kind of storage, the storage of symbols.

If we go way back to the earliest examples of storage metaphors we get to Plato's suggestion that we should think of memory as analogous to making impressions on wax. Why wax? Not only because of its physical properties, but because in Plato's time tablets coated with wax were commonly used as writing surfaces, and it was specifically the impressions made in writing that were to be regarded as analogous to memory traces.

> It seems to me that the conjunction of memory with sensations, together with the feelings consequent upon memory and sensation, may be said as it were to write words in our souls (Plato).

Plato was only the first in a long line of authors who depicted memory as a kind of inner writing. It's an analogy that runs like a red thread through the history of memory and is implicitly acknowledged in more recent theories of symbol storage.

Memory storage is a metaphor, yes, but it is based on something more than a mere analogy. It is based on a real peculiarity of human memory, the fact that people do their remembering in interaction with memory aids. These aids, whether they be marks on stone, letters on wax tablets, or programmable silicon chips, serve as an external memory that greatly magnifies the scope of whatever internal memory capacity humans may be endowed with. By the time people began to speculate about a memory inside them, that internal memory had already become quite dependent on an apparatus of external memory that kept on growing. It is hardly surprising that this visible apparatus always supplied the models for conceptualizing the operations of an invisible memory usually located inside our heads. Metaphors of memory were usually derived from the technology of memory, and as that technology developed, so the metaphors changed their concrete form, from wax tablets to books to computer programs. But technological developments only delivered improved variants of the operation of symbolic inscription that remained the basis of external memory. As long as people made use of an external memory based on technologies of inscription they had an ever present source for metaphors of internal memory.

Our visible success in building up the apparatus of external memory should not make us forget that there is a long history of attempts at developing a technology of internal memory. In ancient Greece and specially Rome there was already a body of written advice on memory training for the use of lawyers, philosophers and politicians. The context of public persuasion placed special demands on memory and called for specialized memory skills. Methods of memory improvement, known as mnemonics or sometimes mnemotechnics, were developed and eventually collected in well known manuals. It is clear that this technology of internal memory had an influence on speculations about memory. Aristotle's texts, for example, contain indications that he was familiar with the mnemonic advice of his time and used that knowledge in formulating his own ideas. The same holds for his medieval successors.

So it is not altogether true to say that the past only speculated about memory, whereas nowadays we practice planned interventions in its operations and scrutinize the results. In fact, since ancient times, theorizing about memory was accompanied by planned attempts to intervene in its operations. We have to add mnemonics, the technology of internal memory, to the factors that link different periods in the history of memory.

Just like the technology of external memory, the technology of internal memory underwent considerable change in the course of time. From Roman times until the Renaissance period visual imagery remains, not the only, but the most favoured tool for memory training. To get some idea of how this was supposed to work, let us look at two illustrations from a widely used book on memory training published in Venice in 1533 (Romberch). Imagery was to be used in two steps. In the first step you establish a stable background image that you can conjure up at any time. This background image should have several distinct locations or "places", such as the chapel, barber shop, etc. of the village used as a background in the first illustration.

Then, in the second step, you mentally place images of various concrete objects at specific spots in the background image. These concrete images are chosen so that they will remind you of particular topics you want to address during your speech, sermon, or argument. While speaking, you take a mental walk through your background image and come across your previously chosen topics one by one in a pre-determine sequence. The main point to note about this example is the way the whole process assumes that people already have or can easily acquire great facility in forming and manipulating visual imagery.

I am sure you have all been wondering how anyone could actually believe that such a cumbersome procedure, which seems to impose extra demands on memory, could function as a memory aid. Yet, procedures of this kind were taken very seriously over many centuries, even by individuals about whose considerable memory skills there is no doubt, for example St. Thomas Aquinas. How does one explain this?

One thing we do know. The popularity of imagery as an aid to memory training takes a nose dive after the end of the 16th century. By the mid-18th century the most successful manuals on the art of memory were emphasizing altogether different systems of mnemonics that privileged purely verbal methods. Some historians have linked this to the rise of Protestantism and the Puritan suspicion of imagery in general: the truth is in the word, not in 'graven images'. There was a drive to dump religious imagery – iconoclasm, a call to smash the images. In some influential circles this was generalized to human thought: serious thinking, scientific thinking, is incompatible with the analogical thinking encouraged by imagery. It has been suggested that this constituted a kind of "inner iconoclasm" (Yates, 1966). Be that as it may, the point I want to emphasize here is that the technology of internal memory, like that of external memory, is not static but is subject to historical change.

Perhaps we should regard the more recent switch to empirical investigations of memory phenomena as, among other things, a further development in the technology of internal memory. Historically, concern with memory always had a speculative, theoretical, aspect and a practical, interventionist, aspect that often influenced each other. This continued after the introduction of empirical methods. Certainly, memory discourse changed after the introduction of modern methods of investigation. But the change was not absolute; there were elements of continuity. Quite generally, mnemonic procedures and the procedures of memory research resemble one another in that both involve deliberate, planned interventions in the spontaneous operations of memory. More specifically, the most favoured *materials* during the first century of memory research strongly resembled the kind of material that had become prominent in post-Enlightenment mnemonics. I have just mentioned that in the history of mnemonics we can observe a significant break around the 17th century when the old preoccupation with visual imagery is replaced by a new focus on verbal elements and the associative links between them. When memory research made its appearance in the late 19th century it showed the same preference for verbal or quasi-verbal content over imagery that had become common in modern mnemonic systems and the same tendency to present this content in the form of lists of discrete items to be memorized.

It seems the relationship between psychology's long past and short history may be more complicated than Ebbinghaus had imagined it would be. In the case of memory, which was after all the topic on which his reputation was founded, there are inconvenient facts which make one skeptical about his simple scheme. There are continuities which bridge the break between ancient past and recent history, and there are previous historical breaks which, from a broader perspective, may be as important as the break in Ebbinghaus' lifetime.

My point is that the change from long past to short history involved more than the adoption of empirical methods. I do not want to question the importance of that adoption, but taking a longer term view enables one to see that the more recent period has links to the past that are rendered invisible by insisting on a complete break. Any exclusive focus on the advent of experimentation also obscures *other* differences between the more recent and the more remote past that deserve at least equal attention. I don't really have time to go into this, but I would like at least to point to one of these other differences to give you an indication of what might be involved.

One profound change began at roughly the same time as the break between psychology's long past and short history, that is to say, the latter part of the 19th century. When one has been following memory discourse over the centuries one becomes aware of an unmistakable change of focus at that point. Within a relatively few years there is a surge of interest in the negative aspects of memory, forgetting and memory pathology. In the past, there had been plenty of interest in memory but almost no interest in forgetting. The conviction that to understand memory one must pay attention to its failures and malfunctions is specifically modern. So when psychologists began to follow Ebbinghaus in studying memory by analyzing forgetting they were being more revolutionary than they perhaps realized. The more general fascination with memory pathology that starts in the 19th century and gathers momentum in the 20th is absolutely distinctive for the modern period.

Where does all this leave the distinction between long past and short history? Not altogether in the dust, because in many respects things did take a radically new turn around the time that modern psychology made its appearance. But this was not the beginning of history any more than 1990 was the end of history, as was once claimed. Ebbinghaus may have been a bold scientific innovator but history was not his strong point. He thought the history of modern psychology would become a story of cumulative progress, where facts would put an end to a long past of conflicting speculations. Well, that is not quite what happened. If anything, the switch to empirical methods increased controversy. Experimental research had too many unrecognized ties to the past and to the broader historical context to provide the answers that would make interpretive controversy obsolete.

At this point I can reveal that Ebbinghaus did not invent the distinction between psychology's past and its history, he changed its meaning. If we turn to what would have been a standard text in Ebbinghaus' environment, Max Dessoir's *History of Modern German Psychology*, republished six years before Ebbinghaus' book (Dessoir, 1902), we already find the distinction between a past and a history, though it has a different significance. Ancient Indian psychological thought, according to Dessoir, forms part of modern psychology's past, but it is not part of its history because there is no historical link between the two. History implies a continuous connection between past and present:

> A historical connection exists when the past continues to affect the present (Dessoir, 1902).

Dessoir's text dealt mainly with philosophical psychology, a topic that Ebbinghaus thought was no longer relevant to what experimentalists like himself were doing. By introducing the contrast between psychology's *long* past and *short* history Ebbinghaus was emphasizing the break that had recently occurred within western psychology, whereas Dessoir had focused on the continuity. A century later, it may at last be possible to appreciate both points of view.

NOTE

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11. Prospects of a Historical Psychology (2003)

Abstract: The project of a historical psychology must be distinguished from the history of psychology and from psychohistory. Unlike the latter, it conflicts with the assumption that what psychology studies is an ahistorical human nature. Although human psychology is deeply historical the discipline of psychology bears the imprint of its relatively recent origin. Historical psychology does not address an imaginary unity but explores the transformations of specific objects posited in discourse and targeted by intervening practices. Memory is taken as an example of such an object, some of its most salient features having been subject to historical change. Its history illuminates the background of current priorities.

Introduction

Some years ago I wrote a paper whose title asked the question: "Does the History of Psychology have a Future?" (Danziger, 1994). That question provoked a certain amount of controversy, as I had rather hoped it would. Today I would like to begin by asking a somewhat related question, namely: Does historical psychology have a future? This time I am really going out on a limb. Whereas the history of psychology is at least a recognized, if marginal, topic within the discipline, historical psychology has usually been regarded as being quite beyond the pale for any self-respecting psychologist.

At least, that is the state of affairs in North America, where this year marks the thirtieth anniversary of Ken Gergen's (1973) plea for a historical social psychology, a plea that has been cited an enormous number of times, but that has never, as far as I am aware, led to any actual research in the field. The edited book on historical psychology (Gergen & Gergen, 1984), which followed a decade later, fell on the same stony ground. In Europe, on the other hand, the mutual isolation of psychology and history has been less extreme and psychologists have not left the field of historical psychology left entirely to historians (see Peeters et al, 1988; Scribner, 1985; Sonntag, and Jüttemann, 1993).

On the whole though, the prospects for historical psychology seem rather bleak, to say the least. It is my view, however, that this bleakness is not quite deserved, that in our wholesale neglect of this field we may have thrown out the baby with the bath water. In other words, I think that, though the project of a historical psychology is undoubtedly problematic, there are certain kinds of historical studies that are quite capable of making a contribution to the discipline of psychology and should be pursued by psychologists.

I will develop my argument in two parts. In the first part I want to look at reasons for the exclusion of historical psychology. I am quite sympathetic to some of these reasons, and I will advance some considerations for avoiding the pitfalls that have beset the field in the past. In the second part I want to discuss the possible relevance of historical psychology for contemporary psychological work. To do this I will draw on my own studies of the history of memory.

But I before I go any further, I should explain that by historical psychology I do not mean psychohistory, a field dedicated to the interpretation of historical events, often the lives of particular individuals, in light of current psychological theories. Erikson's (1958, 1969) classical studies of Luther and Gandhi would be examples. Psychohistory assumes the universal, trans-historical validity of some contemporary psychological concepts, perhaps the concepts of psychoanalysis or some other system. Historical psychology, on the other hand, would try to question contemporary psychological concepts in the light of historical evidence.

Reasons and Remedies for the Marginalization of Historical Psychology

There is little doubt that most psychologists, and probably most historians, feel that anything like historical psychology is the business of history, not of psychology. People trained as psychologists are not equipped to handle historical evidence and should stay away from it. There is much to be said for this argument, and undoubtedly much of the primary research will continue to be done by historians. But this does not provide a valid reason for psychologists' *ignoring* of historical information. Consider an analogous case: Psychologists usually leave the technical conduct of neurophysiological investigations to physiologists, but no one would conclude from this that they should therefore ignore neurophysiological information. This unequal treatment of history and physiology suggests that there is more involved than merely the principle of maintaining a strict segregation between disciplines.

If disciplinary boundaries have to be rigorously protected against *historical* incursions, whereas snuggling up to neurophysiology is treated with indulgence, the implication is that psychology has a much greater affinity for some disciplines than for others. Where psychology is counted among the natural sciences information from related natural sciences will be regarded as more relevant to its concerns than information from a human science like history.

Whether we define psychology as a natural, a social, a human, or a historical science has implications for the way we regard its subject matter, or, to use a more accurate term, its objects of investigation. A natural science investigates natural objects, that is, objects regarded as part of the natural order, objects whose characteristics conform to universal regularities and whose properties are independent of human beliefs and practices. Insofar as psychology is defined as a natural science the assumption is that its objects are of this type, and that means they are essentially unaffected by human history.

Regarded as a working hypothesis this position is not altogether unacceptable. In practice, however, it has not been treated as a hypothesis but as something that is self-evidently true and beyond question. To a very large extent psychology *has* investigated its subject matter as though it belonged to an ahistorical human nature. This may have produced results, but the assumption of ahistorical validity remains only an assumption until it is tested against relevant evidence. The relevant evidence in this case would have to be historical, and that leads one straight to the subject matter of historical psychology. In other words, the scientific grounding of the belief that historical psychology is redundant would itself require evidence from historical psychology. This does not appear to offer a sound basis for rejecting the field.

In Europe, and especially in Britain, skepticism about the prospects of historical psychology has taken an altogether different form. There is no rejection of the entire field because of a belief in the natural, ahistorical, nature of psychological processes, but there are worries that the field is unmanageable, that it has no clear boundaries, that there are no acceptable criteria by which one might define its subject matter (Richards, 1987; Smith, 1988). These kinds of concerns are based on the conviction that matters psychological are in fact deeply historical, in other words, a position that is exactly the opposite of experimentalist ahistoricism. But if one believes in the deep historicity of psychology one faces the dilemma that, in one sense, almost everything in human history pertains to psychology, yet, in another sense, almost nothing does, except during the last century or two. Psychology's status as a natural science, and hence its commitment to certain methodologies, impose fairly clear limits on its territory. But if we were to drop these restrictions and regard all the historical expressions of human nature as fair game, then historical psychology would certainly become a field without boundaries and without discipline.

It is also argued that the discipline of psychology is itself a historical formation, a way of regarding the world and a way of acting that is a product of a particular historical context, and a relatively recent one at that. If that is so, then we are not entitled to inflict this modern psychological perspective on times when it did not exist. Doing so would amount to a distortion of the historical facts. But in that case historical psychology lacks legitimacy if it is pursued beyond the most recent period of human history.

If one accepts that such arguments should not be lightly dismissed, and I do, does that spell the death knell of anything like a historical psychology? If one does not wish to flounder in a field that is totally amorphous, nor end up as a crude presentist, are there any lines of work that might constitute an acceptable form of historical psychology? My answer is a cautious yes because I believe that the implications of the critique need not be entirely negative.

In the first place, one must not expect historical psychology to be a unified field: no grand schemes for the social evolution of the human mind à la Wundt (1912), nor problematic constructions of cultural history, such as the unique "mentalities" attributed to different ages by some historians (Lloyd, 1990). I see the field rather as a collection of studies tracing the historical background of specific psychological objects.

There are areas where this approach to historical psychology has been relatively successful, child development and abnormal psychology in particular. A considerable literature on "the history of childhood", some of it contributed by psychologists, has used historical evidence to probe the universal validity of concepts developed in the context of contemporary realities, including of course the contemporary understanding of the concept of childhood itself (see e.g. Kessel and Siegel, 1983; James and Prout, 1990; Niestroy-Kutzner, 1996). In the field of abnormal psychology there are many relevant studies in the history of psychiatric diagnosis, including hysteria (Micale, 1995), schizophrenia (Boyle, 1990), post-traumatic stress disorder (Young, 1995), multiple personality (Hacking, 1995), and many other conditions (Peeters, 1996; Borch-Jacobsen, 2001). Useful studies have also been devoted to the historicity of categories of human emotion (Harré, 1987; Stearns & Stearns, 1988).

These kinds of investigation avoid becoming bogged down in a field without boundaries by limiting themselves to the history of specific psychological categories taken from current psychological practice. The historicity of these categories is then explored by tracing their antecedents. The metaphor of genealogy (Foucault, 1977) nicely expresses the simultaneous attention to historical continuity and discontinuity which characterizes these studies. A genealogy traces intergenerational continuity, but this continuity implies anything but intergenerational identity. Each generation is unique though tied to the others by a historical bond.

Viable studies in historical psychology also tend to be non-Cartesian, that is, their subject matter is treated as neither part of the natural world nor a matter of subjective experience. These studies are concerned rather with discursive objects not found in the natural world but constructed by humans through the use of language and other semiotic devices. Psychological categories have a human history only as discursive objects and as markers defining the targets of human social practices.

For example, in my own recent work I have been concerned with the history of a venerable psychological object, namely memory. Discussions of the nature of memory go back to the philosophy of Ancient Greece and a readily identifiable corpus of writings on memory extends from those early times to the present. Those texts provide a basis for the development of a historical psychology of "memory" as long as we do not expect them to tell us more than they are capable of telling us. They cannot, for example, tell us about the subjective experience of people who lived a long time ago, as some early versions of historical psychology claimed. As long as we restrict ourselves to the material and discursive practices involved in the production of textual evidence we are on relatively firm ground. What texts offer the historical psychologist are discursive realities. The history of these is neither a history of natural objects, nor a history of subjectivities, but a history of discursive objects.

<u>Historical Psychology and Current Psychological Research</u>

I now want to address the question of whether historical psychology has anything to contribute to contemporary psychological research. Many psychologists might well agree that historical psychology is a legitimate field of scholarly interest though they would see it as a field without the slightest relevance for current work in psychology. This assumes that the break between scientific and pre-scientific psychology is absolute. But this is simply untrue.

For example, a juxtaposition of the modern and the ancient literature on memory reveals an amazing continuity in explanations of how memory works. From Plato to theories of information processing these explanations have relied heavily on the metaphor of storage (Carruthers, 1990; Danziger, 2002). In trying to understand how they were able to recall things they learned or experienced in the past people have always made use of analogies from storage devices, especially information storage devices, with which they were familiar, from wax tablets to books and libraries to computers. That metaphor has played a major role in theories about memory right up to the present is uncontroversial. Prominent memory researchers have explicitly recognized this (e.g. Roediger, 1980) and a few years ago metaphors of memory were even a subject for extended discussion and comment in *Behavioral and Brain Sciences* (Koriat & Goldsmith, 1996). But memory is probably not a special case, and antique metaphors are likely to have played an equally important role in other areas of psychology (Leary, 1990).

However, historical continuity operates not only on the level of ideas and concepts, but also on the level of practice. Psychological practices, including research practices in particular, do not exist in some hyperrational space outside of culture. No, they are very strongly influenced by the culture in which they arose and the culture which maintains them in being. These cultures have historical roots, which means that psychological practices have historical roots.

To go back to memory, we note that since ancient times people have not only theorized about its nature, they have also tried to intervene in its operations. For more than two thousand years there have been systems of memory improvement or mnemonics, and in times when external memory aids were far less adequate than they are today these systems were accorded far greater respect than we accord them today. Systems of memory improvement have been described and discussed for many centuries. Traditionally, a distinction was made between *artificial memory*, which was memory operating with the help of deliberate mnemonic procedures and *natural memory*, which was memory operating without these aids.

Mnemonic procedures and the experimental procedures of memory research have something in common in that both employ deliberate, planned interventions in the spontaneous operations of memory. Where they differ is in the primary purpose of these interventions – practical improvement in the case of mnemonics and better knowledge of the nature of memory in the case of research. This obvious difference in purpose entails obvious differences in the process of intervention. However, one cannot intervene in the spontaneous operations of memory in the abstract. One can theorize about memory in the abstract, but in practice, whether in an experiment or in a course of memory improvement, one is obliged to make use of one or other kind of content: word lists, rhymes, stories, pictures of faces, or whatever. The choice of content is potentially unlimited, so it makes sense to ask why one kind of content is chosen rather than another. Many factors are at work here, including considerations of technical feasibility and convenience. But typically these technicalities only enter the picture after the field of options has already been narrowed down by other factors. One of these is tradition. Those who intervene in the operations of memory typically do not do so in isolation. They either belong to a community of scientific investigators who all use similar kinds of content, for example, nonsense syllables or word lists; or they are part of a collection of experts on mnemonics who, historical evidence tells us, have copied from one another since time immemorial.

In both cases the force of tradition imposes a certain inertia when it comes to the choice of content for improving either memory itself or knowledge about memory. But traditions are not unchangeable. Eventually, nonsense syllables lost their popularity and, over a much longer time span, the content of mnemonic systems changed too. No doubt, both continuity and discontinuity in practices of memory intervention depend to some extent on simple pragmatic factors. For example, both nonsense syllables and certain mnemonic procedures were abandoned because it gradually became clear that they just didn't do what they were supposed to do – eliminate meaning in the case of nonsense syllables. But this is not the whole story. The content on which memory intervention procedures rely is not culturally neutral, and for that reason, if for no other, it is liable to be affected by broader cultural changes.

For example, a striking feature of the first century of scientific memory research was the ubiquitous use of *verbal* stimulus material. Towards the end of that period memory for other kinds of material gradually came in for more attention, but in the words of one researcher, "up until the early 1960s, students of memory had been resolutely and single-mindedly concerned with verbal materials" (Snodgrass, 1997, p.202; see also Bower, 2000, p.25). Why was this? In answering this question it is hardly possible to overlook the fact that the concentration on verbal materials was established at the very beginning of memory research and became a tradition that later investigators could not ignore. They needed to link their work to previous work in the field, and that meant using verbal materials, unless they were prepared to launch a far reaching critique of the whole approach, which did eventually happen, though with only partial success.

But how was this fixation on verbal memory able to flourish in the first place? Here broader cultural and historical factors become relevant. For example, it is significant that a concentration on verbal materials was already a feature of the mnemonic systems that were popular around the time experimental memory research made its appearance. In this respect there was a certain continuity between the mnemonic and the experimental way of intervening in spontaneous memory processes. Both were primarily interested in verbal memory and relied on materials that were most often verbal in type.

A historical psychologist would want to pursue this matter further. Mnemonics, as I have mentioned, has a long history, and one does not have to go too far back to find that the focus on verbal memory is historically a relatively recent phenomenon. In fact, verbal memory, though clearly recognized as a specific form of memory, was generally downgraded in the classical texts on what was known as "the art of memory". That only began to change in the eighteenth century. In earlier times there was far more emphasis on the mnemonic importance of imagery and of spatial relationships than on verbal mnemonics. When one adopts a wider historical perspective, one has to face the possibility that the verbal bias of much modern memory research may be essentially a continuation of a long term trend in the cultural meaning of memory. By opening up this kind of perspective historical psychology could help to counter the effects of cultural-historical bias on the direction of empirical research. In areas like memory, historical studies can draw attention to potentially interesting aspects that have been neglected, excluded, or rendered invisible by current fashion.

Let me cite another example. As I have indicated, cultural-historical bias invades experimental research through the medium of the materials used to study one or other psychological domain. In the case of memory research there is one feature that is even more pervasive than the use of verbal content and that shows little sign of being on the way out. I am referring to the use of printed stimulus materials. When such materials are used it means that the memory being studied is memory for content that has been given a lasting form by a process of inscription. This means that there is always a definitive version against which anyone's memory performance can be compared. This is the kind of memory that is paramount in educational and scientific contexts but in many everyday situations memory cannot possibly work like that. When two of us remember a common experience that was not recorded we may or may not agree on what really happened, but the question of memory accuracy cannot be decided by reference to a definitive version because there isn't one. Memory then operates in a different way, becoming an intrinsically social rather than an individual performance, perhaps through the co-construction of an agreed upon version (Edwards & Potter, 1992).

In preliterate societies this is the way all memory must operate, and among illiterate people this must surely remain the dominant form of memory. But until relatively recent historical times there were no societies in which the majority of the population was not illiterate, and even now, of course, a large part of the world's population remains in that state. Remembering information recorded in a definitive text and remembering unrecorded information have coexisted for many centuries. But their relative valuation has changed. Plato, for example, was quite derisive about the value of remembering written texts, and in Roman times the whole topic of mnemonics was subsumed under the study of rhetoric, that is, memory improvement was assumed to be in the service of public speaking. That began to change in the Middle Ages when accurate memory of holy and then legal texts was highly prized. With the spread of printing and secular education accurate remembering of all kinds of texts became increasingly important. A new, largely verbal, mnemonics supervened whose aim it was to facilitate the accurate regurgitation of itemized factual information recorded in printed sources. This very special form of memory became the ideal and was increasingly equated with memory as such. That provided the cultural context in which the earlier psychology of memory developed and from which it derived its unspoken presuppositions. By offering an insight into the historical contextualization of particular research approaches a historical psychology, for instance of memory, should provide an antidote to the common vice of substituting one part of a psychological domain for the whole.

One final example of the relevance of a historical psychology of memory for current work: Most psychological studies of memory, and particularly neuropsychological studies, appear to be designed and interpreted as though the seat of memory were entirely within the individual. Yet everyone knows that this is not true. Not only is remembering an activity that is frequently performed in concert with others, but it also depends on the information that is stored in things, in books, filing cabinets, computer memories, and so on. Effective remembering is often the product of a system that includes both objects and humans as its components. Historical studies are useful adjuncts to the study of such systems because they provide evidence of relationships between internal and external memory that are no longer available for inspection today. It can be shown, for example, that historical developments in the display and arrangement of written material on a page served mnemonic functions. There was constant interaction between the technology of external memory and the way people used the memory in their heads.

Conclusion

Let me conclude with a general observation: The prospects for a viable historical psychology ultimately depend on an end to the unhealthy division between the disciplines of psychology and history, a division that reflects the opposition between two sets of prejudices, those of naturalism and of historicism. Naturalism naively assumes that any entity which happens to be the target of current psychological research, theory or intervention must be a natural phenomenon that has always been there waiting to be discovered. Naturalism goes hand in hand with presentism which essentially junks history and values it only insofar as anticipations of the present can be read into it. Historicism, on the contrary, insists on taking the past on its own terms and emphasizes the discontinuities between past and present. Therefore it would limit the history of psychology to the modern period of scientific psychology.

It is possible, however, to steer a course between presentism and historicism without suffering shipwreck. Presently existing psychological objects must provide a base from which historical inquiry departs and to which it returns. But the present is not privileged in other ways. It does not represent the final truth but simply another point in a temporal order that guarantees its imminent obsolescence. Instead of judging the past in the light of the present historical psychology needs to interpret the present in the light of the past. It then becomes, in Foucault's famous phrase, a history of the present.

Many of the objects which modern psychology targets and investigates did not emerge *de novo* when the first psychological laboratories were founded. Their pre-scientific history opens up questions which are not being asked though they should be asked. For example, were there historical changes in the operation of human memory, and if so, what were the factors involved in such changes? That is the kind of question which, though important and legitimate, is likely to vanish in the deep gap that currently exists between the disciplines of history and psychology. It is however exactly the kind of question that would fall within the province of historical psychology if we had one.

NOTE

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12. The Holy Grail of Universality (2007)

Abstract: Although the sites of psychological research and practice have always been internationally diverse, the knowledge claims of the discipline have always been universalistic. In an earlier period this divergence was often rationalized in terms of differences among “schools” and more recently in terms of “indigenization”. In the long run, the imposition of methodological uniformity led to a questionable kind of universality. Rather than treating psychological universality as an unquestioned default assumption, attention should be paid to historically emergent universality. Two examples of the latter are presented: changes in cognitive technologies over long periods, and "looping effects" in the more recent spread of psychological concepts and techniques.

Universality as a Default Assumption

In most areas of psychology an empirical study will not be rejected for publication on the grounds that all the experimental subjects were North American college students. It is perfectly acceptable to generalize in print about human memory or human decision making or human motivation on the basis of phenomena observed among an atypical fraction of the world's population in one corner of the world.

In other words, there seems to be a default assumption that empirical findings obtained in one location provide an acceptable basis for theoretical generalization unless and until other studies show this assumption to be unjustified. In most cases, we might note, those other studies are never done (see Sue, 1999), and local findings find their way into textbooks in a universalized form, as though they applied to human functions in general. Although in rare comparative studies, e.g. in category-based reasoning, researchers have noted that "undergraduates are [actually] 'the odd group out'" (Medin & Atran, 2004), these practices continue unabated. From time to time, they are subject to critical comment, yet they still constitute "the default condition of much of experimental psychology, which focuses solely on American undergraduates at major universities" (Atran, Medin and Ross, 2005).

One of the reasons why this state of affairs persists can be found in a pervasive belief among psychologists that what they investigate are human universals. This belief seems so deeply ingrained that it has recently been referred to as "a foundational postulate of psychology" (Norenzayan, A. & Heine, S.J., 2005). As such, it usually remains implicit, at least in North America and Western Europe. But when psychologists start wrestling with cross-cultural and cross-national differences, the issue of psychological universals is much harder to evade. So standard texts in cross-cultural psychology always address the topic of universality (e.g. Adamopoulos & Lonner, 2001; Miller, 1996). In spite of its concern with local differences, the field as a whole exhibits an explicit bias towards universalism (Jahoda, 1999; Smolka, 2000).

The topic becomes even more acute in the context of what are nowadays called "indigenized psychologies", versions of psychology that have been explicitly adapted to local conditions by local practitioners. Mention of such psychologies first appeared in the international literature about twenty years ago (Blowers & Turtle, 1987; Paranjpe, Ho & Rieber, 1988; Sinha, 1989) and since then, the idea of psychologies with local roots has come in for a fair amount of attention internationally (e.g. Adair & Diaz-Loving, 1999; Allwood, 2002; Kim & Berry, 1993; Staeuble, 2006).

Recently, I was asked to comment on a dozen or more accounts of "indigenized psychologies" that had been supplied by psychologists from all parts of the world: India, China, New Guinea, Poland, Cameroon, and many others (Allwood & Berry, 2006). As you might expect, variety was a striking feature of the accounts of indigenization I looked at. Each of them referred to aspects of psychology that were considered to be particularly appropriate to one or other local context. Some of them even hinted at versions of "psychology" that would certainly be considered beyond the pale by conservative members of our discipline. Nevertheless, more than half the accounts of indigenization that I looked at included explicit expressions of faith in what was generally referred to as "universal psychology" and no one explicitly rejected this ideal. Indigenization was seen, not as a rejection, but as a way of approaching a "universal psychology", though the manner in which this would be accomplished tended to remain a little hazy. So indeed, some respect for the ideal of universality seems to be part of maintaining one's professional identity as a psychologist. This, in spite of the fact that one's version of psychology may well be profoundly incompatible with other versions.

When Asian or African psychologists simultaneously avow a local version of psychology *and* psychological universalism they are only being explicit about something that has been implicit in the practice of modern psychology since its beginning. Psychological knowledge is not produced in some extra-terrestrial sphere, it necessarily has to be produced at some location (or set of locations) on this planet, and that means, not just a geographical, but a social location. Inevitably, it has borne the marks of its origin. Yet, psychology as a science has always clung to its foundational postulate of universality.

In this talk I would like to pursue this paradox a little further. I will do so in two parts. In the first part I will present a brief historical overview, beginning with the observation that the tension between universalism and localism is as old as the discipline itself, going on to later attempts at homogenizing psychology, and then mentioning some recent manifestations of this tension. In the second part of my talk I will go beyond this intra-disciplinary account and raise the question of whether there is perhaps a kind of psychological universality that is historically emergent rather than purely biologically based.

Dispersal

Modern psychology had universalistic aspirations from the beginning. In the division of labour among the human sciences that crystalized in the late 19th and early 20th centuries each discipline appropriated a particular domain of human activity as its special object of study (Ross, 2003). Linguistics made language its own, economics took over human behavior in markets, sociology laid claim to humans in groups, and so on. The domain of psychology was defined, not in terms of human activity in specific social contexts, but in terms of the features of individual activity that were universal across all contexts. Psychology's core domain was the mental life of the universalized asocial individual.

The categories that defined the domain of modern psychology, perception, memory, learning, cognition, and so on, were all predicated on a prior split between the realm of the individual and the realm of the social. Ideally, the psychological brand of universality depended on the externalization of everything that endowed individual action with social or cultural meaning. Psychologists would build a science of the universal natural characteristics of human individuals. The notion of a science of <u>individuals</u> demarcated the boundaries of the discipline. Culture and society were optional extras, left to the tender mercies of colleagues in fields widely regarded as somewhat less scientific than psychology.

Unfortunately, the universality implied by psychology's trans-cultural aims soon came into conflict with facts on the ground. Scientific psychology was international from its earliest years. Different versions of this new science appeared at more or less the same time in a number of countries, not only in Wundt's Institute in Leipzig, but also in Galton's anthropometric laboratory in London and in Charcot's clinic in Paris (Danziger, 1990). Yet, the psychological knowledge being produced at these sites was assumed to pertain, not only to individuals in Paris, London or Leipzig, but to individuals in general.

There was always a certain tension between modern psychology's unifying disciplinary ideal and the reality of its internationally varied practice. Ideally, everyone was working towards a universal psychology of individuals-in-general that was to be discovered by investigators whose social background was irrelevant and whose methods were nothing if not objective. But this ideal was in stark contrast to the reality of internationally dispersed sites at which representatives of different scientific traditions and individuals with different cultural backgrounds were participating in widely varying projects. The commitment to a common universalist ideal was not sufficient to overcome the centrifugal pull of local contexts.

In fact, the local diversity of psychology and its subject matter assumed dimensions that led to talk of psychology in the plural, several psychologies rather than one psychology. References to "Schools" of psychology became common, Behaviorism, Gestalt psychology, and Psychoanalysis being only the most prominent ones.. The differences between these Schools involved much more than discrepant explanations of the <u>same</u> phenomena. They did not agree on the meaning of many of the phenomena that each School had reported. What was an interesting observation for members of one School was a misleading artefact for those with a different allegiance. Members of different Schools had different conceptions of how to produce scientifically significant psychological information. There were deep divisions about the very goals of psychological investigation.

It is not necessary to elaborate on these differences because we already know about these matters from our history texts. What psychological texts on the history of the discipline do not emphasize sufficiently, however, is the <u>local</u> grounding of the psychological Schools. Although these were all "Western" products, cultural differences between western countries were more marked then than they are now. Professional historians have been more explicit about the fact that behaviorism was essentially an American phenomenon (Ash, 2003), that Gestalt Psychology could never outlive its German roots (Ash, 1985), that psychoanalysis took profoundly different directions in different parts of the world (Hale, 1995; Rayner, 1990; Roudinesco, 1990).

During modern psychology's first half century there was little indication that the disciplinary reality was getting any closer to the disciplinary ideal. By the 1930's the gap between national psychologies had not narrowed but widened.

Hegemony

In the period that followed World War II psychology's international situation changed drastically. To all intents and purposes only one major national center of scientific psychology survived that war, the USA. Not only did it survive, it gained enormously in prestige and resources (Capshew, 1999; Herman, 1996). Internationally, the development of scientific psychology was now spectacularly uneven, an outcome that was powerfully reinforced by the fact that the country in which psychology had flourished so magnificently was also by far the most powerful country in that part of the world in which psychology really mattered. Whereas previously there had been several centers able to participate in an international dialogue on more or less equal terms, there was now one center that indisputably overshadowed all the others. Compared to this center, all the others found themselves in peripheral locations. Scientific psychology now had a world center that provided *models* of theorizing and of investigative practice for those at the periphery.

The result was a growing level of international uniformity in the content and practice of the discipline. With the exception of the Soviet sphere of influence, American textbooks were used all over the globe, or else their style was copied by only marginally different local products. Rules of scientific publication culled from APA manuals came to displace traditional local patterns. For armies of graduate students from every part of the world, American norms for deciding what counted as scientific psychological knowledge became universally valid norms. The overwhelming preponderance of American research made it easy to ascribe universal significance to topics and problems that had been formulated to fit a specifically American context (Teo & Febraro, 2003; Strien, 1997).

Homogenization was particularly noticeable on the level of technology. By the middle of the 20th century, American psychologists were increasingly emphasizing the methodological basis for the universality of their generalizations (Danziger, 1997). Following this lead, psychologists in many countries became convinced that the potential reach of their generalizations depended on the use of the right scientific procedures, what have been called the standardized tools of psychological investigation (Yanchar, 2000). These tools were eminently portable. Quantitative procedures can be learned and adopted by anyone with some basic arithmetic resources. In this respect, psycho-technology works just like other forms of technology; you do not need to become a different person to borrow someone else's technology, a little specific training will do.

For those at the geographical and disciplinary periphery the adoption of a technical discourse and practice that was largely American in origin had obvious advantages. It provided them with a passport that gave them entry to a world-wide community of behavioral scientists whose language they could now speak and whose view of psychological reality they could now share. Outside this disciplinary community it enabled them to benefit from the general prestige of techno-science, a benefit that sometimes translated into greater access to the scarce resources they needed for their work.

The portability of psycho-technology made a particular kind of psychological knowledge transculturally reproducible and therefore, in a sense, universal. A set of methodological prescriptions held out the promise of generating truly objective knowledge that would be independent of the local biases of investigators. It was hoped that these biases would be eliminated by recasting all empirical questions in the format of linear causality and adopting more or less mechanical decision rules, such as statistical significance levels, to adjudicate among theoretical alternatives. A common technology would provide psychologists with a lingua franca that bridged theoretical and international differences.

This development created an appearance of universality that essentially depended on a methodological consensus among investigators. If everyone employs basically similar techniques, believed to be objective, it may well seem that the resulting knowledge has universal validity though there is no necessary connection between the replicability of methods and the universal features of the object of investigation. Universality can be a feature of the discipline and also a feature of what the discipline is interested in. For the discipline, universality means that, provided proper procedures were followed, any findings will be replicable by other investigators. Achieving a universal psychology in this sense depends entirely on employing methodologies that yield replicable data. However, we can also speak of a universal human psychology as something that exists, has in fact existed throughout human history, even when there were no psychologists. "Universal psychology" in this sense has nothing to do with methodology.

Resistance

However, adoption of imported technical discourse and practice was sometimes accomplished in the face of significant local resistance. I know what I am talking about because at an early stage of my career I was among the missionaries for these practices in remote parts of the world. I well remember a five-hour debate in the Senate of my Indonesian university about whether quantitative investigations in the human sciences might provide acceptable grounds for the award of graduate degrees. "Our" side won, as it always did in the end. My own doubts about the value of such victories came later.

In this respect I was not alone. Late in the 20th century there were signs that the shape of psychology as an international discipline was beginning to change once more. Non-American centers of psychological theory and research were multiplying and becoming more responsive to local problems and ultimately to local traditions. Sometimes this growing autonomy became quite self-conscious and led to the proposals for "indigenizing" psychology I referred to earlier. A key principle of indigenization was that "behavior is to be understood..... in terms of indigenous and local frames of reference and culturally derived categories" (Sinha, 1997, p. 132).

The call for indigenizing psychology in Asian and African countries had an interesting corollary. It entailed a questioning of the universalistic claims of the psychology that had been imported from America and Europe. This imported psychology was no longer accepted as a science that floated above society; if psychology has an "indigenous" aspect, it follows that so-called "western" psychology must be as much a product of local conditions as Indian, or any other version of psychology (Owusu-Bempah & Howitt, 2000). Or perhaps we should use "western psychology" in the plural, because the early 20th century Americanization of European psychology provides as good an example of what is now called indigenization as one could hope to find (Danziger, 2006).

By the closing years of the 20th century the social localization of psychology had become a topic to be addressed in the context of psychology's internationalization and there was evidence of greater openness to culturally diverse voices among American psychologists (Mays, Rubin, Sabourin & Walker, 1996; Gergen, Gulerce, Lock & Misra, 1996). There was also a new sub-discipline, "Cultural Psychology" (Cole, 1996; Shweder & Sullivan, 1993), based on the principle that psychological processes were "always grounded in particular sociocultural historical contexts" (Miller, 2002, p.99). This understanding, and the growing practice of international research collaboration on a basis of equality, led some cognitive psychologists to recognize that if they restricted their investigations to one cultural context they were, in effect, doing a kind of ethnography of that culture (Nisbett, Peng, Choi, and Norenzayan, 2001).

This more recent sensitivity to the social embeddedness of both psychology and its subject matter certainly represents a long overdue corrective to the rather mindless universalism that is still quite common. However, a new kind of problem arises when sensitivity to social context is automatically translated into sensitivity to the local and the particular. This link is problematical because social contexts do not necessarily privilege divergence, sometimes their convergent effects are far more powerful.

Usually, the emphasis on divergence of social context is conveyed by the promiscuous use of the category of "culture", a term once used in reference to human commonalities but later monopolized by a discourse of human differences (Kuper, 1999). Anthropologists, from whom psychologists borrowed the concept, used it specifically to wage what one of them called "guerilla warfare against psychological universalism" (LeVine, 2001, p.xi).

But it is also possible to replace the abstracted features of individuals with the abstracted features of cultures, so that one ends up with a universal psychology of cultural differences. The trouble is that this relies on a somewhat metaphysical view of cultural essences that is itself culturally grounded. For example, R. E. Nesbitt (2003) finds that an impressive array of data on cultural differences reveals a profound divergence of so-called "Eastern" and "Western" cognitive styles. One of the observed features of the Western style is its tendency to operate with dichotomized and essentialized categories. In that case, how can we be sure that the dichotomization of "Eastern" and "Western" styles is anything else but yet another manifestation of the Western style?

It seems odd that culture is so often territorialized and essentialized in an era marked by vastly increased levels of homogenization, standardization, blurring of cultural boundaries, in short, globalization (Hermans & Kempen, 1998). Sometimes, a reduction of complex global interrelationships to cultural differences is a way of ignoring crucial institutional, economic and political factors. At other times, the concept of culture is mobilized to emphasize the local and the traditional in an attempt to resist the juggernaut of globalization. However justified this resistance may be, it should not divert our attention from the common features of modern life, whether in Delhi, Berlin, or Soweto.

Historically Emergent Universality

From the first part of my talk you will have gathered, I hope, that there is a historical dimension to the universality of psychological science. I now want to turn to the related question of whether there is a historical dimension to human universals as potential *objects* of psychological science. A positive answer to that question is obviously implied when evolutionary psychology refers to our phylogenetic history. We can find common features of human life in our remote past. But what about common features that are emerging at the other end of the arrow of time? History did not stop with our hunter-gatherer ancestors, it is still going on. In looking for sources of universality, perhaps more recent history is worth a second look. This is not to deny the common human constitution as a source of psychological universality. My concern is rather to draw attention to the *inherently* historical nature of important aspects of that constitution. To the degree that there is historical convergence among different localities universality may be an emergent feature.

A little over a decade ago, an international group of distinguished natural and social scientists (the Gulbenkian Commission on the Restructuring of the Social Sciences) came up with this suggestion, among others, for "the future of the social sciences":

> The question that is consequently before us is how to take seriously in our social science a plurality of worldviews without losing the sense that there exists the possibility of knowing and realizing sets of values that may in fact be common, or become common, to all humanity (Wallerstein, 1996, p.87).

I'd like to draw your attention particularly to the words “or become common”. Usually, when we look for human universals, we look for things we have always had in common. This group, however, suggests we consider the possibility that some of the things we have in common may still be taking shape *now*. Their advice is, not to overlook the *historical* aspects of universality. Of course, the history we are part of is always the hardest to see.

How might this be relevant to psychology? It would indeed be relevant if we could identify conditions that are producing a general convergence of human experience and required behaviour in the modern world. There are many candidates for such an identification, but on this occasion I want to single out two cases with particularly strong psychological implications. The first of these concerns the technology of cognition, the second, what have been called the looping effects of psychological categories. I will address these in turn.

The Technology of Cognition

Most human cognitive activity takes place in an environment of human artifacts. And this environment has never stopped changing ever since there were humans to invent new artifacts and to improve on what they had. Human technology has always been on the move, at first quite slowly, and now very rapidly - it is inherently historical. But it is not only mobile across time, it is also mobile across space. Humans have always engaged in technological borrowing and copying, though there were also barriers that limited the spread of technologies. More recently however, those barriers have been collapsing at an astounding rate so that a condition of technological universality is within sight. On this level surely, we can all see things we have in common that are in the process of becoming universal.

That has to have important psychological implications because human technology and human cognition are tightly interlocked; the one cannot change significantly without the other. Insofar as human cognitive performance is tied to human technology there is an inherently historical aspect of human nature. Humans are what Andy Clark (2003) calls "Natural-Born Cyborgs", that is, their cognitive functions have always depended on the use of artifacts of their own making. Human cognitive capacities are an unknown potential until they are made manifest through the use of some historically evolved technology.

Symbolic technologies, such as writing, provide some of the clearest illustrations of this process. The *potential* of doing all the things people were able to do with the help of writing was there long before it became manifest in the form of specific skills. But this did not happen overnight. The technology of literacy took many centuries to evolve, involving changes in hardware, stone, wax, parchment, paper, ink, print, and so forth, as well as changes in software, leaving spaces between words, page layout, indexing, use of images and diagrams, and much more.

Why did it take so long? Well, there were many reasons of more interest to historians than to psychologists. But one reason is that human cognitive functions had to adapt to each new development. During the last few years I have been amusing myself by studying the history of memory, an old man's pastime (Danziger, 2008). It is possible to do this because writings on memory go back as far as Ancient Greece and because memory aids in the form of written manuscripts have been preserved. Over time, there are correlated changes in the uses of memory and the technology of memory.

One quick example must suffice: Classical Roman texts were written without spaces between words (*scriptura continua*), they also lacked paragraphs, chapter headings, indices, diagrams and other technical aids that we take for granted (Saenger, 1997). Imagine using this kind of text as an aid to memory in your academic tasks! It would not work, because the technology is not designed for such tasks and we lack the cognitive skills for using it. Yet we know that documents of this type were once valued as a form of external memory. How did that work? First of all, the Romans particularly prized a form of memory performance that is not so important today: public declamations, speeches and recitations relying on written material learned by heart. Texts were meant to be remembered auditorily, as speech. Accordingly, reading was taught on the basis of phonic recognition, so that a piece of writing represented a pattern of sound that the reader could chunk according to the demands of oral delivery. The topic of memory was nearly always discussed in the context of rhetoric, a highly esteemed discipline in those days.

In the Middle Ages quite different memory functions became more important, functions that involved relating texts to each other, biblical concordances for a start, then theological and legal disputes carried on between a network of dispersed localities by means of travelling texts. The technology of external memory changed, making ever more use of visual aids, spaces between words, paragraphs, headings, and so on. When you follow the further development of external memory: the adoption of alphabetical indexing, library catalogues, increasingly abstract diagrams, and much much more, it is clear that the *organization* of memory is changing. This took many centuries, but eventually, cognitive skills associated with advanced forms of literacy became part of what it meant to be a normal human adult in many parts of the world.

When 20th century psychologists used visually presented lists of separated words to their experimental subjects in order to study what was called "verbal memory" they were relying on a historically evolved form of cognition. A better name for what they were studying might have been "modern western literate reference memory". If and when familiarity with particular technologies of literacy becomes universal there will be a basis for a universal science of literate reference memory, a science that will also be concerned with persisting local variability.

The technology of literacy has a deep historical background. But a relatively short history does not preclude a technology from having significant psychological effects. The tightly organized human-machine systems of the modern world are one example. To study human cognitive functioning in the context of such systems we turn to experimentation. Hooking individuals up to laboratory apparatus often involves simulating specific aspects of human-machine systems that exist outside the laboratory. In these studies we do not measure timeless human attributes. What we can observe are, historically evolved, specific expressions of a potential that we can only know about insofar as it has become manifest in a particular technological context; "working memory" is a good example. It may be that this context becomes more and more familiar in more and more localities. In that case, knowledge pertaining to cognitive functioning within that context may approach, though probably never quite reach, a kind of universality.

Scientific generalizations, whether in the natural or the social sciences, typically apply only under certain conditions. In the physical sciences this is often expressed in the form of constants: a relationship holds provided other relevant conditions do not vary. In the social sciences, in a somewhat analogous way, we may recognize the existence of large scale historical formations and trends that form a necessary setting for observable empirical relationships. So there may well be aspects of modern life which form a context that must be presupposed for the generalizations of modern psychology to be validly applied. Without the appropriate socio-historical context those generalizations would lose their validity.

In my earlier historical sketch of modern psychology I suggested that the discipline's later claims to universality were closely linked to the hegemony established by the American model for the discipline. The technological slant of this model enhanced its portability, as did American power and influence. No doubt, the American role was particularly prominent, yet it is also true that there was nothing irredeemably American about many of the exported techniques; they were a product of social institutions that were being duplicated all over the planet and models of living that were spreading fast. The world-wide dissemination of psychological techniques was not an isolated event but only a relatively minor example of the massive transfer of scientific and technical artifacts in the course of the 20th century. And this transfer was very much part of broad economic, administrative, political, and social changes that affected almost everyone to some degree. With the global spread of an artifactual world exhibiting certain common features the generalized human individual who provides psychology's subject matter may actually be emerging as a product of historical circumstances.

Looping Effects of Psychology

The spread of certain cognitive technologies provides one reason for the existence of historically emerging universals. The spread of modern psychology provides another. For humans are organisms with a special feature, self-understanding or self-interpretation. These organisms not only act and react, they also have particular ways of describing and explaining what they are doing, both to others and to themselves. But they are only able to do this as members of some language community. Because historical change is an inescapable feature of language communities the categories of human self-understanding tend to change, especially over longer periods of time. In the course of the past century the vocabulary of modern psychology became an increasingly tapped source of reconstructed models of self-interpretation. Detailed studies have shown that in Western Europe and North America modern psychology had a significant share in spreading modern forms of human subjectivity (e.g. Pfister, 1997; Rose, 1996; Thomson, 2006).

Understanding human actions in psychological terms was not simply the preserve of specialists but often an important characteristic of the human subjects of psychological research. Modern psychology is most at home with individuals that provided psychology with what Graham Richards (2002) has called a "constituency", which I would see as people who are receptive to a psychological understanding of their lives.

Let me return briefly to my experience, many years ago, of acting as a missionary for modern psychology in a country, Indonesia, far from the localities of the discipline's origin. Along with others who have been in that position, I can certainly confirm Fathali Moghaddam's observation (Moghaddam & Lee (2006) that the appeal of modern psychology is usually greatest among those sections of the population that have been drawn most closely into the orbit of modernity: young, well educated urbanites, students in particular. All too often, these are the very same people who supply the subject pool for cross-cultural psychological investigations. They are the ones who understand what a psychological investigation is and what is expected of them. When they begin to play their role in the investigation they already share with the investigator a certain pre-understanding of the meaning of what is about to happen. In the words of Michael Cole (2006, p.913): "the very form of social interaction that makes it possible to assemble and instantiate psychological tasks presupposes familiarity with the forms of discourse involved and their appropriateness". And because this familiarity can now be found everywhere, it is possible to claim a world-wide validity for some psychological generalizations.

Take for example the phenomenon of what the sociologist Steven Ward (2002) calls "the psychotherapeutic self": individuals framing their understanding of their own and other people's feelings and conduct in terms of categories derived from the discourse of the helping professions. Yes, these categories vary greatly among themselves, but when you trace their historical emergence it becomes pretty clear that they all share certain features that distinguish them as a group from anything that existed previously. It is a mode of self-understanding that did not exist 150 years ago but that became quite common in those parts of the world in which psychologically informed professions exerted a significant social influence. And now that influence is spreading virtually everywhere.

A rather telling instance of this is provided by a recent South African study that considered the influence of psychologically trained counsellors on victims of apartheid terror who gave evidence before the country's Truth and Reconciliation Commission. From the counsellors these witnesses learned, not only a completely new psychological vocabulary, but new ways of presenting their distress and suffering to others. Narrating one's story was taken out of the traditional context it once had and placed in a new, psychotherapeutic, context. "Suffering becomes psychologized", as the author of the study puts it, and this process has "the potential to create new forms of subjectivity" (Louw, 2006, pp. 24/5). Analogous effects are being produced in many parts of the world, and it is not too difficult to foresee a time when a psychological mode of self-understanding will have become natural for almost everyone, as it already has in some communities.

There are other forms of psychological self-understanding than the psychotherapeutic one, for example, the experience of oneself as *calculable* (Rose, 1996). Historical studies have shown how closely the very idea that human characteristics might be calculable was linked to the development of mental testing.

In a recently published study, the historian John Carson (2007) documents the transition from a time when humans had "talents", or perhaps "talent" in the singular, to a more recent time when they had skills, aptitudes, and above all, intelligence. A key difference was that talents were not measurable, and therefore not precisely calculable. Their transformation into measurable attributes of individuals largely depended on the theory and practice of mental testing. As psychological instruments became part of the standard equipment of modern institutions, from schools to armies, the previously strange notion of human calculability became quite natural and taken for granted. Increasingly, people were not only treated as the site of calculable attributes, but also thought of themselves that way. Psychology not only assumes individuals to be the site of calculable attributes, by its practices it brings such individuals into existence in ever more locations.

Humans are often changed when they eat from the apple of psychological knowledge; they are not quite the people they would have been had they never encountered this knowledge. The reason for this lies in the reflexivity of human action: its meaning depends on the way it is represented symbolically. These representations are usually part of public discourse and are therefore subject to historical change. Currently, human action and experience are increasingly defined in terms of psychological categories and this implicates psychology in what the philosopher Ian Hacking (1995) has described as a process of "looping". As psychological knowledge and psychological interpretation achieve more social prominence they affect their potential subject matter.

I am reminded of something Theodore Porter (1995) said when tracing the history of quantification in the social sciences: "numbers work most effectively in a world they have collaborated in creating" (p. 213). Perhaps one could also say that psychology works most effectively in a world it has collaborated in creating. With the expansion of that world, psychology may be helping to bring about the universality it takes as its default assumption.

From Default to Possibility

The theme of this conference is "transdisciplinarity and internationalization", implying that there is a link between the two. I certainly believe there is, and I have implied as much throughout my talk. First, I described how internationalization was inseparable from a fundamental tension within the discipline between the universality of its knowledge claims and the local features of the sites at which this knowledge was produced. I then tried to indicate ways of escaping this polarity by exploring the idea of historically emergent human universals. I pointed to two examples of this, one based on the likelihood that the spread of psychological knowledge is having effects on its potential subject matter and the other based on the fact that human cognitive functioning often depends on technologies that change historically. In doing that I had to draw on the work of historians and sociologists as well as psychologists. But what I came up with remains a sketch, at best. To get further, it would help if people shut off by inter- and intra-disciplinary boundaries started talking to each other occasionally.

Even within psychology, there are two rather different discussions going on. On the one hand, there is the discourse of culturally sensitive cognitivists, cross-cultural psychologists, and increasingly, I should add, evolutionary psychologists who recognize the importance of cultural diversity but look for so-called "human universals" beyond that diversity. On the other hand, there is the discourse of psychologists, mainly in under-industrialized countries, whose attempts at "indigenizing" their discipline raise questions about the universality of psychology *as a discipline*. These concerns are shared by some historians of psychology, like myself, by some who are interested in the internationalization of psychology (Brock, 2006), and by some theorists of the human sciences, Nicholas Rose (1996) for example. These two discourses approach the problematic of universality in different ways. For those who look for "human universals" the *object* of scientific investigation is the site of universality, and any concern about the reach of their scientific tools is a by-product of that primary interest. For those who start with an interest in the universality of psychology as a scientific discipline, the question of human universals is secondary. Although it is entirely possible for an individual to participate in both of these discourses, there is very little cross-referencing between the two sets of literature. This is a pity, because they are surely relevant to each other. Questions about the universality of psychology as a science have implications for questions about human universals as objects of scientific investigation, and vice versa.

If universality is to be treated, not as an assumption to be quietly pushed under the table, but as a problem to be explored, we will have to overcome the perspectives of specialties. And that brings me back to my starting point. I began by suggesting that much of the discipline operates on the principle that the onus of proof is on researchers who wish to question the default assumption of universality. The suggestion with which I want to conclude is that we reverse the onus of proof. In the modern world, it would be far better for the relevance and general health of the discipline if any new findings, theories or procedures were regarded, firstly as local curiosities, and secondly as potential contributions to the psychology of modernity. In order to assess this potential, cross-national and cross-cultural studies would be expected as a matter of course, and not as a rare optional extra, as happens now. That seems to be a better way of helping the actual growth of such a science than the easy option of treating universality as a default assumption.

NOTE

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