Introduction (2010)

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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 sub-categories 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 non-

professionals. 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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