CHAPTER 10

CONCLUDING COMMENTS

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AGAINST THE GRAIN

History and psychology do not make easy bedfellows. Where undergraduate students are free to concentrate on two subjects of their choice the combination of history and psychology is rarely encountered. Where institutions limit students' choices this combination often becomes a curricular impossibility. But here pedagogy and popular stereotypes merely reflect the fact that in modern times the disciplines of history and psychology have tended to define themselves against each other. History has been closely identified with narrativity and the rich contextualization of particular events whereas psychology has strenuously sought the status of a natural science producing universalistic generalizations that would apply across all times and all places.

What the contributions to this volume all have in common is their transgression against this rigid boundary. In that sense they all go against the grain of prevailing disciplinary orthodoxies. In different ways they all temporalize, and therefore historicize, the subject of psychology. Historical considerations become critical for gaining an understanding of what modern psychology is all about. This holds for those contributors, Bayer and Walsh-Bowers, whose gaze is primarily turned to the discipline's future as much as for those who are more directly concerned with its past. There is implicit agreement that reflection on the status of the discipline must start with the recognition that psychology and its subject matter are situated in historical time.

Although this shared insight involves an assault on the wall that separates psychology and history, this is by no means an "interdisciplinary" enterprise. There are no historians among the contributors, they are all psychologists. Their focus is on disciplinary history, which, in several cases (especially Bayer, Staeuble, and van Hoorn), takes the form of disputing its boundaries. With one exception (Stam) these authors are not concerned with history as such, the question is what historical studies can do for psychology.

For the contributors to this book that question has become quite explicit. It was not always so. Early examples of disciplinary history were hardly notable for their historiographic interest or sophistication. However, this did not preclude an implicit commitment to a particular agenda. When psychologists turned to the history of their discipline certain themes always seemed to emerge. One theme that appeared early was the special position of experimental psychology for the development of the discipline. This theme famously informed the field's most successful text, E.G. Boring's (1929) *History of experimental psychology*, though in a less strident form it is already present in Klemm (1914). It was a theme that lived very comfortably with psychology's aspirations as a natural science. It also provided a convenient way of dealing with the tensions that had arisen in the wake of psychology's premature institutionalization as a discipline, when it actually lacked a core and was simply a loose assembly of "schools" and special interest areas. The experimental method, as understood by the psychologists of the time (see Winston, this volume), would supply the missing core of psychology. A history of the discipline which privileged its experimental component would therefore supply a sense of unity that counteracted the centrifugal influence of diverse goals and practices. This proved to be a highly acceptable recipe within the discipline so that Boring's text became the fountainhead for many subsequent, mostly American, textbooks prescribed for several generations of students who would find in them an intimation of disciplinary unity that they would not find in the rest of their psychology curriculum.

A second theme that emerged in earlier disciplinary histories was the claim that the subject matter and the concerns of modern psychology were of ancient origin, a reassuring thought for a parvenu among the sciences. In its most explicit form, exemplified by the work of R.I. Watson (1971), this claim led to what one historian characterized as "ahistorical history" (Ash, 1983). On this view the fundamental problems of psychology had always been the same—problems of 'personality', for example, were already an issue in Homer's time—though we now have better methods of dealing with them. Of course, this approach was not only compatible with the privileging of contemporary methodology, it also converged beautifully with the presuppositions of a discipline that was dedicated to the production of universally valid, ahistorical generalizations about human nature.

These prominent tendencies within the field of psychological disciplinary history presented striking examples of what has long been referred to as *justificationist* history (Agassi, 1963; Young, 1965). This meant providing a historical justification for currently favored presuppositions, biases, practices, conceptualizations, and interests within the discipline. Historical scholarship came a distant second to the primary function of the field which was pedagogical, imparting an appropriate group image to aspirant members of the discipline.

Though the scholarly contributions of the field may have been insignificant the sheer size of its pedagogical enterprise, especially in North America, led to the formation of interest groups that began as little more than hobby groups but grew into a recognizable sub-discipline with its own journals, associations, and scholarly networks. This process was certainly fostered by the inhospitable climate within the discipline for any combination of a historical perspective with more conventional psychological interests and practices. Any tolerance for history was limited to the pedagogical services that it was expected to supply. Those whose historical interests went any further were obliged to seek out the company of their own kind. History of psychology's emergence as a sub-discipline of psychology was also fostered by the widespread lack of interest in the topic among those whose disciplinary affiliation was with history. This was not the kind of topic to which historians had traditionally dedicated themselves.

Perhaps it was inevitable that the formation of even a loosely institutionalized sub-discipline should prove favorable to the development of a certain sense of autonomy among its members, especially as it often attracted individuals who were not quite content to plough the furrows prescribed by conventional disciplinary norms. That sense of autonomy enabled an increasing number of those affiliated with the sub-discipline to reject the traditional servant role of the disciplinary historian and to turn away from the discourse of justificationist history. What was also important was that disciplinary historians were potentially far more exposed to developments in the humanities and the social sciences than their colleagues who were protected from such influences by the rigid boundaries that defined the discipline of psychology in general. At any rate, a different, more critical, kind of disciplinary history did emerge during the last quarter of the 20th century.

The contributions collected in this volume are all representative of this trend. They have in common a historicizing of the subject matter of psychology and an emphasis on the role of culture and society in the formation of both psychology and the objects of its attention. It is perhaps surprising that such an orientation should exist within a discipline notable for its pervasive ahistoricism and aculturalism. But in spite of undeniable pressures for disciplinary isolation from the social sciences and humanities the barriers that psychology has erected around itself are not impermeable. There are weaknesses in the rigidity of these disciplinary boundaries for which the present volume provides some telling examples. Some of these weaknesses are associated with local variations in the cultural geography of the discipline. Thus, as Johann Louw's chapter indicates, the interdisciplinarity of my own tastes was surely assisted by a quasi-colonial environment in which disciplinary ties were relatively weak and the salience of socio-political factors incredibly strong. However, one does not need to be existentially thrown into such a situation, as I was; one can choose to abandon the blinkers of a first world outlook, as Irmingard Staeuble has done.

The relative prominence of Dutch and Canadian contributions to this field (not only in this volume but also more generally) seems to confirm the importance of marginality for its practitioners. In the American heartland of disciplinary psychology isolationism may be supreme but psychologists in the Netherlands have long had to come to terms with often contradictory influences from the major centers, especially Germany and the US (Dehue, 1995; van Strien, 1988). This was often conducive to a more flexible perspective and a critical sensitivity to fundamental issues. Of course I am not suggesting that all Dutch psychologists were strangers to dogmatism and superficiality, but over the years there does seem to have been more mobility of orientation and openness of viewpoint than in the culturally more parochial centers of psychological inquiry. As for Canada, its marginal position with respect to the US hardly needs emphasizing. Disciplinary integration with American psychology has gone a long way but is far from complete. Openness to European and other influences has never been abandoned, not least because of a deliberate rejection of the culture of the melting pot.

Although the creation of a core disciplinary identity has long been a prominent theme in psychology's modern history there has also been a strong push towards disciplinary colonization of more and more areas of human activity. The constant multiplication of "divisions" or special interest groups within professional institutions like the American Psychological Association, as well as the phenomenal growth in the number of new specialist journals, provide evidence for this process. In many cases new areas have been effectively domesticated in terms of the dominant disciplinary ethos. But in other cases disciplinary colonization has not been altogether successful. One such case is that of community psychology, an area represented by one of the contributors to this volume (Walsh-Bowers). Another area, affecting much larger numbers, is that of feminist psychology (Bayer, this volume). It is to be expected that these and other areas which are marginal from the perspective of the core disciplinary ethos will provide a more hospitable climate in which a socio-historical approach is more likely to gain a foothold (Danziger, 1994).

The contributors to this volume have addressed many issues that are relevant to the future development of the field. However, it is hardly possible to comment on all of these issues in this concluding chapter without lapsing into superficiality. In what follows, I have therefore selected some of the more contentious topics for further discussion, because it is these, rather than the issues on which there is broad agreement, which provide the best opportunity for a clarification of my own position within the field as a whole.

THE PERILS OF HISTORY

A common interest in the project of historicizing the subject matter of psychology does not exclude deep disagreements about what exactly such a project entails. The present volume presents a wide spectrum of views that range from the traditionalist intellectual history of van Rappard to the sociologically oriented "history of the present" that can be found in the chapters of Staeuble, van Strien and Walsh-Bowers. To my mind, what is most significant about this variety of voices is not the dissonance that surfaces from time to time but the fact that psychologists should be discussing such issues at all. A few years ago the critical mass for launching an international project along these lines would not have existed. With some highly localized and mutually isolated exceptions, the disciplinary boundaries protecting psychology's principled ahistoricism would have been too strong, the interest in historical issues too weak, the knowledge of the relevant extra-disciplinary literature too undeveloped. My own work in this field would not have progressed as it did without the emergence of a potential community of interlocutors among some of my psychologist colleagues.

Differences of approach are of course to be expected among an international group such as the contributors to this volume. The development of a historically and socio-culturally oriented approach to the discipline of psychology did not follow the same course in Europe and North America. In Europe the ahistoricism of the discipline was never as pervasive, and the aversion to history never as much in tune with the broader culture, as in North America. When van Rappard informs us that psychologists *do* read the works of the founders of their discipline I am reminded once again that he and I live in very different disciplinary sub-cultures.

Moreover, within Europe, a historical psychology that rejected the investigation of psychological objects outside of time and of history, was able to maintain a foothold in the discipline (examples are Barbu, 1960; Jüttemann, 1986; Peeters, 1996; Scribner, 1985; and Gergen & Gergen, 1984, as the North American exception). Sometimes this led to a historicizing of psychology's disciplinary project (e.g. Jaeger and Staeuble, 1978). As a result, there has been an element of continuity in the field which is lacking in the relevant Anglo-American literature.

That continuity has some advantages, but it has also entailed a certain traditionalism that limits the potential critical impact of this work on other segments of the discipline. One important example of this is the relative neglect of more recent developments in the sociology and historiography of science, though van Strien's chapter in the present volume shows that this may be changing. Those developments have opened up perspectives on the microstructure of scientific investigation which can mediate between the broad sweep of the older histories and the narrow focus of "normal science". As long as this microstructure remained invisible to both historians and scientific practitioners the work of each of them could easily be perceived as irrelevant to the work of the other.

After the second World War a part of European psychology adopted American models and sometimes imported the corresponding historical narrative along with the practices it justified. But indigenous approaches survived and developed their own counter-narratives. Two of these are represented in the present volume (van Hoorn and van Rappard). They are based on venerable models of cultural and intellectual history respectively. Those approaches may have their uses when it comes to exploring theoretical structures (van Rappard) or pop psychology (van Hoorn), but they offer no means for coming to grips with core problems of psychology's modern history, especially, its successful constitution as a discipline on the basis of very specific scientific and discursive practices. Hence their emphases provide the precise counterpart to the scientistic focus of Boring's widely adopted historiography. Boring had privileged scientific methods and findings while avoiding in depth analysis of theoretical systems and offering only an empty version of the *Zeitgeist* concept as an apology for the absence of socio-cultural contextualization. In contrast, van Rappard's style of history favors theory and van Hoorn's favors cultural trends of the broadest kind. Scientific experimentation, on the other hand, is either dismissed out of hand or not considered worth the same attention to primary sources that theoretical traditions deserve. This can lead to historical assessments whose evidential basis is less than adequate.

Curiously, these cavalier attitudes to experimental practices have an effect that is analogous to the effect Boring-style historiography achieved by its exaltation of experimentation. Whether these practices are not considered worthy of serious historical attention or whether they are regarded as representing the conquest of reality by rationality, the effect is much the same: they are placed beyond critical historical examination. A disciplinary history pursued in this spirit has little to say to the large and influential group of psychologists and lay persons who believe that psychology's scientific status and its claims to a unique expertise depend on its reliance on experimental and quantitative methods. For this group, these methods represent the core of disciplinary endeavor and often of a disciplinary identity defined, not as "psychologist", but as "experimental psychologist". Historicizing the practices that lie at the heart of this disciplinary reality must surely remain a crucial part of the wider project directed at historicizing the subject matter of psychology.

Wittgenstein (1968, p. 232) famously observed that, in psychology, problem and method pass one another by. In the traditional historiography of psychology, whether in its "American" or its "European" form, it is history and method that pass one another by. In the "American" form this effect was achieved by privileging experimental practices and elevating them above history; in the "European" form the same effect is produced by devaluing experimentation to a historically unimportant position, as though psychology were essentially a theoretical affair or simply a cultural phenomenon.

Different ways of historicizing the subject matter of psychology are clearly linked to different ways of interpreting the relationship of psychologist historians to the discipline that is at once the object of their investigations and their institutional home. For historians affiliated with the discipline of history the issue hardly arises: they simply do the job they were trained to do, expecting to find their primary audience among fellow historians, not among the members of another discipline. For those working in interdisciplinary institutional contexts the question of intra-disciplinary effects may also appear somewhat redundant. But psychologist historians that retain, and expect to continue, their affiliation with the discipline of psychology can rarely avoid this question. The way they resolve it will show in their manner of historicizing the subject matter of the discipline.

With one exception (van Hoorn), the contributors to this volume appear to be in agreement that the rationale for the work of psychologist historians is to be found in its contribution to psychology. In my view, historical exercises that do not do this should be left to historians. Psychologists' ventures into purely historical issues are apt to be unsatisfactory, both from a psychological and a historical point of view, though they may not lack a certain pop appeal. More appropriately, psychologist historians will rely on the work of professional historians for the broader contextualization of the issues that are of primary concern to them. What sorts of issues are these?

One kind of issue arises out of the existence of disciplinary mythologies that often play an important role in the self-understanding of members of the discipline. Disciplinization is not just a matter of a more or less rational division of labor—it also affects peoples' careers, life chances, and sense of self worth. Identification with the progress of a discipline can supply the missing meaning for work that would otherwise seem trivial. In the modern world, disciplines provide important sources of identity, and, like other sources of identity—nations, religions, ethnic groups, etc.—they do this partly through the medium of myth. Such myths often have a significant historical component, including so-called origin myths (Samelson, 1974) that provide recent disciplinary ideologies with a worthy past. Disciplinary historians will inevitably be drawn into debates around the historical components of disciplinary ideologies, either justifying a received version or providing grounds for questioning it.

As is well known, a particular historical narrative regarding the origins of experimental psychology formed part of the professional self-understanding of American psychologists for most of the 20th century. Although elements of this narrative existed quite early, it was given its canonical form in the work of E.G. Boring and then repeated in numerous textbooks that were required reading for aspiring psychologists. Very briefly, this narrative established a historical pedigree for the particular version of experimental psychology that had achieved ascendancy in America (see Winston, this volume). The figure of Wilhelm Wundt necessarily played a significant role in this account, but it was a figure that anyone who had actually studied his work would have trouble recognizing. Clearly, this presented a challenge for disciplinary history (Blumenthal, 1975, 1977), and my early work in this field was very much taken up with meeting that challenge. The resources of intellectual history were often quite adequate for the purpose.

From that point of view it made sense to pose historical questions in terms of "intellectual traditions", as I did in an early paper that situated Wundt in terms of "two traditions of psychology" (Danziger, 1980), the Lockean and the Leibnizian, a distinction that would have been familiar to many psychologists at the time because it had been used by Gordon Allport (1955). Strong echoes of the notion of a "Leibnizian tradition" can still be detected in van Rappard's (this volume) notion of an "activity tradition". It is an approach that certainly has its uses. Pedagogically it helps students to see beyond the particularities of this or that theory and to pick out underlying commonalities. As van Rappard emphasizes, it also provides a medium that enables the "insider" historians to communicate with their disciplinary colleagues in a non-subversive way.

Nevertheless, when a revision of the book in which my paper had first appeared was proposed two decades later I decided, with some relief, to drop this chapter altogether and replace it with an altogether different chapter (Danziger, 2001a). What were my reasons? Quite soon after the appearance of the first chapter I came to recognize the severe limitations of a historical approach based on the identification of "intellectual traditions". Logically, such an approach has much in common with psychological explanations of human behavior in terms of instincts, needs, or drives identified by such labels as "aggressive", "acquisitive", "submissive", "gregarious", and so on. These are terms used to *classify* behavior into certain categories, but their reification in the form of hypothetical entities provides at best a spurious form of *explanation*. Similarly, though it may be useful for descriptive purposes to note the way in which certain concepts resemble one another, the reification of those resemblances as an intellectual tradition does not in itself explain anything. Of course, if the hypothetical "tradition" leads to detailed research that establishes the existence of significant historical links between the members of that tradition, the hypothesis will at least have served a useful heuristic function. Unfortunately, in the field of historical psychology, the reification of "traditions", "mindscapes", "mentalities" and the like, has too often proved to be a source of pseudo-explanations than of new historical knowledge.

INVESTIGATIVE PRACTICES

So far, I have identified the critique of justificationist historical narratives as a task for which disciplinary historians may be particularly well placed. But this is a task with limited goals, limited means, and limited relevance. In pursuing this task historians apply themselves to subject matter that has already been historicized. They are presented with a much more challenging and potentially more important task in the form of historicizing the current practices of the discipline. This is the task on which most of the contributions to this volume converge and which has also been the main focus of my own work.

When I first began to work in the field of disciplinary history I accepted uncritically the conventional categories used within the discipline for the self-description of its activities. My background in the classical European sociology of knowledge (Louw, this volume), had predisposed me to take an interest in the social contextualization of disciplinary activities, but it did not occur to me till later that the very description, the characterization, the bringing into focus of those activities was already problematic, so that their appropriate contextualization would depend on how one had conceived of them in the first place.

Activities that generate psychological knowledge in a disciplinary and institutional context result in certain products that are clearly distinguished from one another by strict social conventions. Some of these products are classified as "theories", others as "observations", others still as "mental tests", or "experiments". Those products are marketable in psychological journals or in other forms and yield credit that is convertible into career advancement and reputation (Bourdieu, 1988). Psychologists, like their colleagues in other fields, use the social distinctions among these *products* as a basis for describing and understanding their *actions* in generating them. So they see themselves as engaged in observing, theorizing, experimenting, test construction, and so on. That is fine for functioning adequately *within* a particular regulatory social framework. But it is not fine if one stops taking this framework for granted, steps outside it, and asks how such an activity-framework complex could come to be. We know it is not an eternal and unalterable necessity of human nature, so we, as disciplinary historians, must inquire into the conditions for its existence. For this purpose the conventional categories of disciplinary self-understanding are inadequate—they are part of the problem, not the solution.

Smooth day to day activity within the taken for granted disciplinary framework relies on a pattern of discourse that makes this framework invisible (Steele & Morawski, 2002), whereas, if the historicity of this framework is to be investigated, it must first be made visible. This requires a different kind of discourse. First of all, the social embeddedness of all such activities as theory construction, experimentation, quantification, and so on, must be recognized. These are all *social practices*, though when defined by their products, they are other things as well. The historicity of the disciplinary framework can be made visible by studying the history of the relevant social practices. I began to do this in the nineteen eighties with studies of the social practices of experimentation, quantification, data analysis, and so forth (Danziger, 1985a, 1987a, 1990a).

In studying the historical trajectory of the psychological experiment as a social institution I was trying to fill a historiographic void that had been created by the way questions of methodology were commonly treated within the discipline. Looking at textbook treatments of methodology one would never guess that a technique like experimentation had a history. Yes, the adoption of experimentation had occurred at a certain point in history, but there was no sense of this "experimentation" being itself a historical entity that changed quite significantly over the years. In part of course this ahistorical view of experimentation resulted from the didactic, generally unscholarly, treatment of the topic in terms of rules engraved in stone. Harping on the fact that these rules were recent human inventions, replacing other such rules, would only have sown confusion and undermined the faith that methodological instruction was meant to engender.

The traditional presentation of experimentation was not only ahistorical, it was also asocial. There was some recognition of social *psychological* factors as a source of experimental "artifacts", but blindness to the fact that experimental situations were inherently social in character and that their products were social products. Recognizing *that* would have cast grave doubts on the universalistic knowledge claims commonly made on the basis of experimental data gathered under quite specific local conditions.

Another aspect of the prevailing professional ideology which appeared highly problematic was the implication, strongly suggested by methodological teaching and practice, that there was only one kind of knowledge compatible with psychology's prized scientific status. This was the kind of knowledge that was tied to the employment of the techniques of experimental design and statistical analysis which had been widely adopted around the middle of the 20th century. Even a cursory examination of psychology's modern history shows, however, that these beliefs were a later development and were preceded by quite different conceptions regarding scientific psychological knowledge and how it was to be achieved. It therefore seemed appropriate to undertake an analysis of the major variants of psychological experimentalism that had emerged during the foundational period of the discipline's history.

Such an analysis would focus on experimental situations as sites for generating psychological knowledge. Different forms of this knowledge would be produced by different arrangements within experimental situations. As one is dealing with social situations, these arrangements would be social in character, affecting the normed relationship between the participants. These socially structured experimental sites are a sort of workshop specifically designed to come up with a certain product, namely, psychological knowledge of a particular kind. So there are two sides to experiments as social institutions—there is their internal social structure involving a certain distribution of power and tasks, and there is their social function as sites for the making of a product that is accorded some value outside the laboratory.

One set of historical influences that affected changes in experimental practices certainly emanated from psychological work in non-academic settings, so-called "applied" or "practical" work (Danziger, 1987b). In that sense, van Hoorn (this volume) is correct in emphasizing the role played by "fields of psychological practice". But this understanding of "practice" harks back to a time when research in academic institutions was not regarded as an example of social practice. Historical and sociological studies of science (see Golinski, 1998 for an overview) have long abandoned this usage for one which depends on the recognition that the activities which create science are no less examples of social practice than the activities which create more efficient work environments or less repressed individuals.

My studies of experimental practice arose out of an interest in the microsociology of scientific knowledge, but they also attempted to answer purely historical questions regarding the emergence of certain patterns of psychological investigation in the latter part of the 19th century. This accounts for the labels I adopted to identify various kinds of experimentation in the early days of modern psychology. They were based on the places and the figures that had been historically most closely associated with the emergence of these paradigmatic patterns of investigation, Leipzig and Paris as well as Galton and Wundt. My analysis was also limited to a particular time period that ended in the middle of the 20th century. Extending this period, and switching from historical origins to other criteria, Pieter van Strien has emerged with new labels for various identifiable patterns of experimentation. This strikes me as a sensible development. Any expansion of the scope of studies in this field to include new places and periods, as well as new perspectives, is likely to be reflected in new classifications and a new nomenclature.

Thus, van Strien proposes a fundamental distinction, based on strictly *methodological* criteria, between a "natural science model" and a "differential model" of psychological investigation. However, there is more than a difference of terminology behind the fact that he consistently refers to "methodology" where I would prefer to look at "investigative practice". My historical studies of experimental situations had their origin, at least in part, in my dissatisfaction with the disciplinary category of "methodology" that depended on the isolation of certain formal relationships from the social practices that constituted investigative situations. The category "investigative practice" was meant to encompass the social as well as the methodological aspect of these situations. The criteria for distinguishing between different sorts of investigative situations would then be social and historical as well as "methodological". Although van Strien recognizes the existence of social factors, it seems to me that his chapter presents the history of psychological investigation largely in terms of purely methodological changes.

Van Strien's chapter makes an important contribution in raising the question of historical continuity as it pertains to some of the more recent investigative practices employed in cognitive psychology. He regards the method of "protocol analysis", used to generate data applied in the construction of various AI programs and expert systems, as a continuation, or at least a revival, of the "systematic introspection" practiced by the Würzburg School at the beginning of the 20th century. This is surprising, because it runs counter to the investigators' own account of the matter (Ericsson & Crutcher, 1991). Drawing a sharp distinction between "earlier introspective techniques" and "current verbal report techniques" used in protocol analysis, they draw attention to the fact that J.B. Watson, the founder of behaviourism, and not any Würzburger, "was the first investigator to publish an analysis of a think-aloud protocol" (p. 63). It was behaviorism, they claim, which was responsible for two innovations that made it possible "for psychology to achieve status as a science" (p. 62).

The first of these innovations was the use of subjects untrained in introspective procedures, the second was "the introduction of methods for collecting observations in which trust was not an issue" (p. 63). The point here was that subjects' responses would be limited to the kind of verbalization whose reference could be checked against non-verbal task performance so that one did not have to take the subject's word for anything—trust was not an issue. Clearly, the practice of systematic introspectionism contravened both these requirements. It depended on sophisticated, not naïve, subjects, and it was based on trust in the validity of subjects' introspective reports on their conscious experience.

Both of these distinguishing features, the training of subjects and the trust placed in their reports, are social features. In other words, they involve differences in investigative practice, not merely differences in formal "methodology". Although there are no features of investigative situations that are not "social" in some sense, there are certain core social features which play a crucial role in shaping the overall pattern. In any investigative situation one will find a complex of features, some relatively superficial, such as the number of subjects, others of constitutive significance, such as the social relationship of investigators to their subjects and to the potential consumers of their research product.

For example, the question of whether there is symmetry (equality, exchange of roles) or asymmetry in the relationship of experimenters and subjects is critically linked to questions about what exactly is being studied in the experiments. In asymmetrically constituted "verbal report" studies the subject's activity is analysed in terms of task requirements and the nature of the subject's cognitive processes is determined inferentially by the investigator. This has the far reaching consequence that *descriptions* of knowledge and information are equated with knowledge and information (see Clancey, 1997). By contrast, in symmetrically constituted classical introspection studies the imposed task and the subject's activity were carefully separated and the subject's report was essentially incorrigible.

Van Strien has pioneered a potentially very fruitful extension of the analysis of investigative practice into the latter part of the 20th century. However, the question of whether the dominant cognitivism of that period entailed significant changes in the investigative practices of psychologists remains open. The next step in answering it will require an extension of the analysis beyond the purely "methodological".

PSYCHOLOGICAL OBJECTS

Several contributors to this volume take up the question of how the history of psychology relates to psychological theory. Two mutually incompatible viewpoints emerge very clearly. Van Rappard argues for a “history of psychological thinking” that is to be separate from the (social) history of the field which is to be left to professional historians. On this view, theorizing is clearly an affair of pure thought whose products are historically autonomous and whose relevance is therefore permanent: the theories of the past are participants in current debates. In contrast to this wonderfully Platonic view, Stam points out that the activity of theorizing is always socially contextualized so that its creations are essentially historical products. As a result, history and theory are deeply interwoven.

My own sympathy for the latter view arose directly out of my background in the sociology of knowledge. But after I began systematically analyzing psychological research publications, starting with the *Philosophische Studien*, I became increasingly aware of the role played by psychologists’ investigative practices in mediating the relationship between the broader social context and their own theoretical constructions. The major change that had occurred when psychology became an experimental discipline working with quantitative data was that theories now had to be justified in the court of “scientific” empirical investigation and not by an appeal to philosophical argument or popular intuition. But, to continue the analogy, the judges in this court were biased from the start, because the choice of “methods”, of investigative practices, had been largely determined by pre-existing assumptions that were congruent with the theories that were to be tested by means of these methods (Danziger, 1988). When that congruence did not exist, for example in attempts to test Freudian theories experimentally, it was restored by transforming the original theory into one whose presuppositions matched those of the methodology (Danziger, 1985b).

In day to day disciplinary practice theories are not deployed independently of methodological questions and empirical observations. That happens only in texts devoted to “Theory” with a capital T. Outside this rarefied atmosphere theories only exist as part of a complex of constructive activities that includes the planned evocation of phenomena, the intentional suppression of others, the focusing of attention on specific features, the following of particular rules of procedure and of interpretation. Theories in isolation are a creation of metatheory (Danziger, 1993), they are posited as objects of attention for a particular specialty in the academic division of labor.

Outside this specialty, theories have to be studied as part of a complex of social practices that is of course subject to historical change. That complex includes the so-called “data” that are the product of the application of these practices *in* the real world, not *to* the world. This distinction is important because it is not the case that a solitary investigator, occupying a god-like position outside the world, turns his or her gaze on some phenomenon in this world. What happens in psychological practice is that investigators, who are very much *in* the world, apply a complex apparatus of presuppositions, theoretical models, instruments, labels, interpretations, social influence, coercion, numerical and practical skills, etc., to some other part of the world with which they are in interaction. That application has certain results; it leaves the world in a slightly different state from its state before the psychologists' intervention. Some of these results take the form of empirical data categorized and described in a specific way; other results would show up in the form of certain social relationships peculiar to the investigative context (expressed in the experimental subject role, for example); yet other results might take the form of purely discursive structures and representations.

These changes are not random. When they occur on a large scale, and over longer periods of time, definite patterns become apparent, and these patterns constitute a large part of the subject matter appropriate for a disciplinary history. What shall we call these things that are the product of psychologists' activity *in* the world, these things that would not have existed in quite this way but for psychology's apparatus of intervention? I believe the most appropriate name for them is "psychological objects". They are the objects at which the actions of psychologists qua psychologists are directed: experimental subjects, mental tests, quantitative empirical data, and so forth. But, because these actions are not passive states but interventions in the world, the objects at which they are directed are also in a significant sense their *products*.

Is this "social constructionism"? Certainly, in the sense that "construction" refers to a generative metaphor (Danziger, 1990b) I have found useful, but certainly not in the sense of a general belief that everything that exists is the product of human construction (Hacking, 1999). The "ism" in constructionism is extremely hard to pin down. At one time I agreed to review a series of about a dozen books on "social construction" because I hoped it would help me to get to the core of this slippery term. I failed because I suspect this category has no core, no prototypical member, only a huge variety of self-proclaimed instances, some of which even lack a family resemblance (Danziger, 1997b). So I doubt that anything is gained by the application of this category label. It seems to me more fruitful to ask what the metaphor of construction can do for the advancement of historical and theoretical studies in psychology.

It is certainly useful in historicizing the conceptual apparatus of the discipline. One of the functions that this apparatus performs is the labeling and identifying of psychological phenomena. These phenomena do not have labels attached to them by nature but are given an identity when psychologists (or lay people) subsume them under one or other psychological category. Those categories are of vital importance in the construction of psychological objects because they define what psychologists consider themselves to be investigating, for example, personality, motivation, or memory. They are also pre-theoretical in that psychological theories are typically theories about objects that have already been given a certain identity by the category label under which they are known. But these category labels carry a great deal of implicit theoretical baggage because they come with rich connotations that they have acquired through their everyday usage. Cultural learning provides people, including psychologists, with an intuitive understanding of what it means to have a personality, an emotion, a motive, or a memory. The constructions that are commonly thought of as psychological theories are superimposed on this understanding and generally do not place it in doubt. A few theories have done so however, and they may well be the only psychological theories worth having.

Because psychological categories are cultural products they all have a history, and one of the tasks of the disciplinary historians is the explication of that history (Danziger, 1997a, 2001a). In my opinion the mere demonstration that categories of psychological understanding have a history already constitutes a contribution to psychology, and not simply to history, because it suggests lines of psychological research that differ significantly from those based on the common interpretation that these categories are accurate reflections of the deep structure of an ahistorical human nature. Research on ‘personality’ by means of verbal ratings, for example, would then be recognized as a study of a certain semantic space rather than as an investigation of a bit of reality that has the same sort of objectivity as a chemical compound (cf. Semin, 1990).

But beyond this, historical study of psychological categories can also make more specific contributions to psychological theory. For example, it can help to elucidate the foundational role of root metaphors in theoretical constructions that are meant to account for a set of phenomena identified by a particular category label (Danziger, 2002b).

Many of the categories with which contemporary psychology operates are in fact categories whose modern form is closely tied up with the emergence of psychology as a scientific discipline. The more recent history of categories like “behavior”, “learning”, or “intelligence” can hardly be separated from the history of modern psychology and its role in society. The relationship between common understandings and scientific usage is a two way street. Psychologists soon ceased to be the passive recipients of folk knowledge and became an increasingly powerful influence on the way people explained their experiences and their actions to themselves (Richards, 2002a; Rose, 1996). In that sense, the twentieth century was indeed the century of psychology. This “looping effect” (Hacking, 1995b) constitutes a further dimension in the construction of psychological objects. Because of the social impact that the past activity of psychologists has had, some of the objects they seek to investigate do not need to be constructed afresh in the laboratory or the therapy room, they walk in ready made, displayed as traits, states and disorders of various kinds. That too provides a useful area of historical investigation (Young, 1995).

Studies along these lines are very much concerned with recent history. I see them as examples of what, in Michel Foucault’s felicitous phrase, is now often referred to as “history of the present” which forms the antipole to the historicists’ construct of “presentism”. Whereas presentism referred to the projection of modern forms of understanding onto people, events, and ideas that flourished long ago, the history of the present problematizes the taken for granted quality of modern forms of understanding and studies their historicity. This is an undertaking to which psychologist historians can be expected to make a contribution. It will not be quite the same sort of contribution as that of cultural historians, sociologists, or anthropologists who choose to participate in this essentially interdisciplinary undertaking. But it is an undertaking in which psychologist historians have a role.

A HISTORICAL PSYCHOLOGY?

Everything I have said so far about the role of historically oriented psychologists limits their work to that relatively recent period of history in which the discipline of psychology existed or was at least on the historical horizon. However, one of the contributors to this volume (van Hoorn) argues strongly against this limit and another (van Rappard) adopts a historical approach (in terms of intellectual traditions) for which no such limit exists. I believe this is a genuine issue which needs to be addressed.

Within psychology, opposition to the very notion of a historical psychology has taken two very different forms. The first kind of opposition arises out of the fact that in their everyday research and theory testing most psychologists proceed as though their subject matter belonged to a historically constant human nature. In their world there is simply no room for a historical psychology. However, belief in the trans-historical validity of experimentally produced psychological knowledge will remain a matter of faith, not of science, until its reasonableness can be empirically demonstrated. But any such demonstration will require considerable reliance on historical evidence. 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.

A variant of this position is based on simple minded scientism, the notion that the only way to obtain valid knowledge is by means of quantitative and experimental procedures. Historical psychology is largely unable to make use of such procedures and is therefore outside the pale. Virtually the whole of the humanities and parts of the social sciences will of course be rejected on the same grounds. Again, this is essentially a confession of faith that is more likely to produce puzzlement than assent among those outside the faith.

There is however a much more telling argument against the backward extension of the history of psychology into pre-disciplinary times so as to produce a historical psychology. This argument is based on the concern that there are no acceptable criteria which might define the subject matter of such a field (Richards, 1987; Smith, 1988). Those who have these concerns are in no doubt about the deep historicity of matters psychological, but they are then confronted by 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. From a historicist perspective 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.

I regard these as very telling arguments, but do they 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? I believe that if one follows a few general guidelines some promising possibilities do open up in this field. In the first place, one must not expect historical psychology to be a unified field: no "grand narratives" purporting to describe the historical evolution of the human mind but rather a collection of studies tracing the historical background of specific psychological objects in particular contexts. The already existing studies of the history of emotions provide some nice examples (Stearns & Stearns, 1988; Harré, 1987). 'Consciousness' might well be a candidate for similar investigations that would begin with its emergence as a discursive object in the 17th century and trace its vicissitudes up to its transformation into an object for experimental intervention in the 19th century.

But restriction to the history of specific objects only constitutes a first step in reconstituting historical psychology as a field of scholarship rather than hazy conjecture. In the past, what proved most damaging to the reputation of the field was the fact that some of its pioneers displayed a rather cavalier attitude to questions of evidence, relying excessively on the intuitive interpretation of arbitrarily selected examples. This was usually linked to a clinical approach to history, more often found among psychiatrists than psychologists, which often claimed to have direct access to the subjectivity of persons living centuries before our time (e.g. van den Berg, 1961). Its disregard for the ordinary precautions of both historical scholarship and clinical practice did not endear the work of these historical psychiatrists to specialists in those areas, though it was popular among general readers. Today, the certainties that this approach promised seem quaint, essentially because they were undermined by developments that fostered a more skeptical, a more critical, approach to the problems of historical psychology.

Among these developments one is of particular importance; it has to do with the role of language and other semiotic signs. Claims of direct intuitive access to the subjective experience of people who lived in bygone times appear plausible only as long as one treats language and other semiotic media as completely transparent. All that is left of those times are linguistic traces, symbols and artifacts, not a single living person. That has serious consequences for the kinds of conclusions we can draw. We cannot confirm our understanding of their world by questioning them or by arriving at a mutually agreed interpretation. We are trapped in the semiotic, and particularly the textual, evidence. What we have are their descriptions of their lives and the artifacts they produced.

This leads to the recently much discussed problem of historical rediscription (Hacking, 1995a; Haddock, 2002; Sharrock and Leudar, 2002), because, as we go back in time, our descriptions and labels for actions, experiences and artifacts will differ more and more profoundly from the accounts and representations a person living at that time would have produced. We therefore have to distinguish between *our* accounts, that we might consider to be true *of* past times, and *their* accounts, which might have been true *in* those times. If we substitute the one for the other we end up with those pseudo-insights that have given historical psychology a bad name. Such a substitution is almost bound to happen if we treat discourse as transparent and pretend to have direct access to past subjectivities, mentalities, etc.

A major part of historical psychology's empirical basis comes in the form of texts, more often than not published texts. But, whether public or private, the contents of texts are circumscribed by historically variable norms and conventions of discursive form. As long as we restrict ourselves to the discursive practices involved in the production of the textual evidence we are on relatively firm ground. But I am afraid that any historical psychology which avoids this ground in favor of claims about the feelings or the private experience of an age will not be taken seriously. What texts do offer the historical psychologist are discursive objects embedded in a discursive domain. The history of these discursive realities is a history of objects, not a history of subjects.

The same applies to the non-linguistic evidence, symbolic and artifactual, that a historical psychology would have to work with. There are social and material practices that cannot simply be assimilated to the category of discourse. Therefore, in my most recent work on the history of "memory" as a psychological object, I have considered it necessary to examine the role of the social practices known as "mnemonics" and material practices used in the apparatus of external memory (Danziger, 2002b) as well as strictly discursive practices, such as the deployment of metaphor (Danziger, 2002a).

Because of the Cartesian tradition of treating psychological objects as either natural objects or as subjectivities, their history as discursive objects has been neglected. That leaves historians with a twofold critical task. On the one hand, they need to investigate the role of material and discursive practices in conferring whatever historical persistence psychological objects possess. 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.

LOOKING AHEAD

Charting the vicissitudes of investigative situations in terms of their social structure and function certainly plugs a big hole in the traditional historiography of psychology. However, this field of study is not simply of historical interest. As Richard Walsh-Bowers shows, the social contextual approach to the conduct of psychological research has important implications for current issues and future trends. He devotes particular attention to the topic of research ethics, suggesting that "it exists to legitimize conventional methodological practices" (p. 104). Not content with merely noting this state of affairs, he concludes his chapter with a thought provoking section on "the potential for change", meaning change in the direction of a less dehumanized form of research based on more participatory research relationships. I am entirely in sympathy with his activist orientation, but I believe his analysis of the levers of change needs amplification in one important respect.

The question of changing a dehumanizing investigative practice so as to accord greater respect to those on whom it was imposed is not a new one for modern psychology. In fact, this question assumed considerable political significance three or four decades ago. I am referring to the anti-test movement, which not only resulted in serious legal challenges to psychological testing but also led to changes in this practice which at least limited the potential for future abuse (Rogers, 1995). Undeniably, there are important differences in the social impact of psychological testing and psychological research which make it problematic to draw an analogy between the two. But there is one aspect of the earlier conflict which I believe does have some relevance for the issue Walsh-Bowers is concerned about. From the history of the anti-test movement and its consequences we learn that, in the main, the role of the psychologists was essentially reactive. Although some of them may have had some criticisms of testing practice the effective impulse for change did not come from them but from those who had been wronged by that practice. Only when there was a significant withdrawal of support from those on whose participation the practice of testing depended were conditions created in which changes advocated by internal critics were actually implemented. I suspect that unless a similar situation develops with respect to research practice there will be severe limits to the changes one can realistically expect.

As Walsh-Bowers notes, the maintenance of prevailing orthodoxy with respect to research practice depends on "deeply internalized modernist norms (are) powerfully present among psychologists concerning what constitutes 'rigorous' research" (p. 116). But beliefs about the demands of science and scientific research are not limited to psychologists, they exist also among those who participate in research situations as "subjects". If a dramatic illustration of the power of such beliefs is needed, the notorious Milgram experiments will supply it. In that case those who were the targets of experimental manipulation demonstrated a faith in the scientific credentials of psychological research that was strong enough to be compatible with the apparent infliction of severe pain. As long as "investigators" and "subjects" share a similar set of beliefs about the value of "rigorous" research and what it entails the situation is likely to remain relatively stable. Of course, this does not exclude cosmetic changes in the style of research reporting, as Walsh-Bowers notes for community psychology. It is true that a participatory style of research lends itself to what he calls "relational reporting", but the converse does not hold. Merely changing the rules of reporting will not necessarily produce any essential change in the relationships among participants in an investigative situation.

For change to occur, widely shared beliefs would have to become problematic. This might happen in a number of ways. Theoretically, there could be a revolutionary change in the ambient culture, such that humane conduct comes to be valued more highly than precision, efficiency, technological advance, and similar idols of the present age. More likely, changes of that kind could affect certain sectors and certain localities. The conditions favouring that kind of development would be quite diverse and difficult to predict. Rather than engage in speculation along such lines I want to indicate another possibility, one which arises out of the link between research ethics and research product.

A recognition of the fundamentally social nature of psychological research situations has been the cornerstone of my work in this area. By contrast, standard practice is based on an understanding of such situations in terms of "methodology", a purely formal description of conditions, manipulations, and outcomes that brackets out all specifically social features. Those features are either relegated to the status of a separate subset of "conditions" or, for many investigators, rendered completely invisible. As Walsh-Bowers mentions, the very notion of a research relationship seemed to baffle many of the investigators he interviewed. This marginalization of the social in investigators' understanding of research situations results, as a matter of course, in the marginalization of research ethics. Ethical considerations arise in social situations. If situations are not experienced as inherently social, ethical questions will either not arise at all (as was the case for many years in psychological research) or they will be relegated to a separate compartment. Only external links will be recognized between this compartment and another quite separate compartment representing "methodology" or "procedure". What cannot be recognized within this framework is the intrinsic connection between research ethics and research procedure because that would imply a recognition of the inherently social nature of research situations and the artificiality of an autonomous domain of formal “methodology”.

Research situations are set up for the sake of some sort of knowledge product. But not all knowledge products are of the same kind. What standard psychological research situations are designed to produce is what is called “scientific” or “objective” knowledge (often mislabeled “empirical”). The fact that situations have to be specially “designed” to produce that kind of knowledge indicates that there is an intimate connection between the nature of the situation and the nature of its knowledge product. Different situations produce different knowledge products, and if it is quantitative, causal, third person psychological knowledge we want we know what sort of investigative situation to set up. One reason why exploitative or coercive research relationships have been tolerated in the past is that their products were so valued that the ethically problematic nature of their origins could be overlooked. But if and when those products come to seem less valuable ethical scruples are less likely to be discounted.

Human situations in which ethical considerations are ignored, dismissed or circumvented are to some degree dehumanizing. Situations in which people get together to create some knowledge product are no exception. In many scientific research situations this may be of little consequence for the nature of the knowledge product. The properties of a chemical will not be altered by the fact that their discovery was based on the unrecognized contribution of underpaid and overworked laboratory assistants. But in psychological research situations with human subjects things are different. In this case, the information on which the knowledge product depends pertains directly to those participants whose role is that of data source. These participants contribute more than their labour to the knowledge generating process—they contribute themselves. If the situation in which this occurs is to some degree dehumanizing that is likely to be reflected in the information they contribute for the construction of the knowledge product. For example, if the investigative situation prevents human data sources from demonstrating any personal autonomy in their actions no information regarding this aspect of human action will find its way into the research product. The knowledge that results will be knowledge pertaining to individuals unable to demonstrate significant aspects of their human potential. Dehumanizing aspects of the research situation lead to dehumanized psychological knowledge. As long as there is a demand for such knowledge these aspects will be tolerated.

What I am suggesting is that the question of change in research ethics cannot be separated from changes in research practice and changes in the kind of knowledge product that is in demand. Questions of research ethics are closely linked to questions of methodology and ultimately to questions of psychology’s scientific project. Traditionalists often react to moves for significant change in research ethics as though there was something subversive about them. They are quite right. Potentially, such moves call into question much more than narrowly conceived ethical issues. What is called into question is the close relationship between the scientific investigation of human persons and positions of power. As a rule, it is the less powerful who are investigated by the more powerful, and to make effective use of the results of such investigations one must again have power.

The best way to defang ethics is to segregate it into a separate compartment, quite distinct from the issues that really matter to traditionalists. That way one can pass off changes in reporting style for changes in what actually happens on the ground.

That might prompt one to ask what hope there is for any significant disciplinary transformation, a question that can only be considered against a wider background. Ultimately, its resolution will depend on social conflicts and developments beyond the confines of the discipline, and these are likely to show considerable local variation. Perhaps the best conditions for a crucial change in investigative practices exist in those quasi-colonial contexts in which the link between social power and research methodology has become glaringly obvious (Smith, 1999).

Whatever impact disciplinary history has, it will not be the same everywhere. The idea that the discipline as a whole might one day be transformed by the efforts of critical disciplinary historians seems to me preposterous. But these historians may be able to play a role in certain local developments, where "local" refers both to geographical and intellectual space. A juxtaposition of the rather pessimistic tone of Bayer's chapter and Richards' (2002b) highly optimistic assessment of the history of psychology as "the discipline of the future" provides a telling illustration of these local differences, American and British in this case.

But, as both Bayer and Staeuble indicate, whatever their outlook, the historians among psychologists will have to pay more attention to issues of disciplinarity than has been the case hitherto. One of the more important tasks facing disciplinary historians is the historicizing of prevailing structures of disciplinary authority and the policing of disciplinary norms and boundaries. Winston's chapter provides an exemplary product of such work. Any serious discussion of disciplinary change must start from the recognition that disciplinarity itself is a relatively recent, eminently historical, phenomenon that has not followed and will not follow the same course everywhere.

In psychology there has been a close link between rigid forms of disciplinarity and an ahistorical approach to the subject matter. But this may be changing. Interdisciplinary structures are multiplying. Within some of these structures historical approaches are not unwelcome; in others they are more unwelcome than ever. The situation has become more fluid, and that is beginning to corrode some of the structures of disciplinary authority and what Staeuble refers to as the "disciplinary construction of reality". This, as she points out, is particularly clear in a post-colonial context.

However, unresolved problems abound. For example, disciplinarity draws much of its strength from self-reproducing social structures, a property not often shared by multidisciplinary structures (Abbott, 2001). What historical knowledge might be relevant to the possibility of change in this state of affairs? Moreover, the simple replacement of universalist with non-universalist forms of knowledge would replace one set of problems with another, parochialism and non-portability of knowledge being only the most obvious examples. Renewed historical study of the disciplinization of psychology is likely to acquire a new relevance in the context of these problems.

Some years ago I wrote a paper with the title "Does the history of psychology have a future?" If I were to write a follow-up to that paper today I might entitle it "Does disciplinarity have a future?"

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