

**Rediscovering the history of psychology:
Interview with Kurt Danziger (2003)**

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I would like to begin by asking if your work on the history of psychology is connected in some way to your early experiences in South Africa.

History was an important subject in South Africa and it was a loaded subject. South Africa was an intensely divided society. It was a society that was under threat and felt itself to be under threat. Eventually the threat became a very serious reality and resulted in social change. Throughout the 20th century, history had been mobilized in South Africa to legitimize, first of all, racial oppression and, subsequently, attempts to advocate reform. So by the time I was a student at university, there were two kinds of history and I was very much aware of that. There was an older, conservative history that assumed that racial antagonism was part of the order of nature and not something that needed to be questioned or explained. That had been replaced from the 1930s by a more liberal kind of history. This inverted the problem and led to the question of race prejudice becoming an issue whose genesis had to be explained historically. It was not a part of the natural order of things.

There was never a time when you saw history as a simple factual account?

My earliest experience of history was not some kind of Rankean account of definitive historical facts (1). History was always a contested area. It was something that was open to alternative explanations and those explanations were closely connected with current agendas. When social psychology was in its earliest beginnings in South Africa, a classical work on race prejudice appeared there. This was in the 1930s. It was a social psychological study, based upon the administration of attitude scales, but that was preceded by a long historical section. The social psychological data were then explained historically, using a framework recently introduced by the first wave of liberal South African historians.

Which book are you referring to here?

The author of the book was J. D. MacCrone, who was Professor of Psychology at the University of Witwatersrand in Johannesburg (2). It was one of the very few indigenous psychological products in South Africa and so everyone who was learning or teaching psychology there would have been acquainted with it. That link between empirical investigation of race attitude in a social psychological way and a liberal interpretation of history was not entirely peculiar to South Africa. MacCrone spent some time in the United States and acquired some of his orientation there. It was an interaction between local conditions and certain foreign influences.

Perhaps it is social conflict that brings many people to history in the first place.

I see this as related to one of the distinctions between history and antiquarianism. Much that is called 'history' is really antiquarianism but they are quite different. There are many people who have an antiquarian interest in history and that is fine. But if there is no interest in social conflict,

in opposing forces and interpretations, it usually leads to superficial accounts for which the truly historical questions of how and why things came to be as they were never arise.

One of the things that has always attracted me to history is a sense of the remoteness of it, to be able get in touch with previous generations who are dead and gone but who have left traces of their lives behind.

I think in terms of justifying historical research, that has to be a primary consideration. It has been suggested that, potentially, the major benefit to be derived from historical studies is an acquaintance with, and hopefully an understanding of, the other. To me, the hallmark of poor history is the kind of history that tells us that the past is just like the present. There is no value in that. The value of history lies in turning to the past to find things that were different from the present, to find different ways of doing things and different ways of understanding the world. That is a large part of the justification for paying considerable attention to a figure like Wundt in the history of psychology. There is some continuity between what Wundt did and what was done later but the major part of Wundt's work, both theoretical and practical, is other than what subsequently developed in psychology in the twentieth century. This is what makes Wundt interesting, not the little thread of continuity between his laboratory and later laboratories, but rather the yawning discontinuities between his ideas about psychology and what came later.

History can show us alternatives to the status quo but, from a contextualist point of view, is it realistic to expect that the alternatives can be transported from one time and place to another?

If history is really a way to get to know the other, it is going to be a way of opening up alternatives. But one has to remember that these are counterfactual alternatives. We have to ask a question: 'Why this way and not some other way?' This way of doing research was a real alternative at one time and yet the discipline went in a different direction. That kind of question can be answered to a large extent by historical investigation. I am not saying that we will get every last answer but if we get some of them, we can have a better understanding of why we ended up where we have and what possibilities for change there might or might not be.

I have heard some people claim that there is an inherent contradiction in your work; that you try to identify the social determinants of psychology and yet believe that we can have a different kind of psychology, even if society does not change. My response has been to refer the people to your article, "Does the history of psychology have a future?" (3), where you point out that psychologists do not speak with one voice. The marginalization of certain groups provides the social context to their criticism of mainstream psychology.

I was making the point in that article that there was a divergence between the moral community within which the active scientist-experimenter worked and the moral community within which the historian worked. I was saying that a good historian could not take the moral certainties of the scientific community for granted but had to question their historical basis. That led to the question, 'What is the future of the history of psychology?', because presumably it is not going to be particularly popular with some psychologists. In the latter part of that paper, I turn to the question of what social conditions might favor the continuation of a more critical kind of history within psychology. I pointed out that psychology was not a homogenous field, that it was not a

static field. It seemed to me that within the discipline there were groups and tendencies that had been heavily marginalized by the mainstream. For example, I mentioned feminist psychologists. I mentioned the growth of psychology outside the most developed countries. Psychology there is marginalized but also has interests that are very different from those of the mainstream in the most advanced countries. Then of course there are marginalized fields. Community psychology is just one example. Taking the discipline as a whole, I suggested that there was indeed a significant social basis within the discipline for the pursuit of a more critical history.

Can marginalized minorities provide the level of support that critical history of psychology needs?

A more critical history of psychology is being pursued by individuals whose professional affiliation is not with psychology but with history or history of science. One could ask: 'Why should someone whose professional affiliation is with psychology be doing history? Why not just leave it to the professional historian of science, as already happens with the physical sciences?' First of all, there is a practical argument that the number of historians of science who specialize in psychology is very small and I can't see it ever being a great priority in that discipline. The other point is that there can be a certain division of labor. I don't think that psychologist-historians can or should compete with professional historians in terms of their handling of the complex texture of historical conditions. We always will have to defer to the historians as being the people who will supply answers to the questions that relate to the complexities of the historical texture. On the other hand, I think that people who have a training in psychology and then enter into history have an advantage over the professional historians in that these people would typically have had a hands-on, direct experience of what it is like to do psychological research or to be engaged in professional practice. They will know what it is like from the inside, sometimes through years of experience. They are in a position, at least potentially, to communicate with their fellow psychologists in a way that perhaps professional historians cannot do. Professional historians will always come up against certain barriers, experienced perhaps as technicalities, which will make it difficult to speak to psychologists in the appropriate language.

Psychology is divided into 'areas' or 'specialities', with history of psychology being regarded as one of them. This is surely a problem for the view that historians of psychology should try to communicate with practising psychologists.

It has often been pointed out that a good way to render potentially subversive ideas harmless is to segregate them out into some kind of ghetto. That is something to be resisted. The fractionation of psychology is a fact that one has to recognize and it is something that historians of psychology should take into account by specializing in their own work. There has been excellent work on the history of developmental psychology and the history of social psychology, some of it very recent, that has contributed to these fields. The work of historians of psychology will have to concern itself with specific sub-fields. I don't think that is a bad thing. One of the historically generated myths that historians do have to come to terms with, and will have to explore as a historical legend, is this idea that psychology is a unitary discipline. That would be a very interesting historical topic: 'Where did the legend originate specifically and what have been its vicissitudes?' One of the basic requirements for an adequate history of psychology is that it

should not tend to be a history of some unitary thing called, 'psychology'. Studying psychology is essentially studying the history of how psychological objects were constituted. The historical study of psychology duplicates the fractionation of psychology that we find in contemporary reality.

Would you say that, in your book, Naming the Mind, you were examining the history of specific sub-disciplines of psychology, such as learning, motivation, personality, intelligence?

These terms refer to what I call psychological objects. When they use certain identifying categories for describing psychological phenomena, psychologists discursively construct these phenomena as certain kinds of objects existing out there in the real world. So phenomena, that people from other backgrounds might classify and identify in totally different ways, become instances of learning, motivated behavior, attitudes and so on to the person approaching those phenomena with pre-established psychological categories.

You have written that the possible correspondence of psychological objects to reality is an empirical question (4). You clearly see an important role for evidence in your own work.

First of all, in a very general sense, history is something that all human products have in common. Anything that human beings use in a practical or an intellectual way has come into being. It has a history and, typically, also a future. What we see is a temporal moment, a cross-section in time in the history of something or other. So in exploring, elucidating and interpreting the time-course of phenomena, we are saying something very fundamental about them. Secondly, the search for evidence, the exploration of evidence, the interpretation and understanding of evidence has, like anything else, a temporal trajectory. We cannot expect that anything that is said now represents the final answer but we can expect one answer to be better than another at this point in the light of everything that we know. And that is simply the best we can do, which is a lot better than nothing.

I suppose this has implications for what is sometimes called, 'revisionism' in history.

There has been some very interesting recent discussion of the implications of re-interpreting history and one of the contributors to that discussion (5) pointed out that, for organized human activities extended in time, the judgment of what was happening was bound to change with time. He referred to the analogy of a game, let us say a game of soccer. When the game is going on, you can see that a goal is being scored but what you cannot then see, or cannot then say, is that this is the winning goal. The game has got to run its course before that judgment can be made. Now we are making judgments about an ongoing game. The game is not over and we are trying to make the best judgment we can in the middle of the game.

Some people are surprised when I tell them that you identify with philosophical realism. Could you explain how this is compatible with your sociological or 'social constructionist' point of view?

To continue using the game analogy, the fact that interpretations of what happened during a particular part of an ongoing game change with the course of time, and the availability of

subsequent evidence, would not lead anybody to conclude that there was no game! Of course there was a game and it is the very fact that there really was a game that leads to all these reinterpretations in the light of new evidence. Psychologists construct their objects, the things that they take themselves to be investigating. They also reconstruct them; that is, they change their definitions of those objects. They even abandon certain objects and invent new ones. It does not follow from all that that there are no objects. The interesting question is that of the adequacy of the constructed objects to the real objects, which I am not denying are there. It then becomes a question of strategy. The normal naïve empiricist approach is to assume that the way we have constructed a particular object happens to conform to the real object. In other words, the investigator should always be given the benefit of the doubt. Naïve empiricists give the investigator the benefit of the doubt because they do not see that there is any doubt. We can hardly blame them. However, historians should know that there is some doubt. The only way to proceed to better knowledge of what is going on is not to give the investigator the benefit of the doubt but to say, 'This is a constructed object and I will study how it has been constructed.' When that has been done, let us ask the question: 'What is left of the possible correspondence between the constructed object and something out there in the real world?' I am not adopting the Kantian position that the world of real objects is a world of noumena which can never be known. I am quite prepared to agree that something can be known about that world. It is a very slow process, a process that demonstrably has innumerable blind alleys and false paths, but I am prepared to be optimistic in the long run.

One of your main concerns has been to show that both popular and scientific views of 'human nature', or whatever we want to call it, have been 'made up' in order to serve certain social ends. Why do you think it is important to point this out?

Let me go back to the South African example that I mentioned earlier. There was a claim that racial antagonism, which would not have been constructed as 'racial prejudice', was a part of the order of nature. It was supposedly the way that things were. That doctrine was however part and parcel of a social order based on the oppression and exploitation of the majority of the people. If people really believed in the doctrine, the tendency would be to accept this order as something inevitable, something that would always be so. Then it was suggested that this racial antagonism was something which had been 'made up' in some sense. What had been made up was the image of the 'savage', the savage that had to be kept from the door. That was something that had its origins not so much in the order of nature but in the social order and therefore was a social construction. That insight was potentially very liberating for all concerned, liberating the dominant group from its own prejudices and, of course, liberating for the oppressed. At any rate, the recognition that existing categories, used in particular social contexts, are not natural categories but made-up categories frees people from the potential tyranny that those categories can exercise. In science, received categories can exercise a kind of tyranny that prevents investigators from reformulating problems and approaching them from a different, perhaps more illuminating, perspective.

How much of a difference can a historian of psychology reasonably expect to make?

I think the idea that social influence only works through immediately visible effects is an illusion. Let me go back to another South African experience. For nearly three decades, I was

unable to go back to South Africa. When I went back for the first time, just as all the big changes were happening, I found something very striking and it surprised me. I had always thought of the motor for radical social change as being political, economic, or even military. What I observed on going back was that, in the course of all those years, there had obviously been an accumulation of minute changes. These were composed of innumerable tiny acts of resistance on an everyday basis. It could be seen in the way that people related in a different way to each other across the racial divide; in the streets, and very definitely in the stores. It was the way in which shoppers that belonged to the wrong race group were treated in department stores, in service stations, and so on. There had been a kind of sea change. It was a change that could only have come about through the medium of imperceptible tiny acts of resistance on a day-to-day basis by ordinary people who would not have thought of themselves as 'political'. This taught me that one should not underestimate the importance of these small and often imperceptible changes. In the long run, they may be more important than the spectacular conflicts that fill the pages of newspapers.

You said that your South African background led to an early awareness of history as a controversial and ideologically loaded field. Are there any other ways in which this background gave you a perspective on psychology that might not have been shared by your North American colleagues?

Finding myself suddenly transposed from that society to this one made some things salient for me. One of the things that struck me from the very beginning was the enormous significance that their professional identification seemed to have for most North American psychologists. Where I came from, there were a lot of people, including myself, for whom professional identification played a very subordinate role. One's source of social identity was largely fed from elsewhere, such as one's identification with political causes and a more general identification with academic values. The oppression was not simply racial oppression. There were books, including academic books, that were banned. The political security police also operated on the campus. One was very much aware of that, especially in the social sciences. The powers-that-be considered certain ideas to be a step too far and discussing those ideas could lead to serious repercussions. So the resources for one's identity were not particularly connected with being a psychologist.

Who or what were your most important intellectual influences?

At the University of Cape Town, where I did my undergraduate degree, the emphasis was on moral and social philosophy. So this was the direction that my interests took. At a very early stage, I developed an interest in classical German Idealism, mainly Kant and Hegel. At a later stage, I came across the writings of the Frankfurt School - Adorno, Horkheimer and later Habermas - and found them very congenial. I then became interested in German sociology; particularly the work of Karl Mannheim on the sociology of knowledge.

Did you acquire any more intellectual influences after moving to North America?

After coming to North America, I was exposed to another set of influences. Because of my emerging interest in professional institutions, I began to read more widely in the history, sociology and philosophy of science. I arrived in 1965 and Thomas Kuhn's book on scientific

revolutions had appeared only three years earlier. A lot of things took off from that. The attempts by the new sociologists of science in the 1970s to get into the laboratory in order to study the genesis of scientific knowledge, at the coalface as it were, fascinated me (6).

I can see how it would fit in with your earlier interest in the sociology of knowledge.

It was a kind of sociology of knowledge or more a microsociology of knowledge. This was the time, at the end of the 1970s, when I was looking at the Wundtian literature. Partly because of my background as a social psychologist and partly because of my exposure to this new sociology of science, I looked not only at Wundt's theoretical texts, like the Grundzüge and the Logik, but also at the experimental reports in the Philosophische Studien.

That leads us nicely into the notion of 'practice' and its importance in your work. Perhaps the best way to approach the topic would be to ask what you mean by the term?

Unlike many of their colleagues in other disciplines, research psychologists do not speak of their 'practice' but of their 'methodology'. The trouble with the concept of methodology is that it abstracts certain rational features of human action from all the other features in the context of scientific activity. What is involved here is the very fundamental issue of whether scientific activity is a human activity, essentially akin to many others, or whether it differs from all other activity in some very profound way. Earlier attempts at producing a sociology of science, or for that matter a sociology of knowledge, tended to put the content of science on a pedestal. As David Bloor has pointed out, science had something like the social status of a religion; with the result that there was a sociology of science that was not a sociology of the content of science but a sociology of scientists (7). Those are two very different things. A sociology of psychological science must target the actual content of that science, and the sociology of individual psychologists could never substitute for that. In one of my first papers in this area, I criticized an example of this applied to the case of Wundt (8). It seemed to treat individual motivation as the motor of historical change. In the end, it almost looked as if the emergence of the first psychological laboratory was the outcome of some private career choices on the part of its founder. I should add, by the way, that Karl Mannheim himself suffered from the same veneration of science because he did not believe that the sociology of knowledge could be extended to the content of science (9).

He believed that it could be applied to the content of the social sciences but not to the content of the natural sciences.

Yes, but by the time I was going into the field, there were currents of thought that were running counter to that. Thomas Kuhn's work pointed in that direction. One of its most important aspects was the link between the social organization of scientific communities and the cognitive products of those communities. The 'subject' of history was no longer conceived as an individual, or even as a group of autonomous individuals with their own private motives, but rather as a scientific community. Then there was the characterization of the activity of scientists. The concept of methodology covered certain rules of scientific behavior which are rational. The tendency had been to extrapolate from the existence of such rules to the actual behavior of scientists. That meant partitioning a certain example of human behavior, namely behavior in a scientific context,

into two: one that was completely logical and rational and another which was socially and culturally determined in the way that human behavior usually is in all other contexts. It seemed to me that this was an unsustainable dichotomy which did not correspond with what I had seen of science.

Would it be right to say that you associate the term, 'practice' with what psychologists usually call, 'methodology'?

In terms of the way in which the concept had been used in the literature, the term, 'practice' or 'social practice' seemed to be a suitable one for describing activities that would have both a rational and a socially contingent aspect to them without separating them into two different compartments. So I prefer to use the term 'investigative practice' as incorporating both the quasi-rational rules that scientific activity follows, to a greater or lesser extent, and the social contingencies of scientific activity as well. It is an integrative concept.

Your more recent work on the history of psychological concepts and categories is not a history of practice in that particular sense. Is this history of a different kind?

There is another aspect to the meaning of practice. Ideas and concepts emerge in the context of human practices, and human practices can take various forms. They can take the form of manipulating material objects or people but they can also take a discursive form. One does not need to accept Foucault's various applications of the term in order to appreciate the analytic value of the notion of discursive practice (10).

So even your history of psychological concepts comes down to practice in the end?

I am always concerned to show how the emergence of psychological concepts and categories, and changes in them, are tied up with practices and changes in practice in the real world. One could mention the tie between the emergence of the category of 'motivation' and emerging managerial practices in the larger corporations in the early part of the twentieth century. Another example would be the emergence of the 'attitude' concept in its modern form. This changed quite fundamentally in its meaning as a result of marketing practices and both consumer and public opinion research.

There are differences of emphasis in your career as a historian of psychology. You published most of your work on Wundt in the 1970s. In the 1980s, most of your work was on psychological methods and your work since the publication of Constructing the Subject in 1990 has been centered mainly on psychological objects. Where is the continuity in this work?

I defined what I was doing in terms of the search for the historical constitution of psychological objects at a very early stage in my work on the history of psychology. Although I have given the topic more explicit emphasis in recent years, it has been there all the time. To be more explicit, I have used three fundamental concepts to organize historical information. The three concepts are subject, object and practice. The conceptual content of psychology consists of psychological objects which are produced by certain psychological practices. However, those practices don't just hang in the air. They are always the practices of actual people, historical subjects. So there

are three histories to pursue, the history of psychological objects, the history of psychological practices, and the history of the subjects involved with these objects and practices. I have concentrated on the first two of these histories but tried not to forget about the third.

Your use of the term, 'subject' is different from the way it is typically used in psychology; that is, to denote the research participants but not the psychologists themselves. You also seem to be using it to refer to groups of psychologists rather than individuals.

The term 'subject' is used in many different ways in different contexts; it has also undergone profound changes of meaning over the centuries. More specifically, the difference you allude to is an illustration of what one might call the curse of disciplinary solitudes. Participants in psychological experiments were not always called 'subjects'. Earlier, they were frequently called 'observers'. The quasi-scientific use of 'subject' derives from medical usage. In the nineteenth century, a person could already be a subject for surgery, for example. Then people became 'hypnotic subjects' and finally 'psychological subjects'. In this usage the subject is an individual to whom something is done, though not entirely without his or her co-operation. However, there is a much older and broader philosophical context for the use of the term 'subject', and here the meaning is virtually the opposite of the medical-psychological meaning: a subject is a source of actions and ideas, an active principle, not someone to whom something is done. When individuals act in unison they can be said to constitute a social or historical subject in the broad sense. That is how I use the term in a historical context.

How would this apply to your work on Wundt?

Because a lot of the early work on Wundt was essentially concerned with deconstructing certain myths and legends, there was a great deal of emphasis on the content of his work. I would regard that as belonging to the history of psychological objects. For example, a concept like 'apperception' is a psychological object which has a certain history. Similarly, Wundt's concept of 'drive', 'instinct' and so on. Secondly, I explored Wundt's investigative practice, analyzing the reports in the *Philosophische Studien*, and eventually placing his practice within a history of psychological practices that continues long after Wundt's lifetime. But in the background there is also the fact that Wundt was a typical representative of what Fritz Ringer called, "the German mandarins" (11), the academic elite in Wilhelmian Germany whose members had certain common values and beliefs which were utterly different from those of the American psychological community.

You said earlier that you criticized an early attempt to explain the establishment of Wundt's laboratory in biographical terms. What is your view of the focus on individual biography that is common in American work on the history of psychology?

My view is that historical biography of individual figures is a perfectly legitimate and interesting field in its own right but I think it is a different field from history. The categories of individual biography are often unsuitable as explanatory categories in the field of history. Their use can even be destructive if they carry the implication that history is, and can only be, the history of individuals. Historical biography works with certain categories for the explanation of individual human action and, quite appropriately, it can take those categories from contemporary theories of

human action, motivation and so on. History, on the other hand, works at a trans-individual level. It works with trends, phenomena, forces that are not to be equated with individual action at all. It works with changing cultural patterns, social institutions, ideological formations, social rules, customs and power relationships. None of these can be reduced to individual biography.

What you are working on at the moment?

My last book, Naming the Mind dealt with the historical emergence and development of essentially modern psychological objects, enshrined in categories like ‘behavior’, ‘motivation’, ‘attitude’, ‘intelligence’ and so on. These were categories which either emerged with the emergence of modern psychology or else took on their modern meaning with the existence of modern psychology. In order to make that book manageable, I intentionally excluded certain topics. For example, I excluded most cognitive categories, the only exception being intelligence. I had intended once that work was complete to turn to cognitive categories, which would require a lot of additional work. The other issue that was sidestepped in Naming the Mind was the issue of the antiquity of psychological categories, and therefore psychological objects. I have tended to agree with people like Roger Smith and Graham Richards that the history of psychology is limited to the period when psychology recognizably emerges as a disciplinary subject matter and that it is extremely problematical to talk about psychology having a history before that (12). It is extremely problematical because, for one thing, the very notion of the ‘psychological’ is historically a very recent notion. The term is not used in a modern sense, in a way that we would understand as psychological, until the eighteenth century. In some countries, it was not used until the nineteenth century. So to talk about, let us say, the psychology of Plato or Aristotle is problematical. Nevertheless, there remains the question of whether there is a sense in which modern psychology had some kind of prehistory. Nothing historical ever emerges out of nothing. That leaves open the question: ‘What was the soil out of which this quintessentially modern notion sprang?’ If we raise that question, we cannot be concerned only with the immediate historical antecedents. We will likely find ourselves going back much further. There are three aspects to the problem. First, there is the sociological aspect. In terms of my previous schema, that concerns the historical subject. How do communities of psychologists or communities of psychological discourse arise? The history of the constituting subject raises questions about the organization of knowledge production; for example, the organization of universities and the organization of disciplines within them. The second aspect involves the history of psychological practices. One might, for example, go into the history of the practice of introspection. The religious use of the notion of introspection predates the secular use by a considerable time. So there is what Foucault and Nietzsche would call a ‘genealogy’ to be followed. And then, thirdly, there is the history of psychological objects, which have their own genealogy. One can even speak of the ‘biography’ of a psychological object. This does not come down to the history of a particular term because concepts can easily change, sometimes quite profoundly, while still being called the same name. The modern concept of intelligence is one example. It has nothing, or very little, to do with the more ancient usage of that term, even though the word is the same. What was the ‘Divine intelligence’ referred to in earlier times? Not something measured by a paper and pencil test I suspect.

Are there any cases where a word has changed without there being a significant change in the underlying concept?

Terms can sometimes change and there can be relatively little change in the underlying concept. That is perfectly true. Nevertheless, the correlation that does exist between term and concept can be used as an initial guide to tracing the prehistory of psychological objects. In many cases, one finds massive discontinuities. For example, if you trace the antecedents of the category of 'psyche', a category that is presumably central to psychology, you will find massive discontinuities because it does not begin by being a psychological category. For Aristotle it is a quasi-biological category. There are a limited number of category terms that exhibit a relatively high degree of continuity. I can't think of a better example of that than memory. True, there have been gross discontinuities in the way that memory was practiced, in the way that memory was understood, and in the way that memory was valued. Nevertheless, there are elements of continuity which make it possible to construct something like a genealogy of memory. To go back to Foucault, I see an analogy with his history of sexuality in which he is very much concerned with all kinds of discontinuities in human sexuality but, nevertheless, there is an element of continuity which makes it possible to talk about these things in terms of a common genealogy.

How different do two things have to be before they cease to be the same thing?

The way to decide that is in terms of the discursive and practical context in which the various terms are used. The concept of memory that occurs in some of the Platonic dialogues is quite comprehensible to the modern reader. Yet, although it is clearly not 'memory' as we understand it today, the resemblance is clear from the context in which it is used. There has to be a balance between continuity and discontinuity. To some extent, of course, there is an arbitrary element and that leaves room for disagreement. To some investigators, in a particular instance, it might seem that the elements of discontinuity are so massive that we shouldn't talk about the history of this object any longer. They are different objects. There can be legitimate disagreement about that and I don't think that we should even aim at some kind of total consensus. Further insights are going to come from discussions of this kind. So I think that the best we can hope for is that there will be some objects, and memory is one example, where the elements of continuity are so convincing that there will be relatively little controversy about the notion that we can treat this as the history of what is in some fundamental sense the same object.

Historical psychology is one of your main interests (13). What do you understand by the term, 'historical psychology' and how you would distinguish it from 'history of psychology'?

To me, the history of psychology is intimately bound up with the existence of a professional class of psychologists, the existence of groups of specialists in psychology who employ specifically psychological practices and specifically psychological concepts in order to intervene in the world in some way. I therefore don't feel comfortable with extending the history of psychology beyond the last 200 years, or perhaps 250 years, but no more. I don't think that one can speak of a history of psychology beyond that. However, there is also something that Irmgard Staeuble has characterized as the history of human subjectivity (14). The term is a bit

vague but I think for present purposes it is precise enough. It is completely uncontroversial that in the course of human history, all kinds of things have changed. Technology has changed, artistic styles have changed, human values have changed, cultural patterns have changed, understandings have changed, philosophies used for understanding the world have changed, and so on. Unless you think of human culture existing completely outside of human individuals, and therefore able to change while leaving human individuals unchanged, the uncontroversial fact of cultural change in history entails changes on the subjective side in human individuals, the human individuals who incorporated those cultures and cultural changes. The history of those subjective changes that are intimately associated with cultural changes would, I think, form the subject-matter of what one would have to call a historical psychology.

There are many different perspectives on historical psychology. Even the small number of authors whose work is available in English – Barbu, van den Berg, Gergen for example (15) – have radically different views on the subject and I would assume that your views are different again.

Because the field is inherently difficult to distinguish from other areas of investigation, it has led to what is virtually a jungle of proposals and projects, none of which has gone very far. It is a minefield and there are certain cautions that arise out of that situation. For example, the idea that a historical psychology, if it existed, could be a unitary discipline makes no sense. The best that one can hope for are histories of specific psychological objects or specific psychological practices. Because one is dealing with subject-matter that is scattered all over the place, it does not have the kind of coherence that only comes with the appearance of a discipline, and the institutions appropriate to that particular discipline. Because of that, you will never have more than an assembly of specific histories; the rest is speculation.

NOTES

(1) Leopold von Ranke (1795-1886) is associated with the ‘objective’ approach to history. See Novick (1988) for a discussion of this topic.

(2) MacCrone (1937).

(3) Danziger (1994).

(4) Danziger (1993).

(5) Haddock (2002).

(6) Danziger is referring here to work such as that of Latour & Woolgar (1979).

(7) Bloor (1976).

(8) Danziger (1979b).

(9) See Mannheim (1936).

(10) The concept of ‘discursive practice’ is central to Foucault’s work. For a discussion of this topic, see McNay (1994).

(11) Ringer (1969).

(12) Richards (1987), Smith (1988).

(13) See for example, Danziger (2003).

(14) See for example, Staebble (1991).

(15) Barbu (1960), Berg (1961), Gergen & Gergen (1974).

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