

Constructing the subject: An interview with Kurt Danziger (1994)

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Could I start by asking you to say a few words about your background - where you were born, educated and so on?

I was born in Breslau - at that time part of Germany (1). When I was 11, my family emigrated to South Africa and so I finished my education in Cape Town, South Africa up to the M.A. level. I then went to Oxford to do my D.Phil.

The psychology department at Oxford was quite new at the time - was it not?

Very new. It had only just been established as an experimental psychology department. As you know, there had been individuals there within psychology - the best known, of course, having been William MacDougall - long before but, as an institute, it was new. It had just been established the year before I came (2).

With George Humphrey as the head?

Yes. And George Humphrey had been at Queen's University in Canada for a long time before returning to Oxford (3).

Oxford had a reputation as a hotbed of critical philosophers. I wouldn't say anti-psychology but certainly anti-psychology in the way that it was being done. I'm thinking of people like Gilbert Ryle (4). Did you encounter any of this criticism?

Yes. There certainly was a lot of that kind of thing. What I mostly encountered was the anxiety among the psychologists not to run afoul of the philosophers. They had the feeling that, as soon as they ventured into the realm of theory-conceptualization, they would be vulnerable to attacks from that quarter and so they didn't do it. The department tended to be almost dustbowl empiricist in its approach - except for George Humphrey himself who, at the time, was putting the finishing touches to his book on thinking, which is still a very useable historical study (5). I didn't appreciate that at the time because I wasn't into history. It was only afterwards that I realised the opportunity that I had missed. I didn't talk with George Humphrey about history at all - only about rats, which is what I was doing at the time (6).

You started out in Australia I believe.

It was very difficult to get an academic position in Britain when I graduated in 1951. There was still a backlog of ex-servicemen around who would, of course, get priority. I was the first cohort of a younger generation that had been too young to participate in the war and so the best opportunity that presented itself was in Melbourne, Australia. That was a far more lively

department at the time than Oxford. It too was new. It had only just been established but its resources were far superior. There was a whole different outlook. It was almost at the opposite end of the continuum. There was a lot of theorizing and a lot of critical debate going on.

You were still in experimental psychology at the time?

I was supposed to be setting up the animal laboratory there and I did but I got interested in other things - although that had already started in Oxford. While I was there, Niko Tinbergen - the ethologist - arrived. His fame didn't take very long to spread. Especially for someone like myself who was working on animal behaviour, it clearly made sense to listen to what this man had to say. His lectures were just down the road in the zoology department. After listening to Tinbergen for a while, and after reading some of the ethology literature, I came to realise that what I was doing - which was pretty standard 1940's psychological rat experimentation - didn't make a whole lot of sense. While I was too far into my project to abandon it at that point, I decided that I wasn't going to continue doing that once I had graduated. I then had a brief period when I tried to work with less artificial creatures than laboratory rats - mainly wild rats - but that just didn't work out.

You eventually moved from Australia to South Africa. When was that?

That was in the mid-50's. I had a period of about three years at the University of Natal. By that time, I had made the switch from animals to children. I was doing Piagetian-type work with children (7) but, being back in South Africa, there were enormous pressures to get back into social psychology. I had done my M.A. thesis in social psychology at the University of Cape Town. While I was in Natal, Gordon Allport spent about half a year there on a sabbatical. That was quite interesting in terms of getting a more, shall we say, intimate insight into the strengths and weaknesses of American social psychology - seeing it transplanted into a very different environment.

Would you like to expand on that? Allport was very famous for his work on prejudice.

Well exactly. This was right after the publication of his book on prejudice (8). He was very committed to the topic and strongly convinced of the social good that was likely to result from studies of prejudice. It was difficult for him to appreciate the relatively secondary nature of the psychological aspects in a context such as that in South Africa at the time - which, of course, was a time when apartheid was really forging ahead. It was to be a generation or more before it finally met its end.

If I remember rightly, Allport uses the term, 'group fallacy' - a term that was popularized by his brother (9) - in that book. When one looks at the way in which South African social psychology has gone, with an emphasis on group relations, it is certainly different from what Allport was doing (10).

Yes. That's right. That was a domain that just couldn't be overlooked and attempting to give an account of individual prejudice without a strong grounding in the context of group relations just wasn't going to lead to any very useful results.

It almost goes without saying that a psychologist would emphasize psychological factors when the explanations may lie outside that particular discipline - or at least the approach that one should take (11).

That was the other thing that began to strike me at that time: the tremendous hold that disciplinary loyalties had on social psychologists in North America when compared to their counterparts in some other parts of the world. For us, it really wasn't that important whether a person was a psychologist or a sociologist or an anthropologist.

That may have something to do with the size of the academic community in South Africa.

Yes. That is certainly one factor.

You moved to Indonesia after that?

Yes. That's right.

You've often spoken of how formative the experience of being in Indonesia was. Would you like to say something about that - in particular, encountering psychology in a non-Western culture?

It was really the first time that I found myself completely immersed in a non-Western culture and, more particularly, my relationship to the people was not a quasi-colonial one. I went there as an employee of the Indonesian government. They paid me. I didn't get paid by a Western agency. I think that made a big difference - both in their attitude to me and in my attitude to them. I found that I had to learn the local language - which I did. After a while, I began to lecture in Indonesian. I had picked up some knowledge of Zulu while I was in Natal but, of course, the conditions there were so different and couldn't be compared with the kind of immersion in another culture that took place in Indonesia where most of my academic and non-academic contacts for two years were not with Westerners but with Indonesians. Participating in the internal workings of the university to some extent was extremely instructive.

There was an indigenous psychology (12) there - was there not?

Yes. That's another aspect. When I arrived there, I found that there was an Indonesian colleague lecturing on psychology. It wasn't called, 'psychology'. It was called, 'ilmu djiwa' which means, 'science of the soul'. It wasn't Western psychology at all. It was psychology based essentially on Indian philosophy and some of its modifications in Java. Of course, before I went there, I had no idea that such a thing even existed and so I was rather intrigued. With all the brashness of my thirty-odd years, I suggested to this man, who was considerably older and wiser, that maybe we should conduct joint seminars. When we began to get down to the details, it became obvious that

we weren't going to get very far because the categories for classifying and describing psychological phenomena were totally different in the two cases. There were, to all intents and purposes, no psychological equivalents for the basic psychological categories that were used in his world and vice-versa.

One of the arguments that is often used against relativism is the possibility of translation - I suspect by people who have never encountered a situation like that where translation seems to be impossible (13).

Some kind of translation, of course, can be achieved but I think that one's grasp of the alien concepts remains quite tenuous and is not to be compared with the absolutely taken-for-granted character which the categories of one's own culture have. You need a paragraph perhaps to express something that you say with one word because you have to supply some of the connotations which these words have for us.

You were saying to me at one point that the indigenous psychology had disappeared - or was beginning to disappear - in Indonesia.

When I eventually went back there for a visit much later - in fact, for two visits in 1980 and 1986 - I visited my old department and found some of my old graduate students established as Deans and such like. I certainly had the impression that the traditional psychology had very much disappeared from the university at any rate. I don't know what might have been going on elsewhere.

I'm sure it was a fascinating time. You then went back to South Africa?

I then went back to South Africa - this time to Cape Town as head of the department. I spent five years there before coming to Canada. That was the early- to the mid-60's.

Quite a few interesting things happened in those five years I believe.

I suppose that my approach to social psychology had become even more culturological and sociological.

You now had social psychology as your main speciality?

That had certainly become my main interest. Much of the work that I did in South Africa in the early-60's was, on a theoretical level, inspired by people like Karl Mannheim (14).

There is that very interesting paper on the successive legitimations of apartheid that you published. Was it the British Journal of Sociology?

I called the paper in the British journal, "Ideology and Utopia in South Africa" which is a direct take on Mannheim's book (15). I think the paper you are referring to was something I did much later when I was already in Canada. I did publish a paper which was more about the topic you

mentioned: the successive legitimations of apartheid. I called that, "Modernization and the legitimation of social power" (16).

I've read some of your work from that period in South Africa. Much of it was published abroad. It's very clear that this work would not have endeared you to the apartheid régime.

That aspect didn't have terribly serious consequences. I couldn't get that work published in South Africa at the time. It was published abroad. Of course, we were also in trouble if we used certain well-known texts which were banned. All that wasn't what brought things to a head. What really brought things to a head was that there was a serious deterioration in the overall situation in the early-60's which took the form of states of emergency - the abolition to all intents and purposes of habeas corpus and so on. I became very directly and actively involved in the resistance to these developments among the academic community in the English-speaking universities.

You were eventually expelled from the country?

Strictly speaking, I wasn't expelled. I did have the choice to stay without the possibility of ever leaving - as it seemed at the time - and taking the consequences. These were only too clear looking at what had happened to some of my colleagues and people that I had been associated with. It would have meant banning and so on.

Were you not in some way connected with the trial of Nelson Mandela back in the early-60's?

There was a big trial of the top A.N.C. leadership at the time. It was only one of a large number of political trials that were being staged at that point. The prosecution's case partly relied on the evidence of witnesses who had been held in solitary confinement for very long periods of time. There was a general atmosphere of tremendous fear in the country at the time - understandably. It was very difficult for the defence to find people who were willing to give evidence as defence witnesses. One thing they felt they would attempt to do was to call expert evidence on the reliability of witnesses who had suffered the kind of treatment they had been exposed to. Another psychologist from the University of Natal and I said that we would give such evidence (17). In the biggest trial, the judge didn't permit us to take the witness stand. We got as far as the courtroom but he ruled that he did not need expert evidence on that point.

Was that the main source of your problems?

Of course, there were other things as well. We circulated several petitions - one among academics in general and one among psychologists and psychiatrists. I took a very active role in that. I'm sure it didn't endear me to the powers-that-be either.

Obviously you had to leave. What brought you to York University?

When I realised that I would probably have to leave the country - if I didn't want to face banning and eventual imprisonment - I began to look around at opportunities. At that point, the

opportunities in Canada were far better than anywhere else. From a number of offers, I chose York and I'm really not sorry for that.

You have a diverse background up to this point, having done work on animal behaviour, child psychology and social psychology. You continued to do research in social and developmental psychology after arriving in Canada - for example, the work on immigrant children (18). What led you to history and theory of psychology as your main interest?

First of all, I had always had an interest in history. The high school that I attended did not offer history and so I arranged to do history on an individual basis. Secondly - and this developed a little later when I was an undergraduate - I developed an interest in philosophy. Both these interests continued all along while all this that I have been describing was happening. I had been teaching the history of psychology - certainly from the Cape Town days if not before. Even in Indonesia, I was doing so to some extent.

When did you start to publish in the area? Was it the late-70's?

That's right. Although I'd had this interest for a fairly long time, I decided to make use of the opportunity presented by a sabbatical in 1973-74 to really acquaint myself with the primary literature in the history of psychology - in particular, the German literature - Wundt, Fechner, Helmholtz and many less prominent characters. My experience at that point was a bit like a subject in an Asch conformity experiment because what I was reading didn't seem to jibe with what I had previously read in secondary sources like Boring and some others (19). Of course, I had simply trusted these sources previously.

As generations of teachers and students had done.

At first, I wondered whether I was somehow misunderstanding things but, after a while, it did become clear that wasn't the case. There really was a discrepancy between the primary and secondary sources.

The timing of your early work on Wundt was perfect given all the attention that was focussed on Leipzig around the time of the 'centennial' (20). Did that encourage your interest in Wundt?

No. That had already been established. That really became established in the mid-70's as a result of the reading that I had done. I must say I developed a considerable respect for Wundt at that point. I found that many of his comments were quite sagacious - occasionally quite prophetic and generally wiser than most of his contemporaries.

I think that is particularly striking given the traditional image of Wundt - a tireless worker but without much intellectual substance (21).

He had a different quality, for example, than Fechner. When one reads Fechner, one does get the sense that there was almost a quality of genius to the man. It is not easy to see how anyone, at the

time, could have had the insights he had. He had an originality that I don't think Wundt had. But simply as an all-round scholar - reliable, comprehensive, balanced - Wundt takes a lot of beating.

I suppose one could do a citation count on this but I have the impression that your article, "The positivist repudiation of Wundt" from 1979 is one of your best-known works (22). Your work on the history of method (23) began quite early - did it not?

I'm not sure. It's not something that it is easy to put a precise date on.

I was thinking in particular about that chapter in the Alan Buss volume where do a cross-national comparison of method and link it to the institutional organization of universities in Germany and the United States. That also appeared in 1979 (24).

I think that, at that point, my approach was heavily institutional. The emphasis on method and practice emerged gradually in the 80's. I think that it first crystallized for me in 1983 when I attended a workshop in Bielefeld on the probabilistic revolution. I gave a paper there which was subsequently published in a two-volume collection called, The Probabilistic Revolution (25). I think that experience made me aware of the specific emphasis that was quite unintentionally creeping into my work.

It is very much a central topic as far as psychology is concerned. I think here of another work of yours, "The methodological imperative in psychology" (26). To what extent has this interest been informed by your frustration - or former frustration - with the rigid methods that psychologists use?

I had developed a very critical attitude to the kinds of imperatives that were being propagated in the 1950's.

Which still continue to a large extent.

In the 1950's, these things were probably at their height and very much in the ascendant. In the empirical work that I began to do after I left animal work, I tried to use a more open-ended kind of approach. For example, in working with children, I used a Piagetian type of approach. Although it is not peculiar to Piaget, it tends to be associated with Piaget in North America because the other people who use that approach are not known - for example, Henri Wallon in France. In my social psychological work, I used open-ended questions that would yield qualitative material which could then be analysed using content analysis. So that kind of interest I've always had. But, of course, when I then went into the historical aspects, the criticisms that I had previously had on a local empirical level moved onto another level.

I suppose one of the reasons why it hasn't been such a central topic in the history of psychology is the failure to realise that these methods have a history and didn't drop down from heaven - which is how unquestioned assumptions are often viewed. I suppose one of the more subversive aspects of history is that it can tell the story of how these methods were 'made up' as it were.

Exactly. That they are very much human products.

Your work has been informed by the literature in the field of science studies - history, philosophy, sociology of science (27). This is highly unusual for a psychologist who writes on the history of psychology. How did you encounter this literature?

I really don't know. As I mentioned, I had always been reading philosophy and I had always been interested in science. I didn't begin my university studies with psychology. I began as a chemistry student and, in fact, took a degree in chemistry before I switched to psychology. So I had that background and I didn't leave because it repelled me or anything like that. I just felt that psychology would give me a better opportunity to develop my other interests - and it has.

That's interesting. Hans Eysenck has often told the story of how he became a psychologist because he didn't have the right qualifications to study physics (28). In a sense, you are coming from the opposite direction.

I think it can be useful to have a bit of a background in science, if only because it demystifies science. My own participation in chemical research was quite limited, and didn't occupy more than a few months, but was enough - as I say - to demystify it for me. I've always been interested in the possibilities and limitations of science. For one thing, that was important in getting into psychology. That was just after the end of the Second World War - not long after the dropping of the atom bomb and so on. There was a very general feeling among the younger people at that time that science ought to be applied far more massively to human concerns than it had been, and not just to the material universe.

That was quite a big movement after the Second World War I believe.

Yes. And I was certainly swept up in it. If it hadn't been for that, I probably would not have gone into psychology at all. The question of the pros and cons of the applications of science to human affairs has always been there as a constant - perhaps because of my interest in philosophy which is at least as old as this. It's never been an open and shut case for me. For many people, it is. They feel that only good can come out of that application. I think it's not so simple.

I think that has been the driving force behind a lot of the so-called "anti-science" sentiment among critical historians and sociologists of science - particularly since the 1960's.

You obviously get exaggerations on both sides but one has to try to keep the ambiguity of the situation in view.

Let's move on to your more recent work on the history of psychological concepts (29). I would say that this has become a central focus of your research.

The roots of that go back to the experience in Indonesia. I have never forgotten that. I had always intended to get back to it one day because it seemed to me that it was potentially of quite

fundamental importance. Now that I am hoping to have more time, that is what I am doing. I am looking at the historical emergence of some of the basic and often taken-for-granted categories that are used to organize psychological knowledge - categories like intelligence, personality, the self, motivation, emotion etc.

What relevance do you see this work having for psychology itself?

I think that everything in science depends on the kinds of questions one asks. Ask the wrong questions, and you're dead from the start. It doesn't matter how good your methods are. You'll never get a worthwhile answer. I think that, by and large, psychologists have not paid sufficient attention to the kinds of questions that they ask. That is a large part of the reason for the poor pay-off of psychological research over the years. When one does research on the history of empirical research in psychology - as I have done - you certainly get an abiding impression of very poor pay-off - to put it bluntly. Enormous resources have gone into this enterprise with not a lot to show for it in comparison to some other fields that one can think of. I think that a major reason for this is that psychologists have much too often asked the wrong kinds of questions. One reason why they've done so is that they don't take time to reflect on the questions they ask, and they don't entertain the possibility that some or many of these questions are not good questions. I think that the kind of historical study that I've talked about - a study of the origins and background of basic psychological categories - can help, first of all, to question one's questions and, secondly, lead to better questions.

You must have seen a tremendous growth in the history of psychology community since you began to do historical work in the mid-70's. Virtually all of the institutions were in the United States. I am thinking, for example, of the establishment of Cheiron-Europe, the sections for 'History and Philosophy of Psychology' in the British and Canadian professional organizations, the 'Fachgruppe für Geschichte der Psychologie' in Germany and so on. It does seem to be expanding quite a lot.

The development hasn't been a straight line - it never is - but it is unmistakable that, over the last 15 years or so, the field has been alive, has grown, has proliferated - at least to some extent.

Do you see any differences in the way that historical work is being done in different parts of the world - for example in Europe and North America? Are there any distinctive traditions or approaches emerging?

I think that it is not easy to generalize because there are so many varieties out there - especially when you take into account different parts of the world. One thing, of course, that one encounters everywhere is a confusion between history and antiquarianism (30). There will always be a certain amount of antiquarianism. There is no harm in that as long as one doesn't mistake it for history.

Which, unfortunately, is very often done.

One can only assume that, over time, people will become more sophisticated about these things. Also there will probably always be history as celebration (31). It need not be harmful as long as it is not the only history that there is or unless it is accepted as the last word.

What about critical history and its place within psychology (32)? It obviously arouses an extreme reaction on the part of some psychologists.

This gets into the kind of question that I've tried to deal with in a paper that is on the point of appearing in print called, "Does the history of psychology have a future?" (33). I think that the question is connected with the older question of whether psychology is more like a natural science or more like a human science. You have two different models of the use of history there. In the natural sciences, by and large, historical studies do not play a contemporary role. It is assumed that anything that is important in history will have been absorbed into the ongoing research tradition, and so it is only necessary to review work that is no more than 10 years old. In other words, you do history of a kind but it is a very shallow kind of history. Now that, of course, is based on the assumption that science is always progressive - that the best results from past work will, in fact, be reflected in new work as it gets done. I don't know about that assumption, even at the best of times. As far as psychology is concerned, I don't think there is much evidence for it. We can contrast that with the situation in a discipline like economics, for example, where critical consideration and confrontation of past work has often informed current work. In the other social sciences, such as sociology, there is much more of that than there is in psychology - much to the cost of psychology.

Except that it works in two ways. By jumping into the natural science camp, as it were, it brings not only greater respect for psychology but also more support in terms of research grants.

That has been the story of psychology. That is one of the reasons for the poor long-term performance that I talked about earlier. I'm sure that it's good for individual careers but, whether it helps the discipline in the long run, I think is questionable.

Another point that you've referred to in your work is the historicity of psychology's objects (34). Do you think that historians of psychology have something to contribute to psychology here?

There, I think, we do have to confront a genuine cultural difference. Psychology in the Anglo-Saxon countries has, of course, been heavily influenced by the empiricist tradition in philosophy. The whole notion of an a-historical human nature goes along with that. The idea that the object that psychology is concerned with is itself a historical object that changes in the course of history tends to be a very strange idea - a difficult idea for many psychologists in the empiricist tradition. Certainly in Germany, and in France, people find it much easier to accept the historicity of psychology's subject-matter (35). At any rate, if one goes along with the notion that one is dealing with an object that is inherently historical, then the history of psychology, which studies the history of the discipline devoted to studying that object, becomes all the more important because the two things are not unrelated. The way in which people think about human nature -

the way in which they study it and research it - is not without consequence for that object itself and vice-versa. The changes in human psychology over longer historical periods are not irrelevant to the knowledge that develops.

It would require a major revolution in terms of epistemological commitments and I see few prospects of that happening among the majority of psychologists.

That is certainly something for the future, but I think it may come about sooner than one imagines, because of the overall development, where psychology, like so many other things, is becoming globalized and is no longer essentially an American enterprise. Certainly for people in rapidly changing societies, it is much easier to accept the historicity of human nature.

One of the themes that has constantly re-occurred in your work is cross-national comparison. It seems to me that one has to look at different societies in order to see the importance of the social and historical context. If you look at only one, it can be taken for granted. That may be a positive result of this globalization.

You have two kinds of history - apart from the other classification that we talked about - that is, local history and universal history. A lot of the history of psychology until now has been local history and clearly a lot of that is going to continue. But the hope is that, increasingly, it will be possible to build a more universal kind of history that doesn't privilege one particular local set of developments but which is comparative in its approach and attempts to arrive at the bigger picture (36).

History is not your only interest. You have taken an active role in the International Society for Theoretical Psychology (37). The programme here at York is in 'History and Theory of Psychology'. How do you see the relationship between the two? I ask because they seem to be going their separate ways - particularly in the United States where the American Psychological Association has two separate divisions (38). Do you see them as closely related?

It depends on how one does history and how one does theory. If one does history as antiquarianism, if one does history as celebration, if one does history as local history, one can do it without getting involved in theoretical issues. But if you think of history in more ambitious terms, then there is no way that you can avoid the link with theory. On the other side of that relationship, if you perceive theory without the historical dimension, you are constantly in danger of re-inventing the wheel - and it happens.

As part of your commitment to pursuing history and theory from within psychology, you played a central role in establishing the graduate programme at York University. That seems to have started around the same time that history and theory of psychology became your main interest.

Some years later actually. As I said, I would date my own involvement with that area as my primary interest to the mid-70's. It wasn't until the early 80's that this option within the graduate

programme at York was inaugurated. By then, I had become fully committed to that line of work and was publishing in it and so on. We had a commission that looked at our graduate programme around that time. In discussing the strengths and weaknesses of the programme, they made special mention of our strength in that area. That made us reflect a bit, and I thought why not formalize that strength. Of course, it wasn't only me. There were people like David Bakan and Ray Fancher, and others (39). It was generally thought to be a reasonable idea and so we went ahead with it.

You were saying a few days ago that you were surprised at the way in which it has grown over the last 10 years or so.

At the time, of course, we had no idea what would happen. Nothing might have happened. We didn't know if we would get any students, how many, and so on. But it has shown itself to be viable and we have had good students, who have done interesting work.

We spoke earlier about the way in which the field has grown over the last few years and yet this doesn't seem to have been followed by institutional support. For example, the graduate programme in History and Theory of Psychology at the University of New Hampshire was established in the late-60's. This is the only one in North America that has emerged since then.

That has some relationship, of course, to the wider context. This intradisciplinary development hasn't come at the best time as far as external sources of support are concerned. If we had thought of doing this in the mid-60's, then maybe things would look different now.

Maybe we are seen as something like champagne or expensive perfume - a luxury that has to go when times get hard economically.

It's difficult to establish what are perceived as luxury items in times of rigorous budgetary restrictions and controls.

Do you think that historians of psychology could have done a much better job in convincing psychologists of the utility of their work - to try, for example, to overcome the view that history is antiquarianism or celebration and irrelevant to the central concerns of the discipline?

Certainly there's room for improvement in that direction. There's no question about that. I think that, in the past, there hasn't been as much of that as there might have been because historians of psychology have lacked self-confidence. Knowing that they are regarded as a luxury item - a dispensable item - they have perhaps been timid about putting forward the view that they can make a significant contribution.

The appearance of professional historians of science who do work on the history of psychology has changed the picture quite remarkably - in the sense that psychologists who do this kind of work can see professionals who have this as their main area of specialization. It's certainly not as high-status as the history of physics but at least it is seen as legitimate research (40).

That's right. And also, of course, it makes for greater exposure to the work, and the literature, in the history of science, the philosophy of science, and the sociology of science. Knowledge about these developments can only increase the self-confidence of historians of psychology.

That's a very good note to end on. Thank you for agreeing to give the interview.

ACKNOWLEDGEMENT

I would like to thank Kurt Danziger for checking the written transcript and for helping me to locate several references.

NOTES

(1) Now Wroclaw in Poland.

(2) Experimental psychology came very late to the University of Oxford. Its Institute of Experimental Psychology was not established until 1947.

(3) Humphrey was a British psychologist who spent much of his career in the United States and Canada. He returned to Britain in 1947 to head the Institute of Experimental Psychology at the University of Oxford (Zusne, 1984).

(4) See Ryle (1949). Ryle was only one of several philosophers at Oxford who were critical of psychology. Much of the criticism can be seen as part of a territorial dispute. Experimental psychology may have come so late to Oxford because of opposition from the philosophers.

(5) See Humphrey (1951). This work contains what is still the most comprehensive account of the Würzburg School of 'imageless thought' in English.

(6) For an example of this work, see Danziger (1953).

(7) See, for example, Danziger (1958).

(8) G. W. Allport (1954). See also G. W. Allport (1956).

(9) F. H. Allport (1924).

(10) See Foster and Louw-Potgieter (1991).

(11) Allport briefly mentions the visit to Natal in his autobiography: "my own psychological bias had perhaps led me to underestimate the forces of history and of traditional social structure ... in South Africa" (G. W. Allport, 1967, p.20).

(12) This term refers to the ordinary explanations of behaviour that are used in different cultures (Heelas and Locke, 1981).

(13) See, for example, Newton-Smith (1982).

(14) German sociologist who is generally regarded as the central figure in the development of the sociology of knowledge. His most famous work is entitled, *Ideology and Utopia* (Mannheim, 1936).

(15) Danziger (1963a). See also Danziger (1963b). This work was extended by researchers at the University of Cape Town in the 1980's (Du Preez, Bhana, Broekmann, Louw and Nel, 1981; Louw, 1983; Du Preez and Collins, 1985). The work continues to generate interest. According to Nelson (1992): "The most satisfactory analysis of contemporaneous thought styles to date has been Danziger's studies of styles of thought in South Africa" (p. 30).

(16) Danziger (1971b).

(17) Danziger discusses the various techniques of police interrogation - and why they work - in the first chapter of his book, *Interpersonal Communication* (1976).

(18) Danziger (1971c).

(19) See Boring (1950). This work was regarded as the authoritative account of psychology's history for many years. Danziger was one of the first writers to point to its many flaws.

(20) Tradition has it that Wundt established the first laboratory of experimental psychology at the University of Leipzig in 1879. The American Psychological Association designated 1979 as psychology's 'centennial' year and the 22nd International Congress of Psychology was held in Leipzig to mark the occasion. A great deal of literature on Wundt appeared around this time (e.g. Bringmann and Tweney, 1980; Rieber, 1980).

(21) For an example of this, see Miller (1966): "Wundt's genius was of the kind that Thomas Edison described - one per cent inspiration and ninety-nine per cent perspiration" (p.39).

(22) Danziger (1979a). See also Danziger (1980a, 1980b).

(23) This is the topic of Danziger's book, *Constructing the Subject* (1990a). See also Danziger (1985a, 1989).

(24) Danziger (1979b).

(25) Danziger (1987).

(26) Danziger (1985b).

- (27) See, for example, the preface to *Constructing the Subject* (1990a, pp. vii-ix). Danziger has also published in journals devoted to the history, philosophy and sociology of science (e.g. Danziger, 1984, 1985b, 1991, 1992).
- (28) See, for example, the interview with H. J. Eysenck in Cohen (1995). Eysenck has relentlessly pursued a natural science approach to psychology.
- (29) For an example of this type of work, see Danziger (1990b).
- (30) This term refers to historical work which has no relevance to present-day concerns. The antiquarian is similar to the person who climbs a mountain 'because it is there'. This kind of work is widespread in historical writing by psychologists - possibly because of the empiricist assumptions of the discipline.
- (31) Celebratory history chronicles the 'great achievements' of the past. It is often written to commemorate centennials and other anniversaries. For an introduction to this topic, see Hilgard, Leary and McGuire (1991).
- (32) Danziger (1984) has outlined a conceptual framework for a "critical history of psychology".
- (33) Danziger (1994).
- (34) See, for example, Danziger (1993).
- (35) Historical psychology has been growing in Continental Europe in recent years (e.g. Staeuble, 1991, 1993). There is some interest in this field in English-speaking countries (e.g. Gergen and Gergen, 1984) but it is very small compared to the interest that exists in Germany, France and the Netherlands.
- (36) Danziger has often pointed out that much of what we take to be 'the' history of psychology is actually the history of *American* psychology (e.g. Danziger, 1991, 1994). He will return to this topic at the 26th International Congress of Psychology in a paper entitled, "Towards a polycentric history of psychology".
- (37) See, for example, Danziger (1988).
- (38) Division 24 is devoted to 'Theoretical/Philosophical Psychology' and Division 26 is devoted to 'History of Psychology'. There is very little interaction between the two.
- (39) David Bakan and Raymond Fancher are both prominent figures in history and theory of psychology (e.g. Bakan, 1966, 1969; Fancher, 1985, 1990). Together with Kurt Danziger, they formed the backbone of the History/Theory Option within the Psychology graduate programme at York University for many years.

(40) History of Science departments have traditionally been heavily focussed on the natural sciences. It is only fairly recently that historians of science have begun to do research on the history of psychology (Danziger, 1994).

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