

Presentation of Kurt Danziger with a Lifetime Achievement Award

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First of all, let me point out that Kurt Danziger will not be attending this session. Today is his 85th birthday. Although he is in good health for a person of 85, few people reach that age without it having some kind of the effect on their health. When the news of this award was conveyed to him, he said that his ability to attend would depend on his health. In spite of his non-attendance, he is very pleased to receive the award and has given me a short paper to read out. Before doing that, I would like to give a brief summary of his career. The remaining time in the session will then be used for questions and comments from the floor. Let me also draw your attention to the certificate that will be presented to Kurt.

Kurt Danziger was born in Germany exactly 85 years ago today. When he was six years old, a momentous event occurred: the Nazis came to power. As it became increasingly obvious what they were about, Danziger's parents did what many other Germans did around this time: they emigrated; in this particular case, to Cape Town, South Africa. There he later took a degree with distinction in chemistry but rather than pursue a career in chemistry he switched to psychology. He shared a general feeling at the end of the Second World War that science had been successful in helping us to understand the natural world and that it should now help us to understand the social world (Danziger, 2009).

At the University of Cape Town, Danziger came under the influence of Hullian neo-behaviourism. He was due to do a Ph.D. in Iowa but a chance meeting with a social anthropologist from Oxford led him to go there instead. His doctoral research involved standard 1940s experimentation using laboratory rats (e.g. Danziger, 1953), though he became increasingly disillusioned with this type of work. An important influence was the ethologist, Niko Tinbergen who was teaching in Oxford at the time. Danziger came to realise that the neo-behaviourist search for universal laws of behaviour would prove fruitless.

He now accepted a position at the University of Melbourne, Australia. At that point, his research was influenced by the work of Piaget who had visited Oxford during Danziger's time there. But whereas Piaget had largely concentrated on the child's understanding of the natural world, Danziger attached more significance to the child's developing understanding of the social world and studied such topics as children's understanding of kinship terms and of economic relations (e.g. Danziger, 1958).

His return to the politically charged atmosphere of South Africa led to a concentration on social psychology. He now began to analyse highly divergent expectations for the future collected from different social groups in South Africa. Except for a two-year break as a visiting professor in Indonesia, this work continued until Danziger's migration to Canada.

In Indonesia, Danziger's mandate was to teach Western-style psychology. He relates an encounter with an older professor who was teaching a local form of psychology called, "ilmu djiwa", which roughly translates as "science of the soul". It was virtually impossible for them to engage in dialogue about these different approaches since they had no concepts or categories in common and this brought Danziger to the slightly disconcerting conclusion that

the concepts and categories of Western psychology also reflected their cultural-historical origins (Danziger, 1997).

In 1960, Danziger returned to South Africa to take up an appointment as Head of the Psychology Department at the University of Cape Town. His work in social psychology had always shown strong sociological influences. Karl Mannheim, a central figure in the development of the sociology of knowledge, had titled his best known work *Ideology and Utopia* (Mannheim, 1936) and in a clear allusion, Danziger's most important work of this period was called, "Ideology and Utopia in South Africa" (Danziger, 1963). Its empirical basis was provided by future autobiographies and future histories written by members of the different "racial" groups of South Africa. It remains one of the few examples of linking an empirical approach to the theoretical framework of the sociology of knowledge.

Danziger's time at the University of Cape Town was also one of political activism. For some years, he had been one of the few white members of the "Congress Alliance" that later merged into the African National Congress, the South African liberation movement. Among other things, he became involved in forensic psychology. Many opponents of apartheid were being tried for plotting the overthrow of the regime based on the testimony of witnesses who had been held in solitary confinement for long periods. In some of these trials, Danziger provided expert evidence on the questionable reliability of such testimony. At the request of the defence, he was also in court to give such evidence in the famous Rivonia Trial at which Nelson Mandela and other top leaders of the ANC were given lengthy prison sentences but the judge refused any presentation of this evidence. These activities led to ominous public attacks on Danziger by the minister of police and covert attacks by white vigilantes. Eventually, he was allowed to leave on condition he never came back.

Danziger arrived in Toronto in 1965 to take up an appointment at York University. It was not possible to continue the research that he had done in South Africa but his interests in developmental psychology and social psychology combined to produce a widely read textbook titled, *Socialization* (Danziger, 1971) that was translated into several languages. His interest in police interrogation methods in South Africa led him into the area of interpersonal communication and he also published a book on this subject (Danziger, 1976).

Danziger's move into history of psychology began in 1973. He had never stayed anywhere long enough to earn a sabbatical and now had one for the first time in his career. One of his plans for the sabbatical was to read the original works of some of the early figures in the history of psychology, such as Helmholtz, Fechner and Wundt. He developed a strong interest in Wundt, which was fortuitous since there was enormous interest in Wundt around that time to coincide with the 100th anniversary of the establishment of Wundt's laboratory in Leipzig in 1879. In papers such as "The Positivist Repudiation of Wundt" (Danziger, 1979) and "The History of Introspection Reconsidered" (Danziger, 1980), he helped to correct some common myths about Wundt's psychology.

Danziger's interest in the social psychology of the psychology experiment led him to examine not only Wundt's ideas but also the methods that were used in his laboratory. He found to his surprise that these methods were radically different from the methods that were subsequently adopted by the discipline. For example, the role of the research participant was frequently considered more important than that of the experimenter and this situation was reflected in the fact that the research participant was often a person of higher status than the experimenter. More often than not, however, the two roles were interchangeable. Another

striking difference was that data from single subjects rather than statistical averages were used.

Given that psychology had a number of competing models for conducting research, the question arises as to why it adopted a particular model as opposed to one of its alternatives. Here Danziger came up with that memorable phrase, "marketable methods". The methods of mainstream psychology were adopted because they produced knowledge that would be of interest to the agencies that had an interest in controlling behaviour and the means to fund psychological research. As Danziger has pointed out, the people who are studied by psychologists and the people who fund their research are rarely the same (Danziger, 1987). These results were published in Danziger's book, *Constructing the Subject: Historical Origins of Psychological Research* (1990). One reviewer described it as "the most important book on the history of psychology to come along in years" (Stam, 1992; p. 629).

In spite of this emphasis on the history of psychological methods, or psychological practices as Danziger prefers to call them, he had defined his interest in terms of "the history of psychological objects" as early as 1981 (Danziger, 1981). Even before the publication of *Constructing the Subject*, he had already started the work for his next book, *Naming the Mind: How Psychology Found Its Language* (Danziger, 1997). Here he examines the history of the basic psychological concepts like "motivation", "behaviour", "intelligence", "learning", "personality" and "attitude". Perhaps the most surprising thing about them is how historically recent they are. None are older than the 19th century and some are products of the 20th century.

Danziger's next book, *Marking the Mind: A History of Memory* (2008) is a continuation of this work. Not all the concepts of psychology are of recent origin and "memory" is one of the oldest concepts that has been in continuous use. It can be found in the earliest texts of Ancient Greece, though it has changed over time. For example, in pre-literate societies, stories are rarely told the same way twice, just as music is rarely played the same way twice. The issue of accuracy in memory only arose when there was some kind of record with which it could be compared. The notion of pathology in memory was an even later development.

Danziger's work on the history of psychological practices and his work on the history of psychological objects are not to be seen as separate histories. He repeatedly shows how psychological objects are related to psychological practices. For example, the prominence of the psychological notion of "intelligence" was connected to the practice of educational testing, just as the notion of "motivation" was connected to certain managerial practices. Different concepts of "memory" were connected to different mnemonic practices.

Danziger's views on the relationship between psychological objects and practices can be found in an article titled, "Psychological objects, practice and history" (Danziger, 1993). It forms part of a group of works on historiography. Many of these works were collected together for a free web book titled, *Problematic Encounter: Talks on Psychology and History*, which was made available in 2010 (Danziger, 2010).

Any overview of Danziger's career would be incomplete if it did not include a mention of his role in establishing the graduate programme in history and theory of psychology at York University, a role for which he received the Canadian Psychological Association's Education and Training Award in 1994. It continues to be the biggest programme of its kind in the

world and the field would be considerably poorer without the contributions of the faculty, the students and the former students from this programme.

I said at the outset that Danziger would not be with us today because of his health problems. Another aspect of old age is that one's energy levels begin to decline. In spite of this, he continues to work. He is currently writing a chapter for a book on the psychology of persons. He has also written a short paper for this meeting which I will now read out.

Note: The paper by Kurt Danziger referred to here begins on the next page.

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Hyper-Loops or Ten Minute Musings from the Rocking Chair

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First, let me express my appreciation for an award that is particularly dear to me, coming as it does from colleagues with whom I have shared so many interests and concerns over so many years. Unfortunately, the physical limitations of old age prevent my active participation in gatherings, such as today's, at which these interests and concerns continue to be discussed. But that does not mean that they are no longer important to me, and I thought I might return to your midst in spirit by using this opportunity to share some musings about a rather significant problem that has been with us since the formation of this section. It is the problem of our somewhat precarious existence within the discipline of psychology.

I am sure nobody here needs convincing that there is indeed a problem. Everyone will have encountered colleagues who are at least puzzled by our claim to have a legitimate home within the discipline, not to mention other colleagues who explicitly reject this claim. Many of us have encountered pressures to restrict our presence, whether in the area of teaching or research. Some of us, including myself, have discussed the problem in print, but there has been no resolution; from what I hear, the problem seems to have become more acute over the years.

Our legitimacy within the discipline may be questioned for various reasons, but here I want to concentrate on one reason that is very frequently encountered, namely, the apparent irrelevance of our work for contemporary psychology, for the questions psychologists are interested in and for their day to day work in grappling with these questions. On our part, the natural response has been to point to the historical continuities that do in fact link the present to the past.

But we would not be able to construct such a response if our historical studies were not informed by certain metahistorical perspectives. Pure history can only tell us what happened once upon a time. To point out a link between past and present you have to adopt a perspective that incorporates both the time of the past and the time of the present. It is possible to do this quite superficially, for example on the basis of a continuity of place. You can reveal perhaps that "William James slept here". Unfortunately, some of what passes for history of psychology has not gone too far beyond this level.

More interesting links between past and present require a more significant perspective than continuity of place. One example would be a metaphysical perspective that posits transhistorical entities, such as ideas, which persist over time. There are other ways of constructing interesting historical relationships, but they all require the adoption of some philosophical, or at least theoretical, perspective. That is a major reason for linking history with theory or philosophy on a pedagogical and an institutional level. It is what many of us had in mind when we set about establishing the programme at York and also this section of CPA. The "and" that occurs between history and either theory or philosophy was not meant to indicate a merely additive connection but one that was intrinsic.

But we have to recognize that when we pursue these kinds of studies within a department of psychology we open up a pretty deep gap between what we do and what our colleagues normally do. That gap has to do with the objects of our scientific attention. What most of us share with our mainstream colleagues is a commitment to the subject- object distinction that is foundational for both the natural and the human sciences. The whole enterprise depends on the distinction between a group of subjects, who are specially qualified experts, and a set of separate objects investigated by those experts. The objects range from physical particles to the movement of human populations, but the distinction between physicists, demographers, or psychologists and the objects of their investigation remains fundamental.

The objects investigated by mainstream psychologists are psychological reactions and attributes of, usually human, individuals who may be students, members of clinical populations, children or what have you, but in any case, not psychologists. Contrast this with the situation of those who specialize in the history/theory of psychology. The objects of their studies are usually other psychologists and the products of their work rather than members of the lay public. This rather fundamental divergence in our objects of study may be sufficient to lead to a certain estrangement.

Mainstream psychologists are not accustomed to finding themselves in the position of objects for scholarly investigation – they expect to be the subjects that guide such investigations. True, in most cases contemporary psychologists become the objects of scientific regard only by implication: the direct objects of investigation are their forebears, the people who founded their fields and established traditions of work that have survived. But when your clan history becomes an object of study your own historical identity is no longer something to be proudly taken for granted but an object vulnerably exposed to someone else's analytical dissection. This is not a comfortable situation.

And that brings me to the crux of my argument. In the human sciences subjects, that is investigators, and their human objects are linked by loops of mutual influence that are unknown in the natural sciences. This can happen on an interpersonal level, when it becomes part of the social psychology of psychological experiments, or it can happen on an institutional or sub-cultural level, as in the case of a sub-discipline that potentially takes the entire discipline as its object. We know that in experimental situations we encounter both acquiescence and resistance (as in cheating or non-cooperation) among those who are objects of investigation. But of course experimenters also modify their approach in response to their scientific objects, or as we misleadingly call them, their experimental subjects. That is why we have pilot studies.

Now how do these loops of mutual influence play out on the level of disciplinary culture? One big difference between the two levels involves a reversal in the relative power of subjects and objects. In experimental situations those who conduct the investigation have considerable power, those who are the objects of investigation much less. By contrast, those who wish to take aspects of disciplinary culture as their object of study have little power in relation to representatives of that culture. Among the latter, reactions of acquiescence are therefore less likely than those of resistance. These can take the form of challenges to the legitimacy of sub-disciplines such as ours, disagreements about sub-disciplinary boundaries, and so on.

How do we meet these challenges? In the past, I have suggested we rely on the fact that psychology is not in fact a unified discipline but rather a fairly loose set of rather diverse sub-disciplines, some of which may even see us as an ally. Today I want to allude very briefly to another aspect of this problem, the pedagogical aspect.

Traditionally, historical perspectives were permitted a certain role in the psychological training curriculum, even when research founded on such perspectives was denied any real legitimacy within the discipline. Our teaching role was perhaps the source of what little strength our voice did have. I know that this tradition is fading, but before all trace of it is lost, it might be a good idea to pay more attention to pedagogical issues in our fight for survival.

When I say "pedagogical issues" I am not only thinking of classroom teaching but broader issues involving the formation of professional identities in psychology both now and in the past. Here we might well benefit from some recent trends in science studies that have switched from questions about the production of scientific knowledge to questions about the production of scientists. How are skills transferred from one generation of experts to another? How are professional norms internalized? What can we learn from an examination of divergent pedagogical cultures? What do changes in the style and content of textbooks tell us about their users? These kinds of question have not received the attention they deserve in studies of the history and theory of psychology.

Quite apart from their intrinsic interest and importance, I could imagine that such studies might help to build bridges between historians and practitioners in relatively new ways. For pedagogical practice in the training of psychologists depends much less on precise scientific knowledge than on historical experience, theoretical perspective and normative preference. And those are issues for which the expertise of members of this section can hardly be regarded as irrelevant.