# CONFESSIONS OF A MARGINAL PSYCHOLOGIST

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## *Lehrjahre* – Years of learning

My formal introduction to the discipline of psychology was the result, not of hopeful enthusiasm, but of purely pragmatic calculation. In 1945 I was a dedicated student of chemistry at the University of Cape Town in what was then the British Dominion of South Africa. I was set on a career as a research scientist that would require further years of study in my chosen field. As the child of parents who had arrived in the country as penniless refugees from Nazi Germany not many years before, I was however dependent on scholarship money to continue my scientific training. But scholarship money, at that time and place, was extremely scarce. Only those with the very highest grades had any hope of qualifying. I had been the class medallist in chemistry, but I was now about to prepare for specialization in biochemistry and wanted to minimize demands from other courses that I regarded as mere distractions from my main task. So I asked around about any "soft options" of which I might avail myself. The consensus among my fellow students was quite clear: Psychology was unquestionably the softest of the soft options on offer. And so I enrolled in the introductory psychology course with every intention of keeping my acquaintance with the subject brief and uninvolving.

The contents of the course gave me little cause to change my mind. As I realized later, they probably had not changed in twenty years and hardly reflected the hopeful new trends that had characterized the field during the twenties and thirties of the last century. There was old fashioned sensory psychology, but, like so many students before and since, I thought that psychophysics was just about the most boring, pointless subject I had ever come across. It took me about four decades to revise that opinion. Apart from Fechner, it was the figure of William McDougall that loomed large. Indeed, his *Introduction to Social Psychology* of 1908 vintage was still a required text in these outer reaches of the British Empire. As a science student, I was unimpressed by the quality of the empirical evidence, but I was intrigued by the theory of the sentiments that McDougall had taken over from Shand. It reminded me a little of the structural models of complex molecules that were so useful in chemistry. Maybe this was the kind of theorizing that might one day provide the foundation for a scientific approach to psychology? However, I felt no inclination to treat such playful thoughts seriously. At the end of the year I took my leave of psychology as intended, preferring the firm ground of *real* science to the dreams of would-be, one-day, maybe science.

But a year later I was back, and this time for good. The reasons for that reversal had little to do with the relatively narrow content of psychology as I knew it and everything to do with complex matters for which that much abused term, *Zeitgeist*, still provides the most serviceable shorthand I can think of. Everywhere, the end of World War II marked, not only the end of a nightmare, but also a new beginning, an opening up of possibilities that had previously seemed unrealistic. For many, the mere possibility of a return to "normality" was extraordinary enough, but for those of us whose entry into adulthood coincided with this historical moment it was not a return that was on the agenda but a new construction. This was a time of great fluidity, socially and politically of course, but also intellectually. Old moral certainties were being exposed as dangerous delusions, and if one was young enough, the challenge of "year zero": building a better world, could be experienced as very real.

Manifestations of this general sensibility would take many different forms depending on individual circumstances. In my own case there was an upsurge of interest in matters that took me a long way beyond the rather single minded fascination with natural science that had marked my adolescent years. Not only did I follow current social developments with an ever greater sense of involvement, but the application of a scientific approach to the social as well as the natural world began to seem both urgent and feasible. My acquaintance with any social science had been limited to that pitiful first course in psychology, but gradually the all too evident shortcomings of the subject began to seem more and more like a challenge. Was psychology now at the stage chemistry had reached when it emerged from alchemy? An exciting prospect, especially when entertained against a background of chemical work that was losing some of its fascination as it became more routinized.

I had loved the sense of things falling into place, both theoretically and practically, that the study of chemistry had provided – a sense beautifully recaptured in the reminiscences of other ex-chemists of my generation (Levi, 1984; Sacks, 2001). But with the consolidation of my grasp of chemical laws, principles, and models, and with my growing facility in the tasks of the laboratory, the whole enterprise seemed to be dissolving into an assembly of specialized projects of limited scope. These could still be fun, but the grand vision had gone, to be replaced by a sense of filling in the missing parts of a structure whose basic design had already been decided on. Had circumstances forced me to persist I would probably have learned to appreciate the intellectual rewards that work in an established science offers. But circumstances were quite otherwise, as I have tried to indicate. This seemed to be a time for bold new beginnings, specifically for extending the scientific spirit to knowledge of human affairs. In this direction the excitement of pioneering work beckoned. Here one could hope to be an architect rather than a mere plasterer.

Accordingly, after completing my chemistry degree with the kind of result that opened up funds for further studies, I confounded expectations by using the time so gained to immerse myself in subjects quite remote from the path I had hitherto followed. Had I found myself at a different institution at this point I might well have taken up the study of sociology, but at my University at that time this subject was perceived as little more than a training ground for social workers. In the meantime I had also read enough psychology on my own – Woodworth's popular introductory text, for example – to realize that there was more to modern psychology than McDougall and friends. I spent the best part of two years catching up with the state of the discipline at the time.

In view of where I was coming from it is hardly surprising that what I found most interesting were the attempts at developing universalistic generalizations on the basis of quantitative data. I was introduced to these attempts in two forms, a British form derived from Spearman that employed correlational techniques, and an American, neo-behavioristic, form that relied on animal experimentation. The latter not only seemed to be closer to the understanding of science I had brought with me from my chemistry days, it also seemed to share my goal of improving the human condition by the application of science to human affairs.

At the University of Cape Town this approach was represented by James G. Taylor, one of the few non-American psychologists to embrace behaviorism early and passionately and to make his own significant contribution to it. When Hull's neo-behaviorist system took shape Taylor adopted it enthusiastically and carried on a correspondence with Hull over a period of years. My own introduction to learning theory took the form of a step by step exposition of Hull's *Principles of Behavior* (1943) which was then considered to be at the cutting edge of psychological science. The last thing Taylor could be accused of was eclecticism. When eventually I became aware, through my own reading, of other systems of neo-behaviorism and asked him if we could be told something about these he declined to do so himself, suggesting that if I considered it important he would give me class time to do so in his stead. Certainly, there was something impressive about the pseudo-Newtonian elegance of Hull's system of behavioral axioms and corollaries when expounded by a disciple such as Taylor, whose logical and mathematical sophistication manifestly exceeded those of the system's founder. With my background, I greatly respected these qualities and clung to them for a while even after I had realized that the whole structure was built on sand.

A further very appealing element in Taylor's version of Hullian neo-behaviorism was its preoccupation with the application of its basic principles to broad areas of psychological theory and practice. Having acquired a thorough knowledge of what was considered to be the basic science, Taylor's graduate students were introduced to models for the application of that science in three major fields: the psychology of perception, behavior therapy, and social psychology. Only the first of these ever resulted in a major publication (Taylor, 1962), and that many years after I heard him develop the outlines. This aspect of his work still has some interest (Wetherick, 1999). His pioneering role in the field of behavior therapy was largely enacted behind the scenes. His ideas concerning social psychology hardly left the seminar room, though at the time I knew him they were particularly dear to him. Taylor was not only a behaviorist, he also considered himself a Marxist, and one of the humanist variety at that. The earlier work of Erich Fromm came in for the same careful exposition as that of Hull, and somehow Taylor, the intellectual juggler, managed to keep both these balls in the air at the same time. Only much later, after some first-hand acquaintance with American behaviorism, did I realize that, for all its apparent orthodoxy, Taylor's understanding of behaviorism deviated subtly yet deeply from the original.

During my student days, however, Taylor's idiosyncratic blend of apparent scientific rigor and social interest suited my own inclinations exactly. His presence and example certainly facilitated my decision to cut my ties with chemistry and to pursue a career in psychology instead. The first step was the completion of a Master's degree, and this entailed my first foray into psychological research. It was not difficult for me to decide that my topic would be in the field of experimental social psychology, although my Department actually had no experimental tradition and any interest in social psychology was purely theoretical. But, still thinking of myself as very much a scientist, it was unthinkable that my research would be anything other than experimental. Social psychology it had to be because it was an interest in the possibilities of social science that had brought me into psychological research in the first place. Fortunately, I was allowed to do as I pleased.

At that time, the experimental demonstration of the formation of group norms by the Turkish-American psychologist M. Sherif (1936) was widely regarded as one of social psychology's most significant experiments. Sherif made use of the *autokinetic phenomenon*, the fact that a small point of stationary light is generally perceived to move when looked at for a time in a totally dark room. He asked subjects, who did not know that the light was stationary, to estimate the distance of its movement. When two or three subjects heard each other's estimates in the same room there was an unmistakable tendency for these estimates to converge around some apparently consensual value, the emerging "group norm". This apparently spontaneous human tendency for consensus formation in ambiguous situations could be used to carry a considerable theoretical load.

Unfortunately, I failed to replicate the phenomenon. My subjects, psychology undergraduates, showed no tendency to adjust their estimates to those of others calling out their estimates at the same time. When I discussed possible reasons with Taylor, he said: "You know, the Americans pay their subjects". I remember being shocked by the idea that a student would have to be paid for being given the chance to advance the cause of science, let alone that this would be a standard practice. Moreover, I had no funds to provide meaningful rewards to white South African students from mostly very affluent backgrounds. But an alternative was at hand. I now recruited my subjects from among the colored service workers, whom the University employed at pitifully low wages, so that they appreciated even tiny monetary rewards. Indeed, their experimental performance showed exactly the converging pattern that Sherif had found.

Obviously, my two experimental groups differed in many respects other than the variable of monetary reward, so one cannot draw firm conclusions. But I never forgot the lessons of this first adventure in psychological research. The "control" of potentially relevant factors was clearly a vastly more difficult matter in social psychological experiments than in chemical experiments. Compared to the incisive techniques available in the physical sciences the manipulations at the disposal of an experimenter in the social sciences were incredibly crude. Chemical experiments only worked properly with purified substances, but in the social world there were never any "pure" materials. As a consequence, one's best efforts as an experimenter were likely to produce a messy combination of effects, most of whose components remained hidden from view. This made any interpretation of the meaning of one's results extremely tentative, at best. Moreover, it was clear that social experiments could not easily be transplanted from one socio-cultural environment to another without thereby introducing significant change in the experimental conditions. In the course of time, this deeply learned lesson led to the question of whether the socio-cultural environment was not always a crucial, though generally unrecognized, part of the experimental conditions.

But I was not ready to pursue such questions at that time. It simply seemed to me that a direct experimental assault on social behavior was perhaps premature, that the indirect route advocated by learning theory was therefore more promising: One should first establish the basic "laws of behavior" by experimenting on sub-human organisms and then, when one had firm scientific ground under one's feet, one could investigate the application of these laws to human social behavior. Such ideas, entirely orthodox at the time, provided the framework for my doctoral research. My previous experiment, as well as my reading of the learning literature, had convinced me of the importance of motivational factors. I therefore became interested in studying these factors at a sub-human level, and, being now at least theoretically immersed in the sub-culture of American learning theory, there could be little doubt that my organism of choice would be the laboratory rat. In fact, Taylor had already arranged for me to do my doctoral studies with Kenneth Spence, Hull's right hand man, in Iowa.

But no sooner had I been placed on this very well defined track than I was jolted out of my complacency. The jolt was administered by Meyer Fortes, an eminent British social anthropologist, then at Oxford and later at Cambridge. Fortes, who was just then on a visit to Cape Town, had started academic life there as a student of psychology. He then went to London to study with Spearman but had subsequently found work in West Africa that led him into Social Anthropology. His beginnings were therefore somewhat similar to mine and he took a fatherly interest in my plans. He was frankly shocked by the idea of Iowa and very appropriately pointed out to me that there was more to being a graduate student than perfecting one's technical competence. He suggested that my academic record would make me quite acceptable at Oxford, which, as he hardly needed to point out, could boast of one or two advantages over Iowa. I did not need much persuading, as I was not entirely happy about having to adjust to an academic environment that sounded rather too regimented for my taste. In any case, apart from my interest in learning theory, my intellectual world was very much oriented towards Europe, and from my Euro-African perspective America seemed a strange and alien place.

When I arrived in Oxford early in 1949 its Institute of Experimental Psychology had barely been established. Housed in a converted residential property it boasted fewer facilities than I had had at my disposal in Africa. George Humphrey, the Head of the Institute, had recently returned from Canada where he had done work in the area of learning and published a thoughtful text on that topic in 1933. However, by the time I met him he had clearly lost interest in the area and told me quite frankly that his supervision of my doctoral research would be little more than nominal. This did not bother me too much, as I was quite happy to push ahead on my own. Much later I discovered that I had missed out on a wonderful opportunity because Humphrey was one of the very few psychologists who had been actively engaged in historical studies. These led to the publication of his excellent book, *Thinking* (1950), while I was nominally his student. But historical work was very far from my mind at that time, and even if Humphrey had been less modest about his own interests he would have found me a less than receptive audience.

No, I was quite determined to pursue the line of behavioral experimentation I had decided on. The trouble was the Institute had no animal laboratory at that time. But this was not necessarily the end of the road because there were several animal laboratories in Oxford serving the biological sciences. So Humphrey provided introductions to some of them and eventually the Laboratory of Human Nutrition agreed to let me have a corner for my work and some laboratory rats. Of course, any experimental apparatus I would have to build myself, which is why my experiments featured a simple runway.

These arrangements suited me very well. I had already had some experience of starting from scratch when I embarked on social psychological experimentation in Cape Town, and as it turned out, I had to do it again in my first academic job after graduating where I was expected to establish an animal laboratory *de novo*. But this kind of activity confirmed for me that I had done the right thing in switching to psychology from chemistry. If psychology provided opportunities for pioneering work on the practical as well as the theoretical level, so much the better. At least there was no danger of becoming bored by repetitive tasks.

My admiration for the conceptual universe of learning theory did not survive my years at Oxford. The corrosion started from an initial skepticism regarding some of the specific *content* of Hull's system, much though I respected its form, its scientific ideals and the empirical practice to which it was tied. I had never felt convinced by the physiological reductionism of Hull's theory of motivation, nor by its postulation of drives as separate entities reminiscent of McDougall's instincts. In a series of experiments, resulting in my first psychological publications, I was able to demonstrate that, even in rats, there were sources of motivation which depended on central processes rather than on so-called primary drives, and that the latter, i.e. hunger and thirst, did not operate independently of one another.

By themselves, such experimental findings need not have upset the Hullian apple cart beyond requiring the replacement of some specific hypotheses by different ones. But in the course of my reading and research I became convinced that the model of behavior implied by the

Hullian postulates was fundamentally wrong. In essence, this was a mechanistic stimulus-response model that Hull shared with many other behaviorists. As an alternative, I presented a different model in the theoretical part of my dissertation, one which incorporated concepts of feedback and cybernetic regulation. These ideas were very much in the air at the time and my interests were shared by one of my fellow students, Anthony Deutsch, who was developing his own approach to psychological model building (Deutsch, 1960). For me, however, this turned into another road not taken.

One reason for this had its origins in another Oxford influence. While I was working on my doctoral research I heard about the lectures on animal ethology that Niko Tinbergen was giving in the Zoology Department. When I attended them and read the relevant literature it felt as though the rug had been pulled from under my feet. The whole enterprise of experimenting on laboratory rats in order to generate and verify general “laws” of organismic behavior, applicable also on the human level, no longer made any sense.

I had already begun to feel uncomfortable about the way behaviorism either ignored the physiological basis of behavior or, in the case of stimulus-response psychology, adhered to a hopelessly discredited physiology that was at variance with contemporary physiological research. Now, in the light of the ethological studies of animal behavior in natural environments, it became apparent that behaviorism was also at variance with some of the fundamental principles of evolutionary biology. It had disastrously underestimated the difficulty of cross-species generalization and had replaced the comparative, evolutionary perspective of the biologist with abstractions that were quite inappropriate in a living context. There would never be any psychological “laws” in the behaviorist sense because behavior as an attribute of an abstract organism did not exist. What existed were members of different biological species whose behavior represented adaptations to particular natural environments. Studying that behavior by employing invented environments and arbitrarily chosen species might have its uses in elucidating specific mechanisms, but outside the laboratory these would always be operating in specific contexts for whose analysis the concepts of behaviorism were hopelessly inadequate.

I now had to recognize that the approach I had turned to as the great hope for the application of science to human life was in fact a travesty of science. By the time I finished writing up my dissertation I was no longer committed to the approach it represented, and I knew I would not return to this sort of project. Although my exposure to Tinbergen and ethology had provided the coup de grace there had been other experiences that had sapped my confidence in the significance of the work I was engaged in. I had imagined this work as supplying the basic science that would one day be applied to genuinely important problems of human social life. But the more I learned about alternative approaches to these problems the more inappropriate did the approach I had adopted seem.

For example, my interest in the topic of motivation had led me to the work of Kurt Lewin which appealed to me greatly and which I therefore studied with some care. Here was an approach that combined empirical work with a degree of theoretical formalization, precisely the combination that had seemed so promising in the Hullian synthesis. However, though both systems claimed to incorporate the essence of the scientific mode of inquiry, I knew enough about physical science to recognize that Lewin had a much better understanding of what this involved than Hull. But if Lewin was right then the laborious detour via animal experimentation was at best unnecessary and at worst misleading. In the field of motivation, the value of laboratory studies of animal behavior seemed quite dubious compared to the fascinating demonstrations and original conceptualizations that characterized the human experimentation of Lewin's Berlin group.

One aspect that bothered many in the neo-behaviorist camp was the relatively subsidiary role that quantification played in those Lewinian studies. But this never bothered me at all. I knew how important qualitative observations were in chemistry. The fact that one solution added to another produced a precipitate, that this precipitate was blue rather than white, that substances changed state, from solid to liquid to gas, that heating of a liquid might result in various distillates that could be distinguished by their volatility, viscosity, color, and so on, – all this qualitative observation provided the necessary basis on which a superstructure of quantification could be erected. Of course, in the end measurements were crucial, because their precision enabled you to develop efficient theoretical models and to eliminate inefficient ones. But without a rich domain of qualitatively described phenomena quantification would be an empty gesture. The animal ethologists and the Lewinians seemed to have constructed such domains whereas the neo-behaviorists had not.

A particularly rich domain of phenomena had been opened up by the techniques of psycho-analysis, and my time at Oxford provided me with an opportunity to gain some limited acquaintance with it. I discovered that a number of my fellow students at the Institute of Experimental Psychology had a very strong interest in psycho-analysis and were in fact undergoing a personal analysis. This meant that they led a sort of double life, as though they had a respectable daytime self devoted to experimental science but also a secret nighttime self that dabbled in the black art of psycho-analysis. It had to be secret, at least as far as the faculty were concerned, because of the perception that these official instructors would have questioned the suitability for a scientific career of any student prepared to give credence to the "mystical" notions of psycho-analysis. Indeed, among this group of students there was much dissatisfaction with the uninspired empiricism, the dessicated curriculum, and the extreme intellectual caution that characterized official studies at the Institute. So there was a strong element of intellectual revolt in their secret defection to psycho-analysis. In this context Freud's theories played a subversive role, and this made them seem all the more attractive to me.

Identification with orthodoxy, with the official view, with the established order has always made me uncomfortable. I tend to assume that the truth is likely to be found elsewhere. In chemistry the issue never arose, and perhaps this was part of the reason for my incipient boredom with the subject. But in psychology things were different. For one thing, psychological theories had social, even political, implications and could therefore be seen as either in tune with the prevailing ethos or subversive of it. In racist South Africa, where prejudice was institutionalized and myth rampant, scientific positivism had distinctly subversive implications. But in Britain it was more like an official doctrine, and for me this may have helped to sow the seeds of doubt. At any rate, I was persuaded that I ought to find out what subversive psycho-analysis had to offer. I knew that book knowledge would not suffice, and so I embarked on a brief psycho-analysis (nine months) while continuing with the experimental research about whose significance I was feeling increasingly doubtful. In fact, my confidence in my previous goals had been shaken to the degree that I began to consider whether I should not leave academic research altogether and pursue a career as a clinician. For that alternative an excursion into psycho-analysis made a lot of sense.

Unexpectedly, one of the firmest (and most lasting) results of the analysis was my recognition that I was much more suited to academic than to clinical work. This was simply a consequence of the opportunity for getting to know myself better which the analytic sessions provided. Of course, any other form of "talking cure" would probably have done just as well. For the rest, I was left with a certain respect for quasi-analytic techniques as potentially valid methods of psychological investigation. Psycho-analytic theory, however, appeared to be an extraordinary mixture of brilliant insights and poorly supported speculations. On the whole, the concepts on which the clinical discourse of psycho-analysis was based, repression, ambivalence, defense, transference, and so on, seemed well founded, whereas Freudian metapsychology seemed more like a metaphysical system – which is not to say that it might not have its uses.

For several months, I was in the position where, on the same day, I might be collecting quantitative laboratory data to test a behavioral hypothesis, doing intensive library work on Lewinian concepts and experiments, as well as undergoing a personal analysis along modified Freudian lines. It was apparent that in each case method and theory were fused into one indissoluble whole. That is why attempts at testing Freudian theories by means of quantitative measures obtained under laboratory conditions always struck me as misguided. The theory that was being tested was not the original theory, whose ostensive meaning depended on clinical observation, but some modification of that theory which provided it with a rather different ostensive meaning. Though I did not develop this point until much later (Danziger, 1985), I believe that the early experience of being simultaneously immersed in three very different modes of psychological investigation formed the origin of my emerging recognition of the intimate link that exists between theory and method.

## *Wanderjahre* – Years of Journeying

When I completed my doctoral dissertation academic positions in Britain were few and far between. My one job offer was to do research that would help the military in the training of dogs to sniff out buried land mines. I did not see myself as a doctor of dog training and therefore looked further afield. Academic jobs were opening up in Australia, and so I ended up at the University of Melbourne in the middle of 1951. Rather like Oxford, Melbourne had been a retardate as far as the formation of a modern psychology department was concerned, and when I arrived the Department had not existed for very long. However, unlike the small operation that Oxford had cautiously supported, Melbourne had tried to make up for lost time by establishing the new Department on what was for those times a rather generous scale. I had been hired to teach physiological psychology and set up an animal laboratory which I duly did.

But I was not going to go back to rat running in the neo-behaviorist mold. Instead, I explored two possibilities for using animal behavior research in a scientifically more defensible way. Both attempts were unsuccessful, one of them spectacularly so. Inspired by animal ethology, I had the harebrained idea of studying wild rats instead of that artificially created organism, the laboratory rat. But I still wanted to study them under experimental conditions. When the municipal rat catcher began to supply the desired specimens, however, I quickly learned that truly wild organisms don't play by experimenters' rules. Their overriding goal is escape, and wild rats display amazing ingenuity and agility in doing just that. They also show their unhappiness about being caged with others of their kind by indulging in cannibalism. Moving them from one place to another is extremely tricky and not without danger. As if on principle, they never do what they are supposed to do.

Given a great deal of time and patience, as well as considerable resources, wild organisms and laboratory environments could be gradually adapted to each other by the slow habituation and artificial selection of organisms on the one hand and the redesign of laboratories and experiments on the other. But I lacked the resources and felt there were better ways of investing my research time. Still, the demonstration of the union of organism and environment had been a powerful one. The organism apart from its environment was an abstraction that one never encountered in reality. One could study the behavior of organisms in their environment of adaptation or in some other environment, but not behavior as such. The unit was not the organism but an organism-environment couple, a point which Kurt Lewin had been trying to drive home in the context of human behavior.

I was becoming more and more convinced that studies of animal behavior should be left to zoologists, and that little of relevance for human psychology would emerge from such studies in any case. In other words, I was no longer thinking of psychology as essentially a biological science. However, before leaving this field for good, I made one last, somewhat half hearted, attempt at continuing my involvement.

In the course of my doctoral studies I had become aware that there were two views regarding the value of animal behavior experiments for psychology. There were those, stridently represented by B.F. Skinner, who thought that empirical laws of behavior could be directly generalized from the animal to the human level without the need to construct theoretical models of what was happening inside the organism. This always seemed to me so absurd that at my oral examination my examiners asked me to please tone down the relevant passages in my dissertation. My indignation had been partly due to my feeling that this approach amounted to a betrayal of science by people who professed to speak in its name, for my days in physical science had taught me that without theoretical models one would have, not science, but a cookbook. The Hullians at least seemed to understand that, although their theoretical models were based on a hopelessly out of date physiology. But the notion that generalizable physiological models would have to mediate between animal and human behavior stayed with me even when my belief in the psychological value of animal studies had been thoroughly eroded. Such generalizable models would probably have to focus on the functioning of the cerebral cortex.

A prominent figure who had long held the view that animal experiments were a means for investigating cortical functioning was Pavlov. In distinguishing this approach from that of American behaviorism he had made the point that, whereas for the latter the establishment of the principles of conditioning had been regarded as the end goal, he Pavlov had always seen the conditioned reflex as a *means* for investigating cortical functioning (Pavlov, 1932). He had in fact developed physiological theories on this basis. Whatever the fate of those, his general argument had merit, and it applied to the behaviorism of Hull as much as to that of someone like Guthrie whose approach Pavlov had addressed directly. More generally, it appeared that theoretical positions did not travel well between diverging social and cultural settings, an issue I was to encounter again much later in connection with the American reception of the work of Wilhelm Wundt.

Unfortunately, Pavlov's physiological models turned out to have limited predictive value and to be resistant to confirmation by more direct studies of brain function. But his critique of behaviorist misuse of the concept of the reflex had profound implications, perhaps more profound than Pavlov himself was able to appreciate. What he seemed to be pointing to was the futility of attempting to generate explanatory "principles of behavior" out of apparently simple instances of behavior without any recourse to extraneous theoretical models. For me, at any rate, that buried what was left of the behaviorist project, and I turned my attention in an altogether different direction.

While I was at Oxford Jean Piaget had come over for a visit and I had been sufficiently impressed to increase my acquaintance with his work. As I became more and more disillusioned with animal experimentation my immersion in the Piagetian literature became more systematic. Then, when I finally abandoned my previous research field, I was ready to launch into some Piagetian type studies of my own. I had begun to suspect that if there were any broad scientific generalizations to be discovered in psychology they were likely to be developmental in character, and in that field Piaget's conceptual framework was then the only variant that had grown beyond hints and sketches. The English translation of his *Psychology of Intelligence* (Piaget, 1950) had been published recently, and for many years after that any theoretically informed developmental study would have to come to terms with the Piagetian colossus. I was also interested in trying out Piaget's "clinical" method of questioning children so as to reveal the structure of their conceptions about the world.

I was however unhappy about Piaget's marked and growing tendency to pay far more attention to children's concepts of the natural world than to their concepts of the social world and to base his general scheme of conceptual development on the model of an individual child's interaction with the natural world. Early on he had devoted one major study to the development of children's social concepts (Piaget, 1932), but since then his theory of cognitive development had essentially been based on studies of concepts of volume, mass, space, time, number, and so on. He did recognize a gradual socializing of the child's thought, but this left him with a model that, as his French critic Henri Wallon observed, was essentially Rousseauian. If this Piagetian bias was to be overcome it seemed that the underdeveloped area of children's social concepts needed more attention. I therefore embarked on a study of children's concepts of kinship, which Piaget had touched on earlier, and concepts of economic relations which he had not.

The earlier work had shown a clear tendency for younger children to understand terms referring to social relationships, brother, uncle, etc., in a non-relational, categorical way. How then did they ever come to understand social relationality? Piaget's answer had relied on an internal maturation of formal, quasi-logical capacities. However, I found that the grasp of relationality developed unevenly over diverse social domains, kinship relations being grasped before economic relations. But, within each domain, each relation was first seen as independent of other relations and only later took its place within a system of relations. These observations suggested that the development of social concepts depended on an interaction of formal and content related factors.

I did not pursue this line of research because a revival of older interests pointed me in a rather different direction. When I switched from chemistry to psychology I had hoped that the latter would provide ample scope for the study of human social behavior, and my first piece of psychological research had indeed been in the area of social psychology, as I have indicated. I had then been diverted from this quest by the positivist belief that the general principles of human behavior, social or otherwise, could only be established on the basis of animal experiments and quasi-biological theoretical models. However, over a period of three or four years I had become convinced of the futility of this approach. I was therefore very open to any influences that might provoke a return to my earlier interest in the direct investigation of social behavior.

The intellectual environment I found myself in at Melbourne University was not lacking in such influences. Research in the Psychology Department was dominated by a large project, involving several faculty members, devoted to the study of personality and social structure in some Australian communities (Oeser & Hammond, 1954; Oeser & Emery, 1954). The term "personality" may be somewhat misleading because the target of this research had little in common with the meaning that this term had acquired in the work of American psychologists. Perhaps "social consciousness" conveys a better sense of what this Australian study was trying to elucidate. The inspiration was Lewinian in part, but the practical execution and interpretation of findings was much closer to what one would expect in sociological rather than in psychological research.

Although I never participated in this project actively it helped to rekindle my previous interest in this kind of work so that I was always ready to engage in discussion with those who were directly involved or to read their unpublished manuscripts. I came to appreciate the critical intellect of Paul Lafitte, whose critique of psychometric personality research (Lafitte, 1957) became a beacon that was of great help in my later studies of psychological research practice. Of more immediate practical effect was the spark provided by the sociologist Geoff Sharp, whose erudition and personal example launched me on a study I should have undertaken much earlier, that of the sociological classics, Weber, Marx, Durkheim, Mannheim. As a result, I developed an understanding of social psychology that was far removed from what went under that name in American psychology.

I was very happy in my Australian environment but the longer I stayed there the more I felt the depth of my ties to South Africa. Although the country of my birth was Germany, South Africa had become a genuine second home where I had spent my adolescence and early adulthood. Only after I had left it did I come to realize that it would never be simply another country in which one had lived for a while and developed a certain affection. The South African tie went deeper than that. It manifested itself in strong feelings of concern about the fate of the country, in a longing to be once again able to experience the quality of its light, the sound of its voices, its outward appearance and even its special menace. After an absence of several years homesickness could no longer be ignored, but there was something else as well. I had left South Africa less than a year after the critical change of government that ultimately resulted in a turning back of the political clock which became known the world over as *apartheid*. By the time I was in Australia the full viciousness of this system of tightening racial oppression had become apparent. It was difficult to avoid the feeling that one's place was among those who were confronting this evil. Men who had openly sympathized with Nazi Germany were now in government. Did I as a Nazi victim really have a choice?

I left Australia in 1954, having accepted a position at the University of Natal which soon took me to Durban, South Africa's third city. This part of the country had a far more pronounced "African" character than Cape Town. It had been colonized much later and under different circumstances. Whereas Cape Town had always retained the character of an outpost of Europe, the African population of Natal not only constituted a large majority numerically, they also preserved African traditions in a salient and self-confident manner. I began to learn Zulu and learned to appreciate the importance of cultural contexts for psychological research in a much deeper way than I had in Cape Town. Durban also had a large population of Indian origin whose ancestors had been enticed to this part of the world as indentured labor. I developed strong friendships with members of this group.

My return to South Africa would have been pointless if I had not become involved in the political struggles of the time. I became one of a handful of white supporters of the African National Congress which had not yet been declared illegal. During the next three years, the focus of my interests, which had previously been overwhelmingly academic, underwent a crucial shift in a political direction. Not that I had had no interest in political issues before - on the contrary, political philosophy had been the third area of concentration during my years as an undergraduate, after chemistry and psychology. But this had still been an essentially academic interest whereas in Natal I became involved in actual political work that brought me into contact with people who were at the receiving end of the brutal South African system of oppression and exploitation. Some of them lived in shacks, some of them were leaders of the liberation movement, Nelson Mandela among them.

This experience was critical for my own development as an intellectual. It provided me with a basis for constructing a new professional identity. Years before, I had started out with the belief that the only worthy goal of one's work was that it should serve the cause of science. But, as I have described, I had gradually discovered that, in a field like psychology, it was far from obvious what might count as a genuine fulfillment of this goal. Consequently, delusions and blind alleys abounded. This was not a viable basis for constructing a professional identity. In the social sciences the value of one's work also depended on its relationship to the human world of which it was a part. Its contribution to that world, immediate or potential, was critical in assessing its value. In the South African context this was particularly clear. Some of its social scientists, supporters of white supremacy, had attempted to demonstrate the existence of race differences in intelligence. Others, busy with arcane investigations that imitated the intellectual games of the developed world, seemed to have nothing better to do than fiddling while Rome burned. But a third way was possible, and this meant working towards the kind of knowledge that would be part of the movement for social emancipation.

I now returned to my original interest in social psychological research, but there could be no question of taking abstract human individuals as the objects of my investigations. In the kind of research that had become the norm in American social psychology college students had been treated as representative of some general human subject, so that empirical findings based on their responses could be presented as universalistic generalizations that might apply to human individuals in general. In other words, the human subjects in these investigations were treated as though they had no social identity, or at least, as though their social identity was unimportant in a social psychological context. This resulted in a social psychology that was curiously disconnected from the specific social conflicts, power struggles, oppressive practices and social myths of the real world. On the contrary, the kind of social psychology I now envisaged would have to regard individuals' social identity as primary. What one's research would be directed at would be the links between social identity and social consciousness, not the principles supposedly at work in the social life of individuals hypothetically without a social identity that mattered.

In my South African research I too used groups of students, but they were distinguished from each other on the basis of their most salient social identity, namely the "racial" categories into which they had been divided by history and by political decree. The general question then was how these differences in social position manifested themselves in different patterns of social consciousness. To tap the latter I used responses to a highly charged slogan of the time, "white civilization", as well as questions about broad social values (Danziger, 1958). Large group differences emerged as expected. However, I soon narrowed my attempts at sampling the potentially vast field of social consciousness to one specific aspect that seemed to me to be of particular importance.

As the tensions in South Africa mounted, as each turn of the screw of state sponsored racial oppression led to further and more desperate acts of resistance, a spreading sense of the precariousness of the situation became quite palpable. This sense of precariousness was often expressed in terms of sentiments about the future: the country was felt to be heading for a crisis, and things could not go on the way they were. The future was regarded with hope or fear, but in either case it was an ever present “horizon” that imbued many everyday events with a special meaning.

Although psychology had had a great deal to say about the importance of the personal past it had been almost silent on the topic of the psychological future. Two notable exceptions were Kurt Lewin, who had explicitly emphasized the importance of the topic, and G.W. Allport who had stressed that human conduct was often “proactive” rather than reactive. As chance would have it, Allport paid a visit of several months to the University of Natal just as I was becoming interested in the possibilities of investigating the psychological future empirically. I learned from him that he had in fact been engaged in such an investigation on an international scale. The instrument used had been the so-called future autobiography, in which individuals are requested to project their lives into the future, usually by imagining to be writing fifty years hence and looking back over their lives. There had been earlier applications of this method by the Hungarian sociologist, Alexander Szalai, but Allport’s resources enabled him to launch a cross-national comparative study with contributions from South Africa and elsewhere. I gathered from him that his collaborator, Gillespie, had not fulfilled his hopes, so that the final report on the study was quite lacking in theoretical or any other kind of analysis (Allport & Gillespie, 1955). Nevertheless, it provided a useful starting point for my own subsequent work on the psychological future. As most of that work dates from the period of my return to South Africa, after a temporary absence in Indonesia, I will defer discussion of it for now.

Quite apart from the use of future autobiographies as a research tool, Gordon Allport’s extended visit provided another, more general, benefit. This was really my first opportunity to get to know an American psychologist reasonably well. My previous intense encounter with American psychology in the form of neo-behaviorism had been at second hand, mediated by an atypical representative (Taylor) and by publications. It had also ended in complete disillusionment, as I have related. But the presence of Gordon Allport, as it were on my doorstep, made me appreciate the existence of countercurrents in American psychology that were not in sympathy with the behaviorist mainstream. What was particularly impressive was the obvious fact that the humanism which Allport had argued for as a psychologist was not simply a matter of professional rhetoric but a deep personal commitment. The fact that he had chosen to immerse himself in this tortured racist cesspool of a society, when he could have spent his sabbatical under far more agreeable circumstances, spoke volumes, especially as there had been other visitors whose idea of cross-cultural research did not include even brief contact with those to whom their research “instruments” had been “administered”.

But there were aspects of Allport’s style of humanism that most of my South African colleagues and I found it hard to relate to. There was first the religiosity. That an enlightened 20$^{th}$ century intellectual and social scientist would attend church regularly was beyond our experience and comprehension. In South Africa at that time religious humanism was known mainly as a cloak for a rather nauseating racist paternalism, a description that did not fit

Allport. Much more serious was the divergence in our assessment of the roots of racism. Though his South African experience may have produced a slight shift, Allport had difficulty thinking of racism as ultimately not a matter of personal prejudice. Although his student, Tom Pettigrew, who had accompanied him to South Africa, was discovering that systemic racism did not depend on personality correlates (Pettigrew, 1958) Allport never abandoned the deeply individualist basis of his social psychology.

It became clear to me that this bias was deeply embedded in the whole field of attitude research, largely an American invention for which Allport had provided a thoroughly individualist conceptual basis many years earlier (Allport, 1935). In my search for a different approach to this field I came across the work that had recently been done at the Frankfurt Institute for Social Research. This work had its roots in Theodor Adorno's (1955) critique of the presuppositions and methods of American sociology and social psychology as well as earlier European work in the area of public opinion. What emerged was an approach that rejected the conception of social attitudes as entities inhabiting individual minds. People's expressions of their opinions should rather be conceived as existing in an interpersonal social space, attributes, not of abstract individuals, but of individuals interacting with each other. Therefore, the way to assess and explore social attitudes was, not by questioning individuals isolated from others, but by examining the expressions of opinion that took place in discussion groups (Pollock, 1955). In a variant of this approach the groups would have a socially significant identity, consisting for example of trade unionists, of housewives, or of ex-prisoners of war (Mangold, 1960). Topics relevant to the social identity of group members were discussed in these groups and recorded. Then the protocols of these discussions were analyzed for characteristic thematic content.

I was greatly impressed by this approach and confirmed its viability in a pilot study that I carried out with South African groups. However, except for a watered down version that I tried out in Canada many years later (Danziger, 1977), I was never able to follow up on this promising lead. In South Africa the deteriorating political situation, manifesting itself in an ever expanding network of police spies and draconian punishment of dissent, made the use of a method that depended on the open expression of opinion on sensitive topics more and more questionable. In Indonesia, where I was soon to find myself, the cultural preconditions did not exist for setting up group discussions that would not be dominated by rules of precedence depending on ascribed social status. By the time I had settled down in Canada my major interests had shifted to topics other than attitude research.

However, the intellectual climate of the Frankfurt Institute, out of which this line of research had sprung, had a lasting effect on my thinking. In Australia I had found one of the few original copies of *Autorität und Familie* (Horkheimer, 1936), containing fascinating early papers by members of the first Frankfurt Institute. Then, in Natal, I proceeded to the later work of Horkheimer and Adorno. Towards the end of 1957, while on my way to Indonesia, I visited the Institute and talked with Adorno. He seemed fascinated by my association with what were to him wonderfully exotic, even romantic, places. It made me aware of my marginal perspective. In Africa I had habitually sought inspiration through the cultivation of Central European philosophy and sociology, but confronted by a major representative of what I admired, I became aware of the distance that separated my view of the world from a truly eurocentric perspective.

How did I find myself on the road to Indonesia? That was partly accidental. A South African social anthropologist who had been at Oxford at the same time as me had ended up there for professional reasons after he graduated. We corresponded, and he painted the culture and the research possibilities of the place in the most glowing colors, urging me to join him at least for a while. He had kept this up over a period of years, and as the situation in South Africa became more and more tense and ugly I began to feel tempted. There was no question of a permanent departure, but when I was offered a temporary contract I accepted. In the event, I spent a little under two years in Indonesia before I returned to South Africa, but they were years that had a lasting impact.

I was fortunate in landing up, not in Indonesia's capital, Djakarta, which, as the Dutch Batavia, had been the epicenter of colonial rule, but at the smaller town of Yogyakarta that had been the capital of the Indonesian Republic during its recent war of liberation from Dutch rule. There the rebels had established their own university, Gadjah Mada, which was enjoying government favor during the post-colonial period. Unlike the majority of the foreign academics at this University, I was not part of some aid scheme paid for by other countries, but an employee of the Indonesian ministry of education, a temporary civil servant. With my South African sensibilities I noticed almost at once that being on the same payroll as my Indonesian colleagues (and therefore subject to the same bureaucratic obstacles and economic uncertainties) was a great advantage in terms of reducing social distance and sharing local hopes and concerns. I also understood right away that if my stay was to produce anything of value by way of research I would have to master, if not the local vernacular, then at least the national language, Bahasa Indonesia. After six months, and with wonderful encouragement from my students, I began to give my first halting lectures in Indonesian. Soon I became quite fluent, though jokes were always a bit of a problem.

In colonial times the town and area of Yogyakarta had preserved a certain autonomy within the Dutch colonial empire, and this had helped to preserve traditional cultural patterns and keep western influences at bay to some extent. In the years I was there this even extended to the University, which was after all a western institution. But in colonial times tertiary education had been the privilege of a minute number of individuals, most of them drawn from conservative aristocratic backgrounds. These men (they were all men) were now running the University. They acknowledged western superiority in natural science and technology but were divided in their opinions as to whether western assistance was necessary or even desirable in the humanities and the social sciences. When I arrived I was told quite explicitly that there was an interest in the methods of western psychology, but as to content, well, I should teach it by all means, but I should also realize that in this area judgment would be reserved.

I soon discovered what was behind this advice. Any courses I taught were identified as *psikologi*, but there were other courses, taught by an elderly Indonesian colleague, on something identified as *ilmu djiwa*. This term translates as science of the soul if one takes "science", not in the Anglo-Saxon sense of natural science, but in the broader German sense of *Wissenschaft*. This other psychology turned out to be based on a local philosophical tradition with its own literature that had historical roots in Indian predecessors. It differed from western psychology, not only in lacking quantitative and experimental methods, but, much more profoundly, in using altogether different categories for mapping and conceptualizing its subject matter. As I have described elsewhere (Danziger, 1997), one to one translation between the categories of these two psychologies simply did not work.

At the time, both my Indonesian students and I experienced this situation as part of the wider issue of modernization or westernization. Everyone agreed that now that Indonesia had become part of the modern world certain things would have to change. But there was also a reluctance to give up traditions that were still meaningful and that had been a source of pride and solidarity in the recent anti-colonial struggle. With considerable justification, the application of western style psychology to human affairs was seen as part of the modernization process. Inevitably, there were differences of opinion as to how far this process should be allowed to go. At the one extreme were traditionalists, mostly older men with aristocratic pedigrees, who did not see any need to import an alien psychology at all. At the other extreme were the determined modernizers, mostly young, who thought that the traditional psychological wisdom was of no contemporary value at all.

Today I might be inclined to take a more balanced view, but at the time my own training and my social position as representative of the West combined to make me an ally of the modernizers as a matter of course. There was a certain irony in this because I was hardly a typical specimen of mainstream western psychology. But I had retained enough confidence in the value of a scientific approach to human problems to know which side I was on when the choice was between that approach and reactionary obscurantism. So the high point of my stay in Indonesia was reached when, in a five hour debate in the University Senate, I successfully defended the legitimacy of quantitative methods in the human sciences against a last ditch attempt by the conservative opposition to turn back the tide.

These experiences permanently affected my relationship to the discipline of psychology. They firmly established the recognition that this discipline was intimately tied to a broad complex of social and cultural conditions. It did not speak for some universal abstract truth but for a truth that would hold in particular circumstances. Different psychological positions were also tied to different social positions. Whether one accepted or rejected the psychological positions would therefore partly depend on where one stood in relation to their linked social positions. More generally, the Indonesian experience cemented an already existing tendency to look at modern psychology from the outside, to defend it or criticize it on the basis of standards that were external to the discipline itself.

Among psychologists, however, the converse of this position was the generally accepted one, namely, the belief that the phenomena of the social world had psychological causes. I had already encountered a major instance of this in connection with the phenomenon of racism that, for American psychologists like Allport, was essentially a matter of “prejudice”. In post-colonial Indonesia I encountered an analogous instance in connection with the problematics of modernization. There was an ongoing discussion regarding the factors that might accelerate or impede this process. Psychology’s best known contribution to this discussion took the form of D.C. McClelland’s theory of achievement motivation (McClelland, 1953, 1961) which attributed economic growth to an individual motive to excel that was strong among the members of some societies, weak in others. What determined levels of individual motivation were certain patterns of child training, notably early independence training.

A satirist wishing to lampoon ideology masquerading as psychology could hardly have imagined a more crass example. All the elements that militated against psychology being taken seriously as a social science were present in abundance: the simplistic conceptualization of complex social processes, the mechanistic model of social causation, the treatment of social formations as an aggregate of individuals, the universalizing of one’s own culturally limited experience, and so on. Yet, in spite, or more likely because, of this the achievement motivation model was being widely disseminated.

Quite apart from its intrinsic weaknesses it seemed to me, as well as to several of my Indonesian graduate students, that this model was singularly inappropriate in the local context. It recognized only one kind of human motivation as conducive to modernization, prescribed one path and ignored others more in tune with local conditions. That led to some empirical studies (e.g. Danziger, 1960) which problematized “patterns of child training” rather than their specific psychological effects.

Early independence training appeared to be part of a complex pattern of child rearing practices that accentuated the separateness of parent and child as individuals and their required conformity to external social rules. By contrast, a more traditional local pattern emphasized the maintenance of an undisturbed union between parent and child as one aspect of a broader harmonious collectivity. The degree to which mothers followed one or other pattern varied with their degree of exposure to modernizing influences in the form of western type schooling, mass media, urban background, and the non-traditional nature of their husbands’ occupation. Intervention that simply targeted one aspect of child training would either fail or would amount to a promotion of values already associated with the more privileged sections of society.

The link between the psychological and the political was one to which I had become sensitized in South Africa, but Indonesia provided the first opportunity for that to be directly reflected in my research. Soon I was given plenty of opportunity. I returned to South Africa rather earlier than expected because I had been offered the headship of the Psychology Department at the University of Cape Town, my old alma mater. My return meant a resumption of work exploring the psychological future that I had begun before the Indonesian interlude. I now extended this work in three directions.

First, I developed a theoretical model that linked the structure of future autobiographies to the socially circumscribed life chances of their authors. Life in Africa had sensitized me to the enormous difference in the psychological demands that bureaucratic-industrial societies and pre-industrial societies made on their members. Indonesia had made this issue a central concern. Conceptualization of the issue depended on a suitable characterization of the common features of bureaucratic-industrial societies, and this was precisely what Max Weber’s notion of *rationalization* had accomplished. These societies operated according to norms of instrumental reason that treated human actions as means chosen for the sake of efficiency. Karl Mannheim (1940) had pointed out that where this principle applied individuals would have to organize their own lives accordingly, that is, as a system of rationally sequenced instrumental actions leading efficiently from one goal to another. He called this *self-rationalization*. Although he had probably never seen a future autobiography his description of self-rationalization fitted many of them to a T. They exhibited just the plodding realism, avoidance of fantasy, focus on career and money, calculation of contingencies and predetermined time structure that one would expect from a thoroughly self-rationalized individual. By applying ordinary techniques of content analysis one could even compare autobiographies in terms of the degree to which they exhibited these tendencies (Danziger, 1963a).

Levels of self-rationalization were far higher among white students than among black students, a difference that could be attributed to the effects of the apartheid system of racial oppression. According to its chief ideologist, the former psychology professor, H.F. Verwoerd, it was the aim of the segregated system of African education to teach its charges that there was no place for them in the “white” social order beyond certain (menial) forms of labor. This aim had apparently been achieved. The “white” social order was in fact constituted by an instrumentally rationalized set of institutional relationships that enfolded the lives of the black students as much as those of the white students. The difference was that the former were legally prohibited from filling any but subordinate positions in this order. Under these circumstances, self-rationalization became pointless, a purely imaginative exercise. The more realistic black students therefore turned, not to self-rationalization, but to the promise of collective political action as the means for improving their life chances. In their future autobiographies they linked their personal future to that of their social group. They wrote of the changes in the social order that would also fulfill their personal aspirations.

The linkage of the personal future and the collective future suggested a further extension of this line of research. I now collected, not only future autobiographies but also “future histories”. These were essays in which students were to imagine themselves as historians writing the history of their country fifty years hence and describing what happened between the actual time of writing and the imagined time half a century in the future. A system of content categories allowed a classification of these productions according to the type of future history that was projected. Some saw a future of revolutionary change in the social order, for others no such change seemed conceivable, while yet others foresaw gradual changes. For some, revolutionary change was foreseen but regarded as a catastrophe, for others it was the way to a much better world. These differences were highly correlated with the social position of the authors, black future histories welcoming revolution and social change, while those of whites were more often oblivious to the possibility of social change or regarded it with apprehension. That provided a striking illustration of Karl Mannheim’s (1936) conceptualization of the link between social stratification and social consciousness. I realized that I had strayed far over the artificial border that was meant to protect psychology from contamination by the social sciences and published this research in a sociological journal (Danziger, 1963b).

A third extension of the work on future autobiographies entailed a similar transgression of disciplinary boundaries. Mainstream psychology had distanced itself even further from history than from sociology. Psychological research was almost invariably limited to the study of changes occurring over time periods that, by historical standards, were minute. Except for the area of developmental psychology, the desirability of studying long term psychological change was hardly recognized, and when it was, the reference would be to changes in monadic individuals cut off from the historical events that might be changing the ground on which they stood. In South Africa I found such an approach incomprehensible. History had us by the throat. The old colonial order that had circumscribed every aspect of life was collapsing all around us. Yet our masters had decided that we were to be the rock against which the tides of change would break – for ever. It seemed unlikely that these circumstances would have no effect on people’s psychological future.

Work on the future autobiographies of South African students had been going on for more than a decade, beginning with Allport’s earlier protocols that he had made available to me. That provided a basis for a modest attempt at studying psychological change in historical context. The time frame was still tiny on any historical scale, but, given the intensity of the historical conflict, it was not too short for the demonstration of significant shifts among black South Africans whose life chances were being deeply affected by the political events that unfolded around them (Danziger, 1963c).

Long after I had left the scene, some of my successors at the University of Cape Town continued this line of work with a considerably extended time frame (Du Preez et al, 1981; Finchilescu & Dawes, 1999). However, an empirical historical psychology based on psychological assessments over relatively long periods of time remains a dream and will remain so as long as the institutionalized structure of research support is totally dominated by an emphasis on short-term goals and an insistence on short-term results.

In the meantime, the historical circumstances whose effects I had been noting in my research were fast catching up with me. Three months after my return to South Africa the situation in the country boiled over and over two hundred people were killed or wounded in a single police massacre at Sharpeville. A state of emergency was declared, the African National Congress was banned, and mass arrests were the order of the day. I was soon approached for help and a hidden room in the Psychology Department became the place where the local Congress organization produced its illegal leaflets.

In due course, a particular aspect of the tightening system of oppression seemed to call for protest on specifically psychological grounds. New laws and procedures, undoubtedly influenced by expert advice from abroad, had ushered in new norms of police practice. Political prisoners were now held in solitary confinement for periods that often extended over many months with the aim of forcing compliance. This might take the form of confessing to illegal acts, providing information on other suspects, or, best of all, appearing as state witness in political show trials (see Sachs, 1967). When these trials got under way I was consulted by some of the defending lawyers about the possibility that long periods of solitary confinement might have psychological effects that would affect the reliability of testimony that witnesses subsequently offered in court. I agreed that this was indeed possible and subsequently gave expert evidence to that effect. However, at what was by far the most important of these trials, the so-called Rivonia trial at which Nelson Mandela and other leaders of the liberation movement were convicted to life imprisonment, the judge refused to admit any expert evidence on the effects of solitary confinement, saying he did not need any expert to tell him whether a witness was reliable or not.

Partly in order to prepare myself for these court appearances and partly because the topic was intrinsically interesting I now immersed myself in the psychological literature on sensory deprivation and so-called brain washing, as well as police literature on interrogation techniques and published first person accounts of political prisoners who had experienced long periods of solitary confinement. At the same time, I interviewed South African detainees who had been released after having been subjected to this treatment. In some cases their release had been a reward for having supplied information, in other cases the police had given up before the prisoner broke.

This work brought home to me the profoundly social nature of the human self. Virtually all the ex-prisoners I spoke with reported significant disturbances in their experience of their own self, their sense of identity, self-confidence and self-worth. As social contact of any sort was cut off for weeks and months, not only did the need for human communication become intense – cases of prisoners begging to be interrogated were far from unknown – but horrendous doubts began to threaten each individual's inner compass. It seemed that, in the long run, the integrity of the human self required a certain level of social feedback for its survival.

Characteristically, the essentially social nature of the deprivation suffered under conditions of solitary confinement had been side-stepped in the psychological literature by introducing the red herring of “sensory deprivation”. That made it possible to treat the deprivation of the social self as a mere instance of what was basically a physiological deprivation. In the witness stand I certainly appreciated the rhetorical value of this appeal to natural science, but privately I knew that “sensory deprivation” was a misnomer based on a crude category mistake. However, I also knew that cultural bias would tend to treat claims made in the name of physiological psychology as fact whereas similar claims in the name of the social self would certainly be dismissed as mere opinion. As it happened, the whole court exercise was futile in any case, given the police state atmosphere which was then beginning to grip the country.

That did not mean that all resistance should be abandoned. Prolonged solitary confinement was a form of psychological torture whose systematic employment by the organs of the state should not be allowed to proceed without some protest from those who claimed the psychological welfare of individuals as their special professional concern. Together with a medical colleague, I therefore drew up a statement of protest which was subsequently signed by a significant number of psychologists and psychiatrists and published. It was one of the few avenues of legal protest left and therefore had to be used even though there was no expectation that it would have any effect on the powers then in control.

Of course, such activities did not endear me to the powers that be. An even bigger black mark must have resulted from a more general protest we drafted and circulated among the academics of the country’s English language universities. In initiating this step I was particularly conscious of the sad historical precedent of the early days of Nazi Germany when a protest petition circulated among German academics had evoked only a limited response. I hoped we would do better, and we did – the number of signatures we collected in just four universities was about the same as had been obtained in the whole of Germany. To me personally this was a source of pride, almost like a successful act of vengeance for past wrongs. Obviously, there was a part of me for which recent German history would always form a kind of prism through which many later developments would be seen and judged.

There followed a period during which all sorts of signs pointed to the fact that I was now a marked man. The minister of justice (*sic*) mentioned me in the white parliament as a leading agitator and communist, reports came back from released detainees that the police had interrogated them about my activities, there were vigilante attacks on the family house and car, the police came to confiscate my passport, and so on. Of course, compared what the country’s black population had to put up with every day, these were mere pinpricks. But from what had happened to some of my friends and colleagues I also knew that I had to treat these events as the writing on the wall. Given my past associations, there could be no doubt that in order to remain in South Africa I would either have to desist from any further acts of protest or face much tougher repressive measures. Neither alternative was particularly palatable, and so I decided that the time had come to take my leave while I still could. I was only permitted to travel on condition that my return to South Africa would incur automatic imprisonment. The form declaring me to be a “prohibited person” I regarded as the closest thing to a medal I was ever likely to get. At least I had done enough to be officially marked as a threat to the prevailing order. To go further would have meant adopting an essentially political rather than academic identity, and this, I had realized long ago, was not what I was cut out for.

**New World**

When I arrived in Canada in 1965 I was nearly forty, the same age my father had been when he left Nazi Germany. A superficial adjustment to life in North America presented no problems – a deeper accommodation was difficult, in fact impossible. Although I had by then lived and worked in four continents the New World was in many respects alien territory. It did not help that my migration had been the result of external pressure, not the result of a calculated career move. I had reached no promised land; I had simply escaped from somewhere I no longer wished to be.

York University in Toronto, where I had accepted an appointment, was then very new, but its location in a fast growing metropolitan area ensured its rapid expansion. Those years of growth provided me with an excellent opportunity for learning the ropes of the North American way of managing tertiary education. For two crucial years I took over the chairmanship of the Psychology Department and became immersed in university administration. This was fascinating for a while because systems of university governance and funding were so different from what I had previously encountered, being run on what was essentially a business rather than a civil service model. However, I soon decided I had learned all I ever wanted to know about this side of things and returned to my research.

That was not as easy as it sounds because my research had been so intimately linked to the special circumstances and problems of the society in which I had made my home before. I sensed that my work on the psychological and historical future, on solitary confinement, on problems of modernizing societies, would not be easily transportable to a North American environment, and I certainly had no intention of going back to animal behavior. There remained the developmental work, especially in the form of its extension to the topic of socialization, which had remained an ongoing interest. Quite soon, I found what seemed like an obvious way of building on this background in the new environment. Canada, already a country of immigrants, had opened its doors to new waves of arrivals after World War II. The assimilation of immigrants was certainly a major social issue at the time and seemed to provide an appropriate context for the kind of research I had come to favor. I had been turned off the direct assault on psychological abstractions and had come to believe that the road to generalization in psychology lay through deep involvement with local material.

Together with some of my new colleagues, I now embarked on a relatively large scale project concerned with the socialization of immigrant children. Concurrently, I wrote a little text (Danziger, 1971) on the subject of socialization. There was much that was unsatisfactory in the field at the time. On the level of theory, the wax tablet model of the child predominated – socialization being understood as the forming of the wax by external influences. Among psychologists there was also a tendency to focus on the mother-child dyad to the exclusion of broader social influences. Empirical data mostly consisted of mothers' reports taken at face value or of measures of limited aspects of child behavior collected under experimental conditions. I tried to counteract these tendencies, more effectively in my book than in the empirical work I believe. One reason for that was that I was still under the illusion that a field investigation of the kind I had embarked on ought to cast its net wide and operate with large numbers.

At the end of this project I was left with the feeling that we had failed to engage with the topic in any depth. The more I thought about it the more I came to suspect that this was the inevitable result of using techniques that provided mere snippets of information from many research participants, not one of whom had been allowed to enter our data as a real person with real human problems. This insight made me reluctant to undertake any more major empirical studies with relatively conventional techniques and helped to turn my interests in a more theoretical direction.

However, to be truthful, I have to confess that my heart was never in this project. I had thought it would be, for had I too not been an immigrant child? But the differences were too great. We had been political refugees, the people whose problems were now the object of scientific interest were economic migrants. We had emigrated from an intellectually and technologically highly developed environment to a place of underdevelopment whereas for most of the immigrants in the Canadian sample it was rather the reverse. A different set of issues predominated. I did not find it easy to enter this world, neither did I encounter the sorts of theoretical issues that had kept me involved with previous research projects. I began to understand that it had always been the theoretical issues that had aroused my scientific enthusiasm in the past, that empirical work had always been a means towards essentially theoretical ends. I was about to shed the last element of my old identity, that of empirical scientist. The ground was now prepared for a venture into rather different scholarly pursuits.

But first, one last attempt to hold on to the old identity. Among the transportable interests I had brought with me from the Old to the New World there was one that still held me. It had sprung up in South Africa when I was looking for ways of studying attitudes in a group context and became more explicit when I was finding out about the effects of solitary confinement. In both instances I was made aware of a micro-world of interpersonal events, a world of communicative gestures, pressures, influences, that was usually below the level of awareness but could be quite powerful in its effects. It was a world that police interrogators, or good salespeople, knew much more about than psychologists. Indeed, psychologists were so focused on what was presumed to be going on inside the monadic individual of their professional imagination that they hardly bothered to notice, let alone investigate, the existence of a structured order of acts that regulated what passed between individuals. Some sociologists – Mead and Goffman were well known examples – had been much more sensitive to this interpersonal order, but empirical work on this basis had been quite limited. There had been plenty of cases, my own studies of the psychological future among them, where the influence of macro-sociological factors on individual responses had been demonstrated, but that left open the question of how these factors got into the individual. Socialization studies should have provided some of the answers, but generally they merely pushed the question further back, subsuming the actions of socializing agents under abstract categories like "nurturing" or "authoritarian" instead of recording what actually passed between individuals when they influenced each other.

As part of the research project on the socialization of immigrant children I had developed a system of coding the verbal interaction between parents and children. Analysis of the protocols of these interactions had been intended as a bridge between the socialization project and a future project that would see the extension of the system to other episodes of interpersonal communication. In preparation for this planned next step I reviewed the existing literature, crossing several disciplines, in the general area of interpersonal communication and subjected it to a critical conceptual and methodological analysis. This resulted in the book *Interpersonal Communication* which was completed at the end of 1973 though organizational problems at the publishers delayed publication until 1976. By then my life had taken a decisive turn which precluded any further work along these lines.

During the preceding years I had slid into a mid-life crisis that was marked by, at times severe, depression and by uncertainty about what I wanted to do with the rest of my life. My first marriage had broken up, empirical work in social science was no longer fulfilling or even significant, and continued exile from the country I had thought of as home was taking its toll. In this situation my earlier roots in Germany took on a new salience. Except for one visit of a few weeks, I had not been back to Germany since my childhood, though, as I have mentioned, a certain internal tie had been maintained by means of books. By the early 1970's I had been an academic for more than twenty years, but, because my existence had been that of a wandering scholar, I had never tarried in one place long enough to earn a sabbatical. Now that was about to change. My first sabbatical was due, and I hoped to use it to take a breather and sort myself out. I decided to go to Germany for several months, finish my book, and take up contacts I had made on my previous visit in order to plan a return to the study of attitudes in a group context which had seemed so promising many years earlier in Natal.

But I also had another agenda for this visit. I was going to spend a large part of my time reading up on the old German psychology that had once dominated the field. Why would I want to do a strange thing like that? There was more than one reason. To some extent it was going to be a sort of intellectual vacation, time-out from the world of American psychology that I had come to know better and like less since my arrival in Canada. But there was also the idea of filling a significant gap in my knowledge that had bothered me for some time. My interest in the theoretical foundations of psychology had been a constant over the years, but, with the exception of Gestalt Psychology and its Lewinian derivative, the only relevant literature I was acquainted with had been produced within the Anglo-American tradition. I knew the earlier German literature only from secondary Anglo-American accounts. As I had been referring to this literature in courses on the history of psychology that I had been teaching regularly for many years, I had an uncomfortable feeling that I really ought to improve the scholarly foundation of my lectures. My first sabbatical at last provided an opportunity for doing that.

Immersing myself in the early foundational statements and debates of modern psychology turned out to be an exciting voyage of discovery, an experience that renewed my interest in psychological issues. Within a short while I was hooked on these historical explorations and began to forget about any plans for pursuing research in interpersonal communication. Not that there was any deliberate decision to specialize in historical studies at that stage. I was simply allowing myself the indulgence of pursuing a gripping interest for a while, without thought of where it might lead professionally. And when I returned to Canada I found that I could continue this pursuit in Toronto because relevant library resources were still at my disposal, the early years of modern psychology in that city having been very much under the influence of August Kirschmann, a German import from Wundt's laboratory. By the time I decided to make the history of psychology my major research interest I was merely registering what had already happened.

As my knowledge of the relevant literature deepened, certain issues began to become salient, eventually providing focal points for my own subsequent contributions to this area of scholarship. The first of these issues, that of historiography, became unavoidable simply as a result of my peculiar situation. Modern psychology had taken shape in somewhat different forms in a number of countries at almost the same time. Its history was subsequently written within these national traditions, leading to somewhat different, even divergent, accounts in each case. But I did not belong to any of these national traditions, though I had more than a superficial entry into several of them. This applied particularly to the two traditions that had been of the greatest importance for the historical development of psychology as a whole, the German and the American. I had been trained and had worked in the latter but mother tongue accessibility and a long standing interest in German philosophy and literature provided ready entry into the former. From this vantage point it was inevitable that I should become interested, not just in history but in the writing of history.

An aspect of my formative South African experience had prepared me for this historiographic turn in any case. I have already mentioned the salience of history in the South African context. It was simply a given that the most striking social psychological phenomenon in that context, that of racism, required a historical explanation. The classical study of race attitudes in South Africa (MacCrone, 1937) had led the way in that respect. In doing this it had drawn heavily on a then new school of liberal historiography that had made a profound break with an earlier conservative historiography that had accepted race antagonism as a natural phenomenon. This background meant that, as a South African social psychologist, I was well prepared for encountering histories that were written from a particular standpoint and for the consequent need for historical revision. Having emerged from that school, a great deal of what psychologists had written about the history of their subject struck me as embarrassingly naïve.

A second focal point of my work in the history of psychology also had a strong element of continuity with my earlier investigations in South Africa. It will be obvious that those investigations were conducted within a framework derived from the sociology of knowledge. In my new line of work I quickly adopted a similar perspective, exploring the link between the social consciousness of selected groups and their social position. The selected groups now consisted of representatives of the new science of psychology whose social position varied with the national context within which they had to work. I concentrated on the comparative examination of the very different situation faced by academic psychologists in the USA and Germany and showed how the fundamental divergence of their conceptions of psychology could be comprehended in the light of these situational differences (Danziger, 1979a).

From the beginning, I had been repelled by certain features that were almost always to be found when psychologists attempted to write their own history. First of all, there was the almost ubiquitous tendency to substitute the history of psychologists for the history of psychology. Not that there is anything objectionable about the genre of historical biography – the detailed examination of interrelationships among public contributions and relevant factors in individual lives has yielded some of the most valuable insights in the historiography of the social sciences.

But the role played by historical individuals in the traditional historiography of psychology was seldom that of a target for scientific biography. No, in these accounts the deployment of individual figures had different functions. First, these were accounts that lacked any conception of a public discourse to which many individuals contributed and which represented themes, conflicts, interests, assumptions, and practices that were shared unequally by various contributors. In place of any such notion the traditional accounts presented history as a series of individual "contributions" lined up like pearls on a string. Among other things, this structure was very useful in conveying a sense of cumulative progress where none existed. Linked to that, the visibility of a string of great names pandered to the need for a little ancestor worship that was all that might induce the average psychologist to show any interest in the history of his or her subject. For amateur historians an organization of history as a string of individuals had the further appeal of making it unnecessary to pay any attention to the broader historical context, an activity for which they generally lacked both time and inclination.

Ironically, the first opportunity that presented itself for a public presentation of a “revisionist” history was very much focused on a particular individual, namely, Wilhelm Wundt. For the historiography of modern psychology Wundt had acquired emblematic significance. His name had become identified with psychology’s transition from a branch of philosophy to an experimentally based science. That is why the discipline chose to celebrate its centennial exactly one hundred years after this philosophy professor took the unusual step of setting aside a little space for the conduct of psychological experiments by his students. Special symposia, addresses, articles, etc. were dedicated to these celebrations on an international scale.

Now it so happened that in the course of my intensive reading in the older German psychological literature that had begun a few years before I had developed a particular interest in Wundt. I think that two factors in particular made Wundt’s work attractive to me. The first was the breadth of his scholarly interests, especially the fusion of philosophical and scientific concerns, that made him seem like a kindred spirit. The second factor was his readiness to reflect on scientific practices, not least on those he had done so much to promote.

The corpus of Wundt’s writings was one thing, the role played by Wundt’s image in the historiography of psychology quite another. As I have noted, the figure of Wundt had become emblematic, and as so often happens in these cases, the emblem had little connection with the reality. The emblematic Wundt was essentially a piece of professional ideology. It was a way of confirming psychology’s claims to the status of a natural science by celebrating them in the form of a concrete historical event, the supposed “founding” of the first psychological laboratory. However, Wundt also had a second role in professional ideology and the company history that went along with it: He was also the bad example, the one who had pointed the discipline in the wrong direction, towards people’s inner experience instead of their outer behavior as recorded by an uninvolved observer. He was the arch-introspectionist, which, in the pantheon of professional ideology, was equivalent to Beelzebub himself. (I am referring here to a professional ideology tailored to American requirements, but for most of the second half of the 20$^{th}$ century anything else was of little consequence internationally).

In this situation historical scholarship might have some general relevance if it exposed the story of Wundt the emblem for the legend that it was. That could be done by confronting the emblem with the historical record. Accordingly, my contribution to the Centennial celebrations took this critical form. In a number of studies I examined such topics as the issues at stake in the initial rejection of Wundt’s vision for psychology (Danziger, 1979b), and Wundt’s use of introspection (Danziger, 1980). The deconstruction of the Wundt myth had begun a few years earlier (Blumenthal, 1975), but an examination of psychology textbooks some years later (Brock, 1993) indicated that there had been little fundamental change on that level. The function of textbook history is not to advance the cause of scholarship but to introduce possible initiates to the myths of the tribe.

## The Past in the Present

Confronted by the different uses of disciplinary history I tried to sketch out a preliminary framework for what was at that time often referred to as “critical history”. If the history of psychology was something else than the history of psychologists, what was it? In an unpublished conference paper of 1981 I described my own project in terms of tracing "the historical constitution of psychological objects", a description which applies to everything I have done since then.

In speaking of historically constituted psychological objects I was trying to get away from an implicit metaphysics of timeless psychological phenomena that existed out there, waiting to be discovered and explained by professional psychologists. Instead, it seemed to me that no phenomenon could be transformed into an object-for-psychology without passing through the mill of psychological categorization and practical intervention. The subject matter of psychology was not constituted by "phenomena", which strictly means things that appear, but by objects, things posited by subjects as the target of their activity. There was a layer of constituting action interposed between observers and the phenomena that appeared to them. This layer was itself a historical product that the older historiography had rendered invisible. What now needed to be done was to make it visible.

I can no longer remember my source for the term "psychological objects". A more immediate source may have been the work of Michel Foucault which I was certainly reading with great interest at the time. But in the long run it seems to me to convey echoes of a switch from an essentially Kantian to an essentially Hegelian world view that I had made many years before.

Guided by this general conception, I began to pursue several lines of historical investigation. It seemed that psychological objects had been historically constituted in essentially two ways. One way was discursive and involved the gradual construction of psychological categories that would serve to name, to classify, to give a specific meaning to certain aspects of human experience. Every psychological category, whether it be perception, stimulation, personality, behavior, self, or some other, had a history, and sometimes a rather short history at that. In the usual case many individuals had contributed to the discourse that changed the category's meaning over time, often unintentionally. This was clearly part of the history of psychological objects.

But there was also a second aspect to the constitution of psychological objects. People not only classified their own and others' experience in certain ways, they also *acted* on each other and produced effects. This has always been part of human life, but with the professionalization of the human sciences towards the end of the 19th century new means of producing effects in others were invented. These took various forms, but they all formed part of the armamentarium of psychological expertise. There were so-called mental tests that produced classifications and measurements on the basis of which individuals' life chances could be significantly affected. There were elaborations of intensive psychotherapeutic methods that ascribed new meanings to vast areas of human experience. There were also experimental methods that produced phenomena and aggregations of phenomena that had not previously existed. With these tools of expert power the construction of psychological objects raced ahead.

Studying the historical constitution of psychological objects therefore had two aspects, one that focused on the historical background of the categories of psychological discourse, and another that would have to explore the development of psychological practices of investigation and intervention. The two aspects were of course interrelated, but their closer study involved different sets of historical materials.

In the early 1980's I was pursuing both lines of investigation simultaneously. At that time my work on the history of psychological categories focused on the category of "behavior". It seemed to be the key category for understanding much of 20th century psychology. In its distinct modern meaning it was a creation of the discipline, a prime example of the shaping of psychological objects by the power of expertise over discourse. But in spite of its relative novelty the "behavior" of "behavioral science" did have a history, or more precisely two histories. There was of course the recent history of "behavior' itself, from its appropriation by students of animal behavior to the full flowering of the behavioral sciences. But there was also a kind of "pre-history" pertaining to developments in the 18th and 19th centuries that created the possibility for the emergence of 20th century "behavior". It was hardly possible to get from intrinsically moral categories like action and conduct to the scientifically usable category of behavior in a year or two. The appropriate conceptual space took much longer to open up.

My original plan had been to assemble my work on this topic in a monograph. But a publisher to whom I submitted this plan thought it was too philosophical to interest a psychological audience. This weighed with me, because, although my own interests were never reined in by disciplinary boundaries, I retained enough disciplinary loyalty to regard psychologists as my primary audience. Not long after I received this publisher's opinion I had more direct indications that there was significant collegial interest in the studies of investigative practices that I was beginning to pursue at this time. Gradually, I invested more time in this aspect of my overall project and less time in the historical antecedents of "behavior". In the end, my too philosophical monograph remained unfinished. Fortunately, some of this work was quite acceptable to historians of science (Danziger, 1983), and the rest of it proved very useful when I returned to the topic in the 1990's. For the time being, however, this material was put on the back burner while I concentrated on the history of investigative practices.

I was confirmed in this decision by an interdisciplinary conference I attended in Germany in the spring of 1983. This conference was part of a major project on "the probabilistic revolution", guided mainly by philosophers and historians of science. It certainly made me aware that the historical study of investigative practices was not merely of parochial interest in psychology but that it had a far broader significance. At this conference I also met some of the people who were to be a continuing source of intellectual stimulation during my later years, Gerd Gigerenzer, Ian Hacking, and Lorraine Daston, a psychologist, a philosopher, and a historian of science.

I had been concerned with methodological questions since my encounter with ethology at Oxford, and most of my empirical research after I gave up rats (or they gave me up) had been partly motivated by an interest in developing ways of extracting information from qualitative data. But when I looked into Wundt's role in the genesis of experimental psychology that concern with methodological issues took a new turn. Initially, my established interests simply pointed me in the direction of studying the experimental reports that came out of Wundt's laboratory as well as his theoretical treatises. Fortunately, a complete set of Wundt's house journal, the *Philosophische Studien*, was available in Toronto, and I spent the better part of one quiet summer being fascinated by the contents of these dusty volumes. However, it was not their ostensive psychological content that I found particularly fascinating, but what the reports revealed about the way experiments were conducted in those days. This was certainly different from the methodological orthodoxy being purveyed by the textbooks I was acquainted with. Not only were the old experimenters quite happy making generalizations on the basis of observations taken from the smallest of small samples, the very notion of sampling (and of course sampling statistics) was obviously unknown to them. They *were* acquainted with the mathematics of probability, but only as a tool in the context of assessing the reliability of observations.

Even more fascinating were the *social* aspects of those early psychological experiments. The conduct of psychological research already depended on a certain division of labor among the participants, but this did not lead to the rigid separation and status differential between experimenter and subject roles that is so characteristic of the typical psychological investigation in more recent times. In Wundt's laboratory, I was surprised to discover, experimenter and subject roles were not only quite interchangeable, but the role of the subject was apparently more highly esteemed than that of the experimenter. The juxtaposition of these and other features of experimental situations then and now certainly demonstrated that the social system constituting these situations was not to be taken for granted but had been subject to considerable historical change.

This prompted me to look at the history of social relations in investigative situations more generally. One of the first things that emerges when one does this is that Wundt's laboratory was not the only source from which early modern psychologists drew their methodological inspiration. More important, in fact, was Francis Galton's Anthropological Laboratory in London whose work organization was in almost every respect closer to that of latter day psychology than anything happening under the aegis of Wilhelm Wundt. Moreover, the social organization for the production of scientific data taken from human subjects in London was intimately connected with Galton's innovative use of population statistics that opened up inter-individual variance as a hitherto untapped data source for the emerging social and human sciences.

My first description of the historical differences in the social structure of psychological investigations appeared in 1985, but by then this work had grown into a larger project devoted to a historical examination of psychology's investigative practices from several angles. There was, first of all, the micro-sociology of the situations in which scientific psychological knowledge was produced. Traditional textbook language would say "gathered" rather than "produced", a difference that provides a concentrated expression of a profound philosophical divergence. I have already emphasized that psychological data are not "found objects", and the image of them being "gathered" is therefore misleading. They are *made* objects that would not exist in the form in which they become objects of knowledge without the active intervention of the psychological investigator. They are "produced" in the course of this intervention.

Knowledge production is a social activity that takes place in specific situations that regulate this activity quite strictly. For psychology, as for other sciences, there is more than one kind of situation in which valued information can be produced, although there have often been strong pressures to extol one type of situation above all others. The dawn of modern psychology was marked by the simultaneous emergence of several investigative situations, or "epistemic settings", in which knowledge that counted as psychological was produced. I have already described some of the differences between the situations in Wundt's and in Galton's laboratory. But the crucial point is that in each of these settings a different kind of knowledge was produced. Very briefly, the work of Wundt's laboratory was dedicated to the elucidation of the universal processes that characterized the elementary content of the individual human consciousness, whereas Galton's laboratory concentrated on the quantitative representation of individual differences in human performance. There was obviously a close relationship between the nature of the investigative setting and the kind of knowledge this setting was designed to produce. In each case the social arrangements, the hardware, the mathematical tools, were adapted to the knowledge goals of the investigators.

What this kind of analysis amounts to is a micro-sociology of knowledge, more particularly scientific knowledge. In terms of my own intellectual trajectory this represented a fusion of two interests that had hitherto existed quite separately: the more traditional sociology of knowledge, and interpersonal processes. But in applying this approach to the production of psychological knowledge I was also helped and encouraged by the exciting new work being done at that time in the field of "science studies", mostly in Britain. This work discarded the exemption from any sociology of knowledge that had previously been granted to scientific activity, especially activity in laboratories. On the contrary, the new approach propagated the principle that the social conditions under which knowledge claims were generated should be investigated irrespective of the truth value of those claims (Bloor, 1976), and advocated an analysis of scientific work in the same way as any other kind of work (Whitley, 1984). The aura of the sacred no longer protected scientific activity from critical inquiry (at least not completely), and books on the sociology of science were published under titles like The *Manufacture of Knowledge* (Knorr-Cetina, 1981). During the early 1980's the literature of science studies provided the intellectual atmosphere that nourished my own work. This does not mean that I shared the extreme philosophical relativism characteristic of much of this work, but I was in the fortunate position of only having to deal with psychological science, a department of knowledge production whose truth claims rested on far flimsier foundations than those of the hard sciences.

I have always felt the need to pursue my studies on two levels. I certainly enjoyed empirical work, whether in a chemistry or psychology laboratory, in a historical archive, or talking to children. Continuing such work by analyzing data sets, whether quantitative or qualitative, was always absorbing. But this level of what I call investigative practice always led to reflexive questions about what I was doing when I engaged in these enjoyable activities. I wanted to be in a position to give an account, if only to myself, of the goals served by these activities and of the appropriateness of the means for achieving these goals. This internal meta-discourse lay behind many of the changes of direction that characterized the earlier part of my academic career. I would come to a point where I could no longer justify a particular line of work to myself and abandon it.

But once the history of psychology became my main preoccupation I felt no further need to change course. Reflection on what I was doing continued, perhaps more intensively than ever, but it now tended to take on more constructive forms. While I was engaged in the specific historical studies that occupied me during the 1980's I was also making notes on the meta-historical framework that was providing the guideposts for diverse aspects of these empirical studies. By 1989 the major part of the historical studies had reached a point where I felt they could be published as a monograph, and this duly happened in the following year (Danziger, 1990). The parallel development of a meta-historical framework ripened at the same time, though due to circumstances beyond my control, my sketch of this framework was not published till 1993 (Danziger, 1993).

As previously mentioned, I had been seeing my work as an inquiry into the historical constitution of psychological objects. This necessarily involved a two pronged search for the historical background of psychological categorization on the one hand and investigative practices on the other. But my rejection of a history of individuals in favor of a history of objects did not imply any commitment to the idea of history without subjects. On the contrary, I never considered historical "objects" to be anything other than one pole of a bipolar relationship, the other pole of which would be constituted by historical subjects. But unique individuals are not the only form in which historical subjects exist. In the long run, human collectivities are much more important for the historical constitution of psychological objects. I had indicated this in my very first historical study (Danziger, 1979a). Later, I emphasized the role played by shared professional interests in favoring particular knowledge goals above others. The preference for knowledge of a certain kind would lead to the use of the appropriate investigative tools and situations.

My study of the historical origins of psychology's investigative practices put some flesh on this rather skeletal outline (Danziger, 1990). For this purpose I was able to supplement more conventional historical source material by an analysis of several thousand empirical research reports that had appeared in the scientific journals of the discipline from its earliest days to the middle of the 20$^{th}$ century.

Such an enterprise obviously required skilled help, but fortunately this was now available. Around 1980 I had joined with a core of interested colleagues, notably David Bakan and Ray Fancher, to provide graduate students in psychology with the option of specializing in the history and theory of the discipline. Fortunately, there was unanimity on linking history and theory – the practice of pursuing the one without the other, common among psychologists, had led to too many pretty tales and lifeless abstractions.

For interested students choice of this option involved participation in seminars on specialized topics, supervised experience of research in this area, and of course a dissertation on an appropriate subject. It was most unusual for a psychology department to offer its students the possibility of such a course of study, but fortunately there was a high degree of tolerance in these matters among my colleagues. That work atmosphere also permitted me to ignore conventional disciplinary boundaries in pursuing my academic interests, a situation that many of my colleagues in other institutional environments could only dream of. For psychology graduate students, specialization in the history and theory of the discipline implied a serious commitment, for this was a risky choice, given the scientistic ideology of the discipline in North America. Fortunately, as the numbers involved were always small, career consequences for individuals turned out better than might have been expected. For me, the presence of these dedicated students was a great blessing. The intellectual benefits of an interchange with young minds can hardly be overestimated, and incidentally there was the availability of practical assistance with large scale projects, such as the systematic analysis of journal articles over a long period.

My research program at this time was primarily concerned with the emergence and transformation of the investigative practices that had characterized modern psychology, in other words, the practical interventions that had enabled psychological expertise to supervise the construction of a variety of psychological objects in the late 19$^{th}$ and earlier 20$^{th}$ centuries. This work involved an analysis of the features of different styles of investigative practice and then following the fate of these styles over several decades in the two countries that played the biggest role in the early days of modern psychology, Germany and the USA.

The archival sources for this work consisted principally of published documents in the form of articles in scientific journals. Other published documents, such as textbook expositions and reports on institutional activities, were used in the interpretation of this information. There was almost no reliance on unpublished sources, and this was deliberate. Psychological objects are nothing if not public. In the modern era they are constructed in the public discourse and the institutionalized practice of accredited experts. Their history is deposited in the archived public documents that were essential to this type of discourse and this type of practice. There are branches of history, for example historical biography or diplomatic history, for which the use of unpublished archival material is crucial. In other branches of history this kind of material plays a less important role, and in the kind of history I was pursuing it is of incidental interest at best.

This line of work had begun with a rejection of the intra-disciplinary concept of "methodology" which excluded the social structure of investigative situations and reduced the actions of investigators to a set of formal manipulations. (In the disciplinary ideology social aspects of experimental and other investigative situations were recognized only as complicating *psychological* factors that produced unwanted "artifacts", never as social *structural* factors whose every product was in quite a strong sense artifactual). The micro-sociology of science could supply a corrective here, because of its emphasis on the social relations between participants in investigative situations and the effect of these relations on the information produced. However, it was important not to lose sight of a level of symbolic manipulation involving different uses and understandings of mathematical tools.

In the end, I made only limited use of the micro-sociology of scientific knowledge. A focus on the production of psychological knowledge as a local achievement became less and less appropriate as the historical perspective widened from a focus on two or three crucial centers of emergence (Leipzig, London, Paris) to a survey of changing patterns of investigative practice during the first half century of a new discipline. The cross-national nature of the accumulated historical data greatly increased the visibility of the differing external pressures to which investigators were exposed. Although their actions always took place in a local context, crucial aspects of these actions were determined by shared professional interests shaped by a wider social environment and a deeper cultural history. An interpretation of the meaning of historical changes and differences in styles of investigative practice would have to based on this broader perspective. The result was an analysis that owed more to my earlier involvement with classical sociology of knowledge than the attractions of latter day sociology of science. The crucial exception was of course the fundamental turn that made knowledge labeled "scientific" subject to the same kind of analysis as any other kind of knowledge.

It was not easy to decide when to bring the historical study of psychology's investigative practices to a close. If one is dealing with ongoing historical trends such decisions always involve an element of arbitrariness. However, for the history of modern psychology the period around World War II constituted a clearly discernible watershed. Before that time, psychology had been an international enterprise open to diverse cultural influences from Europe and North America, in spite of the increasing weight of the latter. After the War the discipline not only experienced a period of American hegemony that changed its global complexion but underwent important changes within America itself. More directly relevant was the fact that the profound transformation of the discipline's investigative practices, which had been the main focus of my study, had become clearly established by the beginning of World War II. Some unpublished work indicated that in the post-War period the previously established trends continued to run their course. Continuing this line of analysis would have yielded no new theoretical insights. With the rapidly advancing fractionation of the discipline into widely divergent sub-disciplines specialized analyses for each of these were now required. Although I subsequently outlined one such analysis for the case of social psychology (Danziger, 2001) I had other plans for the remainder of my working years that were demanding more and more of my attention.

In the early 1980's I had put aside my earlier intention of exploring the categorical construction of psychological objects in favor of focusing on their practical construction in the course of psychological research. Once the latter project had achieved results that I felt to be of sufficient significance I decided that the time had come to return to my earlier interests. Even while still committed to the study of psychological practices I was wondering about an appropriate form for pursuing the topic of categorical construction. When I came across Raymond Williams' (1976) book *Keywords* I knew that I had found the format I needed. In that book Williams had taken certain terms popular in modern social thought and described the profound changes in use and meaning that each had undergone. Most of the words on Williams' list were part of socio-political rather than psychological discourse, but there were a few exceptions, such as "personality" and "behavior". The historical sketch provided for each of these items was quite brief and confined itself to the essentials.

For my purposes a more elaborate historical treatment would be required, and I would therefore have to limit myself to a relatively short list of terms. However, as the number of terms used as labels for psychological categories is quite large some criteria of selection would be needed. First of all, it would be better to select categories that were crucial for defining broad domains of psychological theory and practice rather than those that had a more limited extension. I would also limit myself to category labels in wide circulation in recent years and avoid others that might have been of considerable historical interest but that had more or less disappeared from view.

Even so, the number of candidates for inclusion remained larger than I felt I could accommodate in a manageable project. I excluded some candidates because I was aware of work, published or in progress, that would cover some of the same ground. This applied particularly to candidates from the domain of abnormal psychology, the one field that had attracted substantial historical interest with affinities to my own approach. Another range of possible topics was dropped because I quickly realized that the profusion of historical material demanded more extensive treatment than I was able to provide within a format of one chapter per concept. This applied particularly to the categories of cognitive psychology.

What I ended up with was a list of half a dozen categories of broad application and current interest: motivation, behavior, learning, intelligence, personality, and attitude (Danziger, 1997). In each case I was able to trace the history of their emergence as part of the vocabulary of modern psychology, the changes in meaning that each had undergone, and the social-functional aspects of their use. A seventh category, emotion, received more cursory treatment for the reasons I have already mentioned. Some introductory chapters were devoted to the contrast between modern psychological language and what had gone before and briefly explored the roots of the transformation that had taken place.

My focus on the discursive construction of psychological objects did not signal a loss of interest in issues of practical construction. In the course of my work on investigative practices I had come to recognize the fundamental role played by the category of the "variable" in linking the discursive and the practical construction of psychological objects. The fundamental difference between the old fashioned approach to understanding human individuals and the approach championed by "behavioral science" is that the interventions of the latter do not take persons as their objects but "variables". These are psychological categories that owe their very existence to the application of psychological "instruments", mental tests, rating scales and so on, to human populations. Although it has mathematical origins, the category of the "variable" has taken on a special meaning within psychological science. It signifies any feature that has been given the form required for it to become an object of psychological investigative practice. In that practice the notion of a "variable" functions as a sort of master category whose criteria of membership must be satisfied by any other psychological category before it can become part of the canon of psychological science. I therefore devoted what I regard as the most important chapter in the book *Naming the Mind* to the history of the transformation of an innocent mathematical term into a cornerstone of the edifice of post-World War II psychological science.

The psychological categories discussed in that book had all taken on their modern meaning in the course of the emergence and growth of psychological science. The "intelligence" of the intelligence testers has little to do with earlier meanings of the term; the sense of "learning" and "behavior" was changed irrevocably by the massive influence of American behaviorism; "personality" and "attitude" emerged in their modern stripped-down versions after they had passed through the mills of psychological science; the category of "motivation" hardly existed before its appropriation by that science. In other words, modern psychology constructed and reconstructed the categories of things it was investigating as it developed.

By concentrating on categories where this was strikingly obvious, once one had looked at the historical evidence, I was of course side-stepping the fundamental question of what, if anything, was left after one had made allowances for the effects of this busy scene of historical construction. This is one of those "big questions" that clearly cannot be answered on the basis of the current state of knowledge. But it is important to keep the question open and not to forestall any future advance in our understanding of the matter by giving premature answers now. Within the discourse of psychological research the question does not even come up, there being an unspoken but evident implication that the categories currently in fashion among experts correspond to objective structural features that really exist, that these categories "carve nature at its joints", as the saying goes. This amounts to a variant of the quite commonly held belief that "history has stopped with us".

But the ultimate absurdity of this point of view should not lead us to the opposite fallacy of assuming that discourse is the only reality, that there is nothing knowable beyond social construction. At the very least, it is too early to adopt such a position. Before we close the question, it seems to me, we need more work on the historical relationship between past and present psychological concepts and practices. We still have too little to go on, not least because historians and psychologists do not often talk to each other.

Unfortunately, these issues force one to confront the murky "pre-history" of modern psychology. I say unfortunately, because I have always agreed with those who questioned the justification and the advisability of extending the history of psychology backward into times when the very concept of psychology, as we know it, did not exist. This concept only emerged gradually in the 18th and 19th centuries, and one even faces serious boundary problems when one leaves the firm ground provided by the professional, scientific and academic structures of the most recent period. In the absence of convincing criteria of inclusion nothing human is alien to a history of psychology.

Certainly, these considerations are decisive if one thinks of the history of psychology as the history of a discipline. In fact, most of my own work in this field had been concerned with disciplinary history and therefore had concentrated on a relatively recent period. But my approach to disciplinary history had been guided by an interest in the historical constitution of psychological objects. I had recognized that the discursive construction of psychological objects could be traced back to the 18th century but had tried to avoid the crass presentism implied in a psychology before psychology.

However, while I was working on modern psychology's refashioning of categories to suit its purposes, I had to recognize that in some cases the discontinuity of use and meaning, though profound, was not absolute. The degree of historical continuity would vary from case to case. In *Naming the Mind* I had concentrated on categories with minimum continuity, but curiously, there seemed to be a little more continuity in some of the categories pertaining to human cognition that I had set aside for later consideration. After my retirement from active teaching duties in 1994 I had more time to look into these matters and soon realized that the profusion of material would make it necessary to limit myself to a single category whose history surely began long before modern psychology put its stamp on it. This was the category of memory.

If one considers only written sources in the Occidental canon one could make quite a strong case for memory having the longest continuous history of any psychological category still in common use. Although the psychological aspects are only part of what is covered by the concept of memory – now as much as 2000 years ago – it would be foolish to deny that some of these aspects were recognized discursively long before the advent of modern psychology. Plato's preoccupation with the topic of memory as a capacity of human individuals is certainly different from our preoccupation with that topic, but there is a degree of shared understanding that makes his questions and answers intelligible to us. What I am saying is that there are some psychological objects whose history is older, in rare cases much older, than the history of psychology itself (Danziger, 2002).

How does one approach this kind of history? The problem of defensible, non-arbitrary, boundaries has only been partly reduced by the shift from the history of psychology to the history of its objects. If I misspent my youth in too much travel, both geographical and intellectual, I misspent my old age in struggling with problems like that. In any case, after some false starts, I settled on a collection of problematics that represented important elements of continuity between past and present memory discourse. The problematic of storage, whether expressed in terms of wax tablets or hard drives, would be an example.

Such are the amusements of old age. For, needless to say, the topic of memory has attractions for the old that go beyond purely academic considerations. In my own case, this topic has also allowed me to indulge a fondness for transgressing intellectual boundaries that marked my academic career from the start. I am happiest at the location in which I have usually found myself – at the margins.

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