PATH IN PSYCHOLOGY

History of Psychology in Autobiography

Leendert P. Mos
Editor

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Preface

> My psyche is not a series of states of consciousness that are rigorously closed in on themselves and inaccessible to anyone but me. My consciousness is turned primarily toward the world, turned toward things; it is above all a relation to the world.
>
> Maurice Merleau-Ponty
> *Phenomenology of Perception*

Autobiography has an honorable place in the history of psychology. Already in 1930, Edwin Boring and Carl Murchison asserting the importance of the study of history for the discipline recommend that individuals who greatly influenced the discipline as well as individuals on the fringe should put into print their personal histories as these bear on their professional careers. Fifty years later, T. S. Krawiec maintains that autobiographies, although not truly history, do offer a personalized account of psychology, and that the wisdom of the elders can be shared with the reader, because each contributor, in his or her own characteristic mode, is dedicated to the profession, and so as model inquirers of psychological science offer us a personalized account of psychology.

What is challenging to the autobiographer is to locate his or her life within the larger context of history, of the traditions that embed his or her life. Doing so is challenged not merely by the contingencies in the course of their individual lives but by the very manner in which they try to orient themselves relative to the historical context. It is from within the historical horizon that a biographer attempts to center him or herself so as to enable the possibility of an autobiography contributing as a scientific document to the history of science. The biographer as a prospective autobiographer must characterize an objective context, a consciousness unbounded in every which way, but retain a depiction of the self as the point of intersection if the work is to be autobiographical. To the extent that the historical context can be articulated such that the biography of the self is an expression and contribution to historiography, to that extent is the autobiography a contribution to the history of science.

For autobiography is of interest only if readers recognize themselves in autobiographical accounts. Not primarily sympathetically, of course, however intriguing the life recounted in the autobiography, but to the extent that a presentation of the self opens up an understanding of history through the significance of the autobiography.

Autobiography ought to attempt to write the self such that its depiction sheds new light, as a form of historiography, on the course of history. Only in this sense can autobiography challenge and illuminate another reading of entrenched traditions. Autobiography constitutes the kind of historiography, an encounter with ourselves, that enables a renewed understanding of the history of the discipline and a counterpoint to the science that cannot be readily disclosed.

Autobiography necessarily finds its limit insofar as historical movements find their point of intersection in individuals, and to understand oneself in relation to these movements, one has to move outside oneself into the social and cultural traditions that characterize those movements. Autobiography enables us to read the individual's perspective of their life course as it elevates the significance of that perspective within a historical context making this significance both less certain and freeing the reader from the particularities of the autobiographer's life course. One searches here for the distinction between a reflective consciousness of one's life course, and a reflective consciousness of one's place in, and contribution to, the intellectual course of one's life. The biographer of the self relies on his or her reflective understanding of their experience but always in terms of categories that emerge in their reflection on life as constitutive of their professional life as they have lived it. The categories that frame the course of one's life are those wherein one locates oneself in the historical course of the science. Retrospectively, the significances uncovered in reflecting on one's past are always excessive beyond their individual meaning, value, and purpose, and to grasp the coherence among the events in one's life – as one's yearning for wholeness - one is thrown back unto discourses of traditions in which these significances play a role in understanding one's place in the larger social historical context. Autobiography is then a reconstruction of one's place in the larger social-historical order reliant on the discourses of traditions lived and received.

Maurice Merleau-Ponty reminds us that articulation of one's place invites us to rethink and reorient our image of wholeness relationally, as emerging from the relation between self and world embodied in action. This concern with wholeness is crucial in the contemporary context of increasingly specialization and fragmentation of perspectives, as well as the totalizing tendencies of the discipline that have made the individual superfluous even as reality is a consensus of instrumentalities. Wholeness is also a concern of the biographer whose depiction of the self is inseparable from questions of autonomy and responsibility and inevitably proves to be dependent on the language of traditions. This sense of personal wholeness becomes even more telling in a discipline, which putatively takes as one of its tasks to question ascriptions of responsibility and autonomy. Merleau-Ponty among so many others has cogently argued for a dispossession, or marginality, of the self as expressive of the wisdom embodied in traditions that are the background to any and all efforts to find one's place. Dispossession here captures the otherness of traditions and so our engagement with traditions that exemplifies an aura of receptivity expressive of our freedom in relation to the world.

Merleau-Ponty writes of embodiment as a tension between two unattainable wholenesses. The wholeness of a seemingly unmediated experiential ground upon which reflection proceeds, concrete yet mediated, and the wholeness of ideality, language, symbolization, and expressive activity giving voice to the possibility of ideality. Embodiment is living the tension between two promises of wholeness in a broken world. In a way, writing oneself in finding one's place within traditions aims to overcome this tension and to recover the wholeness that is broken. If the wholeness of ideality is a reflection premised on the wholeness of the body, Merleau-Ponty recognizes this premise as one of tradition and institutionalization that cannot be redeemed, and hence the tension between the unattainable wholenesses remains in our every effort at reconciliation. The embodied self remains a mysterious hinge between the speaking-perceiving subject and the historical world wherein the yearning for wholeness is always situated within traditions orienting our individual and communal lives.

Orienting ourselves within traditions is necessarily a dispossession of self yielding, on reflection, a sense of marginality, which is simultaneously a creative resistance to and an affirmation of our collective humanity in yearning for wholeness, openness, and wonder. The challenge of the autobiographer is to recover traditions, lived and thought, without which we should be unable to configure our participation in creatively and critically thinking the ideality of knowledge. Creativity is here the pivot of traditions and the aspiration for wholeness; it is the tension of participating in traditions and engaging in formulating our understanding of the world. Marginality is living and thinking on the borders; undoing the normative in life and thought and of affirming what is meaningful in an alienated world.

In asking our contributors, representing two continents and four countries, to tell of their personal and professional life course in relation to the history of the discipline, I requested that they locate themselves within the discipline such that the reader is given to understand something of the formative power of both. This task demands that the autobiographer knows something of the historical objects, their connectedness and coherence, which characterize these as productive forces exhibiting the development of the discipline. Our contributors understand the discipline in a particular way, as having determinate possibilities, and find themselves as contributors to and participants in a discipline, marginalized, sometimes profoundly, from its various intellectual traditions. To the knowledgeable reader, this will come as no surprise; indeed, it is of their remarkable and yet marginal status in the discipline that our contributors were selected and agreed to contribute to this volume.

It is not my place to retell their contributions yet a couple of reflections are in order not on their contributions but on the contributors' inclusion in this volume. Our contributors take their departure from a strong sense of the "psychological" as belonging to their lived experience both within and outside the discipline. Their thinking about psychology has much to do with what psychology has to offer our understanding of life in living it. There is an equally strong sense that the discipline's self-understanding, in its proffered schools, theories, and explanations, is subject to the intuitions of the life nexus of our contributors. It is from within this life nexus that they find themselves marginalized, and marginalize themselves, in formulating their view of the discipline as a systematic and historical endeavor. In recounting their marginality, opposition, "go it alone," they do so in relation to the hegemony of the discipline's scientific-technological institutionalization, so as to preserve something of the intimations of how things become meaningful at all, not in doing but in thinking and living.

This volume was an extraordinarily long time in the making. At the urging of my friend Bob Rieber, I wrote a proposal for the project and we agreed on a list of potential contributors in 1999 and publishing arrangements were formally in place in 2002. In between, Kurt Danziger, Andy Giorgi, and Joe Rychlak had agreed to participate and submitted manuscripts within a year or so. The manuscripts by Erika Apfelbaum and Robert Rieber, whom I convinced to contribute, came later and went through several revisions. David Bakan's contribution came together once I received the letters and notes, which were in Bob Rieber's possession, and Fred Weizmann agreed to fill in David's last years. Carl Graumann was the last to join our contributors and was eager to revise and elaborate his recently completed German autobiography for a North American readership. Remarkably, all our contributors honored the spirit of our proposed theme: to write oneself into the history of the discipline. The contributions vary considerably in length and an editorial decision was made, given that the proposal aspired to seven contributors and a reasonable length volume, to honor the contributors' judgment of length. Moreover, the authors were granted considerable leeway in to their use of references and citations.

David Bakan died in 2004. As noted above, his contribution is largely based on letters and notes he provided in reply to several questions first posed to him by Robert Rieber, more than a decade ago. The two had been friends for years, and Robert had planned to preserve something of David's rather unconventional career as a psychologist years before the present volume was conceived. Eventually, David agreed to participate as a contributor to this volume but the care of his wife, Minnie, prevented him from reworking his extensive notes. I formatted the letters and notes made available by Rieber, and Professor Fred Weizmann, Chair of Psychology at York University, and David's friend and colleague for many years, contributed materials on David's later years at York. Both Professors Weizmann and Rieber read the final version of David' autobiography.

Carl Graumann accepted my invitation to contribute to this volume just as he had completed his German language autobiography in 2004. He had planned and was working on an extensive revision when he died in 2007. His contribution is a revision of his German language autobiography with some additional notes completed prior to his death and added by his wife Professor Lenelis Kruse. Lenelis Kruse and I are grateful to Professor Raleigh Whitinger, Professor of Modern Languages, and Associate Dean of Arts at the University of Alberta for his very fine translation of Carl's autobiography. The task of translation is a demanding one, and Professor Lenelis Kruse read with enthusiasm the final English translation of her husband's manuscript.

Edmonton, AB Leendert P. Mos

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Against the Tide: Making Waves and Breaking Silences[1]

Erika Apfelbaum

Für Max und Mela
In memoriam

Looking Back to the Future

La pensée elle-même naît d'événements de l'expérience vécue et doit leur demeurer liée comme aux seuls guides propres à l'orienter. (Hannah Arendt, 1972, p. 26)

[1] I am most grateful to Leon Rappoport for his thoughtful and critical reading of the various versions of this manuscript, for his extensive editing of French tainted English and most of all for his constant priceless support and intellectual exchanges.

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In the introduction to her book of biographies (*Vies politiques [Men in Dark Times]*, 1974), Hannah Arendt points out that as we question certain men and women about the fashion in which each has lived their life and evolved on the world's stage, we take the measure of a whole epoch and we illuminate what is common for everyone. The following narrative is directly in line with Arendt's observation, since my life has unfolded and been closely connected with a significant period in the development of social psychology. Accordingly, my story may provide some insights into the socio-cultural and historical changes in the discipline during the period in which I have been both its witness and an active participant/contributor.

Of course, it must be understood that social psychology existed well before "my" time, in the1950s. As I have noted elsewhere (Apfelbaum, 1986), during the second half of the nineteenth century, several factors were responsible for the growth of the social sciences. Most important, in Western societies, the industrial revolution and subsequent urbanisation radically disrupted the established social order. It became urgent to create new mechanisms of social control and categories of knowledge appropriate to the emerging mass societies. Attempts to conceptualize these issues flourished in what I have called the "proto-social psychologies" of the time. But a century separates the nineteenth century formulations from what is today acknowledged as the subject-matter and methodologies of social psychology. The current praxis of mainstream social psychology as well as its more recent dissident expressions was largely developed in the aftermath of World War II, and the discipline did not become fully autonomous until the 1950s.

In 1951, during my first year at university, I discovered psychology and later that year decided that I wanted to graduate in this discipline. By the 1960s, when I became a full time researcher in social psychology, it already had a well-defined subject matter - social comparison and influence processes, aggression, interpersonal and group conflict, etc., - and specific methodological guidelines. I have, therefore, been both a witness to and participant in the growth and chan-ging perspectives of the discipline during its "Golden Age" in the 1950s and 1960s (Apfelbaum, 1993b, p. 15–17), its subsequent "crisis" in the 1970s, as well as its later developments.

My research in interpersonal conflicts and bargaining during the 1960s attests to my initial commitment to an experimental approach to social phenomena. My work was rooted in what was then one of the leading paradigms of mainstream experimental social psychology. The fact that I was asked to review the research on this topic for one of the volumes of the Berkowitz series[2] (Apfelbaum, 1974) indicates the recognition I was granted from one of the leading authorities in the discipline. I had by then already become interested in studying the role of power in social relations but my approach to this was still based on the mainstream game theory paradigm. It was only when I began to directly question the continuing neglect of power and suggest that it may well be the most significant dimension of social relations (Apfelbaum &

[2] In the 1960s, the Berkowitz's series "*Advances in experimental social psychology*" and Allport's *Handbook of social psychology* were two of the major references defining the legitimate fields of the discipline.

Lubek, 1976; Apfelbaum, 1979) that I was criticized (cf. Deutsch, 1976; Triandis, 1979) and considered a renegade to mainstream social psychology.

The discipline was then in the midst of what Israel and Tajfel (1972) called the "crisis of social psychology." This was part of the wider critical reexamination of the theoretical and epistemological foundations of all social science research: "The time had come to take stock and see where we are and where we should go" (Strickland, 1976, p. 4). In this context, I was drawn into the world of history of ideas, and the early development of social psychology, to understand some of the blind spots in the discipline and to see why it had deviated from its initial "raison d'être". However, even when I seriously questioned the capacity of the discipline to take into account existing social conditions and pointed to its inadequacies, blind spots, and silences, I remained convinced that social psychology was important as a discipline that could provide a unique understanding of the historical, sociological, and individual contexts in which persons evolve.

Ultimately, my main interest has been to develop a framework for an integrative social psychology, which explores how individuals evolve/construct their lives at the cross- roads between their socio-historical and cultural experiences, as well as their sense of personal agency. My study of women in power positions (Apfelbaum, 1993a) represents this theoretical perspective, and my recent investigations of uprooting and memory are also part of it. I have only lately again encountered the writings of Maurice Halbwachs and Marcel Mauss, whose analyses and conceptions of social events and behaviors are seminal to such an integrative attempt.

So, it is the narrative of my three successive lives as a social psychologist during a particular period of history (both in terms of world events as well as intellectual climate and strategic scientific choices), which will be the subject matter of this chapter. But I am quite aware of the pitfalls inherent to writing one's own biography. There is a tendency to unfold the facts as if they necessarily had a logical order or some kind of internal consistency, whereas in reality no-one's destiny follows from logical decisions and rules. My "decisions" have never been fully free choices. Instead, one's life is the result of fortuitous meetings, encounters with unexpected events, and the vagaries of luck. Thus, my intellectual itinerary was not only determined by historical circumstances and opportunities, but also by the way in which I have (or not!) taken advantage of these opportunities. From this perspective, the evolution of my work over the years is illustrative of the type of social psychological analysis I consider to be necessary. In the context of this analysis, moreover, I will also discuss changes in the discipline, including the political, social, and intellectual environment in which these changes occurred. Yet in doing so, I do not pretend to take over the task of a historian. I am not an external observer. I am in the position of an engaged participant. Therefore, my narrative is necessarily biased: it is a construction filtered through my position in the world and in the discipline, through my own political and epistemological choices.

It will, of course, be mainly focused on my professional life, but I cannot simply hush up certain aspects of my private life insofar as they have had a direct impact on my praxis of the discipline: Thus, because of my North American partner, I led a "transatlantic" life, which made me, for a few decades, part of the "intellectual jet set" society, a permanent expatriate in both my home and my host country, giving me a decentered perspective. This has become a second skin, an integral part of my lifestyle and understanding of social facts. Even when not forced by political circumstances, uprooting has a painful edge to it (Apfelbaum, 2000b). Yet at the same time, it has some advantages. It has given me a distanced intellectual perspective. As an outsider one is less dominated by the ideological and institutional constraints, which rule society and scientific communities. Cultural idiosyncrasies and diversity become part of one's normal social environment and, rather than negating their reality, I have come to consider them as significant starting points for conceptualising social phenomena.

Nevertheless, both my intellectual home and institutional affiliation have remained French. The political and intellectual climate that shaped the social sciences in France affected my career choices. My generation lived through a number of histo-rical events, which have strongly determined our vision of the world and the way we have approached the social sciences. More than anything else, the key event was World War II. All the biographical accounts by French historians (Nora, 1984) and sociologists (Mendras, 1995; Marié, 1989) acknowledge its impact. Strangely enough, however, these accounts never mention the Nazi genocide, as if it had no impact and no epistemological consequences[3]. When I pointed to this surprising case of collective amnesia in a paper where I discussed Kren and Rappoport's publication *The Holocaust* and the *Crisis of Human Behavior* (Apfelbaum, 1982), it simply fell on deaf ears. The anti-colonial struggles were another important structuring event; for me, as for many others of my age cohort, the Algerian war was a moment of awakening to critical awareness of political life even if it did not have the same impact on the reformulation of social psychology as the 1968 movements did a decade later.

One last word about my position as a woman in the scientific community; especially, since I happen to be the only woman contributing to this volume. I have never been part of the boys club. I had sexual harassment experiences well before public attention was drawn to the issue. But at the same time, being a woman has never really hindered the advancement of my career. I believe that I owe certain invitations and promotions precisely to the fact that I was a woman and therefore not part of the implicit competition that existed among the boys: As an outsider, my promotion or invitation was a way to block the entrance of a male colleague. This might be a case of reverse gender discrimination. Perhaps I have also downplayed the gender discrimination component in my life because of my early exposure to "race" discrimination. I first experienced exclusion and discrimination in earnest, not as a woman, but as a young Jewish refugee when my entry into public school was denied on the ground that I was a foreigner. I learned French reciting the Christian prayers "Je vous salue Marie" and "Notre père qui êtes aux cieux" in a private catholic school where I was welcomed. But let me not jump to the end.

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[3] The historian Michelle Perrot (1987) is the only one who, in her autobiographical essay, comments on the fact that, during the war, she never thought of the deportees while she had often thought of the war prisoners.

Growing Up in "Dark Times"

"One is never through with childhood." Jean Ferrat (2000)

Origins

I was born in Germany, where my parents first met and lived for a number of years. They originally came from small towns in what was then the Austro-Hungarian Empire. I know little of my father's father. On the only photo I have left of my grandparents, he appears as a very handsome man, proudly sitting next to his young wife. Widowed at the age of 34, my grandmother raised my father and his three brothers alone, working for one of her cousins, who employed her in his shoe business. Her destiny reminds me of the fate of English women during Victorian times when, widowed or orphaned with no resources or social status, they were taken in by some member of their extended family and employed as governesses in their households. I did not really know my grandmother, but thinking of her struggle to raise four boisterous teenagers, while employed at the turn of the twentieth century when a woman's status was still fully subordinated to her husband's, leaves me with the image of a strong, opinionated individual.

My father's family was poor, while on my mother's side there was wealth. My maternal grandfather had a flourishing steel business and had his entries at the local squire's estate: he was what Poliakov (1955) has called a "court Jew." He ruled over his family with an iron hand but provided his three daughters with a solid education. My mother learned French, played piano, danced the quadrille, went to university but was never allowed to pursue her dream of becoming a gardener (she later hinted at the fact that she really should have immigrated to Palestine and joined a kibbutz). My grandparents' lifestyle was that of the bourgeoisie so well described in Arthur Schnitzler's novels: there were maids, governesses, and nannies, who accompanied the whole family on their yearly trips to famous Austrian resorts. A couple of years ago, I visited Freud's home in Vienna, at 22 Bergstrasse, and discovered, with emotion, that my grandparents and Freud spent their summers in the very same places where, who knows, they may well have met socially. The décor in Freud's apartment with its colourful Persian carpets and photos of vacations in Bad Ischl vividly evoked the stories, which my mother had so often recounted. Everything in Freud's apartment was so familiar that I felt I had come "home."

I am born in 1934: "dark times" to coin Hannah Arendt's view of events were already under way in Germany. Victor Klemperer's journal (2000) gives a striking account of the rapid deteriorating social and political climate immediately after Hitler's rise to power, even though people were then still profoundly divided about how serious or dangerous the situation could truly become. In our own family, one of my uncles opposed my mother's pregnancy, claiming that the times were too uncertain and the future insecure. But my mother would not yield. Having just gone through the loss of a child (my 7-year-old sister), she saw no point in living without children.

During the gloomy years of the war, this woman in her forties, who had up to then led a sheltered life, showed incredible courage in the face of intense danger, taking great risks to save our lives. Today I know what an invaluable gift her example has been for my own personal growth: She gave birth to me but I also owe my survival during World War II to her. And even more so because, despite the hardships, she maintained a compelling joy in life, which she passed on to me. Many years later, she once confessed: "I knew I would save you." And I, until her death, lived as if I was invulnerable. Being immortal was my way of repaying her for what the Armenian psychoanalyst Janine Altounian (cited in Apfelbaum, 2000b) said about the children of survivors of the Armenian genocide: they owed their parents a "bottomless debt for having received life [from their parents] at such an incommensurable price" (my translation: p. 13).

The courage and strength that my grandmother, my father's mother, and my own mother showed in the face of adversity have provided me with models of exceptionally capable women who likely shaped my own personality. Their strength was undoubtedly the foundation for my own independence, although I have only recently become aware of this. For many years, I dreamed of leading the dependent life of one of those Harlequin novel's heroines, who live happily ever after with Prince Charming, in full security of his attention, love, and fortune. But then I discovered that was just a hoax because these novels stop just at the time when the characters are confronted with the sad difficulties of daily life (Apfelbaum, 2001).

When I think back to my childhood, I remember a fairly matriarchal environment not so much because of the history of my own family and the early "disappearance" of my father (I was six when we were forcefully separated, and two years later, after having migrated from one French concentration camp to another, he was gassed in Auschwitz), but in the French countryside where I spent the war, World War I had already taken a heavy toll of young men, leaving many single women or widows. This came up again in my study of women in leadership positions (Apfelbaum, 1993a), when one of them remembered how insistently her war-widowed aunts had urged her to become a professional-independent woman so as to be always self sufficient and safe.

Wandering Times

As if she anticipated the catastrophe to come, my grandmother urged her sons to leave Germany. My father was the first of the Apfelbaum brothers to migrate. My mother advocated immigration to the US – was it a premonition or simply the occasion to satisfy her appetite for seeing the world? She took with her a little black book, which would until her death never again leave her purse (and now mine) with handwritten, patiently collected recipes of succulent pastries; they were supposed to allow her to earn our living, if necessary. It turned out to be a life saving item when, during the war, she baked for the farmers who paid her back with eggs and milk.

My father procrastinated; he did not think that we had to go so far to escape from the Nazi danger. We would be safe in France, the country of the Revolution and of human rights - had he forgotten that it also was the land of the Dreyfus affair?

My parents settled in Paris. I was barely mastering German, my mother tongue, when I was put into a French kindergarten and rapidly discovered the discomfort of not being able to make myself understood; it was my first painful encounter with "otherness." At an age when little girls played with their dolls and little boys played at war, the real war played its cruel games with me. The first ten years of my life were errant times; they were years of hide and seek, of sudden moves, of an unexplained arrest by the French army and a week later, an equally mysterious liberation. One by one, the familiar objects of my environment disappeared as the German armies advanced. I still remember the white furniture in my parents' bedroom, and the black piano, a Bechstein, which had also made it to Paris. Even today, I have a feeling of loss and estrangement when people recount what a delight it was to rummage among the wonders of their grandmother's attic. I am even more distressed when I think of my parents' library, which had vanished well before I could read. For me, the sensual pleasures of reading in the muffled atmosphere of one's family library constitutes a rite of passage in the lives of intellectuals, and I have always been envious when reading biographies such as Sartre's or Vidal Naquet's and of the privileged moments they spent in their father's or grandfather's libraries. Having never had this luxury, I feel as if I could never fully pretend to the status of intellectual, which Fine and Roberts (1999) have so generously conferred on me.

Rather than learning from books, I learned from experiences of uprooting and humiliation. Childhood was the rough time of uncertainty and daily struggles to survive; but there were also moments of heedless, innocent happiness. I remember the unique taste of pilfered wild cherries in early summer heat, the rustling of autumn chestnut tree leaves in the Pyrenean forests. In fact, I was at the time more afraid of the will-o'-the-wisp as I passed near the cemetery than of the German convoys, which regularly stopped in our school yard. Fear, retrospective fear, came later.

All in all, I led the ordinary life of a country girl, and was lucky enough to have no major interruption in my schooling. Indeed, I was different from the indigenous children and did not take part in all the festivities, which punctuate village life, in particular, those concerning the Church whose influence was still very powerful in the French country side. But this exclusion weighed less on me than the humiliations I witnessed in the classroom in which corporal punishment was still common praxis. (IQ testing was unknown, so that no one had warned our teacher that the 14-year-old daughter of the miller was mentally retarded and would never learn to read!). To this day, witnessing humiliation is something that I find unbearable.

I was ten when the war ended just in time to free us from our forced residency and allow me to enter high school in the neighbouring town. We had no money left, and at the age of 45, my mother took on her first paid job as a worker in a small factory.

Years of Silence

> *His silences are so fierce that I am unable to utter a single word "(Juliet, 1995, pp. 25–26, my translation).*

Following the years of wandering came the years of silence. Of these years, immediately following the war, I have little to say. It is as if these years had hardly left any significant imprint on me. Immediately after the few survivors of the Holocaust returned, "this event which should have never happened" (Arendt, 1964/87, p. 242) was covered over and followed by decades of abysmal silence. In fact, I feel as if I experienced the postwar years as an automaton or an alien in the world that surrounded me. Is this why I feel that I have learned so little during the high school years, even though I was a fairly good student? Or is it that in the well-to-do part of Paris where the school was located, the teachers were more concerned to prepare girls for marriage than to open the gates of knowledge and stimulate their intellectual appetite. School did not stimulate my curiosity or arouse interest in cultural events, may be in part because one of our teachers once scornfully declared that it was inappropriate to attend a theatre performance if one was not properly dressed up, excluding de facto the poorest of us in the class and the few who came from working class backgrounds.

Joining the Communist youth movement was a brief temptation since its meetings seemed to provide the comradeship (accurately described in the film Rouge Baiser), and a sense of belonging I so much wished for. I resisted the call not because of any sophisticated political consciousness, which I totally lacked at the time but because of an obscure fear of further alienation: it was not a deliberate move but rather an instinctual one. For a long time, I suffered from my inability to join "movements" or follow orders for the sake of a common cause; today I know this has saved me from being enticed into various dogmatic and/or sectarian movements.

I find that I have dwelt on the private part of my childhood period, although I initially planned to limit myself to what belongs to the *ego faber* aspects of my life. Is it that because we women are more willing to admit the deep connection between the private and the public aspects on our lives, whereas men, by guile, tend to focus only on the most general elements? Or is it that men take their destiny for granted and do not feel the need to look for its origins, while women, at least those of my age cohort, tend to retrospectively justify their achievements by referring to external circumstances? This is what I found in my study of women in leadership position (Apfelbaum, 1993a), and has also been noted by many other researchers.

Returning to the particulars of my own intellectual development, the account of my childhood belongs here because I have only lately come to realize how heavily the early years influenced not only my personality but also the way I approach and conceive of problems in social psychology. I was only ten when the war ended, too young to have taken an active part in it, and I always have had the feeling, almost a sense of shame, of having been only a passive bystander. In the wake of World War II, as the world seemed Manichean, divided between the brave and the cowardly, the question of how would I have behaved had I been a few years older must remain unanswered. Even later, in the social milieu of the rising social sciences, age has been a major discriminating factor: either one belonged to the resistance network in the same way as one was part of the Marxist or Ecole Normale network, or one was too young for that. Not being part of this cohort increased the outsider feelings, which my earlier wanderings had already given me.

An Exhilarating Discovery: The Sorbonne and the Potentials of Knowledge

I was just 17 when I stepped into the court of the Sorbonne for the first time, preparing for a degree in math without being convinced that this was the right track for me. But I had already refused the professional school, the newly opened Ecole Polytechnique féminine, which my mother, eager to make me financially self sufficient, had suggested. The director promised her students a safe future: "you will become an assistant engineer" or, even more promising, "you will meet and marry a student at one of those prestigious male engineering schools during one of the yearly organised balls." But I wanted neither of these opportunities! I did not see education as a path to marriage, quite the contrary; I dreamed that education would me give access to the world of men and put me on an equal footing with them. This is exactly what I found in the predominantly male math classes at the Sorbonne: true comradeship and passionate exchanges!

But there was much more. The Sorbonne concealed unlimited treasures. Knowledge was immediately available to anyone without distinction. Overwhelmed by the freedom that existed in this space, I became a frantic intellectual bulimic and suddenly very daring, probably because the Sorbonne seemed a magical refuge, an extraterritorial space protected from the burdens of the outside world. I was wonderstruck; I had found Aladdin's cave, and knowledge and learning became for me the antidote to all the lurking dangers of the world. The philosopher Gaston Bachelard was speaking about time and using poetic expressions such as the time crystal (le "cristal du temps"); I was under the charm: philosophy was poetry. In the near-by Collège de France, Maurice Merleau-Ponty or Claude Levi-Strauss held weekly public lectures; for Jacques Lacan, it was necessary to go all the way to the large psychiatric institution, l'hôpital Saint Anne. But equally exciting was the courtyard of the Sorbonne itself; it was a permanent happening. One could see Jean Piaget pacing up and down with his younger colleagues and then rushing to deliver his widely attended weekly lectures, before running out for lunch at the nearby literary café Balzar, where he was holding court and meeting students. From time to time, a social psychologist came running down from a tiny laboratory, located across the street, which also served as an office or meeting room, to recruit volunteers to participate in some group observation experiment (the Bales category system was very popular, as were the scaling techniques for attitude testing as well as content analysis). This all seemed quite mysterious but was yet another avenue to explore.

I discovered, almost by accident, the existence of psychology that first year, during a conversation with a student who had just given up natural science for psychology. It was a discipline outside the realm of the very limited program of philosophy available to science students. I quickly became a regular auditor at psychology classes, and even dared hand in an essay without being regularly enrolled. I decided on the spot that if I was not discovered and if I got at least a pass on the paper, I would give up math for psychology. So among all the possibilities offered by the Sorbonne, I decided in favor of psychology on the basis of a bet, albeit a much more modest one than Pascal's. At that time, there were no career openings at all for social science students, and it seemed like a great adventure. With hindsight, I believe that what attracted me most to the discipline was its empirical perspective, its self-declared scientifically rigorous approach to the understanding of social issues, and human conduct. This line of thought, which represented the generally accepted credo of the time, suited me perfectly then and continued to do so for a long time.

Social Psychology in the 1950s: A Science in Gestation

In those years, the social sciences were in the process of becoming, setting up new institutions, initiating new paradigms, and establishing their respective boundaries. This was happening simultaneously in the university and in the newly founded Centre National de la Recherche Scientifique - an institution, unique to France, which offers full time research positions in all disciplines. Sociology was not introduced as a discipline in the university until 1958, but psychology already existed, although all of its subdisciplines were not equally developed. There was the well-established psychology laboratory headed by Paul Fraisse, with a long standing reputation going back to Alfred Binet and his successor Henri Piéron. Henri Wallon and Jean Piaget insured the renown and legitimacy of child psychology. However, social psychology was still in limbo without clear cut territory nor defined boundaries. The "certificat de psychologie sociale" was created in 1946 and the chair was held by the psychoanalyst Daniel Lagache, a former fellow student of Jean-Paul Sartre, Paul Nizan, and Maurice Merleau Ponty at the Ecole Normale.

A "laboratory" was attached to the chair, and to run it, Lagache gathered around him people with very diverse intellectual and even cultural backgrounds, deserters, or renegades from philosophy, science, and/or politics. Jean Maisonneuve, Paul Durandin, Robert Pagès and Serge Moscovici who were my teachers were also the principal protagonists of this first group. This early generation of social psychologists was a generation without forefathers; their training was not in psychology, let alone in social psychology. This created a climate of euristic freedom; their diversity enriched the enterprise, giving it a stirring atmosphere of intellectual revolution.[4] Each member of the group pushed toward new unexplored spaces and little by little staked their claims to a number of social psychological issues and topics. The boundary between what was in and what was out of social psychology was not yet clearly defined, nor were there well-defined research traditions. The pioneer mentality that prevailed was exhilarating and spilled over into my generation. We

[4] Even years later, the *Laboratoire de Psychologie sociale* still was the meeting place of "marginals"claims one of its members (Jean Pierre Deconchy, 19/04/2000 in Delouvée, 2000 p. 60) "and it has been the *grand plaisir* of these exalting years. The very grand plaisir."

were willing to participate in the adventure despite the lack of safety for the future. We were not career oriented because there were no career possibilities at the time - this only happened much later (Mendras, 1995, p. 40).

During this period, everything was possible. Social psychology was then still closely affiliated with sociology and coexisted in the same institute, the Centre d'Etudes Sociologiques. When it was created, recalls the rural sociologist Mendras (1995, p. 57), "Georges Gurvitch, Raymond Aron and Georges Friedmann, acting like feudal lords, divided sociology into separate fields and distributed them to young researchers: handing the workers to Alain Touraine, education to Viviane Isambert, the women to Madeleine Guilbert, the literature to Roland Barthes, etc. "Robert Pagès invented social psychology"… "This is how the politics of science was at work at the time." The formal institutional split between the sociology and social psychology disciplines occurred in 1967, when financial reasons dictated that social psychologists should turn toward the hard sciences if they wanted to have the means to become competitive with North American experimental social psychology.

The major inspiration during these post war founding years for the social sciences came to a large extent from the US (Apfelbaum, 1993b, p. 16). Speaking of the sociologists, Mendras notes: "Except for the communists and their fellow travellers all my generation went to the US" (Mendras, 1995, p. 44).[5] For social psychology as well in the 1950s, a trip to the US was almost an initiation ritual. I remember the excitement in Robert Pagès's voice when he reported to us his experiences of the T groups in which he had participated in Michigan. By then, he had become the head of Lagache's laboratory, but despite his interest in group dynamics, he gave a firm experimental orientation to his research group, once the times permitted recruitment.

More generally, it was a period of economic expansion, which encouraged the development of the social sciences, including social psychology. In US, the importance of this discipline was recognized because of its contributions to the war effort, and this led to the growth of university positions and research funds (Apfelbaum, 1986, 1992, 1993b, p. 14). In France, a similar evolution occurred, although on a much more modest scale and my generation fully benefited from it. In the late 1950s, social psychology was "in." Now, it had suddenly become possible to make a career in the social sciences. There were jobs in industry (although not for women), counselling, or motivation research for advertising companies. Simultaneously, the Centre National de la Recherche Scientifique began to hire and this is how I became in 1961 a full time researcher at the Centre National de la Recherche Scientifique.

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[5] Upon his return from US, Georges Gurvitch, in 1945, led the project of a sociological center which was to be the Centre d'Etudes Sociologiques (Mendras, 1995, p.19); he was in favor of an empirical sociology; as for Jean Stoetzel, who took over the chair of social psychology after Daniel Lagache, he was very influenced by Lazarsfeld writings but also created the public opinion poll institute, l'IFOP in 1937 (p. 30) and was convinced that sociology had to develop into a Comtian social physics (p. 32).

The discipline had by now reached maturity with its own paradigms and clearly outlined theoretical and methodological orientations. The editorial boards governing its learned societies and scientific journals acted as gatekeepers, protecting the boundaries of the discipline, implementing the theoretical-empirical rules, controlling what was in and what was out. Quantitative methods were in, qualitative out, making subjectivity an outcast; laboratory deception experiments had become social psychologist's stock-in-trade, so that subjects' behavior were manipulated and then monitored within tightly controlled situations. These developments led to acceptance of the "…notion of a man as an emitter of responses, whose social nature and social context might be interesting, but coincidental" (Strickland, 1976, p. 4). As a consequence, Kurt Lewin's field theory as well as his conception of groups was only given lip service; questions relevant to democracy were out as social psychology increasingly shifted toward a behavioristic orientation. John Thibaut and Harold Kelley's (two of Kurt Lewin's former students) "translation" of the Lewinian notion of group into a behavioristic formulation is a case in point here: the analysis results in a "clean" social science, free of any political or ideological overtones. McCarthyism is in part responsible for the growing appeal of behavioristic models, and their increasing hegemony over social psychology. But the shift also contributed to making social psychology more acceptable among the hard sciences. In this process of normalisation, the dissident voices of theorists such as Fritz Heider and Muzafer Sherif were not heard, and they tended to become virtual expatriates from the discipline. As for more integrative views of the social realities, such as those expressed by Halbwachs (1924), Mauss (1969), or Brown (1936), they soon were forgotten. It is interesting to note that of the four volumes on antisemitism edited by Theodor Adorno during his North American stay, and which represent a systematic attempt to deal with a "social issue" in an integrative and interdisciplinary perspective, *The Authoritarian Personality* volume is the only one to have been integrated into the knowledge basis of the discipline. Adorno's extensive discussions of the need for a rigorous multidisciplinary approach, which among other things would integrate sociology with psychoanalysis, were never passed on to the social psychology students of the 1960s. This perspective got lost in the normalization process then at work, whereby social phenomena were translated into aseptic categories thought to be necessary for the development of general laws.

Becoming a Social Psychologist in the 1960s or the Discreet Charms of Mainstream

> *When I came into the job market nothing at first marked me out for this strange occupation: researching…It all seems to be the consequence of a number of chance improvisations that I grabbed (Duby, 1987, p. 111)*

In 1960, I applied to the Centre National de la Recherche Scientifique for a full time research position. My proposal to study the development of cooperative/

competitive social interchanges was based on experimental methods developed by game theorists. The circumstances were quite favorable, and I tend to think now that "I was at the right place at the right moment" borrowing this explanation to the justification given by the first women to access to high leadership positions and become cabinet ministers (Apfelbaum, 1993a). Given the increase in hiring possibilities, Robert Pagès was developing his team in the Laboratoire de Psychologie Sociale,[6] giving it an orientation, which encouraged experimental projects and mathematical formalisation at the same time as he was opening it up to the widest possible range of social psychological topics. In the midst of these developments, I was assigned – or may be I chose – the area of conflicts, bargaining, and negotiation. In US, research on conflict and conflict resolution had already become an important area. Funding was plentiful, partly because of the Cold War and the hope that psychologists would be able to contribute to the resolution of conflicts. The gaming situation borrowed from game theory research in economics was the most widely used experimental technique and helped make conflict research one of the leading paradigms in social psychology. So, the choice of my research topic was not entirely fortuitous. In France, moreover, given the limited number of researchers, I was at first almost the only one to work in this area and soon became part of the international conflict research community.

In brief, I was at the time very much in the mainstream of social psychology, and quite enthusiastic about participating in what appeared to me to be an enticing scientific enterprise aimed at shedding some light on human interchange patterns. Being center-stage in mainstream social psychology and receiving recognition for my work gave me legitimacy and a secure feeling of "belonging." But on closer examination, my theoretical orientation was, from the outset, slightly at odds with the framework in which most of the current research was being done. Therefore, as I look back at the decade when I worked with the gaming paradigm, the unfolding of my career and the reception of my research appears similar to that of John Garcia, which Ian Lubek and I (Lubek & Apfelbaum, 1987) have analysed in depth. The case of John Garcia was for us an illustration of how a mainstream community can resist the necessity for a paradigm shift in the face of anomalous data and dissenting results. Garcia's research was normally accepted for publication by mainstream journals as long as his "off" results remained couched in the language of the mainstream neo-behavioristic vision of learning processes. Things changed radically once he explicitly questioned the validity of the paradigm and from that time on, his articles were rejected by the same editors who previously had been positive. When we examined the origins of John Garcia's divergences with the neo-behavioristic dominant views on learning, we found that he had had a fairly eclectic training among cognitivists and that he received great support from his mentors for his unconventional initiatives.

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[6] Quoique les recherches soient variées au Laboratoire de Psychologie sociale, un trait dominant en serait sans doute la conjugaison de soucis de formalisation (conceptuelle et, autant que faire se peut, mathématique) et d'exploration clinique. Par ailleurs, le modèle mental de la vérification, même s'il n'est pas toujours appliqué (car on pratique aussi des enquêtes) est certainement l'expérimentation." (Laboratoire de Psychologie Sociale, 1960; p.216; cité par Delouvée, p. 38).

My own freedom toward the dominant way of approaching conflict issues can similarly be traced to a fairly unconventional training in psychology. The circumstances of my European apprenticeship at a time when psychology was still quite loosely defined and its boundaries not clearly delimited provided me and my generation with an eclectic training as well as a broad and relatively unified view of psychology ("L'unité de la psychologie" by D. Lagache was a strong major reference for us). The pioneering spirit that prevailed then in social psychology and among our mentors gave us considerable freedom. Furthermore, our evaluation systems at that time were much less constraining than those in North America, and this flexibility allowed me to think critically and develop a research program along less conformist lines.

When I started to work in the area of conflicts, theorizing on the subject rested mainly on two underlying assumptions about human behavior. The first defined social behavior as mainly driven by utilitarian motives, so that the course of interactions was determined by rational calculations concerning the future benefits following from various actions. The second assumption specified personality attributes as determinants of cooperative or competitive behaviors. Both of these views ignored the social, contextual, and relational components of human behavior. In fact, research studies in this area attempted "…to eliminate actual interactions between the players, in particular by matching the subjects with a pre-programmed stooge" (Apfelbaum, 1974, p. 104), a procedure that eliminates the partner's attitude from consideration. In putting the emphasis on linear causal explanations, research deemphasized the circular and reciprocal nature of all interpersonal relations. This excluded the possibility of exploring dynamic aspects of conflict, including the changing attitudes of the participants. Thus, in the research of the early 1970s, the relationship between the participants and their respective behaviors toward one another were not of central importance, and this made conflict primarily an *intrapersonal* rather than an *interpersonal* phenomenon. Also ignored was how the social context of the conflicts might influence their outcomes.

In contrast, my own research emphasized the relational dimension of conflicts. From the outset, I contended that to understand the outcome of a conflict situation, it is necessary to analyze the development of interpersonal exchanges as an ongoing process in which each party responds to the other's moves, and this "reactivity" can be formally described as a two way learning process. Such reactivity was introduced in the gaming experiments themselves by programming the stooge to respond differently depending on the behavior of the experimental subject. This research program combined my interests in mathematics and psychology in the effort to track the dynamics of interpersonal interchanges. I also introduced techniques to explore the subject's initial perceptions of each other as well as of the social meaning of the task (cf. Apfelbaum, 1974, p. 105).

Although my work deviated from the main body of conflict research, it was at first well received by the research community. I was asked to review the literature on conflicts and bargaining for the Berkowitz volumes on experimental social psychology, which, at the time, was the standard reference work defining legitimate fields of study for the discipline. In this review chapter, I devoted a large section to power, which in my later publications became more explicitly the basis for a call for a paradigm shift. But at first, my comments did not seem challenging, probably because they remained couched in terms which did not antagonise the mainstream research community. I limited my comments to pointing out a number of unattended issues. Namely, that (a), little research had been devoted to asymmetrical power situations, (b) that prevailing experimental designs in conflict research were unable to stage power struggles and such phenomena as "revolts, riots, and aggression,"….which have different internal logics and dynamics, and (c), that the gaming experiments were irrelevant because they do not take place within the context and perspective of dynamic social change. Even when power disparity is introduced as a variable, the experimental design is presented as established and legitimate – even if not explicitly defined as such – [which] excludes the possibility (or at least the perceived possibility) of challenging this legitimacy and of moving the conflict to terrains other than those defined by the initial situation. Experimental designs have built-in limits, which inhibit any behaviors other than those permitted within the circumscribed experimental situations.

These criticisms are in line with methodological issues discussed by Billig (1976, p. 310). Paraphrasing him, I would contend that in gaming situations, the most glaring, and yet neglected feature of the whole situation is the experimenter who creates the situation and defines its social meaning. In accepting to participate in the experiment, the subjects have to accept the social context as presented by the experimenter and are unable to challenge his/her authority. Thus, *when forced to remain in interaction*, in the experiments, subjects learned to cooperate, but not necessarily because they are willing to do so. The alternative of refusing to continue and leaving the situation was never considered. Consequently, the experiments are incapable of examining any uprising against authority, whereas in real situations involving conflicting or oppressed individuals/groups this can and does occur. To explore these issues I designed some exploratory experiments together with Bernard Personnaz (Apfelbaum & Personnaz, 1974–75; 1977–78). They were set up in such a way that subjects had the opportunity to debate not only the outcome of the situation but the legitimacy of their power disparity, and they did so (*see* Apfelbaum, 1974, p. 148). This made it clear to me that it was necessary to conceptualize, and to find a framework that would allow me to theorize about dissent, resistance, and the development of a sense of agency among the powerless.

Breaking Away: Shifting Paradigm

> *One expects intellectuals to share the spirit of their time but it is confounding that they remain its victim rather than offering their own view" (Furet, 1995, p. 19).*

In the direct aftermath of World War II, the reconstruction spirit led to the conviction, deeply entrenched in public consciousness, that the horrors of the war and, in particular, the Holocaust had been just "a momentary madness." Thus it became possible to follow the prevailing postenlightenment ideology and maintain faith in science as the royal road toward progress and greater human welfare. I was very much in tune with this perspective. Even the Algerian war did not disrupt the view that my intellectual activity was separate from my civic life. I saw no contradiction in being on the side of the anticolonial struggle during the Algerian war, while dutifully working in my laboratory, running gaming experiments that ignored such conflicts.

The 1968 movement, once and for all, broke the earlier consensus about the knowledge base of social psychology. It was a major turning point in my intellectual trajectory, a break away from my earlier praxis of the discipline. From then on, my work has been animated by the spirit of the 1968 movement. Because my office was located near the Sorbonne, I was in the midst of events challenging the traditions of the old Sorbonne,[7] and I took an active part in them. Furthermore, what was happening in the streets directly concerned me because it raised unavoidable questions about the relevance of my research activities to real world phenomena. The ongoing uprisings against authority and assertions of previously silenced groups were indeed manifestations of conflicts, yet unrelated to what I was studying in my laboratory. In my experiments, the subjects had no opportunity to speak up. I had conned them into believing that the experimental situation did not allow them to walk out. If science was to shed light on real issues in the world, these discrepancies needed to be addressed. I did not know then that I was on the way to losing some of my illusions about the neutrality of the scientific enterprise.

Within a few days, the students' early protests turned into a broader uprising: suddenly we were in a prerevolutionary period in France. During the month of May, the students' marches were met with police brutality; testimonies describing what went on were collected and published in the first book about this period, *Le livre noir des Journées de Mai* (Anonymous, 1968): I contributed to its preparation and publication and, at the same time, participated in as many meetings as possible. In heated debates, the basic values of society, culture, education, etc. were revisited and questioned. It was an exhilarating time. For all the money in the world, comments the hero of Schisgal's (2002) play "Le regard", I would not have wanted to be old during the 1960s, but it is almost a blessing to be old in the 1990s.

The events of that year and the years to follow left deep imprints on our life styles and social values as well as on the epistemologies of the social sciences. In August 1968, the Russian invasion of Czechoslovakia and the resistance of the Czech population against the power of Soviet Union brought yet another encounter with dissent and revolt: I had the opportunity of observing closely this resistance when, in September 1968, I participated in the first East-West conference on social psychology held in Prague. Then, in 1970, I spent the year in the United States. I travelled across the country presenting my research on interpersonal conflict while

[7] Since that time the Sorbonne no longer exists as an academic entity. There are now some 12 universities scattered around Paris. Today, one university has kept the label "Sorbonne" where no psychology is taught. Otherwise it is just a building, which hosts offices and classrooms of several different universities.

also participating in Black Power rallies and discovering various expressions of the counterculture and the rising feminist movements.

In short, the world and history caught up with me and the gap between the social realities of the time and our ways of theorizing about them in the secluded atmosphere of research labs struck me as inappropriate. The reductionist vision that our continued commitment to experimentation imposed upon the way we understood social events seemed totally misleading. The time had come to revisit the gaming paradigm for studies of conflicts and question its adequacy to deal with the current uprisings and struggles against oppression. And, beyond this particular case, it was urgent to explore the limitations that prevailing research practices imposed on the discipline's theoretical and epistemological orientations. If the purpose of social psychology was "to understand the main phenomena of social and political life" (Moscovici, 1970), we would have to reintroduce and take into account the dynamics and complexity of social situations. This meant going beyond the model that considered individuals as simple responders to stimuli while ignoring the broader context in which they evolve and which determine their sense of agency (cf. Apfelbaum, 1997).

I was not the only one to sound the alarm and insist on the necessity to reconsider social psychology's basic assumptions. On these matters, however, the members of the Laboratoire de Psychologie Sociale were deeply divided during the 1968 movement. Some advocated solidarity with the students, and actively worked at changing the research structures and institutions without ever challenging the basic assumptions on which their discipline was based. For others, however, such as Michel Pêcheux or me, the events and debates of that period called for a critical reconsideration of the whole discipline (cf. Kandel, 1999). But, even among us, there were some major differences. Michel Pêcheux, a former student of Louis Althusser at the Ecole Normale, took social psychology to task from a strictly Marxist perspective. Employing rigid party line language, he, together with Bruno, Plon, and Pêcheux (1973), denounced the bourgeois capitalistic origins of social psychology, stressing its individualistic orientation and its denial of the subject's autonomy. They further accused the discipline of serving the "economic and political interests of the ruling class," as well as failing to integrate the materialistic foundations of oppression and the fundamental character of class struggle (cf. Kandel, pp. 287–288). In their view, social psychology was beyond redemption. As Michel Pêcheux once confided to me, "I chose to work in social psychology in order to disrupt and destroy it from inside." Along the same ideological party line, Plon (1974), in another article, focussed his criticisms on conflict resolution research, denouncing its irredeemable flaws.

Even though I shared some of the elements of these critiques, my own position was radically different. I blamed social psychology for its blind spots, for having gone astray, and missed important meetings with its proper subject matter, but I was not ready, without a further "hearing," to throw the baby out with the bath and condemn it unconditionally. Unlike my Marxist colleagues, I did not see myself as a judge, prosecutor, or people's commissar. I remained convinced - and still am - that social psychology could offer a unique level of analysis that neither psychology nor sociology could provide. It could embrace the interface between the individual and the collective, and represent the tension between socio-historical forces and personal agency. This seems to me to be the unique terrain of social psychology. Clearly, the empirical directions taken by the discipline over the last few decades had distracted it from this goal, and it was necessary to understand why. Thus, my interest in the history of social psychology emerged.

A Voyage into the Past of Social Psychology

I ventured into the past of the discipline to examine its early roots and *raison d'être*. Accordingly, I pursued the early pronouncements and formulations of social psychology, and followed the lines of its development in the first half of the twentieth century as it matured into an autonomous academic discipline. And in this process, I unravelled its blind spots, mistaken directions, and ambivalent relations with socio-political matters. The voyage was full of teachings. My efforts in this area, in particular with Ian Lubek, brought to light entire lost social psychologies that had existed in France, such as the work of Hamon and Tarde (Apfelbaum & Lubek, 1982). But even more important in terms of the early existence of integrative views of social psychology was the discovery of Maurice Halbwachs's *The Social Framework for Memory* (1924), and of Marcel Mauss's integrative notion of total social fact (fait social total), as well as, in US, J. F. Brown's *Psychology of the Social Order*. All took into account the structural, cultural, and historical components of behavior together with the individual's personal motives.

As a result of certain realities (the need to be integrated in the scientific community of psychology) as well as for political reasons (*see* Apfelbaum, 1986), the complexities of social phenomena were progressively ignored in favor of oversimplified analytical paradigms. "Taking over the social questions but simultaneously trying to undermine their political components has been a constant result (or perhaps strategy) of the psychologizing of scientific psychologists. This depoliticizing of social questions was the preset condition for letting social psychology in as a subdiscipline. While academic admission was granted, social psychology had to provide legitimating scientific credentials and, in so doing, the social questions it asked were then stripped of their socio-political significance" (Apfelbaum, 1986, pp. 9–10). The interviews which I did, in 1977, with the early generation of social psychologists made it quite obvious how this depoliticizing was enhanced during the anti-communist McCarthy period. The trend then was to emphasize individual factors over social forces, and the result was to pushing social psychology toward a behavioristic perspective. Consequently, the effects of historical social factors (such as economic transformations) and the power inequities between groups remained outside the purview of psychology. In other words, my trip across history acted as a "mirror" reflecting how the discipline had been detoured away from significant questions of domination and power, into more trivial cul-de-sacs of interpersonal conflict. If I engaged in the work of critical history which "takes on a subversive function, destabilizing the very foundations of the discipline (Apfelbaum, 1992, p. 533)," it was not to destroy social psycho-logy but to be able to argue for a reframing of its principles. For me, the history has never been an end in itself, but rather, a means toward the end of reformulating its methods and subject matter.

Social Psychology Through the Looking Glass of Domination

More specifically, I wanted to find a framework to analyze real world liberation movements that could not be simply subsumed and described through the class struggle looking glass, as my Marxist colleagues advocated. Nor could such movements be adequately explored with a model that does not take into account the context and perspective of dynamic social change, or that ignores the relational, embedded, and circular dynamics of social relations. One also has to account for the fact that invisible silenced communities can, under certain conditions, express agency and resistance to the oppressive rules under which they live. How does one overcome humiliation? I was intrigued by such questions as: "Under what circumstances do underprivileged groups initiate resistance, challenge the legitimacy of existing system and engage in norm-breaking behavior?" (Apfelbaum, 1974, p. 149). Why, for example, did the Algerian uprising break out only in 1954? Why was there a rebellion of the Jews in the ghetto of Warsaw? And why did Black Power movements develop in the 1970s?

During the 1970s, groups that had been silenced for centuries suddenly spoke up and challenged the system. With increasing forcefulness, colonized populations, Blacks, women….denounced their oppressive situations and claimed recognition, legitimacy, and emancipation. But the existing models and theories of conflict, whether interpersonal, inter-group, or international, which had guided our research in the past seemed irrelevant to these new social realities. Questions of power and domination had generally been ignored (cf. Apfelbaum & Lubek, 1976). I searched in vain through social psychology but found nothing that addressed these issues or could be of any help to understand the dynamics and dialectical aspects of power relations.

"Where has all the power gone" was the initial disconcerting question, which I raised as an introduction to a chapter titled: *Relations of domination and movements of liberation: an analysis of power between groups* (Apfelbaum, 1979). Here was a major blind spot of the discipline, making social psychology "the late twentieth century hand-maiden to domination" much in the same way as "in the nineteenth century, biology provided the scientific discourse through which social domination and inequity could be justified" (cf. Fine & Roberts, 1999, p. 264).

The Ottawa international conference on *Priorities and Paradigms of Social Psychology*' in 1974, provided the first opportunity to raise and develop these issues publicly. A selected number of social psychologists had been asked to assess the progress of their respective research areas; my task, as I understood it, was to present the balance sheet on conflict and bargaining research. In my talk, later published as a coauthored paper with Ian Lubek (Apfelbaum & Lubek, 1976), for the first time, I unambiguously and extensively questioned the limits of our knowledge base in the light of the recent liberation movements.

Conflict research had originated in the 1950s, in the context of the Cold War. The spectre of two equally armed superpowers, each with a similar mistrust of the other's motivations and a strong desire to win, loomed as the paramount prototype of all conflict. Furthermore, as social psychologists adopted a "game theory" model for the analysis of conflict, they limited their analyses to conflicts of interest, because gaming situations assume that there is a basic consensus between the opponents about the goals each of them wish to attain. The game theory approach, therefore, rules out of consideration conflicts of liberation such as those noted earlier, where there is little or no consensus between the parties involved. Having examined the origins and limitations of current conflict research based on gaming situations, I set out to prepare the ground for a perspective on conflict, which would allow it to be viewed within a context of dynamic social change.

When I first gave my presentation, I was still strongly convinced that science was a self-correcting enterprise with rules for the determination of "truth." I did not believe that personal power issues existed in scientific circles, nor did I suspect that raising theoretical questions aimed at refocusing a given research field could be interpreted as a personal threat, or threat to the research community, and trigger angry reactions. So I was surprised when my discussion, which seemed to me crucial to the future development of this particular area of the discipline, was met with a strong rebuttal from Morton Deutsch (Deutsch, 1976). His remarks, often bordering on the *ad hominem*, seemed more concerned with my professional credibility than with discussion or debate of my ideas. The immediate consequence was a split among people at the conference, between those with traditional views of social psychology who would no longer have anything to do with me, and those who were ready to hear an alternative and/or critical analysis.

Indeed, I was arguing for an epistemological rupture by stating that questions of power should be at the center of social psychological analyses, that domination was the critical issue in social relations, and that we needed to reintroduce a structural perspective to social psychology (Apfelbaum & Lubek, 1976; Apfelbaum, 1979). This would open the way to a major reframing not only of the problematics of conflicts but also general social theory. With hindsight, it seems no wonder that this kind of discourse stimulated hostile defensive reactions (cf. Deutsch, 1976; see also the bitter-sweet concluding comments of Harry Triandis of my chapter in Austin & Worchel's *Social psychology of intergroup relations*, 1979). In the late 1970s, this line of thought, not only in social psychology but in sociology as well, was somewhat threatening to the Establishment, or at least "surprising," argues the French feminist sociologist Colette Guillaumin (1981):

> "…the *relationships* of domination and the actors involved in these relationships…[were] so seldom *thought about* that the discovery of the existence of the dominated actors, so surprising in itself, cannot for a certain period of time be integrated into their thinking" (Guillaumin, 1981/1995 p. 159 – emphasis in original – cited in Apfelbaum, 1999, p. 301).

Any researcher experiencing such criticism as was directed at me can be powerfully thwarted in one or more aspect of their scientific careers – publication, research funding, training students, career security – by the defensive reactions of a scientific community, which feels threatened (Lubek & Apfelbaum, 1987 p. 83). Following Deutsch's harsh rebuttal, I could have easily myself become a renegade against the discipline or at least been marginalized and dismissed from the international research community. But I was lucky: once again I was at the right place at the right time. It was the right time because in the aftermath of the late 1960s movements, there was an opening for alternative views to be heard. Or, to put it otherwise, for its own sake, the establishment needed to include a few token alternative voices and, as a critical social psychologist, I became one of them. I was invited to contribute to the textbook edited by Worchel and Austin titled: *"The Social Psychology of Intergroup Relations."* Alternative scientific circles were emerging in which I found my niche; they became my reference groups, my intellectual family, and helped me construct a new (scientific) identity. Today, these groups have attained significant professional recognition for their work in critical psychology, feminist psychology, theoretical psychology, and the history of the social sciences.

Undaunted by the experience at the 1974 conference, I went on exploring the various aspects of domination. How, for example, could micro social relations and individual behavior be analysed as reenactments of the macro level politics of oppression? And more generally, how could the power disparity between groups generate individual identity strategies? I was struggling to find a conceptual framework and language that could relate individual psychological processes to larger structural and cultural processes. That is, my aim was to analyse the dialectics of intergroup and intragroup processes, including the dynamics of group formation and fragmentation, and how this could influence individual identities, as well as how people might gain a sense of agency in the direst situations.

Clearly, this was an ambitious project and too much of a challenge to the standard practices of a discipline seeking primarily to establish straightforward causal explanations. Yet, the mechanisms by which subordinated groups can regain agency cannot be examined without also considering the strategies by which dominant group maintain their power. It is necessary to examine domination and subordination simultaneously, in a dialectical perspective that can show how they mutually affect each other. Thus, when a dominant group seeks to break down a subordinated group's cohesiveness to maintain its hegemony, the subordinated group seeks means to resist. And in addition to violent modes of domination such as genocide, torture, or terror, there are also more subtle, micro modes of domination at work. Degrouping (Apfelbaum, 1979, 1999) is one of the mechanisms that groups with more resources and privileges use to protect and perpetuate their advantage. It can take various forms such as creating a mythical standard and applying it as a universal law, ordenying diversity to stigmatize a group and exclude its members. As Memmi argued in *Attempt of a definition: dominated men*, "it is not the difference which always entails racism; it is racism which makes use of the difference." Tokenism is another mode of degrouping in which a limited number of individuals are given opportunities to join the dominant group. Conversely, regrouping – that is, maintaining or restoring a sense of community – is a collective response by which subordinates (re)create a common framework, for example reclaiming a common set of traditions, language, and social practices, which in turn provide the basis for individual agency.

Michel Foucault's works as well as Hannah Arendt's conception of the pariah figure (Arendt, 1976/1964) have been true inspirations helping me to overcome the conceptual limitations imposed by the narrowly defined boundaries of my discipline. Both provided important intellectual tools for exploring the potentials for resistance by subordinated and/or silenced groups. Foucault's seminars at the Collège de France, in 1975, were seminal for my thinking when he elaborated, in front of an attentive and dedicated audience, his general conception of power, insisting on its fundamental relational character and on the fact that it cannot be conceived without taking into account the multiple potential forms of resistance to it (Foucault, 1976).

The distinction Hannah Arendt made between the *parvenu* and the conscious pariah indirectly sheds light on the dialectical tension between degrouping and regrouping. The *parvenu* can be considered as enacting tokenism: adopting uncritically the values and norms of the dominant group and breaking away from his/her socio-historical roots, tokenism is the price paid for the privilege of assimilation into the dominant group. The *parvenu* who is always at beck and call of the dominant group remains in a precarious situation, as does the pariah. But the latter has chosen to be an outsider, to remain at the margins while refusing to repudiate his/her socio-historical integrity: this is an act of autonomy and freedom (of "humanity" to use Arendt's words), an active political attitude. To claim one's position as pariah, as Gandhi did in British India, is a way of forcing the society as a whole to acknowledge its responsibility for this exclusion.

When I first published my analysis of domination, there was not much of a response from the social science community. The chapter (Apfelbaum, 1979) was even removed, without my knowledge, from the second edition of the widely distributed handbook of Austin and Worchel on intergroup relations. I only recently discovered that since then, despite its "disappearance," the chapter has had an active, although subterranean life, copies being distributed like *samizdats* to successive generations of students (cf Gurin, 1999; Hurtado, 1999). Not only had my arguments not fallen into oblivion but they "provided a comprehensive and generative framework in which to place understand and reinterpret certain research programs" (Stewart & Zucker, 1999, p. 276).

Interestingly enough, the resistance against my attempt to introduce the subject of domination within social psychology some 20 years ago still exists today. When my chapter *Relations of Dominations and Movements of Liberation* was recently republished, Patricia Gurin (1999, p. 279), noted in her appraisal, "Even today, most social psychological theories of intergroup relations fail to talk about power at all," while Stewart and Zucker (1999) add that arguing for direct linkages between large-scale, macro-level social structures and individual psychology "remains woefully marginal or forgotten in the discipline of psychology" (p. 296). Why is it still so subversive to deal with issues of power? Perhaps it is that the disparity which exists between those who are granted and those who are denied rights and privileges makes it difficult to continue using quantitative methods, which are relevant only as long as one assumes that individuals are interchangeable, similarly motivated, and pursue identical goals. But the burden introduced by a focus on the analysis of power goes far beyond a simple question of choice in methodology. In dealing with power and in stressing the structural disparities exis-ting in society, one cannot avoid exposing the flaws and fallacies in prevailing views of democracy. The pattern here appears similar to the reaction against feminist political scientists (Pateman, 1988; Varikas, 1995), when they denounced the sexist fallacies of "egalitarian" citizenship and the falsehoods contained in the notion of universalism.

Uprooting and Communication Across Cultural and Traumatic Boundaries

With the ending of the Cold War, if not before, uprooting began to be recognized as a major socio-political reality. In much of the world, political upheavals or economic necessities pushed growing numbers of people away from their homes, forcing them into uncertain journeys with little more than suitcases filled with artefacts from their past lives. If the end of the nineteenth century has been labeled as "the era of the masses" (Apfelbaum, 1990; Moscovici, 1985), the end of the twentieth century may be understood as the "era of uprooting." Although the prior century had witnessed massive migrations from rural to urban areas that disrupted traditional social settings, uprooting was now transforming the deep physiognomy and structural features of our social space. At both the societal and individual levels, efforts to communicate across cultural divides created new tensions and identity conflicts. These social and individual problems resulted from the coexistence, within the same space, of communities with different cultural backgrounds, values, and histories. Such issues can only be addressed within a conceptual framework that takes into account the increasing heterogeneity and changing realities of social life. Social psychologists have developed substantial knowledge about the construction of personal, social, and collective identities when people are living in stable conditions environments, but have not really explored how people cope with major social and political changes, and how such changes affect their sense of identity and feelings of belonging. We know very little about how one can "socially be in the world" following major socio-political disruptions. We have failed to explore in earnest the full range of social and psychological injuries associated with uproo-ting, the realities of dislocation, and their profound consequences for the human condition.

In retrospect, the work I did with Ana Vasquez, a political refugee from Chile, was the first step toward my concern with these questions. I met her shortly before the 1976 international congress of psychology, where she was to present a paper, based on the experiences of former inmates in Pinochet's prisons, on the uses of psychological techniques in torture. Together with a few other colleagues, I helped her prepare the paper for an academic audience. This first encounter marked the beginning of our friendship and research collaboration. Ana soon took an active part in the small research seminar that I was running for my doctoral students and a few academics who shared similar interests in institutional and political power struggles. We were trying to develop an appropriate theoretical framework that would allow analysis from the perspectives of both dominant and subordinate groups. We also wanted to focus on resistance, and attempts to gain agency, rather than simply describing submission and passivity. As noted by Cherry (1999), the seminar was "…a group of immigrants, exiles, outsiders of some sort or another, to our societies and to our disciplines (p. 274)." We debated questions of objectivity, unearthed critical early historical formulations of social psychology, and explored the means of giving voices to those who had been denied the right to speak up. These discussions were seminal for the elaboration of a critical social psychological perspective.

Much of my work with Ana Vasquez (Apfelbaum & Vasquez, 1984) was based on the extensive narratives, which she had collected from her fellow countrymen and women as well as from political refugees from other countries in South America. Their voices spoke of personal dislocation and the devastating consequences, which follow when those social frames of reference providing one's sense of identity are shattered. They seemed to echo Hannah Arendt's account of her painful experience of uprooting after her flight from Nazi Germany:

> We have lost our home, our foyer, that is to say the familiarity of our daily life. We have lost our profession, that is to say, the assurance of being of some service in the world. We have lost our maternal language, that is to say, our natural reactions, the simplicity of gestures and the spontaneous expression of our feelings (1964/1987, p. 58, my translation).

The Chilean political exiles, having escaped Pinochet's imprisonment and torture in their home country, now found that the forced uprooting meant much more than just the loss of their home place, or what Norbert Elias (1950/1987) called the *habitus*. It meant the failure of long-standing commitments to values which had defined their *raison d'être*, and thus the disintegration of the basic fabric of their former identity. As a result, they could see no possibility, and perhaps had even lost their desire, to elaborate any new life project, especially within a foreign setting, no matter how welcoming and friendly (Apfelbaum, 1999). They became orphans detached from their life projects and, still bewildered, frequently repeated, "I have lost my identity." This key expression epitomized their pain and distress at having suddenly become politically divested and culturally irrelevant.

But there was a surprising gender difference among these exiles. When interviewed, the women never expressed distress similar to that of men, although they had also been professionally and politically active and had experienced the same loss of their social *persona* and political hopes. Yet, many of the men seemed to be at total loss, whereas most of the women were kept busy carrying their family through the daily hardships of adaptation to the host country, becoming caretakers and homemakers. These highly gendered functions seem quasi-universal, having no territorial, social, or cultural anchorage; they can be performed anywhere. More importantly, through these traditional activities, the women created a bridge between their past and present worlds, keeping alive their cultural roots and the memories of the world left behind. Their traditional home maker activities thus became a truly socio-political role.

In confronting the "identity crises" voiced by Ana Vasquez's exiled compatriots, I increasingly came to doubt that currently accepted theories of identity proposed by social psychology could adequately encompass the full range of relevant issues, especially those manifest in periods of political turbulence, when migrations and uprootings are involved. As I reflect upon my professional trajectory, there seems to be a certain *"déjà vu"* pattern here, because once again, historical social realities caught up with me, and opened the way to a critical reappraisal of mainstream social psychological theories. Just as the observation of the emerging liberation movements of the 1960s and 1970s had earlier suggested the limitations of traditional conflict theories, the realities of uprooting now led me to reexamine the existing conceptions of personal identity. During the past 30 years, mainstream social identity scholarship had mainly adopted a perspective that focussed on the individual, and generally assumed that the environment was unchanging and stable. As a result, little attention had been paid to the role of such broader contextual factors as historical events and socio-political forces in the development of the individual. But the disrupting effects of expatriations on people's lives reveal the importance of these contextual factors and underscore how much one's sense of personal integrity is linked to changing realities in one's environment.

Forced uprootings are clearly not the most common occurrences in people's lives. Nevertheless, they are of theoretical interest because they highlight identity processes, which otherwise might remain unnoticed. Furthermore, even though political upheavals represent extreme cases of social disruptions, they stand as test cases of the much wider spectrum of social ruptures, which, during the course of a life time, modify our social environment, threaten our previous social adjustment, and consequently affect our daily existence. The implications of such changes, at an individual level, may be for the better when socio-political changes provide new opportunities - as was the case when women were granted the vote, or equal opportunity policies were promulgated giving women the option to move beyond their traditional social roles. But they can also be for the worse when new policies deprive whole categories of people of earlier taken for granted rights - as happens, for example, in times of economic recession. Whether they open or close opportunities, the changes induce a sense of insecurity and loss, disrupt established habits, familiar interchanges and earlier socially acknowledged ways of being andcall for a repositioning of the person within the new social context. By analogy with forced uprooting, which is more intense and abrupt, I have come to speak of social uprooting when environmental changes require people to adapt and alter their personal adjustments. Any form of uprooting involves a price that must be paid as people lose the security of their familiar situation and seize opportunities to assert self determination and agency. The case study of working-class British women who have become professionals described by Valerie Walkerdine (1991) in her film, *Didn't She do Well* examines the problems faced by women engaged in a process of upward mobility. This example of social uprooting emphasizes the burdens and the severe feelings of alienation, which the women experienced both in their new milieu as well as in their original home places, even though it was their choice to move from one life space into another.

More generally, the ways in which one takes up and deals with the challenges of social uprooting provide revealing insights about one's identity:depending on idiosyncratic personal characteristics and personal history, the uprooting may put the individual at a total loss, or it may open the opportunity to break away from earlier constraining social norms, customs, and traditions, and become a pathway toward personal development and creativity (*see* Apfelbaum, 2000a). Responses to uprootings vary greatly from one person or category of persons to another. An exemplary case in point is the variability we found between men and women's ways of coping with exile in the sample of political refugees from South America (Apfelbaum & Vasquez, 1983). The personal givens that are often seen as defining us right from birth (sex, social, or ethnic origin) are by no means permanent. Instead, they should be viewed as the personal frameworks or filters through which the changing socio-historical context is processed, takes on particular meaning for individuals, and may serve to initiate or reorient their life project. Each person replays them in his/her idiosyncratic unique way; thus each life represents a unique narrative, which reveals how we cope with change and organize, within specific cultural and socio-political circumstances, the various elements of our personal history.

Rarely does life follow a steady stream, and, to the extent that we do not live in a vacuum or in an invariable social space, ultimately a life course can be conceived as a succession of existential uprootings, all of which follow from the various life challenges that confront us.

As I followed this line of thought, I was progressively compelled to shift away from a deterministic conception toward a more dynamic perspective on identity, viewing it as a fluctuating equilibrium, a permanent, ongoing negotiation with changing social realities.

When viewed in this perspective, the study of identity shifts to the study of strategies of adaptation, and agency becomes a key issue. This also demands a shift away from theories of the person as just another passive source of responses mainly determined by his/her original givens. In others words, all situations provide a certain degree of freedom; it is then up to the individual to appropriate this freedom depending on the price he/she is willing to pay for this move. A case in point is the story of a former dancer who, when ordered by an SS officer to dance as she was about to enter the gas chamber, complies and uses the opportunity to seize the officer's gun and shoot him, thereby regaining an existential moment of agency (Bettelheim, 1966, cited by Apfelbaum, 1974, p. 151).

During the 1980s, I had an opportunity to empirically explore this theoretical framework. In 1974 President Valéry Giscard d'Estaing had appointed four women as Cabinet Ministers. It was the first time, in France, that women were given the opportunity to take part in politics. These nominations were in part the President's acknowledgment and response to the ongoing struggles of the feminist movements. From the late 1960s on, they had strongly challenged the basis of the "gender contract" (Rantalaiho, 1992), calling into question the rules that informally regulate the relations between men and women and determine the socially legitimate *habitus* or social spaces ascribed to women. For all women, this period has been one of major social changes. Laws were being introduced that opened a number of new social opportunities, and the media's changing representations of women's social roles encouraged them to move beyond their traditional ascribed *habitus*. As gender boundaries became more permeable, women could more openly and explicitly nurture professional projects. Making incursions into spaces until then thought to be closed to them became socially more acceptable, therefore less "risky" and more frequent.

This period saw the first large scale movement of women into politics. It seemed to me to offer a unique opportunity to explore, in situ, how women could move away from their traditional locations. What personal qualities and social circumstances allowed them, particularly those gaining high level political position, to take on such a major challenge? What obstacles did they have to face at both the public and private level? In brief, what price did they have to pay for migrating into a new social and professional location? In fact, as they ventured into spaces away from their expected traditional home places and transgressed the boundaries of ascribed social roles, they were often seen as "outsiders and gender expats" and became the object of all sorts of derogatory gibes. This was especially true when they moved into politics, a public space which was considered, especially by the French, to be reserved for males. Thus, women in high level political position were at odds with their female peers, and at the same time were not fully accepted by their professional male colleagues. They had to face and cope with the burdens of the double marginality, which resulted from their "transgression" (Apfelbaum, 1993a). The migration of women into politics became in my view, a test case for all gendered uprootings (Apfelbaum, 1993a), and an occasion to investigate various facets of the issues generated by social gendered migration.

I proceeded to interview the French women who had become high level political and managerial leaders. I later also interviewed their Norwegian counterparts: the cabinet ministers of both the liberal and conservative party because, as opposed to France, which was just then opening up the corridors of political power to women, Norway had already done this for a long time, with 40% of the cabinet positions being occupied by women. Most interesting was the cross-cultural perspective, which made it possible to examine the cultural, political, and value systems influencing the strategies and narratives of their rise to power positions. Here again, my work contradicted traditional social psychology approaches to leadership. I was neither a specialist in this area, nor did I intend to become one. Instead, my study of women in leadership positions was mainly one more occasion to critically evaluate the underlying epistemological assumptions of my discipline and show how they limit our ability to account for the world's evolving realities.

Liberation movements, uprootings, women's migrations into new social and professional location were some of the most pressing social issues confronting us in the last few decades of the twentieth century. To make sense of these phenomena, new approaches needed to be elaborated, which took into account the realities of a world in permanent flux as well as the heterogeneity of the people who make up our social environment. Why were both of these problems almost totally absent from the agenda of mainstream social psychology? One important reason involves the a-historical nature of scientific social psychology. It has generally assumed that we live in a stable environment in which people are defined by tradition and custom, and bound by the rigidities of inherited biological and social givens. But the truth is that we are repeatedly confronted with a world in permanent flux, where old allegiances and ways of being in the world are constantly challenged, shaken, and destroyed, and our established values and normative systems are called into question.

This reality of the human condition calls for a profound reevaluation of our ways of understanding relationships between the individual and society. Immersed in such a world, people are themselves in process, having to come to terms with the burden of seeing their world views altered by new political, cultural, and technological events. In this perspective, new theories of the person emphasizing responsiveness and agency are required, as well as a dialectical understanding of the interactions between ongoing socio-political trends and personal adjustments. The failure of social psychology to recognize and act on this perspective follows from its implicit epistemological assumptions. Their origins can be traced to the conception of the society that prevailed at the time when the social sciences were first formulated (Apfelbaum, 1986). It is the offspring of the liberal egalitarian tradition and of a representation of democracy based on the notion of universality: the *French Declaration of the Rights of Man* and America's *Declaration of Independence*, both, proclaiming all men to be equal. This vision of democracy is itself modeled on the classical Athenian ideal, where decisions were made by an homogeneous assembly of equal male citizens, speaking the same language, sharing to the same worldviews and traditions a priori excluding the "other"; that is, women, and slaves. Clearly, therefore, assumptions about democracy based on the Athenian ideal do not fit contemporary societies with their increasing flow of migration, socio-political, and cultural uprootings. Modern societies are made up of people with different cultural backgrounds, divergent socio-political traditions, values and differential access to power and resources.

Given the fundamental heterogeneity of our social environment, the structural asymmetries that determine the nature of social interchange and shape personal and public personae should be obvious. Nevertheless, they are rarely considered or discussed in the literature of mainstream social psychology. Correction of this situation would require new theoretical perspectives and research methods to understand the complex implications of diversity. It also would demand serious reevaluation of the ideology of equality underlying the praxis of social psychology.

Memorializing and Society's Politics of Memory

Although the foregoing critical comments may appear too radical, they seemed to be confirmed as I continued exploring the various implications of uprooting. It became clear that migration was disruptive not only because of the loss of cultural groundings but also because of the loss of one's historical roots. Individuals must not only adapt to changing realities, but their sense of identity also depends upon the collective meaning of their past. Yet social psychology has not dealt with this issue: the way in which the historicity of the persons, both in terms of their family sagas as well as of general historical events, determines their social being in the world. Social psychologists have so far viewed the social world, as unencumbered by the complexities of a long-term history, and accepted an equally minimalist view of the individual as a-historical, and decontextualized, more of an object than a subject (Apfelbaum, 1997). One can easily trace the origins of this epistemological fiction to the credo of a modernity, which dismissed the past to clear the way for a "new man." But contrary to B.F. Skinner's claims in "*Beyond Freedom and Dignity*", no one can live without antecedents (Piralian, 1994, p. 7). There is no utopian looking forward without looking backwards. The failure to look backwards prevents the possibility of elaborating new forms of subjectivity, argues Venn (2002). For social psychologists, it is therefore indispensable to explore the impact of legacy and conceptualize the processes of memorializing. This has been at the crux of my more recent work dealing with memorialization. Here, as in other facets of my work, the analysis is again grounded on the observation of extreme situations, such as genocide, torture, or apartheid, because the devastating consequences of being unable to take grasp or process the past are in these cases particularly acute and more readily visible.

"I can't throw a bridge between the present and the past, and therefore [I] can't make time move", claims Eva Hoffman (1989, p. 116–117), who, as a child after World War II, migrated with her parents from Poland to Canada.

This comment is a perfect illustration of the devastating consequences when a leaded silence hovers over the family saga, and also shows that to move forward, one must have a strong sense of the past. Knowledge of the past helps to process the present and provides a foundation on which to ground the future. Lacking such knowledge, children of both the Armenian and Jewish genocide survivors have described similar difficulties finding places in a world in which they feel like "cultural orphans" because of the mysterious empty spaces in their life histories. However, even when historical knowledge is available through official accounts, it often remains disembodied and can never be fully integrated with one's own history. It is the memory of our past that serves as a driving force and structuring factor in the construction of our identities.

My interest in these issues had been first triggered in 1977, while I was travelling across the United States interviewing my social psychology "forefathers" (D. Katz, T. Newcomb, F. Allport, H. Kelley, J. Thibaut, etc.) in conjunction with my critical historiography work. I found, in the bookstores of all the universities I visited, an abundance of first-hand accounts of Holocaust survivors, including narratives by their children describing how heavily burdened they felt by the silence of their parents, who refused to speak of their past history. I also was invited on several occasions to attend groups of children of these survivors where their "problems of being socially in the world" were discussed. Why had the silence within the family, enhanced by the collective social amnesia about the Holocaust, been so damaging? And how could one explain the unexpected efforts of survivors, after 30 years of silence, to seek a public forum for their personal history and memories? Clearly, as the years passed by, the memory of the Shoah was becoming more and more distant and ritualized, rather than remembered and directly narrated. Memorializing was a way to save this event from oblivion and delay the time when it would be nothing more than "mere history," but this was only part of the explanation.

I had observed no similar phenomenon in France but this was not really surprising since we have quite a different approach to social problems. However, the Holocaust also gained public attention when the French "deniers," those who denied the full reality of the Holocaust, claimed that no genocide had taken place. In November 1978, *l'Express*, a respectable weekly magazine (4/11/78) reported the statement of the former Commissioner of Jewish Affairs in the Vichy regime, Darquier de Pellepoix, who claimed that only lice had been gassed in Auschwitz. Almost simultaneously, the equally respectable French newspaper *Le Monde* opened its columns to Henri Faurisson who, in a brief article entitled "Good news," announced that there had been no gas chambers (Apfelbaum, 1983).

The simultaneity of the reclaiming of the Holocaust by survivors and of its denial by the negationists was puzzling and deserved attention. It was as if the taboo, which had been responsible for the prior years of silence and collective amnesia, had suddenly been lifted (Apfelbaum, 1981). Although the Holocaust deniers took advantage of the silence to disseminate their pernicious ideas, they were opposed by the testimonies of survivors speaking out against the collective amnesia. Both could be understood as reacting to parsimonious official narratives where the events had been publicly recorded, and their arguments emphasized the contradictions between private and public memory. It seemed to me that this situation was being played out at both the individual psychological level and the broader interpersonal level of society. Consequently, I set out to explore the different facets and interpersonal levels of memorializing, in other words, the interplay between private and public memory and the way in which the state politics of memory determines our social existence.

I have already emphasized that one's identity must be rooted in a historical continuity. But the processing and assimilation of the past is never just a solitary procedure. It is, on the contrary, a highly social process of communications and interchanges. "No one finds peace in silence, even when it is their choice to remain silent," claims Dori Laub (1995, p. 164). The vital importance of telling to exist socially in the world has been strongly documented, in particular by Armenian psychoanalysts such as Jeanine Altounian (1990) and Hélène Piralian (1994). But I also found great inspiration in the half century old writings of Maurice Halbwachs. He had already (Halbwachs, 1924) stressed the importance of interpersonal meaningful exchanges for memorialization. Namely, that storing individual experiences and emotions into memory depends on the possibility of sharing them with others and, I would add, on the trust the narrator has in the interlocutor's capacity to hear. Traumatic personal experiences and memories that appear meaningless to others induce silence and alienation from one's experiences and environment. Survivors of genocide or of other dislocating experiences, such as torture or rape, often report the sense of dissociation they feel between their private and public existence. Halbwachs also extensively demonstrates the way in which different social institutions (such as, for example, the family, schools, and religious systems) legitimize private memory by setting the standards for normative truth, or to put it in contemporary terminology, by determining the official version of events.

My own work followed this line. I went on to examine how telling and memorializing were further influenced - facilitated or hindered – by official narratives accounting for traumatic events, This is accomplished at the collective public level through history books, legal responses to collective violence, and various forms of commemoration. It is noteworthy that the near-continuous chain of genocides, and regimes marked by terror, torture, and gross violations of human rights throughout the second half of the twentieth century, and, on the other hand, the increasing concern with human rights have led, over the last few decades, to the invention of new and distinctive legal forms of responses to genocide, torture, and dislocation (*see* Minow, 1998). In the aftermath of massive violence, as societies transition away from terrorist or dictatorial regimes, they have found it necessary to address their past to establish the basis for social trust and peaceful coexistence between former adversaries.

Whether it is an official government "apology" for past harm (e.g., to the aboriginal peoples or to the Holocaust survivors when President Chirac recognized the responsibility of the state in the Jewish population's deportation from France), a reconciliation process, or an international tribunal for war crimes and mass rape, these actions all represent some form of transitional justice (Teitel, 2000) carried out by the state. Each of these actions provide an official framework to account for what happened. This allows victims to see their suffering and disruptive experiences as the consequence of a broader social cataclysm, and facilitates the beginning of a restorative process. Whatever form the official public narratives may take, they place personal experiences in the larger flow of history, serve to legitimize individual acts of remembering, and helps those who have been victimized to come out of anonymity, to regain their sociality and sense of historicity (Apfelbaum, 2002).

At a more general level, this analysis of memorializing stresses once again how individual well-being and social existence is shaped by broad societal currents and political currents and the necessity for social psychologists to include these dimensions in their analyses. To the extent that the state politics of memory defines public impressions of historical realities and is itself contingent upon compromises between ideals of justice and pragmatic politics, our sense of identity and social existence may be substantially connected with fluctuations of *Realpolitik*.

And What Now? Did She Do Well?

> *Everything, then, seemed clear and righteous. But now, I feel lost. Life is behind me and suddenly everything needs to be thought out again (Makine, 1995, Le testament français, p. 229 my translation)*

As I am writing these pages and looking/reflecting back at these years through the looking glass of the recent/young generation of psychologists, I become increasingly aware how presumptuous, and even ironic, might seem to insist, as I have done on several occasions, on the challenge that these ideas have been for the Establishment. Yet, they have been considered so at the time when they were first formulated. I am part of a whole generation of psychologists who have been significantly affected in one way or another by the changes in the socio-political and intellectual climate of the 1960s - the counter-cultural movements, the antipsychiatry movement, the civil rights, as well as the feminist movements. The result has been a deep commitment to a critical perspective in psychology, and the movement placed its protagonists in positions of outsider to their discipline; but it has also created a strong alternative scientific community, an intellectual family that helped each of us to continue to exist within the institution even when the price to pay was sometimes quite high/substantial (one sees here at work the process of regrouping, which I have described above). Clearly, things have moved along over the last 30 years or so and critical work is no longer the terrain of the margins (*see* Walkerdine, 2002, p. 2). Nor is it limited to addressing the pitfalls of psychology. It has gained visibility and become an established area. Two examples among many other possibilities, illustrates the diversity and variety of heuristically stimulating research trends, which are today fast expanding: the first Millennium Conference on critical psychology, held in Sydney in 1999 and more along the line of explorations in cultural diversity, the book edited by Corinne Squire on *Culture in Psychology*.

As this voyage into the past comes to an end, I feel that my life in social psychology has been well worth living. Is it because the autobiographical process is "first of all a task of personal salvation" (Gusdorf, 1980, p. 39)? Even though I have tried to be honest, no one can ever be sure of this. Autobiographical memory and interpretative appraisal are so intimately related that any final evaluation is likely to be biased in a positive direction.

Speaking of selective memory, I certainly have not given full credit to all the encounters that have been meaningful in my professional life. There is one, however, that I cannot pass over in silence because it marked a major epistemological turnabout in my thinking, which could (or should?) have led me to start working from radically different premises, or to even give up social psychology altogether - I did even for a while consider opening an "epistemological restaurant" as a gesture of protest.

The encounter occurred while I was in Kansas, interviewing Fritz Heider, and met Leon Rappoport, who invited me to give a talk at his nearby university. He drove me from Kansas City to Manhattan (Kansas) in an old, unreliable Chevrolet with a failing heating system, blowing alternatively cold and hot air. I was warned that the car could break down any minute and we could be stranded in the midst of the prairies, which I first discovered on this occasion. This landscape appeared very inhospitable and sadly monotonous, and it triggered off fearful fantasies of solitary confinement. I felt miserable and wondered why I had come until Leon started to speak of his ongoing work on the Holocaust with his historian colleague George Kren. He discussed their immersion in the Holocaust literature and the difficulty of finding ways to properly conceptualize the material, and last but not least, what he saw as the wide ranging epistemological implications of the Holocaust.

This conversation, as later the reading of Kren and Rappoport's book "*The Holocaust and the Crisis of Human Behavior*" (1980), stayed with me over the years because it opened my eyes to another level of critical consciousness and was the starting point of my growing doubts about social psychology as a whole. It later dawned on me (Apfelbaum, 1982) that perhaps Kren and Rappoport's engagement with such profoundly disturbing existential issues and their implications for all of us, including social scientists, had been possible, or at least facilitated, by their isolation in a stark rural environment, and relative freedom from pressures to publish or perish. I neither remember the details of the conversation, nor do I wish to summarize the main theses of their book. But the arguments developed in their conclusion remain all too tragically relevant today given the near-continuous chain of genocides throughout the second part of the twentieth century. I read them as an inescapable demand for social scientists to face the implications of the failure of Western moral values to prevent the horrific behaviors revealed by the Holocaust. "If one keeps at the Holocaust long enough, then…one knows, finally, that one might either do it, or be done to." (p. 126). If this conclusion is accepted, then conven-tional views of the Holocaust as a momentary historical aberration (Apfelbaum, 1982; *see also* Bauman, 1989) must be rejected, and we must reexamine our assumptions about the fundamental dimensions of human nature. The whole social science project has largely been based on the idea that people are intrinsically good, and if we can discover why they occasionally become violent and destructive, we can find scientific "cures" to prevent this.

Have we not as social scientists missed the relevant questions?

Let me end with a final word from the recent Nobel Prize winner Imre Kertesz (1996). In his book "*Kaddish Pour un Enfant qui ne naîtra pas*", he recounts how one of his fellow inmates saved his life one day by bringing him his daily food allowance at the risk of being shot. In this environment, such an altruist act was highly irrational, claims Kertesz, who concludes that ultimately, what needs to be explained about human behavior is the good not the evil.

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Reflections On My Years in Psychology

David Bakan*

Introduction (From a Talk Given at Cheiron, 1994)

I want to start out by indicating that the time I have spent in thinking about what I would say has been an awesome one for me. Not the least is the awareness that my professional life has covered a very significant proportion of what many writers on

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the history of psychology regard as its major significant history; that is, from the time of the adoption of the experimental method as the method of choice for psychology.

In brute fact, I took my first course in psychology in 1936 at the local Y in Brooklyn, taught by a young man by the name of Lit – I do not remember his first name - under the auspices of WPA. He was at the time, a graduate student at Columbia University, doing work under Woodworth. I entered Brooklyn College in 1938 at age 17. I was in a Department of Psychology which was nominally within the Philosophy Department; hence, I took both philosophy and psychology courses. One course was with John Pickett Turner who had been a student of Santayana; several other courses with Kurt Rosinger, including logic and philosophy of science. My special honors work was done with Martin Scheerer who had been a student of Kurt Goldstein. I also took a course in Social Psychology with Asch when both of us were attending the lectures of Max Wertheimer at the New School. And while I never took a course with Maslow, I became acquainted with him then and we continued our contact until he died.

In 1942, having just graduated from Brooklyn, I faced my first class as a graduate student in psychology at Indiana University even as my assistantship was in the philosophy department where I worked with Henry Veatch on the thought of Thomas Aquinas and with Jellema on Plato. I learned physiological psychology from Roland C. Davis, and learned to think of psychology as itself an event from Robert Kantor, from what he taught and the fact of his existence. I am grateful from the accidents of history which allowed me to be in touch with and to learn from some of the major figures in the history of psychology. I also confess that it is an awesome experience to recognize many of one's friends, teachers, and even students, in the many pages of books that pronounce themselves as "historical." I wish such experience on all, and indeed, with the growth of longevity, it may become a normal fate.

My aim is to provide a historical account and in doing so I face two problems. First, as someone once said, an effort at being historical is something like taking a spoonful of water out of the ocean. Second, although proximity to events in history provides one with a kind of empirical exposure not available to historians who come later, nonetheless, the dramatists/actors in history are not always the best witnesses to what it is that they create.

Allow me to state one generalization. I think it is fair to say that the obsession of the discipline of psychology, of at least my half of the century, is in being "scientific." This yearning for being scientific is coupled with an ever-recurring sense that when psychology becomes "scientific," it is coupled with a countersense that the most significant aspects of human psychological life are being by-passed!

It is, as it were, that there is a haunting interiorized tyrannical super-ego figure which we call "the scientific method," and that while it is often necessary to go against this super-ego figure, there is a lingering sense that this is a concession to weakness, moral weakness, or intellectual weakness.

I was once on a final Doctoral committee for one of Henry Murray's students. It was an extraordinary effort to quantify, statisticize, and operationalize some of the notions that seemed to derive from Murray's work. To me, and to others on the committee, the end-result was that there was no intellectual gain, no increase in understanding, a caricature of science in the name of "scientific method" as commonly understood. I had to leave, and Harry, in his wonderfully gracious manner, walked me out to the street to my car. Although there was no question about passing the work, I felt a kind of *emptiness* which I shared with Harry. I asked him why he would involve himself in this way and he pointed in the direction of the building that housed the then strongly experimental psychology department – as contrasted with the Department of Social Relation – and said, "You know we must always satisfy them."

That whole second half of the twentieth century, to the present day, while the essentially unscientific character of the so-called "scientific method" has been variously noted, the situation has not changed.

Robert Rieber in 1994 posed several questions to David by mail, following up on his reflections at Cheiron, and David responded, in writing, with the following.

Robert: ***What was there in your childhood that led to an interest in, for example, the Jewish mystical tradition?***

Ok, you get me at a good moment, having just pulled off a miracle of getting all my kids with their others and grandchildren together at one time and in one place. With seven grandchildren and the cities of Philadelphia, Boston, Montreal, Toronto all involved, but this as you know the day of email and the miracle took place partly because we could coordinate through email.

This is of course not what you wanted to hear about, although in a certain sense it is relevant to what you are asking of me. It is something which is reflected in me and the kind of psychology that I have been inclined to promote. Hence, let me use this occasion to make a preliminary observation about psychoanalysis as a psychological system. Of all the psychological systems available to us, psychoanalysis distinguished itself by the place of kinship in the system of psychology itself. It is the only system in which the facticity of being born of fathers and mothers and having ancestors, siblings, and offspring plays a significant role in connection with both the observations and the theory. How important is it, for understanding human being, to give attention to kinship? I am fully aware of the way in which what I am saying is politically incorrect. For after all we are in a democracy in which we try to transcend the significance of kinship. One example of attending to kinship seriously is aristocracy. We are against that. Another is racism, we are also against that. We are against favoring kin in connection with hiring; we call it nepotism. The democratic principle is that we are all treated equally independent of our kinship relationships. But we have to distinguish between what is the truth about human beings, and our political aims, and not confound the two. Indeed, I think a good argument can be made that the fullest recognition of what is truly the case in connection with kinship and human beings may serve to promote the advancement of democracy, rather than work against it. Although for the moment, as it were, it seems that we are there together with the worse bigot. So this is to the credit of Freud…there are also other credits.

Part of my misgivings about answering your question is that it gives the impression that I consciously knew, all along the journey of my life, what I was trying to do. Instead, I was simply, or so it sometimes felt, trying to satisfy some demons that would possess me. The human being is wonderful. The human being has a mind, is mindful, in a way that is different from any other thing I know of. The human being can apprehend forces that make things what they are and what they can do, and generate afresh the forces that make things what they are and what they can do. And in an extraordinary manner the human being is somehow in touch with the minds of other human beings.

Parenthetically, I really need the word he/she in order to express what I want to say. I would like to say human being, he/she this and she/he that. This is not a concession to the feminist demand that sexism be removed from all writing. It is that we need a term for a human being which both does always remind us that the human being is a sexual being, as the words he and she do. And at the same time we need a term which is not gender-specific. When you speak of a person as "he said" this or that, it always means that you must allude to the sexuality of the person; so it is also with the use of "she."

This is mystical. This is Cabbalistic it is also true that a most significant feature of the human being is somehow lost when we commit to he or to she. And we do not really help things along when we alternate the he with the she in different paragraphs or chapters. The Biblical author, who is the inspiration of all mystics, got it right. He/she said that man/woman was made in the image of God, male and female. Freud, again! He/she spoke of bisexuality, an original form of sexual polymorphy, and then of a genderless libido which was full of sexual content. I must confess that I did not reach clarity in all this until I began to seriously devote myself to the study of Maimonides' thought (see Interlude 3). I have never tried this but let get myself into the habit of using he/she as I highlight three things right at the beginning.

There is a wonderful way by which human being apprehends even when he has only clues and evidence. These are like the detective, and those like him/her, the scientist, the medical diagnostician, and the jury.

Then there is the wonderful way by which human beings create, bring things into existence that had no existence before, by intention and design. There are numerous models: the inventor, the parent, the father/mother, the farmer, the artist, the craftsman, the engineer.

Then there are relationships among people, ultimately cooperation and competition (in connection with discovery and creation). There are many models but there is some instruction in considering the warrior here, because the warrior is an example of the extremes of both cooperation and competition. Comrades in arms comprise an extreme cooperation, whereas war comprises the ultimate in competition.

He/she detects and discovers, invents and generates, and cooperates and competes. The duality and the integration involved in the duality of human beings is something I tried to delineate in my *The duality of human existence* (1966).

After these preliminary reflections, what do I conjure out of my psychoanalytical confession so that I can answer your question? Let me briefly peak of two things: polio and the "being question."

I was born in April (April 23, 1921). My family took rooms at a farmhouse in the Catskill Mountains for the summer after my first birthday. In August, when I was about 16 months old, I was stricken with polio there. My mother tells me how

I had a fever, and could no longer stand and how they rushed me back to New York to get medical attention. The treatment I was given was to have been placed in a plaster cast from my chest down to my toes; virtual complete immobilization. That event was of major importance in my life as I was later to learn in psychological self-examination. Not the least was a strong sense of "what is this about?" for a child, a toddler, who could no longer respond in activity. I have always toyed with the "being question," as Heidegger refers to this neglected topic of our modern world. I compulsively ask it about all things, for whatever is, it is inconceivable that it should not be at all.

This matter of "my" being comes with a kind of animal fear that we have about somehow returning to a condition of total nonbeing. In any case, the polio and its aftermath placed me under a kind of pressure such that I did not have the distractions from the "being question" I might otherwise have had, and as I think others have. Then the gush of sexual feelings: Freud, again. He was absolutely right about the sexuality especially in preschool years. The profound pleasure I felt when Ella Schrieber, my teenage babysitter, came to our apartment; feelings that went with curiosity, extraordinary curiosity about being. Being and sex were one and the same long before I learned that the Biblical author used the same word for knowledge and sexuality. Curiosity, doctor games with my cousin Zelda; but I will spare you these couch-memories. Importantly, there was much in my psyche that prepared me for the kind of thought associated with Jewish mysticism, in which the mystery of meaning and the mystery of sexuality were one and the same.

One couch-memory, just because it allows cultural support associated with psychological readiness. I must have been three years old, or younger. Some neighbor women were assembled in our kitchen, sitting around the table. I crawled under the table and worked my way up to Mrs. Dreyfus' skirt. She screams and giggles. My mother laughs and says, in Yiddish, *Seh nur vi er sicht fun vannen die fees vaksen* (just see how he seeks to find the place from which his feet grow). Such was my lesson: to find the origin of the Nile, as it were. To find from where things originate; that was the euphemism! Sex and origin are one and the same. So I was prepared for science, interpretation, religion, and mysticism: Freud.

Three other influences bear on your question: my grandfather, my friend Lennie Greenstone, and Rabbi Moishe Weintraub. My grandfather introduced me to Jewish mystical tradition by reading to me the stories of Hasidic leaders. Rabbi Weintraub introduced me to the major source of Scripture, Mishnah, and Talmud. Lennie introduced me to mathematics in a way that I got from none of my teachers. He grew up to be a mathematician; I grew up to be a psychologist. Lennie and I were friends all through high school and college days. He had an extraordinary sense of the romance of mathematics; in touch with the Pythagorean tradition and the great model it provided for a combination of rationalism and mysticism. Most contemporary views of mysticism are corrupted in having lost the intrinsic rational feature of the great traditions of mysticism.

An exercise that I engage in, religiously almost, is to prove – mathematical proof is demonstration of the objective existence of necessity – the Pythagorean theorem (that in a right triangle, one of the sides adjacent to the right angle squared plus the other side adjacent to the right angle squared has to equal the side which is opposite the right angle squared). The necessary relationship is intrinsic to the very nature of being, holding even during periods of history during which there are no physical things which are square; that relationship is ever *there* to be discovered. It is not invented; however, much all the apparatus for expressing it may have been invented. It was there even, say, four billion years ago, long before there were any human beings.

From this to mind: the mind of the human being is something which can apprehend the necessity of the Pythagorean theorem. The human mind can invent the methods of proving it. And somehow a teacher can act in such a way with respect to a pupil so that the pupil will come to apprehend it even is he did not apprehend it at first. There is a very special relationship between this ability on the part of human beings, and that which the universe itself is to itself without the human being to apprehend it. In this sense, psychology is fundamentally a kind of *religious* activity, where religion is a mystical religion.

Rieber: ***Why in God's name did you go to the Midwest?***

Let me take your question at its word. In God's name, and the Midwest, and what is the relationship? The easiest answer is opportunity. January 1942, I had been engaged in writing a senior thesis at Brooklyn College. Theoretically it was being done under the direction of Martin Scheerer who was the co-investigator with Kurt Goldstein in the testing of brain damaged people. Goldstein had written *The organism*, a great work which has unfortunately fallen into the great literary sink. I had taken off in my reading and advocacy on an organismic tack, but different from Goldstein with its intellectual Aristotelianism and more toward a biological Aristotelianism. I was reading and writing on von Uexkull, Jan Smuts, and von Bertalanffy (I must dig out the thing somewhere, sometime) and hence in a way was leaving Scheerer behind. One of my disappointments was that Scheerer did not come to the session in which I presented it as was incumbent on everyone who wrote a thesis. He made it clear that while he was my supervisor - and he helped me a good deal - he did not want to be identified with my thesis. His relation with Goldstein was at the level of empirical investigation and testing of patients - and not theoretically.

I confess that in those days the future for me was something like what happens tomorrow – next week always seemed very far off. Anyway I had accumulated enough credits at Brooklyn College mid-year to get the degree. So I went to the library, looked through the university catalogs, identified Indiana University considering costs, and because of Jacob Robert Kantor. Quickly accepted, I borrowed some money from my sister, packed my bags, got on the bus, and crossed the Hudson for the first time in my life, and so continued the [family] trek from Germany to Poland to New York.

That, in God's name, is how I got to go to the Midwest. I operated like a worm and had no bird's eye, or God's eye. Meanwhile, I had met this wonderful girl one night at an adjoining table in a restaurant in Times Square and, by early summer when she had finished at Hunter College, she joined me. We were married in Indianapolis on Christmas Eve in 1942, and celebrated our 51st anniversary in 1993.

The Midwest gave both of us an opportunity to do graduate work. We lived like medieval monks traveling from university to university. Millie had majored in mathematics at Hunter, and she obtained a masters degree in psychology from the State University of Iowa where she worked with Claude Buxton who was into learning with memory drums and nonsense syllables. Her contribution was to have been the first to apply analysis of covariance to the study of learning nonsense syllables. She then got a PhD in philosophy at Ohio State University on a study of actuality of logical propositions, an idea of which I am more enamored than she is at the present time. I got a master's degree at Indiana University in 1944 and a PhD at Ohio State in 1948.

But let me return to our academic pilgrimage later and at this point pick upon your question about the Midwest, again.

There is an injunction in Pirke Avoth, a tract in the Mishnah, which says that one is obliged to know how to answer an *apikoiris*, an Epicurean. The word is commonly taken as meaning a Jew who has become an apostate. But it has a deeper philosophical history and meaning. Sextus Empiricus, an expositor of classical philosophies, characteristically distinguishes between the Stoic and the Epicurean. The distinction is that the Stoic allows that there is meaning and that meaning and matter compromises reality. The Epicurean allows that there is only matter, atoms, their arrangement, and that all meaning is mere accident of the arrangement of atoms and has no reality of its own (see Interlude 2).

This is how to understand the Midwest, and its great Epicurean impact on the discipline. Behaviorism, dust-bowl empiricism so-called, Missouri as the "show-me" state, a language filled with thing-names and relatively vacuous of words representing nonthings, an impatience with whatever does not quickly result in a thing to eat, or wear, or shelter, or transport, or protect. There is desperation with a land so void of man-made things; without a sense of the leisure to reflect on meanings. A land which was so desperate it could not afford even a Sabbath. That is my theory of Epicureanism. It arises out of poverty, out of poverty of a land that is not built-up.

Soon after I had gone to the University of Chicago [as a faculty member in 1968], I visited David McClelland. He said something interesting about the University of Chicago: it was like when they got around to being able to import the piano in the Midwest. David had a deep understanding of the Midwest and about the cultural cost of building on land to the point of modern safety and comfort. When you are building a house you can think of little except the wood, bricks, and plumbing, and all else is luxury. But when you finally have a fine enough house, and you have filled it with furniture, a stove, and indoor toilets – that ultimate great luxury - then you can think of importing a piano.

If you are poor you have to think of things rather than meanings. And one is locked in poverty because one has to think about meanings in order to rise out of poverty. You have got to let go of things. But when one is poor one is afraid to let go and take a chance. This is a hard lesson. I remember Lois Murphy when the Murphys (Gardner and Lois) went to live in Topeka, Kansas. She found that the kids in that part of the world just had a greater appreciation of immediate space and time. And I, from New York, am never oriented with respect to north and south.

I was often the butt of jokes by my Midwestern friends when I did not know, inside a building, which way was north. But when I lived in New York City, I knew well at which station I could change from one train to another. And even though I did not know where Brooklyn was relative to the Bronx in compass terms, I knew which trains could take me where.

The people of the Midwest, whom I knew, knew no Sabbath, a day of compulsory nonwork, no work with material things. Only reflection and consumption were allowed. They give over the time that they are not at their jobs to looking after households and property which they otherwise neglect.

But back to your question, Mid-west and in God's name.

But first, New York City and Brooklyn College which in the years I was there were in ferment. Wertheimer had come to the New School and Gestalt psychology was strongly influencing the faculty at Brooklyn College, Solomon Asch, Rosalind Gould, Helen Bloch Lewis, Austin B. Woods, Herman Witkin, and Abraham Maslow. Their students who later became distinguished in psychology were Ludwig Immergluck and Sheldon Korchin, among many others. One great course in experimental psychology had as its textbook – imagine – Koffka's *Gestalt psychology*. The course had a mandatory laboratory component which was closely supervised and in which students conducted various Gestalt inspired experiments for half the course and then the original experiments for the remainder. Another great course was in animal learning; I cannot recall the name of the instructor but he was a splendid teacher. Milton Rokeach who was about a year ahead of me worked in the lab taking care of the animals and sometimes I would help him. In the course we did one experiment which always stayed with me. It was something from Tolman's lab, as reported in Schneirla's book, about how my rat absolutely refused to run a maze when it figured out that I had put him to a silly task.

We had a large universal maze, perhaps 6 × 6 ft., and we rigged it to make the rat go round and round and come back to get the food. After one run, the rat climbed over the wall and refused to make the longer run. Even the rat had a mind in which it could make a representation of the situation and plan an action which was agreeable with it understanding of the situation! Not until many years later did I learn of the Stoic position that one should study nature, and the act in ways which are agreeable with the nature one has learned about. Rats are Stoics. And I suspect that the human being, whenever he is acting effectively, efficiently, competently, and resourcefully is a Stoic. Never mind the principle that we simply tend to repeat what was reinforced; and never mind the assumption that everyone was essentially blind in the mind and no direction was coming from the mind. I had empirical evidence, knowledge, that the stimulus-response-reinforcement notion was not a sufficient ground for understanding psychological phenomena, not even in animals.

We heard about the work that Kurt Lewin was doing in Iowa. Note that Iowa is in the Mid-west. His great experiments on democratic, autocratic, and laissez-faire atmospheres! Lewin-Lippit-White was like one word, and those of us in the know, knew enough to say Levine and not "loowin."

Maslow was into people and seriously had academic concern with personality. Austin Wood was moving into that direction. Personality had become a topic – foot dragging and dashing, resistance and enthusiasm, at the same time, toward psychoanalysis. The possibility of a career as a psychotherapist for psychologists – the medical people had a monopoly – was showing itself strongly. Indeed, one other reason why I went to Indiana was to take a course with Louttit, who wrote, I think, the first textbook on clinical psychology. Louttit was not teaching the course when I was at Indiana but the course was being taught by Marion White, and I was introduced to actual clinical practice. I took some therapy with Austin Wood, extraordinary valuable hours both from the therapeutic point of view and for the sheer experience of psychotherapy.

My secret was reading. I have always said, in a nonarrogant sense, that I learned psychoanalysis from Freud himself. It is as though everyone who has learned from Freud has to somehow disguise their knowledge and give the impression that it comes from some other source (and this goes along with some Freud bashing at the same time). My Freud library started with the Brill translation in the *Modern Library* edition of Freud's works. I added the *Introductory Lectures*. In those days at the Brooklyn library when you wanted to take out a book you had to sign a card that had the names of everyone who had ever drawn the book from the library (the card stayed in the library until you returned the book when it was reinserted into the book and returned to the shelves). I remember the day when I was about to take out a book by Freud and, confronted by having to sign the card, turned the book back. I did not want a public record of having been interested in Freud. I could only imagine how my teachers on reading my name might injure their opinion of me for reading Freud.

That was Brooklyn. If we did not have the phrase "politically correct" it was distinctly not politically correct to be reading Freud at Brooklyn College. Nor was he politically correct in any Midwestern school with the exception of Chicago in which Freud was introduced into the undergraduate curriculum by David Riesman in Social Science – a course which was to have a vigorous life of its own for several decades. Freud also had a special place in the Department of Social Relations at Harvard.

Let me record the place of Wertheimer who, as mentioned above, came to the New School and took the psychological community by storm. Everybody who was anybody went to hear his lecture. I along with other undergraduates regularly attended his classes; we did not sign up, we just went to listen. Who was there? I remember Kohler there once, George Katona, and David Levy. Indeed, Levy was once invited to address Wertheimer's class with Wertheimer interrupting him with lengthy speeches such that at the end of the hour there was no time left for Levy to speak. Wertheimer good-humoredly and humbly asked if Levy would not like to come some, perhaps next, time and give the lecture he had planned to give!

However, there was a kind of turning point. I remember going to a kind of private lecture/discussion at someone's house in Manhattan. I can visualize the setting but I cannot remember whose house it was – could it have been Isadore Chein? In any case, the buzz-word was "operationism," a hot topic, and I recall Schneirla pushing very hard in favor of it. It sounded terribly disagreeable to me. About 1941, I think, operationism was a black cloud putting out the sun. There was no sun! There were only people thinking that occasionally they saw some light. All at once it appeared to be stylish to pretend to be stupid and ignorant. Maybe, the war, maybe – and this was the thought I noted above – maybe when the demands of life get urgent people get stupid. One weighs things without appropriate leisure. One becomes too invested in action and immediate outcomes to think about what is truly the case. Psychology was not the same before the war and after the war. Before the war, it was a thinking thing, then it stopped thinking and became a doing thing which bypassed the thinking thing. Put another way, there was a loss of interest in theoretical orientations and a rising interest in "research areas." Daniel Bell's end of ideology: the loss of any sense of how the very act of thinking gave direction to human activity. I can and do give myself credit for trying to hold on to a psychology as a thinking thing. Sadly, psychology got a job and left school from the time of the war. It was successful, of course. But it became tied to the "employer" in a way that was not quite the case before the war.

Back to your question about the Midwest: everything in the Midwest felt to me as distant from the mainstream of history. I recall thinking of that image once as we crossed the Mississippi driving toward Columbia, Missouri some 125 miles to the west (of Bloomington). The river was like history and its course. And I was 125 miles from the river. The advantage was that it gave me the most wonderful psychological space which I truly enjoyed while there.

I told you that I borrowed some money from my sister, got on the bus and went to Indiana University. I had already written to its housing office and had made arrangement for a room. The place was an old house with rooms let out to students. A woman greeted me and led to my room and, sitting on my bed I tried to come to terms with the shock of the trip and the unknown future. Before I even opened my suitcases, there was a knock on the door. The woman came in and asked if I was Jewish. I replied that I was. She said that I would be happier elsewhere. It was not, she said, that she was against Jews, indeed her husband – she was a widow – had been a minister and a good Christian. Proof that she had nothing against Jews! (Actually, solving the riddle of how being a good Christian could mean that one could not be hostile to Jews, has been an important exercise for me. I did not doubt the sincerity of her remark.) I walked over to the university housing office and the person there called the Rabbi at Hillel House, a place where I was warmly greeted. Soon some young men arrived – like Maccabeus – with a car, got my suitcases and set me up in another house, one with several Jewish students. It was Friday and that evening I attended the first Friday evening religious service I had been to in some time.

Kantor turned out to be a disappointment to me. He had one song. There were behaviorists, mentalists, and interbehaviorists. He was the only truly interbehaviorist. Sometimes I would think him to be brain damaged; at other times he seemed extraordinarily insightful but keeping it all to himself. His classes were very, very boring. Roland Davis was a treat. He taught physiological psychology and solved the mind-body problem. For every human function there was a coefficient of involvement for every body part. This is a brilliant concept which, to my knowledge, never did find its way into the psychological archives. The brain does not think. That is silly. But thinking cannot take place unless there is a brain, and some parts of the brain may be more involved than others.

A good thing happened to me at Indiana. I needed money but the psychology department in which I was registered did not have any for me. But I was registered in a wonderful philosophy course on Plato's *Republic* with Jellema. He was fond of me and offered me an assistantship in the philosophy department. I suspect I may have been the only graduate student in psychology with an assistantship in philosophy. I was to grade and tutor students in logic which was being taught by Veatch. I took a course with Veatch in Thomas Aquinas, something that stood me in good stead over the years in many ways. Not the least was Maimonides' influence on Aquinas, and in this sense I was already preparing for my detailed Maimonides studies by studying Aquinas. My qualifications for philosophy consisted of a course in Santayana with John Pickett Turner (who had been a student of Santayana), and a course in philosophy of science with Kurt Rosinger who also taught me some symbolic logic. But it was my friend Lennie who taught me logic with a boost now and then from Veatch.

Another great thing at Indiana was a chance to take a course in algebra with one of the finest mathematicians of out time, Emil Artin. His fame had not yet spread which, when it did, had him working at the Advanced Institute in Princeton. He did something on Galois' theory about which I understand nothing. Nor did I have much understanding of what went on in his class. Millie and I took the course together and, of course, with her degree in mathematics from Hunter, she readily followed the numerous steps in Artin's lectures. Giant steps they were and all I was capable of were baby steps. But the vision of a *real* mathematician wandering about easily in high spaces among all the dangers is like my image of a guide who might take you on a climb of the Alps (one can surely take one's images from one's wishes and imagination!). He used to talk about how he would be working on a problem in a continuous manner, through shaving in the morning and accompanying his wife to the concert in the evening. He was totally unassuming, totally charming, and eminently kind especially to those of us who could not follow his every step. His final exam was an oral in which he tailored every question he asked to the precise level of the student's understanding so that when it was all over it was one of the best classes of the year.

Speaking of mathematicians let me jump ahead (of what the Midwest meant to me), to Henry Mann at Ohio State University where I went in 1944 after getting my degree from Indiana. The Mann–Whitney test has become a favorite nonparametric test among psychologists. Mann was giving this great graduate course in mathematical statistics. The class had about five students; I was one, Whitney was another. Mann taught the mathematical ground of statistics in an incomparable manner. I know of two things that resulted from that class: my paper on the test of significance in psychological research (*The test of significance in psychological research*, 1966), and the Mann–Whitney test. Let me note one of Mann's major grievances against Fisher. He was angry that Fisher was either ignorant of, or concealing, the mathematical grounds of small sample theory. On the concealment side, he advanced several hypotheses. Fisher had great intuition and "just knew" a few things without the effort of doing mathematics. Fisher had someone who was helping him. Fisher had a strong foundation in mathematics which he simply concealed. I think that one of the handicaps that psychology has suffered has been that it committed to statistics as a research method of choice without sufficient examination of the foundations of the statistics that were being used. Many of the psychologists who pioneered the use of analysis of variance and covariance came from Fisher, on the one hand, and Snedecor – the statistical cookbook extraordinary – on the other. My chief example of the latter is Lindquist, who was teaching at Iowa, and with whom I took a course in statistics for psychologists and educators, as he called it.

Before turning to Iowa presently, I want to linger a little longer in my reflections on Indiana in those days. I want to mention Edwin Sutherland, and with it what I learned from working in the prison system in Indiana. Sutherland was a criminologist in the Sociology department at Indiana. He had come out of the University of Chicago and the Chicago delinquency studies. I took his course in criminology wherein he advanced a theory of crime, the "theory of differential association," as he called it. It was based on the assumption that crime is a cultural phenomenon associated with membership in some criminal subculture, such as gangs and professional criminals which were complex subcultures with values and norms, recruitment, enforcement, and educational practices. He dealt at some length with what appeared to be exceptions or counter examples, crimes of passion, of secret individual initiative, the insane, and crimes of the psychopath. In each instance, he made the argument that the person was in touch with and participating in a criminal cultural line within general culture. Indeed, he pointed out how some cultures, America and Australia, for example, had within them major criminal components. He would cite literature, the media, and even extensive bodies of literature celebratory of conduct which was criminal under the law. He mentioned the Boston Tea Party as an example of celebratory criminal conduct, and the implicit criminality in some of Nietzsche's thinking.

This perspective put me in touch with the notion of culture, culture and personality in the Department of Social relations at Harvard where I was to go later [in 1968]. For sure there was some beginning of this already among the Gestalt psychologists at Brooklyn College for they were reading Margaret Mead and Ruth Benedict. The idea of a social gestalt was also articulated in some of the Lewinian thought that was around. But Sutherland opened this whole domain of culture to me in a most substantive way.

In the summer of 1942 I went to the Indiana State Farm, a minimum custody institution not too far from Bloomington where the university was located, to work in the psychology department there. My mentor there was a psychologist named Harry Hawkins. In the fall I returned to Indiana University and then went back to Indiana State Farm in January 1943, after I married on Christmas Eve, 1942, in Indianapolis. We spend the whole of 1943 (except for the summer at the Yale School of Alcohol Studies in New Haven) between Greencastle, Indiana, and Indianapolis where Millie was working calibrating instruments for RCA. In January 1944 we went back to Bloomington for a semester to complete my master's degree with a study on the relationship between vacillation and intelligence which I conducted at the State Farm prison.

At the State Farm I spend a considerable amount of time with Alfred Kinsey, learning about method and human sexuality. Importantly, I learned from Kinsey how to work. He came to the State farm every week for a period of about 6 months. My office was relatively private, and allowed for the possibility of truly confidential interviewing. I helped Kinsey line up people for interviews and every week I offered him my office sometimes several days running. Every week we had lunch together and I learned in great detail what he was doing and finding. I never met anyone who controlled his working time and life so methodically; every minute was planned, including recreation, taking care of his own needs and his personal obligations. He even planned for flexibility. His fundamental principle of interviewing was that there was no terminal point. He was prepared to go on for days if necessary, collecting sexual history. His patience and meticulousness were unbelievable.

As a reward for my assistance Kinsey gave me one full day of his life. You have no idea what a gift that was coming from him. He invited me to spend the day at his laboratory at Indiana University where he went over all his gall wasp research, forthrightly answering my every question about human sexuality. But one of the most memorable things I learned was statistical. Everyone in those days was big on small sample theory. Kinsey of course was committed to large samples. He showed me a chart with a frequency distribution of a particular trait of the gall wasp. The distribution was patently normal with some irregularities. He then pulled out another chart based on a new sampling of gall wasps 10 years later. What was amazing that the irregularities were virtually identical! Kinsey said that most statisticians would have regarded the irregularities as random fluctuation, however this was not so. He explained how the irregularities were indicative of important genetic characteristics.

When Kinsey came to the State Farm, he would sometimes bring visitors in an effort to raise funds to support his research. Yerkes was on some committee from which he was trying to get money. He brought Yerkes to the State Farm once. I was thrilled to play host to both of them that day. It was clear to me that Yerkes had an enormous amount of respect for Kinsey's research.

At the prison, psychology and culture and my own learning and maturation came together for me in some interesting ways. Let me mention three.

Dartagnan

I learned about major historical cultural lines. Let me tell you about Dartagnan. One of the things I was trying to do while working at the prison was to learn French sufficiently well to be able to pass the language requirement for the Doctoral degree. I was struggling through a version of *The three musketeers* as I was trying to do my job in the assessment of incoming inmates. At one time we were getting a number of inmates from Evanston which borders Kentucky. Kentucky had a large number of people from remote areas that had been isolated from events associated with the great immigration, urbanization, and industrialization that had taken place in America. Many young men were coming in from the hills, the true hillbillies, on hearing of the availability of jobs. But on Saturday nights they would revert back to their old cultural patterns, be picked up by the police and brought to us. In retrospect, for surely I did not realize it at the time, I will tell you of an insight. One day I was deeply into *The three musketeers* and their shenanigans, and in comes this young man with blue eyes and blond hair – phenotypes reflect recessive traits when there is too much inbreeding – totally charming, gallant, polite, and ready to put his life on the line for the honor of women, drawing his sword or breaking a bottle of beer and using it as a weapon at the slightest hint of an insult. I looked up from my reading and saw Dartagnan. What I realized in the days afterward, reflecting on this "shock of recognition," was that I was witnessing before me the cultural trait depicted in *The three musketeers* written in a wholly different place and time. The Dartagnans of the day were immigrants to America in the eighteenth and nineteenth centuries, some of whom settled in the hills of Kentucky. The culture persisted in isolation, and this young man, coming out of the hills to Evanston attracted by jobs and good wages, manifested this isolated culture.

Mother-Fucker

There were a substantial number of black men in the prison population. In their common discourse, mother-fucker was an expletive just behind "fuckin" where the latter was used virtually as a universal adjective, a word to describe all things. Now I was also deep into reading Freud, as I mentioned above, learning about the Oedipus complex, the urge to kill the father and fuck the mother (I ordinarily use another word, a euphemism, to indicate sexual intercourse. But I have to use "fuck" here to make the point!). So here I discovered an amazing convergence. Freud is talking about mother fucking and this is what I now hear over and over again as I walk around the yard of the prison. I began to ponder the relationship…

But yet another point: In my various clinical contacts with black men I had given up trying to identify anything like what I understood Freud to be talking about. Indeed, I made some notes to myself on this black population being proof against Freud's suggestion that the Oedipus complex was universal. The Oedipus complex assumed some identifiable father, or father figure, in the experience of the child, but in many cases before me now there was no such figure in their lower class background. Many of these black men do not have an Oedipus complex for the simple reason that they do not have identifiable fathers. When queried, the best these men could do is tell me about some man who would come to visit more regularly than others – but there were no father-son relationships. So what is one to make of the ubiquity of "mother-fucker" in their speech? The answer is that Freud's notion of Oedipus complex has two components, the desire to fuck the mother and the desire to kill the father. The latter component is missing in these men, and the result is that the first component of under considerably less repression. In this way Freud is vindicated all the more, for his understanding of the repression that arises out of the desire to kill the father.

Freedom and Responsibility

There was cultural determination and Freudian unconscious determination – and, third, determination of physical, chemical, biological laws. All this determination stood against the message heard over and over especially in the criminal justice system that human beings chose to follow or break the law and, also against my own conviction that human being is somehow linked to the way he or she makes choices, and that these choices are noble and others ignoble. All these considerations are true, even as there is seemingly no coherence among them.

My prison experience gave me no solution to the problem. It did however make me very conscious of it. As a psychologist I was always in the role of the institutional "liberal" who forgave criminals as not being responsible for what they had done. Personally, I could not give up the dignity of human being even as psychology was finding him or her as the end result of a chain of causation. A recent paper I did on causality still deals with this problem; I never really left off thinking about it. My work on Maimonides is very much focused on this question (see Interlude 3).

Alcoholism

Many of the people at the Indiana State Farm were there for the maximum penalty of public intoxication. The State farm was the solution to the problem, mostly the problem of Indianapolis, of getting the drunks off the street. The name of Judge John Niblock – "Black Jack" as he was known in the newspapers – handed out maximum sentences, almost a year, for public intoxication. I had a world of alcoholics to study at the State Farm, a paradigmatic disease. A person crippled in that very mechanism which was also the source of human dignity: the will.

The alcoholic helped me to understand the "problem" – that which is the essence of the difficulty that has to be responded to – of Freud's whole work. And as I now know, this "problem" was Maimonides' as well. In short, it is as follows. Human beings normally have control over what they think, feel, will, and do. There is a range of self-sovereignty. But sometime this self-sovereignty goes, and in comes Freud: the neuroses, hysterias, compulsions, and phobias result in a loss of normal control. Maimonides in his *Shmoneh Perakim* finds that the consequences of transgression are precisely hysterical blindness and paralysis (much as did Freud when he studied the hysterias in Paris), examples of both are found in the narratives of Scripture, and are cited by Maimonides as cases in point. Human sovereignty is the problem, and when sovereignty goes other systems take over as default sovereigns, and what results is psychological pain (see my *Disease, pain, and sacrifice: toward a psychology of suffering*, 1971).

One day at the State Farm I got a call from John Klinger, in Indianapolis, who was head of the whole correctional system in Indiana, and a man for whom I had a good deal of respect. He told me that the psychology department at the penitentiary in Michigan City was in crisis with respect to parole assessments, and he asked if

I would consider going there for a few weeks and help out. I agreed. This was the state's maximum security prison. Prisoners with indeterminate sentences had to be regularly reviewed by the parole board, and the parole board needed a psychologist's assessment.

John Watson said of psychology that its job was to predict and control human behavior. The task of parole assessment was predicting how a person would fare if he were released: a challenge to the art of prediction. A few years before there had been a national investigation of prisons across the country, with Indiana held up as an example of the worst. Indiana counteracted vigorously. Among other things it established psychology departments in all of its prisons (it was for that reason I had a job at all). But the flip-side of that was that psychology departments were given huge responsibility, and authority, and were held in high regard by officials. Thus, the psychologists' recommendations to the parole board were taken very seriously by the board in its decision process.

So there I was, young and green as could be, in a job way to big for me. I was all they could get because of the war, and I was there because of the good/bad fortune of having had polio.

How do you, in perhaps 2 or 3 hours at the very most, come to a recommendation as to whether a criminal in a maximum security institution should or should not be paroled? I did it but may God forgive me for my errors, positive and negative. Another psychologist told me how to do it (another devotee of Sutherland's). Look for criminal identification, he told me. Look for the indications that he is or is not likely to re-enter the same criminal subculture from which he came in the first place. Check for "circumstantiality," he told me. If the criminal act was largely due to circumstances, ask if the circumstances into which the person would re-enter are the same or different from the ones he was in when the crime was committed. If, however, the causes are completely mysterious the recommendation is always "no." This was the case of a young man who for no apparent reason that anyone had ever discerned, went into a restaurant and killed a group of people. Armed with advice, I was making recommendations about people which would be rubber-stamped by the parole board because, I, to the board, was the psychologist!

I read numerous books on parole prediction; correctional studies, most of them. All of them struck me as not only being benighted but also being unjust. You do not make individual decisions on the basis of correlations, especially if they are low, as they characteristically are in the human domain.

There was one night I did not sleep at all; reading and thinking all night. In the morning I went down to prison, and after sitting at my desk for 10 min chatting with the secretary who was the prisoner I mentioned above and who would never be paroled, I got up, left the prison, went directly to the train station, took a train to Indianapolis, took a cab to John Klinger's office, and with eyes ablaze I broke in on him. I told him I could not do the job, and that was all that there was to it; I was resigning.

We conversed for 3 hours; there was little he missed. A wise and experienced man, he covered all my expenses and got me an appointment with Sutherland in Bloomington. I spend 2 wonderful days with Sutherland discussing the problem of parole and parole prediction. Interestingly, from both Sutherland and Klinger I was told that parole recommendations required that I be as informed as possible in coming to some mysterious process of judgment. I went back to Indianapolis to again visit with Klinger, and then back to Michigan City. Both Klinger and Sutherland thought I was doing a great job. To me what was important was that neither Klinger nor Sutherland had any special knowledge with which to be wise in their recommendations/decisions. Hard decisions have to be made and someone has to make them! There is a ceiling of excellence beyond which no human being can go.

In the summer of 1943, Millie and I went to Yale, to its summer institute on alcohol studies – run by Jellema. I could never figure out where all the money was coming from for the institute but we were well treated financially also. Officially only I was covered but Millie sat in on all the sessions and somehow all our expenses paid for. It was exciting. There were two groups of people at the institute: those who came as professionals and those who came with some political-moral-religious intention. What came out of it was my study on the relationship between birth rank and alcoholism, much helped in my discussions with Jellema. Indeed, he gave me one of the best pieces of advice I have ever gotten – something that became the kernel of many of my lectures on method. Jellema said that in your mind you should always do the study backward. Think about what the conclusions might be and what evidence you need in support of these conclusions. Think about what kind of values your data must have and how you would collect your data, and only then press forward in conducting the study.

Years later I had a conversation with Isaac Asimov, at a time when he had just completed his 100th book. We talked about writing fiction and he said essentially the same thing Jellema had said: “When you write a novel start with the last chapter. After that you can write the rest of the novel. You must know how the story ends before you begin. Otherwise it will never end – only with death.”

After the summer at Yale, I returned to the State Farm. During the summer I had been reading Alfred Adler and was particularly taken with the psychological significance of birth rank. Going back to the couch for a moment, I had become very aware of the significance of birth rank in my own life. I was a third child. Although I had two younger siblings, they were much younger than I so that, psychologically, I was the youngest (there were 8 years between me and my next younger sibling). The third child is a kind of outsider, a fifth wheel for a four wheel institution: my father, my mother, older sister, and then older brother. That to me was the system when I came into it. I have attributed much of my sense of alienation to birth rank.

Without data, or at least without statistical data, I had some sense of the alcoholics I was dealing with as being alienated in the same sense as I felt alienated. I do not claim much understanding of alcoholism except some kind of intuition that always made me inquire about their position in the family. I was not until some time later, after I had collected some data, and after I had learned enough statistics to analyze the data properly, that I published a study in the *Quarterly Journal of Alcohol* which came out of Yale (see my *The relationship between alcoholism and birth rank*, 1949). The study clearly indicates that there is a relationship between alcoholism and birth rank; alcoholism being more likely for higher (younger) birth ranks.

After completing a year in the Indiana prison system, I returned to Indiana University where in submitted a thesis on a little Lewinian type study I had conducted at the State Farm. I asked two groups of subjects to copy geometrical figures without lifting their pencils off the paper and without retracing the lines and counted the number of vacillations and plotted these against intelligence test scores. Behold there was a dramatic relationship between number of vacillations and intelligence test scores at the low end of intelligence but at higher end of intelligence test scores the relationship disappeared with few vacillations. Since the literature showed that mixed findings, I attempted to show that the variation among studies could be explained by the range of intelligence of the subjects. The study was an interesting demonstration on the relationship between correlation and the ranges of the variables, and I often use this study as an example in teaching statistics. I no longer have a copy of the study but it has to be in the Indiana University library somewhere. Perhaps my computer can help me access it now!

With my master's degree in hand I took the bus to Iowa City, famous to me because of Kurt Lewin. I went to see Lewin, and also Kenneth Spence who was head of the psychology department. I could not get to see Lewin but Spence was encouraging. However, he told that if I were to come, it would have to be to the psychology department and not "upstairs" where Lewin and the Child Development people were. Furthermore, I was told that Lewin was not there and that there was a chance he was not coming back!

Our stay in Iowa was exciting but rocky. We were there for a year and a half. We learned statistics from Lindquist whom I greatly disliked for his arrogance and the occasional anti-Semitic remarks he would make. I remember telling myself the "department store" story each time I would go to class. When you go into a department store it is not important that you like the store or the people working there. What is important is what you take away with you. And what I took away from Lindquist's course was a treasure. He was on the leading edge in the application of analysis of variance and covariance in psychological research, notwithstanding the theoretical weakness I noted above.

I took a course with Wendell Johnson, and I learned about general semantics from Korzybski and Bateson. I learned about the phenomenon of stuttering. Johnson was a stutterer and a very good student of stuttering. He had written *Because I stutter*, which, I believe, was also his PhD dissertation. But Johnson stressed above all the importance of humanity. This was refreshing in an atmosphere in which the dominant ideology – for it is an ideology – was behaviorism. I remember how once he indicated that a psychologist should be a person who is liked by dogs and children! One great exercise: live the life of a stutterer for several days – the value of the deliberate bounce, stuttering deliberately so that you will not stutter nondeliberately.

We took two courses from Gustav Bergmann, a true European intellectual. He had been on the fringe of the Vienne Circle and was brought to Iowa by Lewin. However, he soon turned against Lewin and became the major promoter of behaviorism, linking himself to Spence so as to become the philosophical voice of the movement. He did give us a fabulous course in the history of psychology, and an introduction to the philosophy of behaviorism largely based on his own papers, sometimes with Spence.

Spence's course covered Clark Hull's *Principle of Psychology* line by line, proposition by proposition. His enthusiasm for Hull took the form of taking him apart with the zestful conviction that the more he could show Hull to be in error the greater Hull became. That is how Spence sought his own greatness, of course, and greatness is clearly what he was aiming at. But Hull was the greatest; even his mistakes were the mark of genius. Spence achieved his greatness by riding the coat-tails of Hull precisely in showing what Hull should have truly said. There was really a kind of sickness about it all, but I am afraid I fell into that sickness to some degree.

Spence was at war. His ideology of science was "purity"; his mission was to prevent the contamination of psychology from impurity; Gestalt psychology being the latest threat. The great demon was Kurt Lewin. But at issue, ultimately, were university jobs. Spence wanted to make it so that he and what he stood for would have enough prestige such that his recommendation would guarantee a university appointment. He would fill those jobs with true believers who came under his tutelage and who would be loyal to him personally.

Spence was a tyrant in the department. He insisted that Claude Buxton, with whom Millie eventually did her master's thesis, could not work with rats. For working with rats entailed certain holiness and required a certain purity of heart that Buxton did not posses. Buxton conceded and limited his studies of learning to what could be studied using nonsense syllables and memory drums. Millie took the analysis of covariance we had learned from Lindquist and applied it to the phenomenon of reminiscence as manifest in the learning of nonsense syllables on the memory drum. Of course, there were members of the faculty who were not enthusiastic about Spence's authoritarianism and this later became the occasion for an uprising.

Bergmann participated in this grand power fantasy with Spence in which they were going to "take over" psychology! Bergmann was a sort of Rasputin or Richelieu to Spence. Bergmann conceived of a "one-up" system derived from the positivist distinction between language and meta-language. Behaviorism entailed the distinction between scientist and subject, and between philosopher, especially philosopher of science, and scientist. I recall the day when Bergmann came into class all aglow with satisfaction and celebration. One of the Gestalt psychologists, of the four Wertheimer, Kohler, Koffka, and Lewin, had died. He was the second of the four to die, and Bergmann joked, "Two down, two to go!"

There is a story here at Iowa that I have never written about; it is unpleasant but it should be recorded. There are two parts to this story. The underlying sordid one involves Bergmann; the other, perhaps one level of innocence higher, involves Spence. But the matter involved them both. Bergmann was a refugee who was deeply wounded in his soul by the Nazis. I have seen many such people but Bergmann's injury was the worst I have seen. He had become profoundly anti-Jewish. He renounced his Judaism. I had been told that if he were sent a Jewish New Year's card he would send it back. His view was that the only way the Jews could prevent repetition of their various historical persecutions was in a relentless assimilation. This meant that no Jew should ever marry a Jew.

Millie and I became of great interest to him. We should not be married. He told her how she had a great career ahead of her as a philosopher, and that being married to me was impedance. Me, he sought to disparage in every way he could.

The fact is that we both were thriving intellectually. Robert Leeper had published a lengthy criticism of Hull and the day it appeared Spence came into class with a copy of it. For the next few weeks Spence put everything aside and concentrated on preparing a response to Leeper. Millie and I holed up for several days, and found every weakness we could in Leeper's paper, and submitted the results to Spence who was overjoyed. Ours, he said, was the best in the class, and joked about having to give each of us only 50% because we did the criticism together instead of the 100% the paper deserved. At the time I did not realize that it was the omen that it was.

Millie was finding her identity as a philosopher, along with mathematics and psychology. I, with my background in mathematics, logic, and philosophy of science, was finding the Hullian stuff really good fun. The truth was that I could get into it better than most students. I was finding things neither Spence nor Hull had seen. Indeed, I wrote a paper examining the mathematical properties of the exponential function which Hull had adopted as the basic mathematical form for representing learning. I found potentialities in the exponential function that neither Spence nor Hull even imagined. In particular, I pointed out that an exponential function must have an exponential function both as its derivative and integral. I showed that there were a number of experimentally verifiable consequences that followed from this (see my *The exponential growth function in Herbart and Hull*, 1952). Spence took the paper as a wonderful confirmation of the power of the hypothetico-deductive method.

From it I designed some experiments and Spence, who openly praised my work, told me that he was communicating with Hull about these experiments. He encouraged me big time. There was no doubt that this would be my PhD dissertation and Spence pressed me to take the written exam as the PhD requirement. I wrote the exam.

Weeks went by but I heard nothing. A kind of strange silence began to surround me. Finally, I approached Spence and asked. He told that I had done very well. However, he decided not to enter the results in the record, and that I would be asked to leave. He also told me that he had hoped I would fail the exam and that would have been that. Furthermore, he told me that I was only at the 85th percentile of graduate students, and that was not enough. As a Jew I would have to be at the 95th percentile.

It was true that they had given a number of degrees to Jews but, he said, that was all the more reason, because of the saturation, he did not want to give the impression that he was turning out too many Jewish PhDs. He told me how sorry he was. He told me that he would do everything he could in finding me another place, particularly, he advised, if I went into some form of *applied* psychology.

Then Spence had a conversation with Millie. He told her, in addition to what he had told me, that he had made the decision to withhold the recording of my passing the examination because I had a bad leg, that it created a bad impression, and that he would have difficulty placing a person with this kind of visible handicap.

When this became known, there was the beginning of an uprising among graduate students and faculty. I do not know much about it because most of the activity was kept secret from us. There was a protest meeting (Gregory Kimble was there). Someone told me about it while the meeting was ongoing. I went to the meeting. The room was crowded. I begged them to do nothing on my behalf. We just wanted to go away quietly. We did!

We returned to New York and moved in with Millie's parents in the Bronx. I proceeded to look for a job, and found one in the statistical department of the Cooperative Test Service of the American Council on Education, housed at Columbia University. I qualified because they were impressed by the course I had taken from Lindquist. His name was magic and, as I noted above, he gave me a treasure. The service regarded me as a gift from heaven in dealing with their huge testing and scoring operation.

Out of nowhere I found myself directing an office of some 30 people, administering, scoring, and analyzing test data from all over the United States. Fortunately there was a wonderful lady who – I forget her name – really ran the show. She did not need me but I needed her. I knew my true place in the operation and everything ran most smoothly. The only problem was that I was bored out of my mind.

Meanwhile I had contacted Don Pelz, who had been a student at Iowa. One day he told me that Leon Festinger had just left his job with the National Research Council in Rochester, and they were looking to replace him. Don was offered the position but he was not interested.

My new mentor at Rochester was Seymour Wapner, who eventually found his way to Clark, and was even President there for a while. It was an office of the National Research Council, Committee of Aviation Psychology. The real force of the whole operation was Morris Viteles at Pennsylvania, who was chairman of this Committee. The Committee consisted of some very accomplished people, mostly psychologists, who had an interest in aviation. Sy and I worked together for a year. It was a year devoted almost exclusively to statistics.

Sy Wapner was indeed my mentor. Here I was a young Pythagorean psychologist from Olesseyce, and he was a kind of Henry Higgens from *My fair lady*. Let me explain. In conversation with Sy, the suggestion arose that we, Millie and I, should change our name, Bakanofsky, too patently Jewish. I confess that some of Bergmann's assimilationism, through contagion, and through the dynamic process of being a victim of anti-Semitism, as was Bergmann himself, had become ours. A modification of our name was part of the process of grooming me. Sy put us in touch with a lawyer, and the name change was legally processed. But let me say that years later when I discovered that the *Encyclopedia Judaica* included the name David Bakan in its list of Jewish psychologists.

Every month there was a meeting in Washington, headquarters of the Committee of Aviation psychology of the National Research Council. Morris Viteles was the chair. There were always a number of notables present at those meetings. It was a level of knowledge experience, wisdom, and *power* with which I had no experience or, for that matter, any inkling such could exist. The prison system gave me some experience of power, but the Council was something different and greater. Moreover, every year there was a grand annual meeting to which all kinds of people, generals, government officials, engineers, psychologists, and heads of corporations were invited. Viteles was absolutely wonderful as in a room of over a hundred people he went around introducing every person to the group by name and affiliation without missing or hesitating over a single one. How all of us presented ourselves was very important. It sometimes passed my mind that Wapner was under instruction from Viteles to groom me; but I do not know whether this is true. Strange world! A few years ago I found myself sitting next to Viteles on an airplane on the way to an American Psychological Association meeting, and we had a wonderful time reminiscing about the days of the Committee of Aviation Psychology.

But let me explain my metaphor ("Pythagorean psychologist from Olesseyce", above). My father came from a small shtetl in Poland called Olessyce. My mother came from a somewhat larger place called Zelechov and from a somewhat higher social class within the Jewish community than my father. Or at least so I was led to believe. While my grandfather on my father's side was a scribe, my grandfather on my mother's side was a master tailor who had the good fortune of being a manufacturer of army uniforms. My mother always had great aspirations with respect to vertical social mobility, and she was deeply disappointed that my father remained a laborer, and never became a business man. My father scorned those aspirations. My mother in her bad moments would scream "Olessyce" at my father a term which, as we understood, referred to his vulgarity and lack of "refinement" and which she often used. My father took his vulgarity as realism and integrity, and suggested that vertical social mobility was vanity and folly. My mother took it as a major deficiency. I am afraid that in these respects my father's influence on me was considerably greater than my mother's. My mother continued to complain virtually to the day she died, that in spite of my education I remained so "unrefined". Her ideal was an actor by the name of Edward Arnold, a somewhat portly middle-aged actor, who regularly played the role of a kindly, charming, well-off gentleman in the movies. I suppose I succeeded in fulfilling her ideal by becoming portly.

I write this because I always had the sense that Sy Wapner was more like my mother, and that it was his aim to do me over with respect to the proper manners. He literally coached me with respect to every detail of dress and conduct, and he monitored me closely when we went to meetings.

But as far as the Pythagorean in me was concerned, Sy and I became a perfect team. The Rochester office was suffering from a common disease that prevailed among psychologists: they were sitting on a backlog of unfinished studies, unfinished because no one knew how to organize and analyze the data.

I had gotten quite proficient in the "design of experiments," the phrase Fisher used in the title of one of his books. What this meant was finding ways to organize messy data, finding appropriate statistical tests, making appropriate tables so that one could interpret the data.

In that year, Sy and I brought a number of those studies to completion, to the great satisfaction of Viteles and the Committee on Aviation Psychology. Viteles decided to give me a special title: Chief Statistician, which he regularly used to call on me at meetings.

Pythagorean because I always experienced a thrill in doing these studies, a religious thrill for somehow pulling a curtain away and finding meaning in a body of data that was not manifest before the analysis (see Interlude 2). I always had this strong sense that what we found *there*, in those pages and punch cards of data, was just waiting to be apprehended. It is like the *thereness* of two things It is like there *thereness* of the facticity of the Pythagorean theorem, and it is also like the *thereness* in the hieroglyphics that were carved in stone and existed for millennia with no one being able to read them. The message was there I the hieroglyphics in, say the year 1000 but in that year no being on the planet could either write or read hieroglyphics. Then at the time of Napoleon human beings found a way of reading hieroglyphics, a way of going from the manifest to the un-manifest. For convenience, and as a manner of speaking: God wrote the Pythagorean theorem; humans wrote hieroglyphics.

I am reminded that Newton once said on this topic after he had discovered the laws of refraction of light. He went to the butcher shop and got the eye of a bull examined it closely and came to the conclusion that whoever it was who designed the eye of the bull could not have done so except someone who already was acquainted with the laws of the refraction of light… of course, this had to be God.

My Pythagoreanism was strongly reinforced by my various contacts with military people for they are also deeply entrenched in a metaphysical position in which that which is not materially existent is the essence of reality. For the military mind is ever concerned with battles and wars that have been not yet fought, which have no existence in the material world.

It is interesting that concern for the future, as a major factor in psychological functioning, has no systematic place in any psychological system, again with perhaps the exception of psychoanalysis. As I pointed out, above, psychoanalysis takes kinship as essential to its psychology, and the future is implicit in that.

Military people have a proneness to converting adjectives to nouns: capable to capability and vulnerable to vulnerability. They are ever involved in the preparation for activity that they hope will never come about. But battle or war is the focal point of their concern and while they hope these would never become actual, they are certainly in the realm of the objective and the real.

Danger was the major focus of much of our work. I recall one meeting in which someone associated with the investigation of airplane accidents said, and which everyone agreed was a basic working assumption: *all aviation accidents were due to human error*. I did not at the time think about how this articulated Maimonides' view that all evil is due to the failure of the fullest functioning of human being. Maimonides' essential metaphor was that all human evil is like the stumbling of a blind man.

The military were turning to psychologists as to how to minimize human error so that failings could be minimized. This was also the major concern with promoting public interest in aviation. How to minimize danger on the assumption that danger was a function of human failing. Some of the deepest satisfaction I have enjoyed in my life was when it would come to my attention that some part of our work was being incorporated into the practices, regulations, training manuals, and curricula.

The combination of *intellect and governance*: the bringing to bear of the best of the human intellect for the management of human problems. I had gotten some sense of that when I was in Indiana. It was certainly in Plato's *Republic* to which Jellema exposed me. It was very strong in the connection with the delinquency studies under Park at the University of Chicago, and the version of that approach to which I was exposed by Sutherland. It was what informed the changes that were taking place in the Indiana prison system, and certainly in the lessons I was getting from Harry Hawkins and John Klinger.

Social class vs. intellect, and governance. Historians have talked about this period of history as the growth of meritocracy. I called myself the Pythagorean from Olessyce; social class as low as one could get. On the European Jewish side, distinctly on the lower class side, and hence nowhere near the "*scheine yiddin*," the upper class within the Jewish community. In the American context, the Jewish community broke into three social classes: the well off business men and professionals, and the lower class, the workers, such as my father, who worked his entire life as an operator in the ladies dress shops in Manhattan. I was a low class both within and without the Jewish community.

I was victimized by my belonging to the lower class in Iowa. For even the Jew who had been successful in Iowa were Jews who had come from backgrounds which were, to use my mother's term, more "refined" than I was. At the time I had not yet gained a reputation for statistical wizardry which was later to become my "merit" in the sense of meritocracy. That came at the time I was in Rochester, when Viteles granted me the title of Chief Statistician for the Committee of Aviation Psychology. While there was plenty of discrimination throughout American history, I suspect that my case kind of represented an end-point at the time, certainly in psychology departments.

But this was the Rooseveltian period of brain trust, involving the application of Keynes' high order economic theory to the economy. The increased tendency to select people in government on the basis of their capability, rather than class and politics, and the increased tendency of government to govern on the basis of information rather than interest and ideology. I may be overstating it, but something like this was going on.

According to Maimonides, the human being should study God's word and nature, apprehend the essential unity that exists between them, and guide his conduct in harmony with that unity. As I indicated, all evil is essentially like the stumbling of the blind.

At the same time there were many factors that interfered with the proper apprehension and the ability to guide conduct properly. During World War II, America had made some considerable progress toward the intellectual guidance of public policy. But after the war a certain regression began to take place.

But I am running ahead of myself.

When I mentioned Pythagoreanism and the military, it is really Pythagoreanism and *power* and, in this context, I want to note several things.

Years later I worked with McClelland who was interested in achievement, worldly achievement, and became renowned for his achievement motive. It was at the time that I was at Harvard (1956–1958, on leave from Missouri where I was between 1949 and 1961) and at work on my *Sigmund Freud and the Jewish mystical tradition* (1958). McClelland was coming up with a strange finding. This was that cross-culturally there was a positive relationship between the extent to which a culture was characterized as mystical and achievement. Mystically inclined cultures were higher in achievement, and in achievement motivation, than less mystically inclined cultures. This finding went against the common notion that mysticism removed persons from reality, provided them with pseudosatisfactions of an imagined world, and distorted their view of reality so that they could not achieve even if they wanted to. It was in this context of working with McClelland that I became conscious of something I had been less than consciously aware of before, namely the enormous power that the human being gains through the process of abstraction.

Of course, it is not this *process of abstraction alone* that provides the *power*. It is rather because there is a certain special significance to those things which are identified through abstraction. It is that the things so abstracted drive events, or at least it is a useful way of looking at those things. This is also the fundamental feature of science. In the same way, laws determine the nature of human conduct so to the laws of nature determine the conduct of matter. This is the way Spinoza put it, who got it from Maimonides who influenced all of the founding intellectual figures of the modern world, Meister Eckhart, Thomas Aquinas, Spinoza, Leibnitz, and Newton (it would require some defense to claim these as the founding figures, but just take it as my opinion).

Indeed, this is my essential view of psychology. *The essence of the psychological is the process of abstraction*. The great defect in human beings is precisely when they become stimulus-bound to a world impinging on them instead of reacting with their abstracted guides, including their values, to conduct their understanding which is the product of an abstractive process.

There is a notion that is articulated in Plato's *Timaeus*, one which is the most fundamental in the Pythagorean position, of how material things may be generated out of mathematical expressions; this is the notion that the driving force of things are the intelligibles. Intelligibles are those things which are represented in human thought as ideas. Or as Leibnitz grasped the relation between intelligibles and material things, there is a "pre-established harmony." This is the basic miracle that grounds the work of the psychologist. It is a miracle that Maimonides notes when he puts it in human beings that they are created in the image of God by virtue of the possession of intellect.

So here I was in Rochester, in possession of masses of unanalyzed data. What was our task? It was to try to identify the intelligibles, which were determinative of the events recorded, and the determination of the records by the intelligibles in the events. For all we had were records. With the help of IBM machines – punch cards, sorter, and collator – we identified those intelligibles. And from that we and others could derive recommendations which promoted safety and efficiency in flying airplanes.

In my time at Rochester I became very aware that I had been exposed to a major lie about the nature of science, that science was value free (i.e., objective).

In fact, just the other way, it is precisely values which provide the energy for science. There is a deep truth in the pragmatic view of science, but it is far from the common understanding.

Superficially, I had the experience of being involved in applied psychology, as contrasted with experimental or theoretical psychology. I had been led to believe that the characteristic sequence was that science is first made in a value-free atmosphere, and then applied; and that, furthermore, the value-freedom was essential because values contaminate truth. This, I realized, was wrong (see Interlude 1).

Jellema had told me that one is supposed to do research backward in the sense of starting with the vision of the finished report and going backward. What I realized is that the vision of the finished report is itself a product of a backward process. All of these studies started in historical contexts and in values. Many examples came to mind. Binet was trying to solve a problem in connection with the education of children in Paris schools. Freud was trying to solve and problem of the causes and treatment of neuroses.

Carnot did the basic work on thermodynamic theory after the development of the steam engine. Indeed, "Student" had developed his *t* test in an industrial context, and Fisher was working at an agricultural research station.

Maimonides made the fundamental distinction between the ability of human being to apprehend the difference between true and false, on the one hand, and the ability of human being to apprehend the difference between right and wrong, as applied to human conduct, on the other hand. Further, the apprehension of true and false was about logical things on the one hand, and empirical things on the other. Still further, the apprehension of right and wrong is about knowing what to do in a skill or craft, know-how on the one hand, and in an ethical value sense on the other hand. For Maimonides, all of this is designed toward the promotion of human welfare, the welfare of the body and the welfare of the soul. As compared with lower animals which are equipped by birth with capabilities to promote their welfare, human being achieves this welfare through mutual interdependence and through the development of the intellect with respect to both true and false and right and wrong.

For sure, there is built into the human condition a great distinction between childhood and adulthood. The child is removed from the hard responsibility of promoting welfare; the child is not and should not be a pragmatist. But that very relief from responsibility is itself a part of God's overall pragmatic design. I say all this just to highlight the essentially incorrect, and even infantile, view that the work of science should be value-free. It was in working with Sy Wapner in Rochester that I came to this clarity and perhaps it is something with which I should credit him.

In any case, in my year at Rochester, working with the Aviation Committee, we ground out study after study. Toward the end of the summer a few things converged that changed my life. Sy decided to take an academic job that was offered to him, and the administration at the University of Rochester had decided that it had made enough of a contribution to the federal government.

Also a crisis had developed in a very large project that the National Research Council was supporting at Ohio State University. Viteles had asked me to stay a few days in Philadelphia with him before going back to Rochester after a meeting of the National Research Council. He had worked out a plan. To persuade Floyd Dockeray who was the Director of the project at Ohio State to expand the project by taking in the Rochester office and placing it under his direction as well. Ohio State owned and operated an airport and it also had one of the finest optometry schools around.

Civil aviation had a major problem in connection with the visual requirements for licensing pilots. Visual requirements were stringent but many argued that these were not necessary for flying an airplane. Remember Wiley Post, the great pilot with a patch over one eye! It was argued that flying a plane was considerably less visually demanding than driving an automobile. It was decided that one great study would settle the question once and for all. There was a massive recruitment offering people a complete course in flight training if they fulfilled certain requirements. There were four experimental groups based on visual characteristics: normal vision without glasses, defective vision with or without glasses, and those who were actually or functionally monocular. They all received the same battery of pretests, their flying performance was regularly tested through instructor ratings and mechanical recordings, and they were given flying tests by those who did not instruct them.

The problem was that Dockeray had fallen ill with a heart condition, barely able to discharge his duties but in denial. Gorham Lane working under him basically managed the project. But the project suffered from the usual problem, huge amounts of data, and few people capable of analyzing these. Dockeray welcomed me warmly, as did Gorham Lane. Viteles (and Ewart) had warned me that I was to be careful not to offend Dockery or Lane. It was a matter of all of us being useful to each other. So I cleared out my office in Rochester and moved to Columbus, Ohio.

I had by then become rather sensitive to anti-Semitism, anti-intellectualism, anti-Europeanism, anti-urbanism, anti-lower classism… and the anti-whateverism that pervaded the culture of the time and the place. When we moved it was of course a double move, out of the office but also out of home. I had carefully segregated my books before the move but when I arrived and was putting my books on the office shelf, I was terror struck as I had clearly failed to properly segregate one book. I quickly grabbed it, put it into a drawer, and secretly took it home in the evening. It was a copy of Goethe's *Faust* in German.

I had taken a course in *Faust* with Harry Slochower at Brooklyn College. Slochower had been identified during the McCarthy period as a communist, and lost his job at Brooklyn. But that was not the reason for my fear. I was frightened because I did not want it known that I could read German and that I had any interest in literature. Spence had made it quite clear that one could not be interested in the humanities and be a good scientist. I recall that he spoke disparagingly about a student from South America when he learned that the student had a volume of poetry in his possession in Spanish. Spence said of him, "I knew all along that he was no good. When I found out that he wrote poetry I knew it for sure. Anyone who writes poetry cannot be a good scientist."

I had become wise enough to know that Goethe's *Faust* in German could not sit on my bookshelf in the office. It was the German, especially. Cultural history is more sluggish in the mid-west. When the waves of cultural change come there, the wave is already weakened, and causes less change. There were still traces of anti-German sentiment residual from World War I some of which were renewed in World War II, especially at universities. Before World War I there was considerable traffic between America and Germany. American students went to Germany, and people from German universities were teaching in America universities. I never had any doubt but that Spence's opposition to Lewin was partly due to the fact that Lewin was German. Nor did I ever doubt that Bergmann's reaction to Lewin and the gestalt psychologists was in part motivated by his desire to assert himself as an American. Gordon Allport once said that to understand America psychology, one had to draw a line not in the Atlantic but in the English Channel. I was quite convinced that to understand my condition I had to understand that there was a line in the Hudson River. And I came from people and culture which were both on the other (wrong) side of the English Channel and the Hudson River. I recall how pleased I was when, one summer teaching in summer school at Harvard, two ladies in the class from Germany told me that I was the closest one to a German professor they had yet met here. At Ohio State to get a PhD one had to qualify with either a reading knowledge of two foreign languages or what they called a comprehensive knowledge in one language. I took a comprehensive in German over the expressed disapproval of the Chair who told me two things. First, that one should know another language besides German and, second, no one had ever taken a comprehensive in German. In fact, my German was not that good; indeed, the negative effect of Yiddish made my spoken German quite impossible. But the exam was only a reading exam, and not that difficult for me, not after plowing through *Faust* with Slochower.

Viteles had told me, "Not knowing is ok, not asking is not ok. What is really ok, is knowing enough to ask". With that advice, I studied the place for several months. I then went to a Washington meeting to report on a project with a long memorandum containing descriptive paragraphs, each followed by a series of questions. It was clear that the problem called for the application of analysis of variance – for the comparison of four groups – but an endless set of problems adhered to this application. Choices of error terms in the analyses, handling of varying number of cases with the cause of the variation being the variation in the wash-outs and drop-outs, they themselves being major criteria, finding error terms within group variances varied substantially, affirming the null hypothesis – the essential feature of the intention of the study – with analysis of variance which is designed only to reject, not affirm, the null hypothesis. What variables in the pretesting to consider for controlling any analysis of covariance. I will not go on. I laid it all out for the Committee.

The angel was Phil Rulon from Harvard: brilliant, charming, totally self-confident, and full of wit. Viteles assigned me to him. He took me home, literally. He had flown his own place from Boston and we flew back together, and I spend the next day with him. Outside of Lindguist perhaps Phil Rulon was the only person I had met so far who had a proper sense of both the potentialities and the limitations of analysis of variance and covariance. We did not complete the task but when I got back to Columbus I carefully went over the various considerations we had come to. Some weeks later Phil actually came to Columbus and we spend 2 days together further considering the analysis. I subsequently wrote up a plan, keeping in mind always Jellema's advice, work backward. Dockeray and Lane were pleased, I presented the plan to the committee in Washington, and everyone was impressed. I did the analyses indicated, virtually single-handed, with the help of IBM machines from the Ohio Business School which we had contracted to use. We completed the study!

But I was still looking to get a PhD. My work in Ohio was three-fourth of time and for the remainder I supposed to get a degree. I transferred a lot of what I had done in Indiana and Iowa and in addition took two great courses.

The psychology department at Ohio State was in the College of Education. While most in the department took this administrative oddity as unimportant, I believe that there was a subtle influence derived from that fact, most especially from the Deweyan orientation in the Education department. The passionate behavioristic-positivistic ideology such as existed in the extreme in Iowa, was much less in evidence here. People like Rotter, Kelly, and Shartle were much more pragmatic concerned with the consequences and consequentiality of psychology in the world at large. This was every evident for example in the easy acceptance of the kind of applied research entailing the cooperation of the National Research Council and Ohio State University.

Let me mention the two great courses: one with Carroll Shartle and the other with Arthur Melton. Shartle gave a course in "occupational information." The idea was that psychologists offering vocational guidance solely based on psychological assessment were fundamentally in error, and that vocational guidance needed to be informed not only by a knowledge of the person but also by the vocational opportunities involved. Vocational guidance in the direction of yacht design during the depression was ludicrous. Shartle had been the major force behind the making of the *Dictionary of occupational titles* and he had a deep conviction in the use of human intelligence at the highest levels of public policy.

The other course was Melton's on the psychologist in crisis. It was not the psychologist's personal crisis, although that too, but a course on learning. Melton who had been a student of McGeoch, a leading figure in connection with the study of learning through the use of nonsense syllables on memory drums, used as his text one by McGeoch going through the book very systematically with the class.

Melton had been at Missouri but he had gone off to do research for the government and, while there, had spent his nights writing a lengthy work for a series on that research. Instead of going back to Missouri – where I was eventually to go myself [1949–1961] – he had come to Ohio. There was a great sadness about Melton, and about his take on psychology. He had, as it were, lost faith in the kind of research he had been doing. He had lost the conviction that the method of nonsense syllables/memory drums which he hoped would unlock the secrets of human learning could do so. Instead, his deepest feeling was that each experiment in psychology was unique, and allowed for little if any generalization. He had had a taste of practical research, with its proximity to the problems, and simply could not find his way back to the memory drum. He once explained to me that it was virtually impossible to write a proper textbook in experimental psychology because all one could do is to recite the details of one experiment after another.

This position was exactly the opposite of the position being advanced by Hull and his followers. Hull's book, *The principles of psychology*, although in truth merely a book about learning, of course reflected the intent that psychology derives from learning. The principles of learning were the principles of psychology.

Melton eventually solved his problem in several ways. He linked himself with the aviation psychology program at Ohio. After Dockeray died – about which more below – he stepped in, with my cooperation, as the head of the office of aviation psychology. He left Ohio and established a major psychological research unit in the Air Forces, and assumed editorship of the *Journal of Experimental Psychology*.

There were three phases to my stay at Ohio State University. In the first phase, we completed the study on the relationship between flying and vision. The upshot was that there could be considerable latitude with respect to vision for licensing pilots. The study and its conclusion made me rather sensitive to the whole question of "accepting the null hypothesis." For while, strictly speaking, one cannot accept the null hypothesis under the analysis of variance "rules" as it were, if the number of cases is sufficiently large, and some indication of how large a difference might make practical difference could be arrive at, then one could say something on the basis of an analysis in which significant variation was not found. Similarly, I became very sensitive to the direction of difference in this and some other studies. For although we blithely talk of committing statistical errors without regard to direction, being wrong about something in one direction could be very consequential, while being wrong about something in the other direction could be quite inconsequential. At the end of the first year, Gorham Lane left and I inherited the project. That is, it was left to Dockeray, and I was working under him. But by this time Dockeray was not too well even as I was meticulous in my routine with him in going over every aspect of the work and, at his home.

The big study we were involved in was a study of the perception of a stall. It was a study I designed and for which I got National Research Council funding. It involved a considerable amount of close contact with and supervision of the personnel at the airport, Ohio State University's airport, Don Scott Field. Gorham was skilled in these matters but after he left it fell to me; fortunately, the airport personnel were very helpful and the data were collected without a hitch. Not quite. One day a call from the administration inquiring about insurance coverage on the subjects in the experiment – which involved approaching stalls at high altitude – ceased all operations. For months we all sat on our hands, collecting money. Since I have often given a lecture I like to call "psychologist as superman" citing the need for the psychologist to know everything. For my simple ignorance, and lack of knowledge even to ask questions, was at fault.

While the inadvertent stall was one of the major causes of fatal aviation accidents, little was known about how pilots detected an oncoming stall. (A stall is a condition in which the airplane loses its lift when the angle of attack, the angle made by the altitude of the wing and the direction in which the airplane is moving, reaches a certain critical point.) What we did was vary the sensory inputs by blindfolds and masking noises, and got judgments of proximity to the stall under different maneuvers. The study was complex and could only be analyzed by analysis of variance. The upshot, of which we were very proud, was that we were able to demonstrate the uniqueness of the cues for detecting stalls for virtually every maneuver.

It was in the late spring or early summer of 1948, the end of our second year at Ohio State. It was hot and I tried to start the day early. In the lobby of the Administration building where I had gone for some business, I ran into the Vice President, a man by the name of Davis. He was fond of me largely because of my role in connection with bringing a very considerable amount of money into the university. He inquired about how along I was with my degree. He said to me, "Why don't you get yourself that degree by the end of the summer, and I'll make you an Assistant Professor."

I went for to see Dockeray for my regular conference with him and told him about Davis' offer. Dockeray then said "You know, David, that 'stall' study you did, it's a lot better than most PhD dissertations. I don't see why we could not accept that as your dissertation." The matter was delicate for another reason. Dockeray in his pride never doubted for a moment but that I was his student for the PhD. I would never have challenged him but in my own mind, my true teacher at Ohio, the one I really respected intellectually, was Melton. I truly could not imagine taking a PhD in psychology at Ohio without his approval. It was a strange thing. He was my teacher, and I was his. The course in learning I took with Melton, albeit with McGeoch's textbook, and Melton's own crisis with respect to it, still engaged me.

I literally studied that material perhaps more carefully than I did other things. Among other things it raised the studies of learning from the animal level, which it was largely when I had studied with Spence, to the human level. I mentioned previously that Millie had been studying nonsense syllables with Buxton at Iowa, and was at least a witness to the rats-mazes/humans-memory drum debate. And I was desperate to find an alternative to the Hullian approach – the inverse probability approach I developed as a higher alternative, to which I will get shortly.

So when Dockeray said this to me, I said, "What do you think Art would do?" He replied, "Why don't we find out, let's call him."

Call him, meant calling him in California, for that is where he was at the moment. It was still early in the morning; California is 3 hours earlier. Neither one of us thought about that. Melton never went to bed earlier than 3 or 4 o'clock in the morning, for he spends his nights dictating and listening to country music (which I knew because when his secretary asked me for help in deciphering something on his tape, I would always hear country music in the background). Melton also had great respect for age. Whenever someone would come into the room older than him, he would rise in a gesture of respect. We woke him and, from his voice, it was out of a deep sleep. I spoke with him briefly and then Dockeray spoke with him for a few minutes. I did not hear Melton's reply but Dockeray said, "Well, its ok with Art."

The study had already been issued in the National Research Council's series. But Ohio State had strict rules about the form in which a dissertation was to be submitted.

I took a copy to the study to the typist who had a lot of experience in typing Ohio State dissertations, and she completed it according to the rules.

I said that Melton was my teacher and that I was his, literally. Some faculty members, Arthur Melton and Harold Edgerton taking the lead, asked me to give them a course in analysis of variance and covariance. The course was organized on a regular basis and a number of faculty and graduate students attended. Although it embarrassed me, they paid me the same respect that they would to anyone teaching a course. I have always felt odd about this, but there was the fact.

Some years later I thought about this in terms of a finding that Charles Curran (at Loyola) had made in connection with the learning of a foreign language when one is an adult. Curran, who had been a student of Rogers, held T-group type sessions with a group of students from a course in French. Curran came to the conclusion that there was a major obstacle. This was the feeling of infantilization generated in an atmosphere in which a language is spoken which one does not understand. Curran believed such a situation rearoused the feelings and emotions of infancy and childhood. He then helped students identify the gross feelings and emotions of inadequacy and fear in the course with their infantile feelings and emotions. The result of this was a dramatic difference in the learning curve with respect to the French of this group and a control group.

The fact is that Melton and Edgerton and the rest deliberately put themselves through this. I certainly felt this, and I feared that some day I would have to suffer some retaliation for it. To the best of my knowledge it never came. But it was a strange thing for me as a graduate student, standing and lecturing and even devising pedagogical "tricks," as one has to do, especially when teaching mathematics, to professors with respect to whom I was the student.

In teaching statistics one faces two major problems, what I have come to call the "down" and the "up" problem. The down problem is that students may not be sufficiently prepared in terms of basic mathematics. The up problem is that the student is impatient to learn the technique before he or she understands the basis of the process. The first was hardly a problem with my group, the second was. Let me say this crudely. The world has slaves and nobles. The slave act under instruction from others without understanding, the noble acts in accord with understanding, and indeed, issues instructions derived from his or her understanding. In learning statistics, one can learn like a slave or a noble. Slave-learning is more directly rewarding. If there is a need to keep the student interested by reward, then the rule is to teach to a level of slavish competence as quickly as possible. I have done that a lot. However, if one begins at a theoretical level, the impatience factor enters, and inevitably the question arises "What is the meaning of this to me?" Like the guy in the Passover Hagaddah! He is in no position to comprehend the answer.

If you teach technique first, the student feels that he can quit, go out, and do it, and so loses interest in learning anything more. There is something here about the character of education, especially American education…but, I will leave that topic here.

So how does a graduate student teach statistics to his professors? The answer is that it is not a problem if the professors understand this, and I remember talking about it openly at lunch with Melton and Edgerton at the Faculty Club and getting their view that they were quite ready to bear their discomforts for the sake of learning what I had to offer. And for that they had respect for me. That lunch was one of the great moments of my life.

If I were to complete the degree by the end of summer, the final oral exam had to be soon. Little did I realize that Dockeray was at the time fighting his own "deadline."

Our first child, Joe, was born on August 2, 1948. My oral exam took place on the afternoon of the 8th day afterward, the day of Joe's circumcision, August 9. It was one of the most memorable days of my life. The Catholic Hospital in Columbus was reputed to be one of the finest in central Ohio. That is where Joe was born and the circumcision took place. I recall one of the nuns beaming, mentioning that she was glad to be present because, according to the Gospel, Jesus had been circumcised. When I took Millie and Joe home, my mother and Millie's mother and father were there for the occasion. The exam was scheduled at 4 o'clock at Dockeray's home with a committee we managed to get together in Columbus, Ohio in August. The day was impossibly hot. By afternoon everyone was tired, and the baby was fussing, focusing on Millie and the baby trying to be helpful, Millie lost her temper and demanded that everyone leave her alone and that she would take care of herself and the baby. In the midst of this emotional storm, I walked out of the house and drove to John Bennet's, an anthropologist, good friend, and outside examiner on the committee. We drove to Dockeray's, jumping a stop sign and almost having an accident, which frightened John and me…

The exam did not last long. The topic of airplanes, stalls, and analysis of variance and covariance was totally distant from the sphere of knowledge of everyone in the room, except Dockeray and me. I remember some horsing around with Horace English about his car which sometimes stalled. John asked me some questions about making inferences from pot sherds. Dockeray just asked me to explain a bit about the nature of the study. After 35 minutes Dockeray said, "I don't suppose that anyone has any more questions to ask of you. David why don't you go into the kitchen, I think that Katie might have something for you." Katie had a cool alcoholic drink waiting for me but I had barely taken a sip or two and Dockeray called me out of the kitchen by saying, "Congratulations Dr. Bakan."

What I did not know is how sick Dockeray was, and how much his own awareness of his impending death was involved in all of this. It was only a few weeks later Katie called to tell me that Dockeray had died and asked if I would be a pallbearer. The circumstances of the exam and Dockeray's death have always left me uneasy. This ended my second year at Ohio.

In our third year, Dockeray and Lane were both gone. All the ongoing projects had been brought to a close. There was a hiatus. The promotion from instructor to Assistant Professor came to me and I took to teaching an undergraduate course in statistics. We drastically reduced the staff of the aviation psychology unit. Melton was nominally in charge; I was actually in charge but there was little to do.

The National Research Council did provide a planning grant in connection with landing lights. The problem was that of finding ways to illuminate runways in such a way as to provide the pilot with the best information for landing. I surveyed everything that was going on with respect to research in landing lights. I went to meetings. I spend time at air force bases talking to pilots. I flew with pilots making observations on their landings. I spend days in the tower in Washington Airport studying the ground control procedures. I checked out some of the pilot training with respect to landing.

After about 6 months – a round Xmas – I woke up one morning with a focus on something which was the ground for all that was going on. My metaphor is figure-ground in the Gestalt sense. The figure is the problem of making landing less dangerous, the ground was the extraordinary discomfort that prevailed among everyone associated with flying about the landing problem. One pilot has said to me, "You know, when you land and airplane, you're always guessing."

One of the most interesting developments in this area had occurred in England. It was called the slope-line system. The lights were arranged like the major beams of a V-roof along the sides of the runway. This caused easily differentiable patterns to be seen from the airplane depending on where the pilot was located. And yet this too was not fully satisfactory. I was an admirer of the system, and had recognized that whoever it was who designed it must have had a good understanding of perspective. I had gone to the library to consult some of the books on perspective but had made little progress.

I went to a meeting at Arcata, CA, where virtually everybody who had any interest in the problem of landing had assembled. One evening I found myself at dinner sitting next to someone whose name I cannot recall but who had designed the slope-line system. He was a burly Irishman. After some reasonable amount of drinking and eating, I asked him "How did you learn the mathematics of perspective?" He answered with a laugh, "The *Encyclopedia Britannica*."

That was the key for me; I went home and studied perspective. I recalled the discussions from Bergmann's class about Titchener, and the "stimulus error" so-called: view-point apprehension vs. mind-point apprehension. We see the top of a cup (except when looking down at it directly) as an oval in our view-point of apprehension. The stimulus-error for Titchener was the confounding of the mind-point apprehension for the view-point apprehension. I then went on to develop the information in the view-point of a pilot looking down at a runway through the use of the mathematics of perspective as I learned it from the *Encyclopedia Britannica*. From this something very simple emerged. The problem could be expressed in terms of the solution of a set of simultaneous equations. The information in the view-point could be expressed in terms of two equations involving three unknowns. It is impossible to solve for three unknowns with two equations. In order to solve for three unknowns one must have at least three equations.

Thus the impossibility of exact instantaneous self-location on the basis of linear visual information simply did not exist. That is, no matter how capable the pilot might be, there was simply not enough information for him to make the appropriate judgment. Additional information is essential to self-location. I was, of course, back into my Pythagoreanism. This work, which was judged at the time to be very important, was completely ideational work. And the idea began to grow in me that I did not want to do research ever again on a proposed plan of work as grant applications then came to require.

Allow me to insert three stories from my life at this point. I share them to indicate something of the sense of dangerousness concerning flying that I had at the time.

The first is a self-location story of another kind. I did some flying, but never enough to qualify for a license. We were up one day, I and my instructor, doing this and that. Time went on and we were not paying attention to where we were. When we decided to go back to the airport, we realized that we had flown off our maps and had absolutely no idea where we were. The land around Columbus is all very flat, and everything looked like everything else. We looked down and saw a water tower with words on it. So we flew down, and my instructor flew some very tight turns around the water tower, while I read the words. The maneuver was of course very dangerous.

The second is about my trip to that meeting in Arcata. I had managed to get a ride on a B-29 from Wright Field. It was the airplane which had made the first completely automatic flight across the Atlantic. The equipment was all experimental, and the Sperry engineer who was chiefly responsible for the development of the system was also on board the plane. When we were coming into the airfield, the pilot decided that he was going to try for a completely automatic landing. I and the Sperry engineer were seated behind the pilot and copilot. They set the automatic controls and set their hands on their laps. Below we could see the giant redwoods of northern California. The wheel moved with slight movements like the keys of a player-piano, and it was going very smoothly. When, very gently, the plane began to descend prematurely, the pilot and copilot gave each other a quick glance and grabbed for the wheel pulling it out of its dive. The pilot indicated that we had missed the trees by about 9 ft. It was not until we landed and got out that I was struck with delayed terror.

My third story was about when I visited Wright Field once to interview some of the test-pilots there. I was having lunch with three of them, and our conversation turned to jets which had just come into existence and Wright Field had received them for testing. "Would you like to see one?" I was asked. I was quite eager and we got into a jeep and drive out to the field. I was shocked at what I saw. I had been flying but always in a plane that had at least two seats, one for someone who knew what he was doing and one for someone who might be learning. But these jets came with only one seat! Which meant you could not have a duo of instructor-student. "How do you ever learn to fly one of these things?" I asked. "Well", one of them answered, "it's this way. No one knows how to fly one of these things, so there is no one to teach anyone. Until we flew them no one ever flew them. There is no book on how to fly them. They send us a book but it is a book on how the plane was made but not how to fly the plane. That's all anyone knows – how to build one of these. So we just sit around and study the book. We go through a lot in our minds. We talk about it. And then… a moment of truth arises. One of us just gets up and says, 'I'm just gonna try it.' And that is it. He goes out and flies it."

So let me come back to the spring of that year. One of my fellow graduate students in psychology was an air force colonel who had taken time off to get a PhD in psychology. He was about to go back to manage a huge budget for psychological research. He had persuaded Melton to come on board the chief civilian in the program. Melton tried to get me to come along with him as second in command. The money that they were offering was something like double and a third more over the going salary for an Assistant Professor at Ohio. I really was at a turning point. I had been offered a job at the University of Missouri [1949] – thanks to Melton's recommendation. Millie and I discussed it.

Let me add something about our family condition at the time. When we arrived in Columbus, Millie had become quite firm in her identity as a philosopher. She had majored in mathematics at Hunter, and attained a Master's degree psychology at Iowa. But perhaps because of the vision of philosophy that came from Bergmann – not withstanding his warped character – she came to see herself as a philosopher. At Ohio she registered as a graduate student in philosophy. One of the people who was there was a young man by the name of Virgil Hinshaw who had just taken his degree with Bergmann at Iowa. Hinshaw took over the nominal direction of Millie's work. Nominally, since Millie was doing fine on her own. She managed to thrive as a philosopher while being pregnant and raising children. Indeed for her there was a kind of convergence which I would leave for her to explain. In her eighth month she took her written exams, being terribly uncomfortable in not getting near the table. She gave birth to Joe about the time I completed. It was in this third year that Millie wrote her dissertation while nursing an infant, and I was reflecting on self-location and landing lights, and teaching.

One of our most interesting observations at the time was that the sight of Millie nursing and getting a PhD at the same time was an extraordinarily disturbing sight for a number of women. Millie needed someone to type her dissertation, and she lost several typists who simply became too emotional when they came to the house. In retrospect the feminist revolution was in the making and this meant, at least for many women, a choice between having babies and a career. The vision of a woman nursing a baby and writing a philosophical tract was just too much. Millie's fundamental thesis, captured in the title of her dissertation, was important: "*On the factuality of logical propositions*."

Psychologically I was drawing inward to the household to take advantage of the greater possibility of living a reflective life. More and more I tended to do my "real work" at home while attending more to institutional politics, paper work, and bureaucracy when I was out of the house. Not the least I could think without the constraining Epicureanism that contaminates the intellectual atmosphere of universities, especially in the mid-west at the time.

So I was growing into an Abrahamic romance. Friends were growing less important. Relatives were growing more important. Recall that Abraham was a nomad who moved with his whole household wherever he went. The first words spoken in the Bible were walk, walk, and the meaning was clear: go with your family together. The point was made by Oscar Handlin in his book on immigration, where he points out how America was largely settled by individuals who came one at a time as individuals. I remember the first time I observed how different the American ideal was, expressed variously in American myths, like that of the cowboy, and that of "have gun, will travel." The American myth is one of travel just like the Abrahamic myth, but the American hero travels as a single male, not with his household. When Joey was born, we began to put constraints on our friendships. Indeed, in the first 2 years at Ohio, Millie and I had many friends. Our house, near campus, was a kind of social and intellectual fraternity house for many people. But from the time Millie became pregnant it began to change, and we were much more inward and kinship oriented.

I was also beginning not to want to be in contact with the jock mentality of a number of people that my interests in aviation brought with it. I truly do not know how to explain this. The protests of masculine virility, on the one hand, and the protests of feminine charm, on the other; booze on the one hand, smiles and perfumes, on the other. I basically did not want to be with the many people I was forced to be with. People who put too much effort into being what they would like to be in terms of social class, or people who are like adolescents on prom night. Something like that, I cannot quite find the words to express it. It all converged on the martini; I never wanted to have another.

Because I never wanted to have another martini, we went to Missouri (and stayed from 1949 to 1961), rather than take the lucrative job in the air forces with Melton…something like that…

And I wanted to find my own way! Again analogous to the retreats into the mountains like that reported in the Bible. Away from interests, shams, and the plaque of sabotaging ideas, the kind of ideas which interfere with intellectual progress. Where one could listen to what one wanted, and not be captive to speeches that occur in the social institutions in which one finds oneself. I wanted time to go out to the library and select the speaker to whom I wanted to listen. And I wanted a chance to determine my own "curriculum."

Melton and I were both leaving and we did a fine thing for Ohio State. Paul Fitts was head of psychological research at Wright Field. And we arranged for him to take over the Laboratory of Aviation Psychology and be a professor of Psychology. He did some very fine work in the few years before his untimely death.

Before I leave the Ohio State account, I share with you one significant thing from that time. One day – it was the day after I made my decision and we were preparing to leave – an interesting notice appeared in the mail of the Office of Naval Research. It was an announcement of a decision to support a major effort at basic research. Indeed, at that time the distinction, which has become famous since then, between basic and applied research had hardly been formed. For sure, there was a history of conflict between basic and applied psychology people in the American Psychological Association, but with the general success of psychologists of various kinds in promoting the war effort, as it was called, the distinction appeared to make less and less sense. The thing is that for many, the success of the war effort was taken as a vindication of the merit of basic research. A view that was becoming very strong was the one still current, that science starts out as basic and is then applied. Application is the vindication of basic research. However, it is a view I have never shared. For my view is, to use Aristotelian terminology, that the human being is both political and reflective.

At the end of the Second World War, the American government found itself in possession of a surfeit of scientific riches. Through government support the scientific establishment had grown substantially in quality and numbers. The military view was conditioned by its fundamental principle that one must practice war conscientiously when there is no war.

The Manhattan project of the atom bomb was a good example. Theoretical physicists had been brought into the war to create a weapon. They did, creating the most awesome weapon in the history of the world. And now it was time, so to speak, to put them back into their cages. But a number of scientists, notably physicists, were demanding a place at the table were policy was being made. Two aims developed, the first was to remove them from policy making, exemplified in the Oppenheimer story. The second was to create situation in which they could continue to develop and train. It was like keeping up a pilot corps. It was essential to keep them active and in training even if there was no war to fight. Just as pilot corps had to be maintained so the science corps had to be maintained. Thus began a massive new program of government support of basic research.

There were two other factors in determining the nature of the support of basic research: meaninglessness and stockpiling. Scientific work had to be meaningless. Thus, the Manhattan project was a great experiment in the application of the principle of the division of labor in scientific work. This principle had shown its power in connection with manufacturing. While there was little by way of tradition of secrecy in connection with research, it was demanded in connection with the development of the atomic bomb. The principle of the division of labor provided the possibility of secrecy. People could be put to work in solving a number of different scientific problems without knowing why they were doing so. Later, those who knew the purpose of the work could take the many results and apply them. Purpose and meaning are inextricably related.

The military also have a principle of stockpiling goods of whatever variety. To the present day there is virtually no limit to the range of things the American armed forces stockpile. The same principle came to apply in scientific research. I remember the shudder that came over me when I read this invitation by the Office of Naval Research. The big point that the announcement stressed was that the criterion of relevance to military purpose would *not* be applied. This made me very uncomfortable. *I simply do not believe that human beings undertake projects without purpose*. I was immediately suspicious of the purposes left unspoken in the invitation. I was suspicious of the purposes that might come to be served by those who would come to be supported by these monies.

Around the 1950s, at Missouri, I began work on Freud, social psychology, and notions of community.

In the 1960s, at Chicago, in the context of the counterculture Zeitgeist, I began to pull some things together: humanistic psychology, politics, and social responsibility.

Interlude 1

I want to make some observations about statistics and psychology. I remember the first time I met Bill Estes at a convention. He smiled at me and said, "How many of you are there?" Over the years I have been asked the question, implicit in Estes' question.

How to reconcile my statistical interests, understood as linked to behavioristic, empiricistic, scientistic, objectivistic, operationistic, mechanistic, dust-bowl, personality-avoiding-psychology, with some other interests, especially Freud and mysticism.

The question is one which is deeply locked into the tie in between statistics and agriculture. Let me make some points in connection with this. The first is the essential dehumanization, depsychologicalization, devitalization in the perception of living things that occurred when agriculture suddenly grew to serve a huge mass market in the nineteenth century. Simultaneously with the great burst in urbanization and industrialization, there was great burst of market oriented agriculture.

This is reflected in the theme, repeatedly found in American children's literature, of rural children entertaining anthropomorphism with respect to animals, and being opposed by the adult world in some fashion. American rural people regard anthropomorphism especially with respect to animals, as grossly immature. Maturity consists of repressing that. This is one of the major factors associated with the development of behaviorism in America. It is a thesis I have dealt with at length in some of my writings. This came to me very clearly when I was working at the Indiana State Farm, in the dining room having lunch with some of the prison guards. A number of them were actively engaged in farming, privately at the State Farm. All of them were culturally out of the great agricultural revolution of the preceding century.

For them it was most important to make as sharp a distinction between humans and animals as possible. I have speculated in the past that this is equally the ground for the American rejection of evolution. For the latter indicates too close a connection between animals and human beings. What is important about the agricultural product is its material quantity. It is counterproductive and painful to think of the agricultural product anthropomorphically when, the day it is nurtured only to be slaughtered.

One of the main concerns of these people was the conversion between corn and hogs. Depending on the price of corn and the price of hogs on the market, one either sold the corn directly, or converted it to hogs, whichever would yield the greatest monetary return.

I do not recall the conversion formula, but all of them knew quite precisely how to convert bushels of corn into pounds of hog-weight.

This frame of reference led quite directly to the development of the methods of analysis of variance and covariance that became the research staple of psychologists. It was first developed by Fisher in his agricultural research station and then picked up by Snedecor and his distinguished student, Wallace, under Roosevelt, an architect of the New Deal agricultural program at the agricultural research station in Iowa. The books from which psychologists were learning analysis of variance and covariance were Fisher and Snedecor. Snedecor was particularly valuable because of the detailed advice on calculation that was to be found in the various editions of his book. We substituted our independent variables for fertilizer; our dependent variable for producing yields.

Statistics also became associated with a kind of carelessness with the individual, a carelessness that continues to characterize a great deal of research in psychology. Indeed, to this day there is a remarkable obtuseness in the literature in its failure to distinguish individuals from measures of central tendency. How many times do you find an author stating that there is a difference between A and B, say, for example, men and women, when only a difference between means has been demonstrated? Let me give an important historical example.

Psychologists were heavily engaged in the development of selection procedures in the military. They conducted numerous studies for the military in which some pretests were administered to a large group of relatively unselected men who were put through a training program and tested for performance at the end of the program. The psychologists provided the fundamental chart, a scattergram, and correlation coefficients. On the *Y*-axis we had the end performance score, on the *X*-axis the pretest score. Hopefully we had low pretest scores going with high end-test scores. The carelessness derived from the fact that the relationship did not have to be good – the correlation did not have to be high. For any improvement over chance was of value to the generals. In connection with expensive training programs, the generals were enthusiastic about using the tests to determine who they would accept for training and who they would reject. They cared less about the first type of error, rejecting those who might succeed. They cared a great deal about the second type of error, accepting persons who would fail. Every person who failed constituted a casualty, as it were, long before the battle. That is, defining a casualty as the loss of a person who had been trained. Even low correlations would reduce the casualty rate. In those days our sensitivity had not reached a point where we could see that any kind of selection based on a correlation which was less than perfect in fact constituted a form of discrimination.

Let me share another story of a secret. At Rochester we were in possession of a massive testing program that had been sponsored by the government in the course of a program to promote an interest in aviation. The government at one time was sponsoring a program for getting people to learn to fly airplanes. In the course of it they collected pretest data and performance data. Among the pretests was something which we called the BI, the biographical inventory. It was a long set of biographical items of various kinds, each of which was then correlated with the performance measures.

We were sitting around reviewing the results we had and planning a presentation. We had some interesting facts. Southerners were inferior to northerners; Protestants were better than Jews, and Jews were better than Catholics. Protestants and Jews were close to one another. The Catholics were far down. I remember we just looked at each other, and by common consent, just penciled all that information out of the report. We know all the qualifications that have to be made in interpreting data of this kind. We are talking of aggregate statistics, not individual cases. We are not talking of overwhelmingly large correlations, only correlations that meet the criterion of statistical significance, which, with large numbers of cases, arises with very small aggregated differences. We know of the possibility of bias in these measures. We know of the role of linked accidental factors.

Yet, from the point of view of, say, a general setting up standards for mass selection, none of these qualifications are very relevant. For, whatever the reasons, he takes it that there is this kind of relationship between, say, religion and performance. And since it is relatively easy to determine whether someone is Protestant, Jewish, or Catholic, why should he not use it to maximize the number of people who would pass, say, a pilot training program, by giving preference for admission to the program to Protestants.

Ironically, statistics, correlation, chi-square, t test, analysis of variance and covariance, all somehow entering to give validity to the dehumanizing contextual associations identified above, can be the mark of psychological professionalism in the minds of many even today.

I had some other understandings of the context for statistics.

I mentioned Kinsey already. His lesson with the charts of gall wasps made 10 years apart and revealing the identity of irregularities has always been on my mind. The aggregate can reveal things that are not manifest in the individual instance. One of the simplest examples is difference. Difference can exist, but difference is not in evidence to the person until two different items are apprehended.

Some time later I came across Durkheim's book on suicide with the clear demonstration of the persistence of the suicide rate of groups, even groups in which there was a total turnover of individuals over time. The fact of the matter is that statistical methods as properly understood and properly deployed can bring us to know things that would otherwise remain unknown to us.

But the methods of statistics have been used so *mindlessly* in psychology. Not the least is the silliness associated with testing for "significance." I have written about that (see my *The test of significance in psychological research*, 1966). I learned this for the first time in Rochester when Sy Wapner was teaching me how to use the IBM machines. As an exercise, and as a demonstration of the power of the machines, we ran tests of significance of the huge corpus of measures east and west of the Mississippi, Maine vs. the rest of the nation, and north and south of the Mason-Dixon Line. Every test we ran produced dramatically small p values: significance! But with large numbers, the test can detect a difference in means even if that difference is very, very small. And there is no reason why it should be that the population means on any of the measures should be identical east and west of the Mississippi, etc. Of course one would get significance by the usual testing procedure. Both the Bible and Darwin agree that variation in nature is ubiquitous. Why should means on psychometric tests be identical? There is no widespread understanding of this among psychologists even today.

For me the test of significance had another great significance; however, significance in quite another sense. It was something whereby I could study one of the fundamental aspects of the mystical position. What is mysticism? At root, the mystic is one who is aware that there must be a realm of the un-manifest behind the manifest, and which is determinative of the manifest.

Think of what it means to identify the mean of say, a finite population or, even more extraordinary, the mean of an infinite population. Say, a finite population, a box of pine needles. Each pine needle has a length. It is conceivable that one may measure each pine needle in the box, run up the distribution and compute the mean of all the pine needles in the box. That mean exists *objectively* even before I measured the pine needles. However, it is unmanifest. It is objective: both objective and unmanifest. *It* is there and it is *hidden*. Hidden, until I discovered it through the process I outlined. There is a process that I can engage in whereby I may go from the manifest, the measurements I make on the pine needles, to the unmanifest, which is their mean.

We can take an infinite population, the same box of pine needles but this time I do something differently. Each time I measure a pine needle, I put it back into the box, shake up the box, and pull out another pine needle, etc. The population of measurements I am making is infinite. It too has a mean. And that mean is different from the one I described above. I may have many things to say about that mean; I may speak of how it approaches the mean above, I can say things about the nature of that approach as a function of the size of the number of pine needles at which I stop to count, the sample size.

Note how I have entered into a world which exists, is objective, and not accessible at all to my senses. And I can even tell you a story of the mean associated with pulling the needles in a way in which to make the number of pulls infinite, and of the relationships among them. And other things!

And yet a third thing associated with these pine needles, we may note that the pine needles are so big and not bigger or smaller. And I make an assumption which appears very reasonable. Somehow somewhere there is a template involved in the generative process of the pine needles making them so that they are so big, more or less, and not bigger and not smaller.

And it is precisely that mean *absconditus*, that hidden mean, which characterizes the template. This is the assumption behind the test of significance. One assumes that there exists a population mean that is determinative of the generation of the distribution of measurement of the pine needles in a sample.

In case of the *t* test for the difference between means, we allow even further, the sampling distribution of the difference between means to have an existence in this statistical heaven, with this as generative of the difference between means of the two samples.

When I first learned about the *t* test, I was haunted by the question of how a table in a book, which had no prior relationship to the phenomenon or to the experiment, could give any information about the phenomenon under investigation. The answer of course is in the metaphysical assumptions that I point to above, about the generation of the manifest from the unmanifest, the table in the book arising from that same heaven, as it were, from which the phenomenon arises.

What is the reality of the population distribution and the sampling distribution? How do they generate the sample? Where is the license to make inferences from the sample to the population? Is some kind of idealist metaphysics not essential?

Thus, the test of significance was a kind of "concrete" example of the dichotomy between the manifest and the unmanifest. In it there is the two-way process. On the one hand, the generation of the manifest, the influence on the sample of the population; on the other hand, the use of the manifest to get information of the unmanifest; the use of the sample to get information about the population.

This way of regarding the test of significance was quite different from the vulgar scientism it had come to serve in psychological research. For many unfortunately, significance has become a major token of objectivity.

Somewhere there is an article by Boring in which he discussed the use of the personal pronoun "I" in the reports of psychological research. He said its use is inappropriate. He made an exception for older psychologists who had established themselves in the field. Boring's point was to emphasize the objectivity of psychological research, where objectivity was taken to mean independence of the experience of the investigator. This was the argument that was being advanced in connection with operationism as well. One was obliged to report on all the operations one performed in conducting an experiment so as to free any part of it from the particular skill of conduct or perception on the part of the investigator.

How bad was this? I recall when Arthur Melton was deliberately taking over the editorship of the *Journal of Experimental Psychology* (he had an important influence on me). He was committed to the operationist position. This meant to him that the personhood of the experimenter was not to be involved in the experiment, only his actions/behavior was involved. What is required in Melton's opinion was a lengthy and detailed statement of everything that the experimenter *did*. Unfortunately, that worked against another aim of the journal's editorial policy, to keep articles as short as possible. The fulfillment of the operationist intent of fully reporting all the operations was simply not feasible. Melton eventually adopted the policy of putting all procedures in small type, a Solomonic solution.

But the worse thing is the encouragement of a deliberate mindlessness in the conduct, and especially in the interpretation, of psychological research. For with the test of significance the investigator could "stay out of it" as it were. The procedures were all laid out and openly revealed. The data put in. And one waited at the other end of the machine to see if it came out significant or not. If it came out significant, one won, and one had a possible publication, if it came out nonsignificant, one lost, as the journals would not publish anything nonsignificant. No one understood how we were thereby filling the pages of the journals with an unknown, and possibly very large, proportion of Type I errors.

This is a problem that persists to this day, and barely confronted. Jack Cohen, who has been one of the few people who did confront the problem (1994), just send me a copy of a pre-print of a paper that he recently prepared and is trying to get published in which he bewails the fact that while the information concerning the limitations of the test of significance have been known for sometime now, the test of significance continues to be used in a mindless way without taking account of objections. He asked me to comment on the paper and I told him the only thing that I could think of is to speak openly about ignorance, and to probe the grounds of ignorance in deficiency, intention, culture, history, politics, bureaucracy, etc.

Interlude 2

On the another matter, the human being is both "*politicus*" and "*sapiens*": both social and intellectual. We find both in Aristotle's *Nicomachean ethics*, which starts with man as a social being and winds up with a set of considerations about intellectual reflection. The big problem with man as social is that whereas human beings cannot thrive without cooperation, there are numerous instances in which some human beings thrive at the expense of others. The big problem in connection with the intellect is the existence of the unmanifest, the existence of things which the human being does not recognize. The great questions in connection with the *politicus* are those of right and wrong. The great questions in connection with the *sapiens* are those of true and false. The great social problems that preoccupy us are problems calling for judgment about the levels of cooperation which are needed for coping with them. Some things need to be done individually; some things need to be done cooperatively and cooperatively at higher and higher levels of cooperation.

Western civilization is said to have begun in Egypt and Mesopotamia around the same time. In both instances there were problems in water management that could be handled only by coordinating the labors of many people. Ironically, war had been a civilizing force, because, in spite of the fact that war entails conflict, it demands an extraordinary level of cooperation among human beings. Governments have come into existence for two purposes: first to directly organize human beings in their efforts which cannot be effectively accomplished individually, or in small groups, and second, to produce and enforce law which promotes cooperation among human beings.

The great intellectual discovery, according to Maimonides at least, was the discovery - not the existence – of God around 1800 BCE by Abraham. According to Maimonides, that discovery was based on the study of nature by Abraham. Historically, that was also the birth of science. Abraham taught that to all the nations of the world. Since God, including the creative force out of which all things in nature arise and derive from, is always in the realm of the unmanifest, we do the best we can to come close to God by studying nature. For Maimonides the commandments to love and revere God, which are to be found in *Scripture*, are to be interpreted as injunctions to study science. For, he argues, if you study science then you will come to love and revere God.

Classically there have always been two approaches to science. There are those who would quickly come to claim uncovering of the unmanifest as exhaustive of the unmanifest and there are those who ever regarded what they uncovered of the unmanifest as but a small part of the region of the unmanifest. The Epicurean, who was convinced that the world of atoms and their motions exhausted all of the unmanifest, exemplifies the first approach. This contrasted with the view of God as existing, singular, incorporeal, creative but unknowable in essence by human beings. For the first view the task of the intellect was, at least in principle, completed. For the second view the task of the intellect could never be completed.

The Epicurean is intellectually impatient and insecure in the same sense that the idolater is impatient and insecure. The Epicurean is just like the idolater ready to settle for something, to take the idol as God, just because it is clearly recognizable. Indeed, in the Jewish tradition, the word Epicurean is taken precisely in the sense of the unbeliever in God.

The great scientific event that brought the world back to the scientific tradition from which it had become alienated was the development of the Copernican theory. For many centuries the peoples of the world had lost the sense of the existence of the unmanifest because they were led to believe that what existed in that realm of the unmanifest was known by their saints and religious leaders. The fundamental idea of the religion that was being taught centered on *revelation*. The fundamental idea of science, and the nonidolatrous religion, is that it is the task of human being to probe the unmanifest, rather than rest content that the truth of the unmanifest had already been revealed.

There is certain knowledge, wrong knowledge, and the unknown. By allowing the possibility that what appeared to be certain knowledge could possibly be wrong knowledge, the door opened to the tolerance for the unknown, for a renewed awareness of the realm of the unmanifest. The unmanifest would again be the unmanifest, without beliefs that this realm of the unmanifest was filled out either by atoms, on the one hand, or angels, on the other hand.

I mention all this to put my situation in some perspective. I was working for the government on the one hand, and I was trying to be a scientist on the other hand. I was being *politicus*, on the one, and *sapiens*, on the other hand. The former concerned with right and wrong; the latter with true and false. This understanding both with respect to the *politicus* and the *sapiens* is indeed very low in the discipline of psychology.

Interlude 3

In a letter, dated October 5, 1986, from David Baken to Robert Rieber about what David called his *Maimonidean Meditations*, and he was considering a subtitle such as "*psychoanalytic perspective on the Guide of the perplexed*".

Maimonidean Meditations deals with Maimonides' *Guide* as an esoteric document, the interpretation of which is facilitated by seeing Maimonides as the harbinger of psychoanalytic thought largely with respect to two items mentioned – the method of interpretation and the understanding of the language of sexuality as a metaphor. I follow the lead of Leo Strauss (who was once my colleague [at Chicago]) who identified the *Guide* as an esoteric book and who suggested that the *Guide* contains intimations of sexuality, albeit to be understood as metaphors, as part of its secret esoteric content.

Maimonides' place in history has not yet been adequately appreciated, however, honored his name may be. He is an extremely important figure for his two different sets of contributions, and their different literary histories. First, he provided a commentary on, and a codification of, Jewish law in the twelfth century. All subsequent Jewish thought and practice has been influenced by these writings that he completed prior to his composition of the *Guide*. Most of the continued interest in Maimonides on the part of the Jewish community is associated with these prior works on Jewish law.

Second, he wrote the *Guide*, and from it flow several different lines of influence. These lines of influence are not as evident as the influence of Maimonides in connection with Jewish law. We note first, the *Guide* is the foundation of whatever might be called Jewish philosophy, especially as this has developed in modern times. The orthodox among the Jews, regard such philosophy as peripheral to authentic Judaism. Second, the *Guide* was major influence on Jewish mysticism, as recounted by Gershom Scholem in his *Major trends in Jewish mysticism*. Third, the *Guide* was a major source for Thomas Aquinas and Christian scholastics of the thirteenth century, and thus a major influence on Christianity. It was a basic work for the integration of classical Greek thought with Christian thought, not the least for its great clarity in presenting Aristotelian thought in virtually a textbook form. (As I point out in my book, Maimonides could be favorable to Aristotle because gentiles, for Maimonides, could be equally excellent and appreciating the existence and unity of God.) Fourth, it was a major source for Christian mysticism, influencing Meister Eckhart, and thereby all the European Christian mystics after that. Fifth, Maimonides' *Guide* was a significant source for the founders of modern scientific thought, including Spinoza, Leibnitz, and Newton.

My book is related to the second point above. The historical line is Maimonides in the *Guide* to the Jewish mystics, and from the Jewish mystics to Freud. I try to identify the bridge between Maimonides and Freud. As for the span between Maimonides and the Jewish mystics, there is much work to be done yet on the particular way in which Maimonides' influence took place. Scholem indicates that the *Guide* was an influence on the mystics, but he is quite at a loss to indicate the precise way in which it did. I believe that by seeing how psychoanalysis may be conceived of as a product of this development, out of the fundamentals developed by Maimonides in the *Guide*, one might be able to go back and understand both the *Guide* and that history better. In my book I try to show how various great obscurities in the *Guide* become very clear through this approach. This history, however, is something that I do not tackle in this book.

Years at York: 1968–1991 (written by Fred Weizmann, Chair of Psychology at York University, Toronto, Canada)

In the late 1960s, David and Millie became increasingly concerned about the racial climate in the US and the political turmoil over the war in Vietnam. Disillusioned by US policies and very disturbed about the incidents of violence in Chicago which made them fearful for their children, David and Millie in 1968 accepted academic positions in psychology and philosophy, respectively, at York University in Toronto. They remained there for the remainder of their academic careers.

York was a new university, founded in 1959, and it was rapidly growing. Because of the shortage of Canadian academics throughout the 1960s and 1970s, Canadian universities hired faculty members notably from the US and also from Europe. York hired a number of American academics, including well-known historians and social scientists many of whom left the US over their disenchantment with the war. In fact, unlike most North American universities, the largest and most prestigious departments at York were in the humanities and social sciences. The Psychology department was the “flagship department” in the university. With some 60 faculty members, it was the largest department and the first to have a graduate program. This gave York a different tone, one that was reflected in the Psychology department itself. Although many faculty members did conventional psychological research and published in standard American journals, the department was more diverse and open to heterodox views than most US research universities. Perhaps, most notably, it was not dominated by behavioristic views. Although York was typical in terms of having graduate programs in various areas of psychology (clinical, developmental, social-personality, and general experimental) it was also quite collegial, with few barriers between members of these different areas. However, David who tended to disregard conventional academic categories both in his work and in his person, became a member of each of the four areas, as much out of principle as interest. Also in the early years, there were relatively few rules, and the department had a great deal of latitude and room for flexibility. For David, who abhorred bureaucratic rigidity or control, this was an ideal environment.

David always defended the rights of individuals. As a faculty member, he was something of a one-person “court of last resort.” He supported students who were having difficulty, usually in the name of creativity and academic freedom, and assumed the role of their supervision. A number of these students were talented but directionless. They were able to use the freedom and tutelage that David provided and so made their way through the program. There were also students he supervised who came to the realization that psychology was not for them and left to make successful careers in other fields. However, he also attracted some weak, manipulative, or otherwise unsuitable students. David’s espousal of freedom was sometimes mistaken for a laissez faire attitude, which on occasion led to poor or troubled students seeking him out. In fact, while David believed in giving students freedom he also demanded that they make good use of it. This sometimes led to problems. In one case, for example, a very disturbed and paranoid student tapes some of David’s remarks exhorting her to work harder and follow through on her work. She then attempted to use the tapes as evidence that David was being abusive. Faculty members who then had to intervene in some of these problematic cases were often irritated with David. Some were also frustrated because they felt that exposing students early in their graduate careers to David’s critiques of the standard methodological and statistical conventions in the discipline often made students too critical (and so also revealed some faculty members’ own unease with David’s critical approach).

David's defense of academic freedom went beyond students. Beginning in the mid 1980s, J. Philippe Rushton, a psychologist at the University of Western Ontario, in London, began publishing articles that argued for the existence of a hierarchy of human races. What was different about Rushton's work was not that he cataloged a number of (so-called) racial differences, but that he tried to incorporate these findings in a genetic and evolutionary framework. In 1989, the controversy about Rushton's work exploded into public view, not least because Rushton's public utterances on the matter, and there was a great deal of public outrage, especially in Ontario. The Premier of Ontario, among others, called for Rushton to be fired, and at one point there was serious consideration given to charging Rushton with violating Canadian hate laws. Several of David's colleagues at York had written and published extensive critiques of Rushton's theory and his interpretations of evolutionary theory. While David was certainly opposed to any claims about racial superiority, he was nonetheless very uncomfortable with these criticisms because, in view of the general political climate, he was concerned that they threatened Rushton's academic freedom. David's colleagues who had written the criticisms of Ruhston's work pointed out to David that their criticisms were focused on Rushton's work, and that they had refused to take part in any effort to drive him out of academia or prosecute him. David remained uneasy and uncomfortable with the issue, although Millie and his children disagreed with him. David was in large measure an antinomian, and the imposition of limits on individual freedom by external authorities, especially in the context of the university, continued to be problematic for David (and not only for David, of course).

In the 1970s David write two books, *Slaughter of the innocents: A study of the battered child phenomenon* (1971), a disturbing book about the universality of child abuse and infanticide, and *They took themselves wives: On the emergence of patriarchy in Western civilization* (1979), in which David tried to explicate the often conflicting textual and subtextual themes in the Bible about the emergence of patriarchy and parenthood. Both books embodied some long-standing themes in David's work, including sexuality, relationships, and aggression, in the form of the coercive use of authority and power. David was especially concerned about the last of these, as we noted in the context of academic freedom.

Slaughter of the innocents originated as a series of lectures he did for the Canadian Broadcasting Corporation (CBC, 1971). David was also interested in the topic of corporal punishment. At the time the Province of Ontario allowed corporal punishment in the schools, and David strongly spoke out against this practice. He became involved in the founding and governance of a school, called MAGU (Multi Age Grouping Unit) which was an alternative school operated within the public school system. MAGU can be described as a cross between a Montessori school and Summerhill, the famous British school founded on the belief that children can govern themselves within a supportive and nonpunitive environment.

One story, shortly after the publication of *Slaughter of the innocents*, David gave a talk on child abuse. On the plane returning home, David met a nun who was the principal of a mission school, in Africa. When the conversation turned to the topic of corporal punishment, she told David that she could not imagine how children could learn if they were not beaten or caned. David asked her: "Would Jesus beat a child?" She had no response.

David also injected himself into another human rights issue, one in which he had a personal interest, mandatory retirement. York had a policy of mandatory retirement at age 65. This was legal in Ontario. However, Canada in 1983 adopted a new constitution which included a *Charter of Rights and Freedoms* and under the *Charter* discrimination was illegal. David who was approaching retirement launched a law suit, arguing that mandatory retirement violated the *Charter*. Although it made it to the Supreme Court of Canada, David ultimately lost the case. However, in the interim, York, anticipating that David might win his law suit, instituted a new retirement policy which moved the mandatory age from 65 to 71. David took advantage of the extension and remained a full-time faculty member at the University until 1991 (also the date of publication of his *Maimonides* book), although there is no doubt he would have remained a full-time faculty member longer if it were allowed. As it was he continued to teach long past his retirement.

There is footnote to this story. In 1990, the University attempted to reinstate mandatory retirement at age 65. York, whose faculty was unionized, reacted to this attempt to change the mandatory retirement policy by going on strike in 1997, a strike which lasted 8 weeks. Although David was suffering because of post-polio syndrome, he supported the strike as best he could. In the end, the strike failed in its attempt to forestall mandatory retirement. The issue was only settled once and for all when the Ontario government outlawed mandatory retirement in 2006.

Soon after moving to York in 1968, David rediscovered Maimonides' *Guide of the perplexed*, a book he had initially read as a teenager. David's interest in Maimonides, the most important Jewish thinker and philosopher of the middle ages, occupied him for the remainder of his life. Although he was initially interested in the possible connections between Maimonides and Freud, he eventually began to focus on Maimonides as a figure in his own right. His last book, a commentary on Maimonides, entitled *Maimonides and prophecy*: was published in 1991. Even after its publication, he continued to explore, debate, and write about Maimonides and his ideas. He introduced a course in the psychology of religion at York that he continued to teach even after his retirement.

David was a significant presence in the Department of Psychology at York. He would speak with anyone interested in talking with him, and a number of those faculty members still vividly remember some of the notable things David said. I still, recall David's comment during one such interchange that technological changes and world-wide mobility would make it possible for small groups of disaffected people to have enormous power, and the world would have to learn how to defuse these threat peacefully (almost 20 years before the rise of Al Qaeda).

David would also sometimes organize seminars based on his latest work or ideas. One notable colloquium, I remember, concerned the limits of science, in which David argued that science had already made most its great discoveries and the question concerning what scientists should turn their attention to in the future? Most of his audience disagreed with his premise leading to vigorous debate (a decade later John Horgan would write a book, *The end of science*, making essentially the same point). David also gave a talk in which he used textual analysis to try and demonstrate how the writers of the Hebrew Bible had erased the evidence of feminine influences in early versions of the text. This, too, led to much passionate interchanges with his audience. As Juan Pascual-Leon, one of David's friends on faculty, remarked, David would take extreme positions that would serve to clarify issues, because of "his brilliant intellect and his taste for shocking people into higher social and intellectual awareness."

Although he took no part in the formal organization, David along with Kurt Danziger helped to inspire the formation of "History and Theory" as a specialized program with the graduate program of psychology at York. The fact that David was on the faculty was also instrument in attracting some younger faculty interested in history of psychology to York, including Ray Fancher. David helped found Division 24 of the American Psychological Association, the History of Psychology division, which he served as President, 1970–1971. During the next few years, he also served as President of Division 24, division of Philosophical Psychology, and Division 32, division of Humanistic Psychology.

In 1999, David's wife, Millie, a philosopher, and important influence on David's thinking and writing, as well as his beloved companion, began to show signs of dementia and, when David could no longer care for her, he arranged to have them both move to a specialized geriatric care facility where they could remain together and have their need addressed. David himself was suffering from post-polio syndrome and had repeated leg infections. Although unable to walk with his crutches any longer, he rode around on a motorized scooter and remained energetic, engaged, and involved with those around him. He continued to study Maimonides and conducted Yiddish poetry sessions and weekly Torah lessons for Jewish patients and anyone else interested. He became a patient advocate, sitting on patient and family committees. He was also made a research associate at the facility where he lived, and led seminars for researchers at the facility. He was also active on the Internet through academic chat lines and lists.

To provide some insight into David's influence at York, as well as insight into David's character and the way he saw himself, one can do no better than tell a story that one of David's younger colleagues, David Wiesenthal relates. On one occasion when faculty members were looking for a suitable faculty member to serve as the chair of the department, David Wiesenthal asked David if he would be interested. David refused, and quoted one of his father's favorite sayings in Hebrew: (in English), "In the end, it is better to be a prophet than a king."

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Significant data

David Bakan was born on April 23, 1921 in New York. He married Millie Blynn in 1942 and David and Millie had six children: Joseph (1948), Deborah (1952), Abigail (1954), Jonathan (1958), Daniel (1960), and Jacob (1966).

David completed his A.B. at Brooklyn College in 1942; his M.A. at Indiana University in 1944, and his Ph. D. at Ohio State University in 1948.

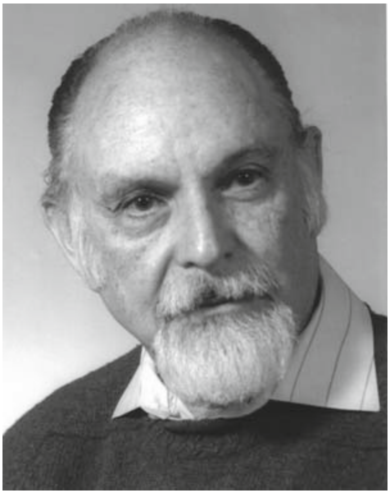
He was employed for 1 year as a clinical psychologist in the Indiana Prison System in 1943. He was Director, Statistical Department, of Cooperative Test service of the American Council on Education, during the summer months in 1945. From 1945 to 1947 he was the Chief Statistician, Committee on Aviation Psychology, National Research Council at Ohio State University and the University of Rochester. From 1947 to 1948 he was an instructor at Ohio State University, and then was appointed Professor of Psychology and the Director the Laboratory of Aviation Psychology at Ohio State University until 1949. From 1949 to 1961 he served as Professor of Psychology at the University of Missouri and, for 1 year (1956–1958), as Visiting Lecturer in Clinical Psychology at Harvard University. From 1961 to 1968 he was Professor of Psychology at the University of Chicago and in 1968 was appointed at York University in Toronto Canada.

David served as a member of the Board of Ethical and Social Responsibility (1974–1976); Committee on Testing and Assessment (1975–1976); and Task Force on Privacy and Confidentiality (1975–1976) of the American Psychological Association. He served on the Advisory Board of the National Council to Abolish Corporal Punishment in Schools (1974 onward); the Canadian Council on Children and Youth (1974 onward); and the Board of Advisors, National Center for the Study of Corporal Punishment and Alternatives in Schools, Temple University, (1976 onward). He served as consulting editor for numerous journals, including the *Canadian Journal of Family Law*.

David presented the Terry Lectures on Science and Religion, Yale University, February 16–19, 1976.

Confessions of a Marginal Psychologist

Kurt Danziger



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.

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Only those with the very highest grades had any hope of qualifying. I had been the class medalist 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 uninvolved.

The contents of the course gave me little cause to change my mind. As I realized later, they probably had not changed in 20 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 and more a matter of routine.

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 neobehaviorism 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 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 with 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, and that the indirect route advocated by learning theory was therefore more promising: One should first establish the basic "laws of behavior" by experimenting on subhuman 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 subhuman level, and, being now at least theoretically immersed in the subculture 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 was just then visiting Cape Town, where, some 20 years earlier, he too had started academic life as a student of psychology. Subsequently, he had gone to London to study with Spearman but, without job prospects in psychology, had ended up as a colonial administrator in West Africa; work 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 toward 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, Humphrey (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 neither 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, that is, 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 that 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 doing. 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 with the fascinating demonstrations and original conceptualizations that characterized the human experimentation of Lewin's Berlin group.

One aspect that bothered many in the neobehaviorist 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 neobehaviorists 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 psychoanalysis 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 desiccated 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 psychoanalysis had to offer. I knew that book knowledge would not suffice, and so I embarked on a brief psychoanalysis (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 psychoanalysis 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. Psychoanalytic 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 psychoanalysis 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 neobehaviorist 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 do not 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. This critique could help one to recognize 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, this recognition 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 Rousseauan. 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 nonrelational, 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 happily for a while and therefore come to like. 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 expressed their Nazi sympathies 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. Cape Town had always retained the character of an outpost of Europe, whereas 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 toward 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 50 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 that 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 twentieth 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 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. Toward 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 increasingly 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 postcolonial 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 also, much more profoundly, in using altogether different categories for mapping and conceptualizing its subject matter. As I have described elsewhere (Danziger, 1997) a 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 postcolonial 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.

Had one wished to satirize the psychological approach to world problems, one could hardly have come up with a better 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 nontraditional 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 preindustrial 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. 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 50 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 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 200 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 perso-nally, 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 with 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 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, 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 whereas 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 toward 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, which 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 1970s, I had been an academic for more than 20 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 to plan a return to the study of attitudes in a group context, which had seemed so promising many years earlier in Durban.

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 was that of historiography, and it 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 other 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 100 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, 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, toward 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 twentieth 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 that 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 of 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's 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 toward the end of the nineteenth 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 1980s, 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 twentieth 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 "prehistory" pertaining to developments in the eighteenth and nineteenth centuries that created the possibility for the emergence of twentieth 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 of 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 1990s. 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 people (and the relevant 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, 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 to emerge 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, and the mathematical tools were adapted to the knowledge goals of the investigators.

This kind of analysis amounts to 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 1980s, 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 metadiscourse 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 1980s, I was also making notes on the metahistorical 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 metahistorical 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 twentieth 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 nineteenth and earlier twentieth 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 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 intradisciplinary concept of "methodology" that 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 microsociology 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, and 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 be 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 postwar 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 subdisciplines 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 1980s, 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 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 could 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, briefly exploring 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 postWorld 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, such 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 "prehistory" 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 eighteenth and nineteenth 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 eighteenth 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, nonarbitrary, 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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Professional Marginalization in Psychology: Choice or Destiny?

Amedeo Giorgi

Introduction

As one makes presumably free decisions in the course of one's life, one hopes that one is acting rationally. Certainly, at the time of the decisions, I thought that I was. I also thought that I was acting freely, bound only by some inevitable contingencies at the time, but no true obstacles. It is only retrospectively, as I look over the course of my life, and a certain pattern becomes discernible, that I begin to ponder that murky word, "destiny." Did I really freely choose all those contingent options that presented themselves to me, or was I only fulfilling some kind of fate as I pursued

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my career? I am not sure that I can judge this issue, but in order to help the reader to decide, I will first present the important factors of my biography as I see them and then I will present what I believe to be the important decisions that I made as a professional psychologist. The reader, as well as I, will recognize that there is possibly a certain bias in the narrative to be given in the sense that I will be giving both accounts, and I will be selecting the events to be reported. This, of course, cannot be avoided. All I can say is that I will be honest in the sense that I will report the events as they impacted upon me and I have no a priori bias with respect to the pattern that emerges – if one emerges – in the sense that I have no precommitment to either destiny or choice. I know that I felt free in making my decisions, but that does not necessarily rule out the fact that I may have lived a "patterned" existence.

I am not sure when the idea that I may have lived a "patterned existence" dawned on me, but inklings of such a notion began to appear in my fifties. It may have occurred in my early sixties when I began to reflect upon the possibility of retiring – not for the sake of a leisurely life – but more for the possibility of doing my own work, or at least giving it priority, as opposed to giving priority to institutional work first and turning to my own only when the work of others was done. While I was very conscious of my marginal status as a psychologist, I never quite realized how marginal I was as a citizen of the US as well. It occurred to me that perhaps "marginality" is my essence, although I can honestly say that I never strived for it for its own sake. In any case, as I look back, here is how things evolved.

Biography

I was born on July 9, 1931, the third child of Paul and Palma Giorgi, in the borough of the Bronx in New York City. I had a brother, Louis, 8 years older and a sister Assunta, 6 years older than me. A younger sister, Elena, arrived in 1940, but by then, we had moved away from the Bronx to Philadelphia and it was there that I was brought up.

The family moved to Philadelphia when I was 1 year old in order to open a grocery store. My estimation is that this move was extremely important for the formation of my character and personality. In New York, the family lived in an Italian ghetto with many members of the extended family nearby. (It was known as the Morrisianna section of the Bronx, not so far from Fordham University where I later earned my MA and PhD degrees). It was an immigrant working class neighborhood, but, I was told, there was much camaraderie and warmth.

The Philadelphia neighborhood was also working class, but we were the only Italian immigrants who lived there. The ethnic constitution of the neighborhood was mostly Irish and German with a scattering of other immigrants, mostly Polish, Scottish, or Slovak. The one or two other Italian families in the neighborhood were second generation and assimilated. What this meant was that instead of being one of many in the same boat, we stood out as different from our immediate neighbors.

Many factors seemed to contribute to this differentiation. My parents spoke only broken English and there was often difficulty in communication and misunderstandings were many. Another difference was due to the fact that we owned the store at which many neighbors shopped and so it was difficult to simply be one of the "regular guys" with our customers' children. Another factor was my name. While "Amedeo" is a well-known name in Italy, no one had ever heard of it in my neighborhood. (Mozart and Modigliani were not household names!) Everyone thought it was Italian for "Andrew" and so I got the nickname of "Andy" while I was still preverbal. Even the teachers at the public school, which I first attended seemed not to recognize the name at all and they had difficulty pronouncing it. I hated going to school and especially being called upon in class to answer a question or perform some task, so I used to sit in the back and tried to become invisible.

I believe that it would be fair to say that I felt that I was discriminated against as much as any black person from ages 5 to 12. I was in many fights until 8th grade, mostly over my nationality, my name, or my parents' status. Name calling was frequent and refusing to speak or play with certain childhood friends happened frequently, and usually ended whenever some larger neighborhood event or some other necessity took place. But these events always happened to me within a context of ambivalence because if the playmates were customers' children then I could not remain angry with them lest they ceased to remain customers. There was a way in which I could not be as carefree as other children.

Another outstanding event of my early years was that my brother decided to enter the seminary in order to study to become a priest. One consequence of that decision was that I transferred from public school, where I was floundering, to parochial school, where for some reason, I blossomed as a student. Perhaps because we were all Catholic and Italian in that classroom gave me a sense of belonging that I did not feel in public school. But, still, it was not without a certain strain. The parochial school I attended was St. Mary's, which was an ethnic (Italian) parish, a bit distant from where I lived, and the church was situated in the heart of the Italian ghetto of North Philadelphia. My parents went to that church rather than the nearer one because Italian was the language of the parish. However, all of my neighborhood friends attended the nearer church and school, Corpus Christi, which was basically an Irish parish. What this meant was that there was a split between my neighborhood friends and my school friends. The latter lived too far away for me to play with them and those I played with, after school, did not share any of my scholastic experiences, nor I with them. Thus, something less than a total belonging was felt.

This feeling of being in between worlds continued, in one way or another, throughout my education, up until the PhD. My friends who went to Corpus Christi were sent to one high school and those from St. Mary's to another and so the separation between neighborhood friends and school friends continued. When it was time for college, the reputation of the local Jesuit college, St. Joseph's, was that it was tough and I wanted to test myself against the toughest and I chose to go there. Most of my classmates who did go to college (it was not so universal in those days) chose La Salle College run by the Christian Brothers. I once again found myself as a relative loner among a group of students, many of whom knew each other from the Prep School circuit. I didn't realize that social class was operating in college choices. In effect, attending St. Joseph's accentuated my sense of being between worlds. The "costume" for collegians in those days was khaki pants, sports jackets and white buck shoes; on the corner where I hung out with my neighborhood friends it was "zoot suits," pointed leather shoes and "lumber jackets" for leisure wear. I straddled those worlds, avoiding extremes, as I tried to participate in both, but never fully in either. I wanted to be loyal to both worlds, but it was impossible to do so because they were so different.

At one time during my collegiate years I remembered an early experience I had had, undoubtedly provoked by a similar situation. I remembered a time when I was about 5 or 6 years old and I was worried because it seemed that I could not keep all of my acquaintances, including relatives and family, happy. I was basically being raised in a continental manner, speaking Italian at home with European values subtly being expressed everywhere, but in an American context with basically pragmatic values and American social–civil customs. These worlds did not harmonize easily and I remember reflecting on this and saying to myself, "You cannot satisfy everyone. So, I should follow my own values, and there would always be someone from my large group of acquaintances who would agree, and always some who would not." Perhaps this is true of everyone, but it was especially poignant for me at an early age because it prepared me to become a marginal person.

Experiencing myself at the nexus of a couple, or of several, worlds persisted throughout my graduate education and my professional career. I shall not detail any more instances, except perhaps in passing, because the basic point has been made. I shall now turn to my exposure to psychology and the difficulties surrounding that initiation.

I should also mention that the grocery store we owned was a complete family operation and only family. My earliest memories are connected with working in the store, doing chores appropriate to my age and size. Full responsibility came at age 15 when my father died and the store was handled from then on principally by my mother, my older sister, and myself. My younger sister also contributed to the extent she could until she, too, reached maturity and was a full contributor. The work in the store was a constant companion through high school, college, and even during graduate school at selective times. Since the business was conducted from the early 1930s, through the 1940s and the 1950s, the hours were long (6 A.M. until 11 P.M.) and it was fully stocked (selling fruits and vegetables as well as fresh meats) as was the custom in those days.

Psychological Training

I entered college as an English major, more because I needed to declare some major than for a passionate interest in the topic. I surmised that I was more of a humanities type than a strict natural science type, but when I investigated what majoring in

English truly meant, I was not so inclined and so I knew I had to find another major. At Jesuit colleges, at least in those days, one always majored in philosophy and one got a good sprinkling of theology courses as well in addition to the declared major. I was somewhat attracted to philosophy, but I did not so much like the issues that were taken up, nor the universalistic approach often used by philosophers. So I decided to research becoming a psychology major, which I imagined still dealt with issues involving humans and their worlds, but perhaps more concretely than philosophy. I spoke with some fellow students who were majoring in psychology, and one of them advised me to read William James' *Principles of Psychology* (James, 1890/1950). I took the book out and scanned it, but I allowed myself to read wherever an interest was sparked. I was drawn to the first chapter dealing with "The Scope of Psychology," but then skimmed quite a bit until I reached Chap. VII, "The Methods and Snares of Psychology," and then read the next four chapters rather thoroughly. The latter dealt with the relation between mind and things, the stream of thought, the consciousness of self and attention. I merely skimmed the rest. Based on this skimming and reading of James, I became a psychology major in my sophomore year of college. (Later I read the *Principles* from cover to cover along with all of James' other works.)

Thus began 7 or 8 straight years of intense study of psychology, from my first introductory course to the completion of my doctoral dissertation. Though I could master the material it seemed as though I was always at odds with it. It was never quite what I expected it to be. Of course, I began to appreciate the difference between the psychology embedded in common sense and what the "science of psychology" was trying to establish, but it seemed to me that common sense was closer to the truth than the alleged scientific approach, even though I could see that common sense psychology was ming1ed with myths and exaggerated distortions. It certainly needed a critical perspective applied to it. But scientific psychology, I thought, allowed the genuinely psychological to slip through its various apparatuses and techniques.

It is also important to indicate the degree of naïveté I possessed when it came to the world of higher education. During my senior year at St. Joseph's, I applied to three graduate schools. Since my father died, my older brother became *paterfamilias* and he insisted that I apply only to Catholic universities. Since there were none offering doctoral degrees in psychology in the Philadelphia area I applied to Georgetown University and Catholic University, both in Washington, DC, and to Fordham University in New York, NY. It turned out that at that time Georgetown had no graduate psychology department and Catholic University did not accept me, but Fordham did, so I went there. It seems to me that this solitary option had something fateful about it. Catholic University was known for its clinical program and Fordham was strongest in experimental psychology. Of course, Fordham also had a clinical program, but the courses I took in it left me cold. I simply did not believe in what I was being taught. Had Catholic University accepted me, would I have remained in psychology until the end?

All of my courses in psychology were traditional, perhaps even conservative. At the undergraduate level I had statistics, experimental psychology, testing, physiological psychology, and so on. At the graduate level, the same basic courses were repeated, often at an advanced level and as I progressed toward the doctorate, the themes were repeated, most frequently with increased specialization. I worked under Richard Zegers, S. J. at Fordham and he had earned his PhD at Columbia under Clarence Graham. Basically, he duplicated the Columbia visual lab at Fordham. Most of his students worked on problems in the psychology of vision and I was no exception. I was still single in those years and so I was able to continue studies without interruption and so I went from AB to PhD in 4 years.

I enjoyed laboratory work and pondering over theoretical issues, but there was one problem: I had difficulty reconciling the concrete work that I was doing in sensation and perception with my implicit, naive understanding of what psychology ought to be about. I could just as easily have called myself a physiologist, neurologist, or even a physicist and I am not sure that my concrete work would have changed at all. Indeed, my doctoral dissertation was published in the *Journal of the Optical Society of America!*

I tend to be a reflective person and my psychological training made me sensitive to an issue that has remained with me for my entire career, namely: What is the meaning of psychology? What, after all, is the "psyche"? What are its boundaries? How does one delimit the field? These fundamental issues announced themselves to me in the following way.

As a graduate student following an experimental program and specializing in the psychology of vision I had to do much reading that was not in psychology. For example, if I wanted to know more about the structure of the retina, or about rods and cones, I went to the biological library in order to find the appropriate material. If I wanted to know more about the chemical reaction in the visual receptors (rhodopsin and iodopsin) then I went to the chemistry library. In order to really understand light, I had to learn its characteristics, and that meant a trip to the physics library. If I wanted to understand experimental design and laboratory procedures better, I could go to the psychology library, but I was really reading about scientific practices modeled after physics and chemistry. Finally, if I wanted to become expert in statistical techniques, then I ended up going to the mathematics library. I spent less time reading psychology than I did boning up on supportive disciplines. It was not that I minded going to other fields, but it was more that I did not experience a psychological center that was holding things together. It was as though what was called psychology was 90% conglomeration of other fields and 10% psychology – but what was that 10%? Was there a psychic glue holding everything together? Moreover, no one else, faculty or peers, seemed to be worrying about this problem. When I did occasionally raise the issue, I was told that I should become a philosopher.

The other strong impression I got from my training was that psychology was more committed to exemplifying that it was a natural science than it was concerned about being faithful to psychological phenomena. All kinds of measures or operational definitions were standing for complex lived actions that basically made a caricature of the latter, and yet it was all accepted in the name of science as practiced with nature. The fact that psychology was perhaps being more faithful to the philosophy known as “naturalism” than to psychology did not seem to be noticed by anyone, except a few philosophers. My objection was not to the idea of science as such but to the idea that science had to be practiced as it developed within the context of the study of nature. After all, the idea of a human science was alive in the nineteenth century and it did not seem all that revolutionary to modify certain procedures of science in order to accommodate human characteristics. However, the very idea seemed to be anathema to most of my psychology professors. Nevertheless, I carried along with me the notion that it should be possible to practice science differently without having to give up the goal of being scientific.

The Italian Immigrant Experience

The above account of my growing up in America is obviously an individualized account, and it was only later in life that I realized that my own experiences were shot through with generalities. It is true that I was born in the US and so, strictly speaking, should be considered first generation American. However, my neighbors and society at large did not observe such subtleties. My identity was clearly with “immigrants” or “outsiders.” Thus I participated in the same identity as my parents. I mentioned that I experienced strong discrimination in my youth. I am sure that every ethnic immigrant group suffered some type of discrimination, but here is what is said about the Italian immigration by those who have studied these issues:

> Perhaps more than any other ethnic groups, Italians faced considerable prejudice in America. They were hired for low wages and, along with other southern Europeans of dark skin, labeled as “swarthy.” Italians became a significant factor in the growth of American nativism…. Italian immigrants in the post-1880 period had the distinction of having all three nativist impulses directed against them: anti-radicalism, anti-Catholicism, and racial nativism (Vecchio, 1997, p. 48).

> Immigrants often sought out Little Italies as a result of the hostility they encountered in American society. As a despised minority rooted in the working class and seemingly resistant to assimilation, Italians suffered widespread discrimination in housing and employment. American responses to the immigrants occasionally took uglier forms as Italians became the victims of intimidation and violence…. Criticism of Italians became integral to the successful legislative drives to enact the nativist Literacy Test in 1917 and National Origins Acts in 1921 and 1924 (Pozzetta, 1995, p. 767).

One of the charges against Italian immigrants was that they did not assimilate easily. But, of course, with such discriminatory attitudes being expressed against them, how easy could it have been to assimilate? As Pozzetta (1995, p. 767) states: “Within Little ltalies, immigrants created New World societies. A network of Italian language institutions – newspapers, theaters, churches, mutual aid societies, recreational clubs, and debating societies – helped to fuel an emerging Italian-American ethnic culture.” What is implied but not explicitly noted in many sources is that Little Italies also provided a protective atmosphere. One could be more emotional, gesticulate freely and be as loud as one cared to be in a way that was not possible in Anglo-Saxon communities. The key point for me while growing up is that while I experienced the discriminatory attitudes and practices, I did not have the protective atmosphere of a Little Italy. I did have my family, which was indeed a source of psychological strength for me, but such a unit was too small to provide complete psychological protection against the larger social world. After all, the family, as immigrants, did not fully belong to the social fabric of America. Consequently, the only place to take a stand against such social slings and arrows was on my own resources, with familial support. But the protection of a larger social group was lacking.

When I did go to my parochial school, I did feel a greater sense of belongingness, but to Little Italy, not to America. And because of my neighborhood exposure to the Anglo-Saxon world, I also experienced the world of Little Italy as provincial. I did not want to be wholly Italian in America. But, somehow, I did not want to be wholly Anglo-Saxon either – although I might have thought of this as not being wholly American. I was being raised with continental values and I did prefer them to Anglo-Saxon values. At home, there seemed to be a sense of warmth, caring, and heightened relationships that I did not perceive among my peers. I did not want to give those values up, so I straddled the cultures, but not without a sense of deep loneliness.

While the above summaries give one a sense that discriminations take place, they do not indicate very well what it feels like to be a target of disapproval for arbitrary reasons. The best sense of the feelings associated with discriminatory practices that I have come across has been provided by an anonymous woman giving advice to a young Italian Jewish boy fleeing Fascist Italy to live in Palestine (Segre, 1985/1987, pp. 106–107). The quotation is long, but it is so "on target" that I want to quote it in full. At the time, 1938, Segre was 16 and the woman was about 35. He writes:

> A tall, blond, sad woman, she listened to me with a strange look in her eyes, a look of curiosity and compassion. With considerable tact she kept warning me of the illusions I was weaving about the country awaiting me. I would not find anything remotely like Italy, she said, either in the landscape or in the people. It was not the physical toil that I should be afraid of but the cruelty of human relations. In Palestine, the differences among people were greater than in other places because of the large number of immigrants. I would probably find myself lonely and misunderstood by other youngsters because of the type of world I was coming from. I would be caught up in a network of abrasive relationships among uprooted people who, because they were uprooted, were now busily engaged in building a world in which to forget their past. I should not expect compassion, pity, or kindness, though I would be able to rely on human solidarity. But it was a type of solidarity that shied away from privacy and individualism. Necessity and ideology privileged the group rather than the individual. For this reason it would be wise of me to find and adhere to a group as quickly as possible. I would suffer less than if I remained aloof. The price to be paid for acceptance would certainly consist of the loss of many of my dreams, not to speak of the tastes and habits I had brought with me from my home, but nevertheless, I should conform. The only other piece of advice she could give me was to try and develop thick calluses on my soul like those that would very soon harden the palms of my hands. Palestine, she kept telling me, is a land where caresses are made with sandpaper.

These are amazing reflections on the immigrant experience and what the anonymous woman said certainly rings authentic to me. While the woman refers specifically to Palestine, my experience is such that what she says is true of all lands in general. A big difference was that I was not given such advice and I had to experience the tribulations of getting along in the world rawly. In addition, I belonged to no group. I was too far away from "Little Italy" and the neighborhood friends did not fully allow integration into their group. Thus, aloofness was my only strategy, and loneliness was my constant companion. The support of family and its values, more than anything, was critical for me as I solitarily determined that I should somehow succeed at something.

Now, I am aware of other persons, including psychologists, who have been exactly in the same cultural situation as I am, and yet they are comfortably establishment persons. They seem to have made the transition from immigrant familial context to mainstream American establishment without difficulty. I really do not know how to account for this, except to state the fact that such a path would have been inauthentic for me. The possibility of belonging to "establishment America" was for me always a temptation, but never an authentic choice. It would have meant too much of a denial of my own essence to make that transition. The price was too high.

Another factor that could have played a role in my case is the fact that I received a straightforward Catholic education. It is well known that the Catholic schools more frequently served the immigrant population since their growth was correlated with the children of the immigrants of the early and mid-twentieth century. Even if not explicitly acknowledged, there is a certain second class status associated with Catholic schools from an American social perspective. Hardly any such schools, even today, would be deemed equal to the Ivy League schools or to certain prestigious small colleges such as Haverford or Swarthmore. Perhaps these Catholic schools need another century before full acceptance is plausible, especially if further secularization takes place as happened with the older Protestant colleges and universities. The ultimate irony, for me, is the fact that most of my teachers at Fordham went to Ivy League schools, including the Jesuits, and brought back with them everything they learned at those schools. My training at the graduate level at Fordham was as secular as at any public or nondenominational private university. The only difference was that a few teachers wore Roman collars. Yet, somehow, the education is stereotyped as a second class one because the culture at large posits the older, once Protestant, but now secular, universities, as superior.

In speaking the way I did in the above paragraph, I am speaking from an external perspective – from the perspective of society. From my personal perspective, I would not have traded my education for any other. I found in the Catholic college that I attended values much closer to the continental values with which I was raised. There remained within that system, a full respect for the human person, a concern for philosophy and ultimate values, a respect for the classics and humanities, and an approach to science that emphasized equally its limits as well as its strengths. Technology had its place, but it was not absolutized. My graduate training was not as balanced. The idea of science overwhelmed the content of psychology and technical and pragmatic concerns dominated where common sense would have cautioned hesitation. Thus, as I was about to launch my career as a psychologist in the US, as a citizen, my upbringing left me feeling something like a second class citizen because I could not fully embrace established sociocultural values and remain authentic to my own values. I also sensed that I had second class status because I was a product of a Catholic education, which was perceived to be correlated with “outsiders” rather than with the establishment. Finally, my self-perception as a psychologist was also that I was different, because I could not fully embrace establishment psychology.

Career as a Psychologist

While I was pretty sure that I would end up having a career in academia, I was not eager to rush into such a job because until age 25 I had spent all my life in school. After about 6 months of searching I was offered a job with Dunlap and Associates, a consulting firm in Stamford, Connecticut. Basically it was to do human factors work, or applied experimental work, and often was called – a term I hated – “human engineering.” All the work was for the government and it was related to military objectives. While the work was meaningful from a “military-systems” perspective, I did not find it satisfying because human presence in such systems was minimal. The system was what mattered the most and fool-proof options were always chosen first because they were allegedly error-free. Hence, my exposure to psychology thus far (not counting independent reading on my part) had been to learn about minimum level functioning of the human person (mostly sensation and perception) or of infra-human species and how to either eliminate, or simplify greatly, human decision making. Yet, I entered psychology in order to understand the “whole person.” I had run some dozen experiments in connection with my work at Dunlap, so I did learn about practical “real-world” research and how different it really was from the conditions in the lab. Nevertheless, after about 3 years, I left Dunlap and began teaching in the psychology department at Manhattan College in New York City.

My stint at Manhattan College was brief, but a significant event took place there. One day a young student came up to me after class and began chatting with me. Then he said “I know you are a young teacher, and I know that you will get better with age, like a good wine.” I could tell that I was being set up for a critique, but I was not ready for what followed. He continued, “The trouble is that whenever you present something, you critique it and practically destroy it, so I don’t know what to believe.” I was stunned by this comment because the student was absolutely correct! I did not believe in what I was teaching. Moreover, I suppose an existential crisis came over me because the comment made me realize that I could not project teaching this material over the next 30 or 40 years. I was at a loss. I was still young enough to change careers, but I did not know to what field I should switch. I did not want to go into business. I did not want to return to

English. I realized that I did like the field of psychology *as I imagined it*, but this field did not exist in the world. Was I to remain an eternal critic, or was there some sort of constructive alternative within which I could work? An event that had taken place about a year earlier came to mind and it determined the direction of the rest of my career.

While I was still at Dunlap, one of my classmates, Ed Hogan, went on to teach at Duquesne University and he wrote me a letter one day about a young Dutch psychologist who came to teach at Duquesne. He wrote to me that this Dutch psychologist "Makes many of the same criticisms about psychology that you did." This Dutchman was Fr. Adrian van Kaam, who while in Europe was exposed to post-war continental existential-phenomenology, but came to America to get his doctorate, and then came to settle at Duquesne University because he belonged to the order of priests, the Holy Ghost Fathers, that ran Duquesne. However, van Kaam was not prepared for the "dust-bowl" empiricism that dominated American psychology and so he was motivated to introduce a different kind of psychology.

I first met van Kaam while I was still with Dunlap. Apparently, some time during the late 1950s Maslow went on a sabbatical from Brandeis University and asked van Kaam to take his place. During that time I was also making weekly trips to Massachusetts and so we agreed to meet one night in Waltham, Massachusetts. We talked for hours, and it was the first time that I even heard of phenomenology in a nonderogatory way. I remember asking if one had to be a clinician to be a phenomenologist and he negated that idea and told me about European experimental phenomenologists that I had never heard of. I was pleased with his response. I asked him whom I should read, and out came another series of totally unknown names: Husserl, Heidegger, Merleau-Ponty, Sartre, Buytendijk, Linschoten, Gurwitsch, and so on. He also told me that at the New School for Social Research in New York City there were philosophers teaching courses that would expose me to this style of thought. Finally, he said that he would like it if I came to Duquesne to direct the research program there based upon a phenomenological perspective. A year or two later I joined Duquesne and became a member of a marginalized psychology department.

The Duquesne Years

Before I left New York for Duquesne in September, 1962 – the year the PhD psychology program started – I did attend courses at the New School and followed courses by Aron Gurwitsch, Rollo May, and Paul Tillich. While the courses by the latter two contributed to my growing background in existential phenomenological thought, it was the thought of Gurwitsch which truly inspired me. His work, *The Field of Consciousness* (Gurwitsch, 1957/1964), was an eye-opener to me about different ways of approaching consciousness and I was happy to contribute to the translation of the English version of his text. Linschoten's (1959/1968) book on James, another book that I helped translate and also edited, was also impressive to me.

These two books helped convince me that phenomenology could make valuable contributions to academic psychology.

I spent practically a quarter of a century at Duquesne, and the best years were the first 15 years or so. Adrian van Kaam had begun an MA program in religion and personality in 1958 and received permission to commence the PhD program in 1962. Being a European, van Kaam was used to the fact that institutes took on the coloration of their chairs and so he decided that Duquesne's program should do only one thing, but do it well. He argued that we should introduce an existential phenomenological approach to psychology and simply specialize in that perspective. It was not an easy task, especially in the American scene. Moreover, such a specialization was necessary in order to start a doctoral program because both Pitt and Carnegie-Mellon University already had doctoral programs and in order to introduce a third in a city the size of Pittsburgh a greater justification was required by the administration. The program was approved and we began our venture.

Perhaps it was the climate of the 1960s, which were just beginning, but we were truly optimistic that a whole new and revolutionary approach to psychology could be implemented. We wanted to raise foundational questions, root psychology in a different philosophy, redefine it, and then show how the redefinition was superior to what was being practiced by mainstream psychologists. Of course, a key question was how our students were going to get exposure to this new philosophy because it was not indigenous to America. Here, we were lucky. The philosophy department had begun, in the mid-1950s, to specialize in existential-phenomenological thought. This was due to the effort of Rev. Henry Koren, another Dutch Holy Ghost father, who updated the philosophy department and reinvigorated a dormant press. Existential-phenomenological publications began to stream from the press and specialists trained in Europe began to fill vacancies in the philosophy department. In addition, the philosophy department had a "Visiting Professor" program, and every semester a different specialist in one aspect or another of existential-phenomenological philosophy lectured at Duquesne. Consequently, the problem of exposure to existential phenomenological thought for our students was solved by having them take one philosophy course per semester while they were pursuing their psychology degrees.

Issues Surrounding the Development of an Alternative Psychology

While I did not realize it at the time, I wonder if our psychology department could have even gotten off the ground if it were not for the cultural openness of the 1960s. I would probably have denied it then, but today I doubt it. I think that we were riding a cultural tide of openness and experimentation and in a sense, subsequent decades indicate this because the phenomenological bubble burst rather quickly and today there are only a few psychologists who pursue it, but that is not necessarily due to a lack of interest in the ideas as I will demonstrate later.

During the 1960s, the psychology department's reputation grew, both positively and negatively, because of our exclusive emphasis. We were attracting more students than we could handle from all over the country. On the other hand, peers and colleagues were calling us "philosophers" and not true psychologists and we were called antiscientific and numerous other names not worth mentioning. Why? The reason is because we claimed a viewpoint that was at odds with mainstream America, and because we emphasized unfamiliar aspects of our amorphous field of study. After all, the ideas we were promulgating came from Western Europe and psychology's "foundation myth" was that it began in Germany, and so of European origin. True, we acknowledged one perspective, but how is that worse than trying to do everything more superficially as some 200 eclectic graduate departments in psychology were doing? However, a closer look at our program would have indicated that our perspective was taught in dialog with mainstream psychology – not by ignoring it. In other words, we really had a coherent vision that could easily have legitimated our project to any open-minded scientist, but many of our psychological peers judged us superficially and dismissed us.

I mentioned above that our doctoral program began in September, 1962. It should be noted that van Kaam left the department in winter of 1965 to start his own institute, which went through several name transformations over the years, and ended up as the Institute for Spiritual Formation, or some such, by the time it closed down in the 1990s. It seems that van Kaam had marvelous talent for getting things started, but he was not so good at maintaining programs or bringing them to true fruition. Adrian van Kaam deserves all the credit in the world for getting both the MA and PhD programs started, but the fact that these programs received significant recognition in the 1970s and early 1980s was really due to the entire faculty that pulled together and decided to continue and to improve what we started despite van Kaam's withdrawal. There was some talk of breaking up in those days but we hung together and plowed forward. I should also add that it was not smooth running such a marginal department and it seemed that we had crises every couple of years, but we managed to survive them, at least in the developing years.

A Sociology of Knowledge Perspective

In the personal section of this article, I indicated how, because of my immigrant life style, I experienced certain prejudices, and even if the prejudices had not existed, I believe that I would still have found myself at odds with many of the values of the culture at large. Thus, it would be fair to say, retrospectively, that I was a marginalized person. Speaking Italian at home, being raised with Mediterranean, continental values rather than the Anglo-Saxon ones of the dominant culture, put me in a place I did not choose to be. Moreover, receiving primarily a Catholic education added to the marginalization and so did the fact that I spent a large block of time teaching at a Catholic university. The factor that sealed this marginalization is the fact that I came across phenomenological philosophy and was completely taken up by its perspective and arguments.

Before I make explicit the consequences of having encountered phenomenology, I want to say a few words about the sociology of knowledge perspective. The sociology of knowledge perspective does for science what sociocultural analyses do for the Life world. That is, the sociology of knowledge tries to account for the role of external or arbitrary factors in the constitution of scientific knowledge. While it is hoped that only relevant rational factors are responsible for the constitution of knowledge, it is well known that humans are prone to error and consequently, specific interests, trends, fads and fashions as well as specific historical beliefs often color the way scientific knowledge is expressed. It seems that the first person to call attention to these factors was Sir Francis Bacon when he warned scholars about the "idols" that could seduce one when attempting to express scientific knowledge, even though the term, as such, did not exist in his time. The term, sociology of knowledge, seems to have emerged in the first half of the twentieth century with the work of Mannheim (1936, 1952) and Scheler (1924/1980), with earlier presentiments from Weber (1946) and Durkheim (1897/1951), among others, and many of the issues were delineated during the famous *Methodenstreit* that took place in Germany in the latter part of the nineteenth century concerning the nature of knowledge in the social sciences. The overriding issue was whether there could be genuinely objective knowledge in the social sciences given the significant role of subjectivity in those disciplines. It is not clear whether this debate was ever satisfactorily resolved, but it is clear in this postmodern era that the handling of subjectivity is far from established and the unresolved issues surrounding the question of objective knowledge are still too numerous to deal with briefly.

I raise this issue because it is my belief that the marginalized role to which phenomenology is relegated in psychology is due purely and simply to sociology of knowledge factors rather than to any explicit postulate of phenomenology correctly understood. I hope to demonstrate this fact in the next section, but I want to make explicit here that the biases based on natural science approaches that have crept into the articulation of historical and contemporary mainstream psychology and the biases that keep alternative expressions from gaining a fair hearing are the reasons for what is acceptable knowledge and what type of knowledge is viewed with suspicion. All of these can be accounted for in terms of issues that fall under the label of "sociology of knowledge," i.e., arbitrary or contingent factors defining the "status quo" in less than critically rationally acceptable ways.

The Meaning of Psychology

I have been emphasizing how I experienced myself as a marginal person in the culture, and, over time, I learned to recognize that I was equally marginal in my chosen field. Of course, I did not know this at the beginning. At first, I simply attributed all the differences to my naïveté. Only gradually did I learn that I was consistently different. What were these differences and upon what were they based?

First of all, I expected to have my own vague understanding of psychology clarified. That was one reason that I wanted to study some more. I thought that some 75 years after its foundation, psychology might have advanced somewhat. I did not expect a full answer, but I was expecting some advance over the generation of the founders. I learned that we went from an elemental structuralism to functionalism or to a Gestalt understanding of consciousness, but we still did not know anything more about consciousness per se, except that it was organized and useful when problems were encountered. Then I learned that consciousness was not the proper subject matter, but behavior was, or else the psychological unconscious. Behavior was more adequate as a definition of psychology because it was observable and lent itself to experimentation better and the unconscious was more important as a definition because more concerning what really mattered regarding humans had their origins there. The idea of a person as a rational animal was more skewed toward falsehood than we realized. All of these options were all presented as if they were up for grabs as equally desirable. One chose the appropriate perspective because of predilections, bias, or interests. There were, of course, polemicists for each perspective, but one always got the impression that the perspective mattered more than what was good for the entire field. Finally, later in my career, the fragmentation of psychology received its official blessing from Koch (1969), a colleague whose criticism of psychology I otherwise admired. He declared that, in principle, psychology could not be unified. The factual state of affairs became a principle. There could only be, he asserted, study cells around significant problems and so it did not matter what one called oneself, just so one contributed toward the solution of problems. At best, we could only have the "psychological studies" grouped around problems. But then, why call these concentrated efforts around problems "psychological" studies if there is no unity to psychology and if there is not something unique to our perspective? I was dying to deal with other issues, such as: What is the difference between psyche and consciousness? If the former is the subject matter of our discipline, why was it defined as the latter? Is it really the case that behavior does not participate in consciousness? That is, by defining psychology as the science of behavior, is what is unique to consciousness really left behind? Did anyone really investigate the matter and come up with the demonstration that experimentation and the natural science approach, in general, were the best ways of studying psychological subject matter? Did anyone really demonstrate that the quantitative perspective was the true key for unveiling the secrets of the psyche? Then there was the Boulder model with its "scientist-practitioner" ideal, but was this ideal correctly conceived? What kind of scientist was being created and did the training of that type of scientist really help the clinical practitioner? Or, were all these decisions assumptions or taken-for-granted attitudes that served another role for the acceptance and establishment of psychology as a science and profession?

All of those issues are alive in psychology today. I did not mean to imply that I was precocious in any special way, for I was not. This is simply the type of question that comes to mind to a beginner. They are asked out of ignorance or naiveté. The only difference I can see between a host of peers and myself is that these questions never really died for me. When I tried raising these questions in class – somewhat timidly, I must confess – I was always told that I should be a philosopher, or that they were really philosophical questions and had no place in a course dealing with science. Not only did I never get an answer, there was not even any acknowledgment that the questions might have some value! They were simply dismissible.

Now, why did I, despite constant rebuffs from a horde of others, keep these questions alive for myself? Why did I, and do I, continue to see them not only as important but also as essential to the field of psychology? Here, I think, I must credit my experiential marginality and a basically logical approach to scholarship. I am now describing a link that was merely lived at the time and not at all planned or even thought through. I had come to trust my own experience. I lived in a peculiar "demi-monde" that was between the world of working class Americans also with very set attitudes and values which were not wholly consonant with mine, and an immigrant world clutching at values that had no place in the new environment. On one hand, I aspired to join that "American world," but on the other hand, not at the price of leaving behind entirely the Italian immigrant world. It had been too good to me. I resolved this dilemma by attempting always to choose to do what I believed was right, knowing that each decision would have positive and negative consequences. But how was I to determine what was right? It was difficult and I was not always successful, but I tried to do it on the basis of what "one ought to do" or "should do." It was an implicit distancing from the more immediate emotions and desires tugging at me.

I cannot account for this tenacity – or stubbornness, if you like. Perhaps it was because I trusted whatever I encountered experientially. There must have been something in my upbringing that enabled me to stand alone if I had to, and this is a characteristic trait I still possess. For me to change a deeply held belief, I would need concrete experiential evidence or strong logical persuasion or both of the above. Sheer numbers of persons arguing rhetorically will not do it.

As I write these lines I realize that it is really a matter of discovering what I was already doing. As a child of the only Italian family of immigrants in my neighborhood, as the only boy of my group that went to St. Mary's school (an Italian ethnic school), as a Catholic in Protestant America, as the only one from my neighborhood who went directly to daytime college, let alone graduate school, as the only one of my group who never entered any military service, I was already living a marginal life, only I did not know it. There was a price to be paid for all of these contingencies and I was well aware of the price. I was not aware of any advantages; these emerged slowly and only retrospectively. I suppose that if I had to use one term to describe the ultimate impact of the contingencies of my existence vis-à-vis psychology, I would use Riesman's (1953/1969) term "inner directedness." For some reason I could withstand social pressure to conform and I could accept secondary status (e.g., not being fully acceptable to all my childhood friends) and hold on to what I believed despite objections and other pressures. I shall now try to indicate how this "inner-directedness" affected my view of psychology.

I learned early on that a science first tried to distinguish its subject matter and then based on the clarified understanding of its subject matter or phenomena, it proceeded to devise a method to study it. In my era as a student, psychology as the study of consciousness was still lingering even though psychology understood as the study of behavior was more prevalent. James (1890/1950), of course, defined it as the study of mental phenomena. However, there was a problem with any definition: how was one to delineate it, enclose it, and differentiate it from other phenomena? I know that at first I had a simplistic, realistic sense of such discriminations based on things. It seemed simple to go from thing to "life," but how does one go from "life" to "psyche," or to consciousness or behavior? If I am sitting quietly at an outdoor café watching passersby, how could one know what hidden interminable and unstoppable processes were going on? The question of access, or method, had to be related to what one was looking for, and if this could not be specified, how could the problem of access be resolved?

It was not so much that I had answers to such questions, but I was looking for answers to questions of that type, but what shocked me was that the questions were not even raised and, as I indicated, if the questions were raised, they were dismissed as being too philosophical. Instead, I was exposed to courses on science and scientific method, statistics, anatomy and physiology, testing and test construction, and repeated courses concerning the experimental approach to sensation and perception. The more fundamental questions concerning the very status of the field were not raised.

If I can now speak from the present, I can say that it took me much searching, sorting, sifting, dialoguing, and reading in order to even get an angle on this problem. While psychology has been almost transformed since the early 1950s, there has been no advance with respect to this problem. Indeed, psychology is still under the threat of being fragmented or disappearing or else being absorbed by other disciplines. The so-called "cognitive revolution" in psychology did not change anything since it was driven not by intrinsic psychological concerns but by the same cultural forces that drove psychology uncritically in the direction of the natural sciences. The so-called cognitive revolution was driven as much by the invention of the computer and the possibilities it allegedly offered for understanding "cognitive processes" as by anything else. The neurosciences are also enjoying tremendous prestige today because of technical breakthroughs. But are we going to understand the "psyche" through understanding the brain, or through knowing cognition better? Can the psyche be reduced to "cognitive processes?" If so, all we need is cognitive science and psychology becomes redundant. Or, if neurology or brain studies can truly unearth the secrets of the psyche, then what is left over for psycho-logy? Thus, the so-called advances within psychology are driven by technical factors or advances in other disciplines rather than theoretical insights into the nature of the psyche – or whatever one wishes to call our intrinsic subject matter. We are once again very heavily riding on the coattails of the more established sciences. We have not yet learned that such a strategy will not lead to theoretical clarification.

The above critique has to be properly understood, although it rarely is without further elaboration. My comments are motivated by my concern for the true independence of psychology. I do believe that psychology has – or ought to have – a perspective that is unique and that cannot be provided by any other discipline. However, if we psychologists do not achieve this goal, who is going to do it for us? Once psychology has clarified and theoretically supported its unique stance, then it can easily dialog with other disciplines and other perspectives. With an unclarified stance, it can easily be absorbed by other perspectives. So, the above comments do not mean that I am against interdisciplinary work, only that the possibility of quality interdisciplinary work requires theoretical clarification. Nor do my critical comments mean that only psychology can understand humans. Human beings are so complex that it will require a host of disciplines to understand them properly. I am only arguing that psychology alone can understand the psychological perspective well and if we surrender that possibility, then the psychological perspective will be understood only in a bungled way.

While I do not believe that the problem of the precise definition of psychology has been completely solved yet, I do believe that some advances on the problem have been made and I think that I can give a better articulated response to the question today than anything that I heard in the 1950s. To me, it is still viable to speak of the "psyche" since it does delineate a circumscribed field of study. In my understanding, which is a synthesis of much reading and which is heavily biased by phenomenological philosophers, psychology is the study of subjective individual meanings bestowed on situations in the world by sensory-mobile creatures. I will briefly elaborate that statement, although more extended discussions can be found elsewhere (Giorgi, 1982, 1984, 1986, 1990, 1992a, 1992b, 1993, 1995, 1997, 2000, 2001). If there is something that "psyche" offers above sheer "bios" or life, it is "world," and according to Straus (1966), worlds appear with mobile creatures that have sensory openness to otherness. There are no psyches without bodies, but psyche is not limited to bodies. There are levels of consciousness that belong to psyche and other levels that do not, and aspects of body that are not psychological. What makes the delineation of the psyche so difficult is that it is not purely recipient nor purely active, not purely bodily and yet highly somatic, not enclosed within itself and yet not all relationships with world or others are psychological. What seems to be most characteristic of it is its subjectivity, which is both unique and general. Pragmatic approaches work well in psychology because subjectivity is all about how to get along in the world and with others, and yet, pragmatic approaches do not account for peak experiences, altruism, or highly imaginative behaviors. So what is this distinct coloring that we want to call psyche and how account for it? Indeed a difficult question, but it will never be answered if we cannot determine its scope. It is equally clear that intentionality has to be included as part of psychological subject matter. This means that relationships are central to psychology and that psychology can never be stopped at the skin, even if one is dealing with "inner processes." The intentional object and its horizons have to be included and that usually implies worlds. The difficulty encountered here is that the psychological aspects have to be qualitatively discriminated against a ground that looks very similar, but which contains much more than merely the psychological.

In essence, what is called for here is a new kind of scientific task and that is why the received wisdom does not work so well. Unlike things that stand out clearly or processes whose causes can be easily controlled, we are dealing with processes that are intermingled at base and yet demonstrate a discriminable peculiarity capable of recognition. Thus new modes of discrimination and new modes of understanding are required in order to do justice to the discipline of psychology. At the beginning, more or less known procedures borrowed from other sciences could carry the day for a while because there was no other scientific approach with which to compare the approach supporting those procedures. There were only the reflections and speculations of philosophers regarding psychological subject matter. That is why scholars interested in such a subject matter turned to other scholarly approaches, and as we know, the natural science perspective was chosen. We shall turn to that topic next.

Psychology and Science

That the subject matter of psychology should have been the topic of a specialized science is not surprising. This process had begun with the sciences of nature in the seventeenth century and the advances in knowledge gained by the use of the scientific method certainly justified the attempt to investigate the psyche in a more concrete way. However, the question whether the correct implementation of the understanding of science was used is still open to debate. Or, at least, it has been challenged by a minority of scholars but mainstream psychology simply keeps pursuing the same goals in the same way with only technical changes.

It is understandable that psychology would begin its separation from philosophy by imitating the natural sciences because of the legitimate success of that approach with nature. What is more surprising is that it sticks with same approach despite limited success and constant multiple criticisms of the approach that span its existence (e.g., Bartlett, 1932/1995; Dilthey, 1894/1977; Giorgi, 1970; Husserl, 1911/1965; Koch, 1959; Merleau-Ponty, 1942/1963; Politzer, 1928/1994; Samson, 1981; Smith, Harre, & van Langenhove, 1995; Sullivan, 1984). However, the context of the natural sciences is the reality determiner of our era and it seems that psychology is reluctant to break away from it, despite several reminders that science's success was with nature and its forces rather than with human beings. It is desirable to wish to bring the same degree of precision and knowledge about human affairs as was gained with respect to nature, but psychology made the simplistic assumption that the same strategy that worked with nature would also work with humans. The fact that "natural phenomena" were being replaced with "human phenomena," or that "things and their processes" understood in the light of cause and effect were being replaced by "humans and actions being motivated by meaningful discriminations" seemed to motivate no difference in approach. Of course, philosophical anthropology makes a difference here. There are those who genuinely believe that humans are as natural as electricity and volcanoes, and others, among whom I count myself, believe that with human beings an ordering in addition to a naturalistic one is present. Psychology cannot resolve what 2500 years of western philosophy has not been able to resolve. However, the human scientist who believes that more than a naturalistic perspective is required to understand humans adequately should still be able to conduct research according to his or her own criteria. That is, such a scientist would have to clarify the assumptions and criteria with which he or she worked and then demonstrate how such a framework could produce rigorous knowledge without reductionistic tendencies. All human qualities and characteristics should be able to be understood without distortion. This knowledge-acquiring task has to be done within the context of Science-at-large but not within the set of criteria that the natural sciences have determined to be appropriate for nature. Moreover, the assumptions concerning the qualities and use of knowledge will also have to be different. I cannot go into this fully here, but one example would be the fact that much knowledge concerning nature is acquired for the purposes of control, but, ethically, this could not be a goal imposed upon humans. Rather, the knowledge would have to be shared with others and appeals to “good science” would have to be made. A current example of this strategy would be what is happening with smoking. There can hardly be a person in America who does not know that smoking is harmful, but I think that it would be wrong to impose a universal ban on smoking. One should share all of the reliable knowledge as widely as possible, but then it is up to each individual to decide. In the human realm, such decisions will be inevitable, and this is but one example of how subtle but real changes take place with the goals and use of knowledge with humans.

Now, when I argue like this, colleagues often respond with the view that they believe that the humanities are important and that the approaches used by the humanities could be helpful for understanding the psychology of humans. Obviously, I believe in the importance of the humanities as well and I do not in any way belittle their value. However, the human science perspective for which I am arguing is not the same as that of the humanities, nor is it the same as that of the natural sciences. And again, of course, a human science perspective does exist and has been practiced for years, but not always well. Historically, it is a mixture of natural science, humanities, and sometimes indigenous logic. However, it does not have the status and success that the natural sciences have achieved. My own perspective is a theoretical and logical one and I am afraid that it calls for a fresh attitude from anything that has been practiced thus far. In other words, more has to be done to develop it than has been done thus far. Basically, it would follow the logic of science, but allow for human characteristics to be present in nonreductionistic ways. This would give us knowledge about humans and their relationships in a way that is similar to our knowledge of nature, but specifically different because human phenomena are not reducible to natural phenomena.

Now, back to the personal again for a moment. Why do I persist in believing that another attitude is necessary in order for a psychology of human being to develop into a mature science? I cannot say for sure. I can only say that I see a possibility that is not being actualized. Moreover, it is a possibility that could lead to a better understanding of human beings from a psychological perspective. Of course, this superiority would have to be demonstrated and should not be taken on my word. But the claims are (1) that the approach will be nonreductionistic; (2) a more authentic sense of the psychological will be obtained; and (3) another perspective on science will be developed that will enrich our understanding of science. It is hard to let go of a possibility that potentially offers so much. But, if it does, why do not others flock to it, or at least acknowledge the possibility? I do not know. I just know that the possibility exists. I also know, from many experiences too numerous to mention, that life does not follow logic. Not even science does. Yet, it seems not unreasonable to assume that this possibility can be actualized. I realize though that it is more possibility than actuality and that a big stumbling block is simply the fact that the philosophy of science that would support such a human science is also a mere possibility. That is my next topic.

The Search for a More Adequate Philosophy of Science

The age of faith seemed to have at least one deleterious effect – it seemed to encourage the human mind to go to sleep. Whatever was not preordained by faith could be rationally deduced. One consequence of the Renaissance was that the awakening of the human spirit also awakened the critical faculty we humans have and it seemed that we dared to question in new way. The weakening of faith also implied that we had to try to do different things; we had to experience how things were before we could speak with some degree of authority. Hence, there was a resurgence of the philosophy of empiricism, the idea that things had to be experienced, tried, and encountered before we could speak about them with authority. The idea of trying also implied testing, experimenting, and manipulating things in order to find out the best way that things worked. A key idea within empirical philosophy was that phenomena had to appear to the senses in order for us to acknowledge their existence. And if the phenomenon itself did not appear to the senses, then at least its consequences would have to. Full authority was granted to experience to acknowledge existence or not. Obviously, this philosophy worked well for all phenomena that appeared to the senses. There were, to be sure, some problematic encounters, but there was confidence that the difficult issues would be resolved over time.

In terms of science, we are still living within the framework of this empirical philosophy which is also buttressed by logic. Some version of the logical–empirical approach dominates all aspects of contemporary science. I want to affirm that there is no problem with empiricism when one is dealing with phenomena that are given to the senses through appearance. However, such an approach does not seem to be as comprehensive as required when it comes to accounting for the full range of givens to which humans are capable of being present. I do not mean esoteric phenomena here, but many types of givens that we deal with in everyday life non-problematically, such as memories, ideas, numbers, thoughts, meanings, and so on. There is an old saying in philosophy that goes “Nothing is in the intellect unless it was first in the senses" to which Kant replied *"nisi intellectus ipse"* (unless it is the intellect itself). Given the recent emphasis on cognition, one could say that it is precisely cognitive phenomena that raise this question: Can cognitive phenomena be fully explained by the activity of the brain; or must other factors be taken into account? Can sensory experience fully account for cognitive achievements?

When empirical philosophy works, it is truly strong. The question is whether it can comprehend all that is given or whether other perspectives can also be legitimated. If other perspectives are necessary to understand other kinds of worldly manifestations, then they should be introduced otherwise empiricism ends up dictating to phenomena – that is, telling them what they *ought* to be – rather than simply accepting them as they are. The reason that I turned to phenomenological philosophy as the basis for psychology is that it can deal with any manifestation of a given without being challenged. It is the maximally open philosophy, and yet, it seems to be more misunderstood than understood. The reasons for this difficulty are given in many places and I shall not reiterate them here. I shall try simply to present those aspects of phenomenology that I think are what make it a philosophy for our time as well as a philosophy that is needed in order to develop the human sciences.

I have discussed the meaning and value of phenomenology for psychology numerous times so I will be brief here (Giorgi, 1982, 1986, 1992b, 2006). Phenomenology is the study of consciousness and of all the objects (understood as broadly as possible) that can be given to consciousness according to any modality. In other words, it is not only the study of what is given, but how the given is presented as well and it is equally concerned with possible objects of consciousness as well as those that are actually given. Husserl has done scholarly knowledge a great service when he specified that a key function of consciousness was to make objects present. This presentational function of consciousness he calls intuition, which within phenomenology means being present or becoming present. Then Husserl (1913/1983) made a distinction among types of presence according to the type of object that was given. Husserl called those objects that were real, that is, those that exist in space, time, and causality, as real or empirical objects. That is, experience is the term he applies when we encounter real objects. However, unlike in empiricism, experience does not exhaust the types of objects that can be encountered. Husserl also affirms irreal objects – such as geometric figures, ideas, meanings, numbers, logical propositions, and so on – and he claims that these objects are given to an ideative type of consciousness. Ideative consciousness is not experiential in the strict sense but it is presentational in the sense that it presents us with irreal or ideal objects. Moreover, Husserl points out another difference between objects that are given ideatively and those that are given experientially: the latter are always given in appearances and perspectivally, the former are given directly. The difference, therefore, with empiricist philosophy as the basis of science of persons is that only "real objects" can be accounted for. However, it is not simply a matter of acknowledging "irreal objects" because in my view – not necessarily Husserl's – the object of psychology is not purely irreal either, but rather something more like "quasireal," that is, a mixture of real and irreal objects or in between the two. Husserl refers to this type of object but he does not develop this sector of givenness. But one could not have "quasireal" objects if there was not a field of irreal objects to allow for this possibility. Merleau-Ponty (1942/1963) adds to this notion of the difference that characterizes psychological objects. He has argued that "structures" should have been the guiding concept of psychological science rather than the "atom." It would have been descriptively more accurate and it would have explained experimental data more thoroughly. But he develops this point even further. Merleau-Ponty argues not only that structure would have been a more fruitful concept but also he states that structures are given to researchers essentially perceptually. The key implication of that fact for him is that "structures are opaque to mind" (Merleau-Ponty, 1942/1963, p. 205). Thus, while the mind of the scientist has to conduct science, the primordial data are given perceptually. One has to at least begin descriptively in order to see what the data are.

If we add the implications of irreal objects to the idea of opaque experiential structures one begins to see that the point of departure of psychological investigations could become problematic. That is why Husserl's notion of the phenomenological reduction is so important. The idea of the reduction is for the person entertaining awareness to be sensitive to all presences that constitute the givenness of the object being presented and not to be biased in the direction of real objects as opposed to irreal or to substances as opposed to horizons or to saliences as opposed to fringes or margins. Everything is counted merely as present rather than as existing, for that is what consciousness is truly about. The reason that this is important is that the field of presences is larger than the field of those presences that can claim existence. Empiricism is biased toward the real, toward existences, and it bypasses the true field of psychology, which is the field of presences, of phenomena in the strict sense. However, the phenomenological reduction, by requiring us to step back and not be seduced by existing givens makes it possible for us to unfold the field of presences. We live through the presences normally and stop with the real. A regressive move is required in order to arrive at the genuinely psychological. Basically, I am saying that psychological reality is discovered in the phenomenal realm.

However, even as we do this, another complication sets in. While Husserl distinguished between real and irreal objects he did not develop an approach to "quasi-real" objects, which is, in my view, where psychological objects dwell. An image is a perfect example of such a purely psychological object. Something is given, it has a kind of appearance but the appearances of an image do not lead to the thing-itself. It is a quasiobject. (Husserl calls them "phantoms.") Of course, many aberrations also have these characteristics, for example, hallucinations, dreams, illusions, false memories, and so on. To perceive normally as one usually does in everyday life is really a metapsychological achievement. In addition, to think logically or to perform rationally also requires metapsychological achievements. In such cases, one performs precisely as one ought to and the psychological becomes a temptation to yield to pressures or diversions that ultimately are not yielded to in order to maintain veridical perception or rational performance. The psychological is contributory to healthy processes in getting along in the world, but if it dominates, then things go awry and psychological factors become determinative rather than the worldly situations to which the psychological ought to be directed.

In attempting to delineate the psychological, I realize that one must remain ever sensitive to aspects of the contextual that surround the given because I am convinced that that is what is required in order to be present to the psychological realm in an authentic way. However, Merleau-Ponty (1942/1963) has already expressed this notion when he said that structure was neither thing nor idea, the two concepts that have guided scholarly activity in the west for over 2000 years; the former concept guiding the activity of the natural sciences and the latter directing philosophical reflection. What seems not to have been developed yet are positive guidelines for comprehending structures. But, of course, this problem will never be solved until it is acknowledged to be a theoretical problem worthy of solution.

I shall not articulate anymore about the difficulties in coming to grips with psychological subject matter. I acknowledge that it is a difficult problem but what I hope is recognized is that the solution requires a new approach. One can no longer work in the old tried and true ways. A new way of recognizing psychological phenomena is being called for and new ways of operating with them so that findings and principles equal to those of the natural sciences can be articulated. Of course, "equal to" does not mean "identical to." Allowance must be made for the peculiarities of psychological subject matter.

The Scientist–Practitioner Model

There is only space to indicate how a different approach could help psychology, both as a profession and a science, with some of its ideals. As we know psychology chose, and has continued to affirm, the Boulder model of training, which is meant to produce psychologist trainees as research scientists as well as practitioners who can help with healing various pathologies. It seems that the tensions introduced by this model have never been overcome, even if noticed and occasionally written about. What is the basis of the tensions?

The basic tension is due to the fact that psychology interprets scientific research as following the model of the natural sciences whereas human psychology should be following a human scientific approach. With respect to therapeutic praxis a concession toward human scientific principles has already been made with practices in the sense that most "talking therapies" do not reduce the human being and respect the dignity of the client. What is required here are research principles that are equal to the therapeutic principles, but theoretically, this cannot be done within a natural scientific framework. In other words, therapeutic practices are closer to what is necessary to be done than research practices are. What needs to be radicalized along human science lines are principles of human scientific research. This means qualitative research motivated to seek psychological meanings in a rigorous way –but rigor defined by research practices and principles that are different from natural scientific ones. Once the research principles fall within the human science framework, the tensions between research and therapy disappear. Of course, differences remain because research and therapy have different interests and goals, but they can be related harmoniously. The reading of research reports by therapists no longer becomes an alienating experience. What is discovered becomes relevant for therapeutic praxis. It seems that what was never appreciated by psychologists was that the experiment was never meant to be the model for psychological research. Its structure does not lend itself to clarification of psychological reality. It is more diversionary than essential. New ways of doing psychological research will have to be found.

The reader will perhaps have noticed that each of the points I have discussed ended up with the notion that new ways will have to be discovered by psychology in order for it to truly progress. Psychology will have to be defined anew, which will require a new understanding of science which in turn will need a new philosophy of science, and all of these new dimensions will create an authentic psychology, which then, will be able to solve many older, persistent problems such as the scientist–practitioner dichotomy.

The emphasis on newness above may seem strange for our age, but it seems to me that the natural sciences have no trouble extending the boundaries of science in overturning established ideas. But then the fit between the natural scientific method and the phenomena of nature is as it should be. Psychology does not progress because the fit is not so good. Wettersten (1975) has demonstrated that the history of psychology is unlike the history of the natural sciences. We fill our pages with names that formulate theories but not with approaches that solve significant problems.

The point that I want to make, is that perhaps it is not strange that the argument for something new to be done within psychology should come from the child of emigrants thrust into a new land where the so-called "native dwellers" behave and think differently from his home environment and these differences call forth a stance whereby what is taken-for-granted by the natives is called into question because of the conflicting perspectives thrust on the individual by his total environment. Not being completely comfortable in his native land, he is equally uncomfortable in his chosen field. After all, the ideas about his chosen field sprung from his conflicted ground, and perhaps, just as he had to forge a unity in his life, he attempts to forge a unity in his discipline.

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Psychology in Self-Presentations
"The Life of a Maverick"

C.F. Graumann

Autobiography as a Self-Presentation and Verbal Communication

For a social scientist – an affiliation by which, as a social psychologist, I set great store – to write an autobiography is to attempt two inseparable things: on the one hand presenting oneself and on the other hand communicating with others. Even though I emphasize these two aspects as one and the same activity, I am referring to two

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research fields of language and social psychology. Before I write down what I remember about my development or personal history, I want to make sure which track I am picking up or, less metaphorically, what kind of text I am to produce. You do not just sit down and write about your life or yourself out of the blue. As a rule, you either feel that the course of your life, mainly your career, is interesting enough or even exemplary for others, or else you believe that members of your scientific community are convinced that your vita is essential information beyond your publications.

Nor is it, for a social scientist and in particular for a social psychologist, possible to respond to the collegial request to compose a "Self-Presentation" without being mindful of the theoretical baggage that the term "Self-Presentation" (in German "Selbstdarstellung" often translated as "Presentation of the Self") and its history carry. The term itself is equivocal. It can be used simply to indicate that the presentation has been written by the individual him or herself rather than by someone else, the German "Selbstdarstellung" thus functioning simply as the equivalent of the "autobiography," which in German can also be replaced by the term "self-biography." But a sociologist or (social) psychologist might also think of composing a presentation of one's own "self," since those specialists are accustomed to working with a multiplicity of significantly diverse concepts and theories of the self. They work with a range of competing constructs and theories of the self, self-awareness, self-categorization, self-concept, personal and social identity, self-monitoring, or even of impression-management involved in the psychologically appropriate rendition of what the literary term "autobiography" implies.

The history of the term "Selbstdarstellung" or "self-presentation" is in one sense relatively short. It does not emerge as a scientific construct until 1959, with Erving Goffman's *The Presentation of Self in Everyday Life*, and the work that Goffman and his psychological followers then went on to do in further defining and refining of his concept of the dramaturgical approach formed the bases for the theories and theorettes mentioned above (Goffman, 1959). To these theories, there is a common denominator that has led today's psychologists to use the terms "self-presentation" and "impression management" more or less as synonyms, and it resides in the notion of the consciously willed control of the impression that a person wishes his or her "self-presentation" to have on intended or supposed recipients, whereby "self-presentation" comes to mean something that by intention is unequivocally and unilaterally more than "mere" autobiography. It is unequivocally more than autobiography if one considers that Goffman linked his concept of the everyday "self-presentation" very closely to the model of the theatre, whose actors, those doing the presentation, offer a "performance" as they enact their particular "role," playing it out in the context of an "ensemble" and thus in principle always for a "public," as if guided by an invisible director and on a stage divided into separate "front stage" and "back stage" areas and involving the "props" that the role requires.

What I found to be an important aspect of this structural model, as I had occasion to clarify it to my students in a lecture I was holding on the stage of the Old Audimax at the University of Heidelberg, was the fact that I myself – as the individual C. F. Graumann – was a clearly necessary but not in itself sufficient component of my entire performance, which, in order to be a success, also required the alert presence and attentiveness of the others involved in that presentation, whose behavior, even though their role was reduced to that of "listeners," also, in addition to the summer-like midday weather, had an influence on my mode of presentation.

So even here I am inclined to make a generalization: I can hardly imagine the autobiography of a university teacher as the presentation of a lone, solitary self (although I have read such autobiographies). Rather, this autobiography must reflect the fact that the written text now emerging is only part of an ongoing process of communication, thus part of a dialogue. On this point, as I shall soon make clear, I have learned something essential from those who have replaced the monologue-centered mode of thought that is still so widespread and tenaciously prevalent especially in psychology with a more dialogue and communication-oriented approach – thus from the likes of Ken and Mary Gergen (1988), who have developed a diachronic-relational concept of the self manifest in narrative form. Accordingly, my "self-presentation" as a university teacher is based primarily on an identity that, while constituted in communication with others and owing much to "significant" others, is nevertheless, as initiator and enactor of my behavior, incontrovertibly I myself. The communicative character of such a "self-presentation" includes that self mirrored by others – as Weimer and Galliker (2003) pointed out a good 30 times in their Festschrift in my honor (*Sprachliche Kommunikation*), with its collection of perspectives and recollections by my colleagues and students – and so it is under these premises that I attempt to reconstruct from my autobiographical memory what is characteristic of my life as a psychologist and university teacher, even though much of that is atypical. This involves some comment on my background, a brief account of how I came to specialize in psychology, and some highlights of my scientific progress as a psychologist with reference to some basic themes and leitmotifs.

My Background

When for the segment that other autobiographies often term "Childhood," I choose the title "Background," I do so for the following reasons. I was born in 1923, the year in which the postwar inflation reached its highpoint, in Cologne, which was part of the still-occupied Rheinland, the son of a father who was born in Kalmar, in Sweden, and who, a year after my birth, died of a stroke – thus a man whom I never came to know. My mother was born in Brooklyn, New York. She was the second wife of my father, who was born in 1856. Since she did not remarry, I grew up without a father, without brothers or sisters. My mother died just short of her one-hundredth birthday, outliving my father by 72 years. My father left me, aside from those pictures from which he gazed at me with visage more stern than benign, a trilingual library, his loyal dachshund – who, as a true hunting dog should, soon followed his master – and above all the all-powerful image of a kind and loving man that my mother had created and then repeatedly attempted, in all of life's situations, to pass on to her son. And so I did not grow up "fatherless," but rather with a strong father image, albeit reflected through a rather tender and delicate woman, and thus I matured without having to confront a man of indubitably strong character. Nevertheless, my mother confronted me quite early with decisions for which a father would have served as the proper authority, and so I came to follow in that generations-old Sauerland line of Graumanns tried and true as iron-workers and engineers. Alhtough my mother saw to it as best she could that I was aware of this side of my lineage, her own side came right out of the blue, when I was 10-years old and the Nazis came to power. My mother's line was declared to be non-Arian. My grandmother in Bruchsal, to whom I was very attached as a result of her delicious cakes, cookies, and spaetzle, proved to be Jewish, a fact that neighbors and acquaintances caught onto more quickly than she did herself, since she was convinced that she and her family were proper protestants and, as long as she lived (until 1937), could not make any sense of her new "racial" classification, a fact that, of course, did nothing to spare her or her "non-Arian" children the discrimination and humiliation that intensified, above all after the instigation of the 1935 "Nürnberg Laws."

With that I myself experienced for the second time, now as a "second degree racial half-breed or half-caste" – or in common parlance "quarter-Jew" – what it meant to be consigned to a minority. As innocuously as my fate, compared with that of others, unfolded, the whole experience sharpened in me a sense for social discrimination that was to interest me much later as a social-psychologist.

In order here is a curious reminiscence about my first experience in social categorization: When I reached school age, I attended a Catholic elementary school, since it was close by in our neighborhood, whereas the nearest Protestant school was far away. Our teacher, strict and straight in her comportment as her hair was parted, was a "miss" M, who would hand out "good behavior cards" to those who gave good answers and showed that they had been paying attention, and ten of these could then be exchanged for a colored "holy picture" of a religious figure. Now it turned out that, for reasons that I can no longer clearly recall, as a 6-year-old protestant, I thought these "holy pictures" were "heathen pictures" – likely because of the proximity of German "heilig" (holy) and "heiden" (heathen); after all, we school children were also collecting tinfoil to help the "heathen children" in far off colonies. For this reason, I did not exchange my "good behavior cards" for the "holy pictures," and instead I hoarded them until I had exhausted "Miss M's" entire supply. Since she very quickly established where her cards had ended up and also why the only protestant in the class could have been the culprit, she resorted to a forced exchange that abruptly worsened our relationship.

Compared with that, the later forced reassignment to a "racial" minority was less amusing. One experience from my military days can serve as an example. In fact, in my later school days at the Kreuzgasse school, a renowned academic high school in Cologne (where only the phys-ed and music teachers wore their Nazi party badges in class), I was never confronted about my background. As a schoolboy of age 10–14, I was able to get out of the "German Youth" (the "Jungvolk," a subdivision of the "Hitlerjugend," the Hitler Youth), and in the Hitler Youth, which in the meantime had become obligatory, I was involved in the radio acting and entertaining company of the Cologne radio station, singing in the choir. One highpoint of these activities I recall clearly: with a voice that had gradually landed down in the bass range I sang in a huge chorus performing the "Ode to Joy" for the broadcast of a much rehearsed rendition of Beethoven's Ninth Symphony. Besides that, I was much welcomed in the Hitler Youth acting and entertaining company (the "Spielschar"), as well as later in the army, as an accordion player. But when, after completing my military basic training, I had unexpectedly been transferred to Münster to train as a radio specialist at the officers' candidate school and then, after four days, had been pulled back out, it was explained to me in dry official terms that "my papers" had arrived, according to which that line of training – for which in any case I had neither applied nor volunteered – was out of the question. My background had caught up with me once again.

The alternative which I found intellectually challenging and that I seized at the earliest opportunity was work in long-range intelligence reconnaissance, an area in which, by 1941, I had been trained as a radio monitor. When, at the end of December 1941 and after a secret Christmas leave in Cologne, I left Germany with marching orders for North Africa, and I had no way of knowing that I would not see my hometown again for five-and-a-half years – thus not until the war had been over for two years – as a Colonia deleta. The sight of that field of rubble, above all those onetime architectural treasures, those Romanesque churches left standing amidst the ruins was disorienting and devastating – for after all, in art class we had labored with brush and ink, with charcoal and chalk, to engrave many of them indelibly in our hearts and in our memories – a consolation after so many years behind barbed wire. This returning soldier's only consolation was that mother and family, for all the repeated bombing out and evacuating and harassment they had undergone along the way, had at least emerged healthy and intact. Yet the most valuable part of my paternal inheritance, my father's library, had fallen victim to British fire-bombers and looting fellow-citizens. The basic set of personal effects with which I was to begin the next segment of my life consisted of a couch, a duffel bag full of books that I had brought back from Canada (psychology and a lot of Nietzsche), and the standard German flute with which I had filled in as second flautist in our POW symphony orchestra. But what I brought back above all was the resolve to study psychology. And this resolve has a history of its own.

My Path to Psychology

The "Kreuzgasse" academy had been my school from 1933 to 1941, and in fact in the best sense of the word. Competent teachers there laid the humanistic and scientific foundations of my later studies, and in their moral courage, too, they were an example for what I learned much later from my long-time friend Serge Moscovici about the power that a minority can have against an oppressive majority (cf. Moscovici, 1976).

Later I was never the adherent of a "school," nor was I ever the disciple or follower of any mentor, however important. I studied with some and I learned from many, and I continue to do so even today. Often those from whom I learn are much younger than I, and to follow up on that point I am not at all convinced that the teacher–pupil or mentor–follower relationship is conducive to the acquisition of scientific knowledge. But of course the road I took to psychology and scientific work contributed to this doubt, and it began years before my first semester at university.

As a member of the Africa Corps in 1942, I was taken as prisoner, and by the winter of 1942–1943 I found myself in a POW camp in Canada. Here I began my lasting acquaintance and engagement with psychology, my joy, and my suffering in pursuit of it, and this occurred in two ways or, more accurately, on two – only at the outset distinct – paths:

1. We POWs – among us many young men who had completed their academic requirements for university, some students, and even the occasional university instructor – formed a variety of work-and-discussion groups, one of which in particular I found very interesting. We called it "Dogs and Power." On the daily rounds we made within the barbed-wire perimeters of the camp, we had observed how some of the soldiers – sergeants and corporals most prominent among them – kept dogs that they trained (or as we put it, using the term for the basic training of recruits, "drilled"). In our work group, we developed theories about what they were doing, and we speculated theoretically about the function of the dogs (as substitutes for actual human recruits), the motivation of the dog owners, the mode of training, and so on. In this way, our occasional amused observations evolved into a systematized ongoing monitoring, which in turn led to objections about our observing and against us as the observers. We soon became known and accordingly stigmatized as "the psychologists." But what we were lacking was a system for our activities, which the second of my two paths to psychology at this time also failed to provide.
2. A "European Student Relief Fund" offered us POWs in Canada the opportunity to take correspondence courses at a Canadian university, and for us it was the University of Saskatchewan. My choice, in addition to American literature, was psychology. Back then, in the 1940s and 1950s in America, psychology was predominantly behaviorist, and even today when I see the old textbooks, certified with the censor's stamp, I am reminded of how, for example, the instruction in methodology began, as well as with descriptive statistics, with the labyrinth and detour methods – but not with behavior observation. From Eugene O'Neill, whose "negro drama", *The Emperor Jones*, I had to analyze for my first assignment in American lit., I learned more psychology than I did in the psych course I was taking at the same time. Nevertheless, for my later acquaintance with German psychology, it was important for me to learn two things early on:

   (a) First, there already existed a sophisticated and complex behavior-science psychology that was already in its third (or "molar") stage of development.
   (b) That this behavior-science psychology was competing with other schools of thought, the psychoanalytical and Gestalt-theory ones of which had European roots, whose surviving representatives had fled to America, escaping the National Socialist system that I had fought as a soldier to defend in

North Africa, becoming in the process a prisoner of war now interned in North America, where I was encountering a German psychology that, in my later studies in postwar Germany, most of my academic teachers would have denied me, had I not encountered psychologists like Hans Thomae. For with the desiderata that I brought with me from my psychological beginnings in Canada, I found resonance with Hans Thomae.

1. On the one hand, with my need for a systematic method of observing and evaluating behavior, I encountered the brief, 1954 monograph by Thomae on “The Observation and Evaluation of Children and Adolescents” (Thomae, 1954).
2. On the other hand, I returned with my need, awakened in America, to see psychology in terms of a critical comparison of its various schools (rather than from the perspective of a “dominant” school, be it Gestalt theory, wholeness theory (“Ganzheitstheorie”) of strata theory (“Schichtentheorie”). For even in 1940, I had excerpted what Woodworth had written in one of the textbooks I used: “The active members of any school are animated largely by motives of personal loyalty to one another and of rivalry towards other schools. These social (rather than scientific) motives lead them to push their own ideas to the limit and to belittle the ideas of other schools” (Woodworth, 1940, p. 592).

Thus it was not from the “mandarins,” the established full professors of German postwar psychology, but more likely from young psychologists like Hans Thomae (whose new stance had earned him the enmity of those revered scholars) that one could find an advocacy of theoretical and methodological pluralism.

Of course, to follow such a trend was not easy for an aspiring young adept in psychology – but it was possible. Even on the occasion of my 18th birthday a friendly colleague reminded me of the astonishment among my cohort of scholars that occurred when I was appointed, in 1963, as full professor of psychology, since, belonging neither to a prominent school of psychology nor to a Teap circle (“Teap” standing for “Tagung experimentell arbeitender Pyschologen” (“Conference of Experimentally Working Psychologists”), I was not really a contender for a professorship.

My Life as a Psychologist: Its Main Steps and Stations

After my repatriation in 1947, I was able to take up my study of psychology in Cologne and, after only a few semesters, begin to develop ideas for a dissertation topic. This was made possible by two factors: on the one hand, by my participation in a seminar in cognitive psychology, on the other hand, by the fact that in Cologne there was still no diploma examination. There were several reasons for my remaining in Cologne until I completed my degree in 1952. One was financial: in Cologne I was able to live at home and, thanks to a scholarship, not pay tuition. Another was the fact that, by enrolling in Cologne, I was also automatically, so to speak, comatriculated at the University of Bonn, where there was an extensive psychology program. The third reason was that I was interested in combining psychology with phenomenology, an area that was very well represented in Cologne by Professors Volkmann-Schluck and Landgrebe. Important for me as well was guest professor Aaron Gurwitsch, a Husserl scholar, who had worked from a phenomenological perspective on a topic in Gestalt theory. In addition, at an intersession course I became acquainted with Hannah Roser, a philosophy student, who had come to Cologne from Kiel. Our acquaintance grew into love. Hannah remained in Cologne, and we married in 1949. In 1954, two years after I had completed my doctorate, she gave birth to our first son, Götz, who was followed in 1956 by Boris and then by our daughter Annerose in 1961. When our daughter was 15 my wife died of a disease then considered incurable. In 1982, I married my long-time coworker, Lenelis Kruse, who did her doctorate and habilitation in psychology in Heidelberg and from1985 to 2007 held a chair at the Distance University of Hagen in ecological (environmental) psychology.

Even as a doctoral candidate I had, through my involvement with the International Student Association, established contact with students at the University of Utrecht and in particular with those who were studying general psychology under Professor F.J.J. Buytendijk with a distinctly phenomenological orientation. I became acquainted above all with Hans Linschoten as a highly gifted scientist. The two of us soon discovered common interests, and we resolved to undertake a project that would develop a phenomenological psychology derived from the work of James, Husserl, and Merleau-Ponty, to which then, once we had found in de Gruyter a willing publishing house and in Heinz Wenzel a committed publisher, we contributed and dedicated our habilitation monographs as the inaugural offerings in a series of monographs in that area. At this time, Duquesne University in Pittsburgh was developing a phenomenological program in philosophy, sociology, and psychology, and, since there were no North American specialists in phenomeno-logy, Duquesne was turning to European guest professors. In 1961, Linschoten was the first psychologist to participate, and I followed a year later. In addition to my first encounter with teaching graduates and undergraduates, I had at this time the good fortune to become more closely acquainted with the emigrant scholars Kurt Goldstein and Erwin Strauss, whose works I knew from the critical discourse. When I visited Aron Gurwitsch in New York and he was giving me a tour of the "New School" and introducing me to the older members of the "University in Exile," I had no idea that its graduate faculty was one day to become my academic home for a year.

Although Linschoten, who had succeeded Buytendijk in Utrecht, died of a heart attack in 1964, it was possible to continue as editor of the series that I had begun with him (under the title "Phänomenologische Forschungen," 'Research in Phenomenology') – and to date has produced 19 titles in all – with the help of Alexandre Métraux and then Max Herzog (under the title "Perspektiven der Humanwissenschaften"). The original intention of developing a psychology that, rather than being tied either to the natural science paradigm or to the tradition of the humanities, defined itself as a "human science" is reflected in the title of the series: "Perspectives on the Human Sciences" (published until 1996).

With my being offered a chair in psychology in Heidelberg as Johannes Rudert's successor – a development that began during my stay in the US – I was in a position to develop a modern psychological institute (The Institute for Psychology, University of Heidelberg). One blatant example of the challenge we faced in beginning with the absolute basics is evident from the fact that in 1963, the Heidelberg University Library did not list a single foreign journal – with the exception of *Psychological Abstracts*. To me that was the same as if Heidelberg University had a telephone book but no telephone connection. It was clear that a large portion of the professional allowance that was part of my appointment had to be used to buy primarily American professional literature (whereas there were tea sets aplenty to go around). Thanks to the funding provided by the Stuttgart Ministry of Culture, I was soon able to create a research and teaching program that I worked with colleagues and coworkers to develop until I retired in 1991. Invitations and offers from other universities (from Munich in 1967, Bielefeld in 1973), I always had good reason to reject, even in turbulent times, with the exception of the offer, in 1972/73, of the Theodor-Heuss-Chair at the New School for Social Research, about which I shall comment here before going on to enlarge upon the research topics that my colleagues and I pursued during my Heidelberg years.

Even though it was only a one-year guest professorship, I found my experience as Theodor Heuss Professor in the Graduate Faculty of the New School to be both intellectually and socially a very significant station of my professional life. With my interest in phenomenology in philosophy and the human sciences, I had become well acquainted with this faculty as the "University in Exile," as the first haven of German emigrants whose founding generation I was to come to know personally, in part in Europe – including, in addition to Aron Gurwitsch, Alfred Schütz, Hannah Arendt, Karl Löwith, and Hans Jonas. As someone from Heidelberg, I wanted to become acquainted with "Little Heidelberg on Twelfth Street" – that was what they called the university, which, at the urging of Thomas Mann, had taken as its motto the phrase "To the Living Spirit" ("Dem lebendigen Geist"), which had been the motto of Heidelberg University in Germany before the Nazis changed it to "To the German Spirit." And as a psychologist, I wanted to become acquainted with the Department of Psychology founded by Max Wertheimer, where in the 1970s Mary Henle still carried on the Gestalt-theory tradition, while the long-time champion of "cognitive dissonance," Leon Festinger, had already turned his back on socio-psychological research (and later even on psychology in general). For me personally, Festinger (we lived in the same house on Twelfth Street) remained a good partner and friend, even into the 1980s in the study group in Bad Homburg on "historical change in social psychology" founded together with our friend Serge Moscovici.

Today when I look back at my time at the New School, I remember how I was a part of its still-living spirit, which, nurtured as it surely was by its living founders and embracing all departments, had created an atmosphere of scholarliness, humanity, and even political engagement that transcended what I had come to know at the German university. One pleasant memory of those times, too: Lenelis Kruse, then a Research Associate for Environmental Psychology working with Harold Proshansky and Bill Ittelson at the Graduate Center of the City University of New York, was an integral member of the group of New School colleagues.

My Life as a Psychologist as Reflected in its Dominant Research Fields and Interests

A look back on what one has achieved or tried to achieve scientifically reveals some topics that strike more as *major efforts* than as completed *achievements*. With these terms I try to embrace both successes and failures, since both are part of what makes up, and is rightly called, one's vita. Although I try to observe a certain chronological order in the presentation of my major research topics, fluent transitions have been the rule, with new themes emerging as aspects of preceding topics and gradually achieving an independence of their own.

Perspectivity in Cognition and Communication

Not until I attempted to write an autobiographical text did I discover that the major themes I have worked on in the course of my professional life are ones that I have carried with me over that entire span of years – developing them along the way, of course. Thus I would like to emphasize that over 50 years of my "ontogenesis" coincide with the "microgenesis" of those themes dearest to my heart. These major themes, which have become the leitmotifs of my research – in the sense of being dominant and ever evolving, ever branching heuristics of my work – include the following: (a) the phenomenologically based perspectivity in cognition and communication, (b) the role that language has in psychology, (c) the ecology of human experience and behavior, and (d) a theme that also pervades all the others, the historicity of human experience, and psychological cognition. Upon these themes, I orient my autobiographical retrospective, admitting as I do so that in some instances I have discovered and reconstructed the early stages of their microgenesis only in retrospect. The fact that my 1952 Cologne dissertation on the criteria of the "aha experience" (of the "Einfallerleben," a term describing an emotional response occurring at a moment of sudden insight following a period of problem solving) contains in itself the seeds of several later themes I now see as the result of the fact that the phenomenon of the "aha experience" itself is so difficult to grasp and that at that time I had neither a relevant theory nor even a half-way reliable method to turn to. The audacity of undertaking, in spite of this, a "theoretical and experimental analysis" of the problem for a dissertation surprises me more now than it did my dissertation committee 50 years ago.

The Inevitability of Perspective and Horizon

In my dissertation, the concept of "perspectivity" is still used in the restricted sense of what I then termed a limitation of the cognitive process by the viewpoint that dominates it (p. 122), which some of my test subjects suffered to the extreme of a rigid "fixation of a viewpoint" ("Gesichtspunktverhaftung", p. 174, a concept leaning on Wolfgang Köhler's and Karl Duncker's conception of "functional fixedness"). It amounted to an extensive rigidification of the movement of thought in problem-solving processes, which I would describe in terms of the subject's experience as falling between (solution-retarding) fixedness of perspective and (solution-fostering) flexibility or mobility of perspective – thus in terms of the positive and negative power of viewpoints. In the ensuing years, I was to focus primarily on the egologically oriented phenomenological study of the viewpoint-aspect-horizon structure (Graumann, 1960). But through the experience protocols of my test subjects, I encountered the dynamics and interactions of diverging perspectives, which I later found very helpful for the understanding of interpersonal perspectivity, that is: the give and take, the *setting and taking* of perspectives in social interaction and particularly in verbal communication (Graumann, 1972a, 1989a–c, 1990a). The dynamics and reciprocity of "perspective setting and taking" strengthened the doubts I harbored about the individualism rooted so deeply (and not just with respect to methodology) in social psychology and made me aware of "the psychologist's unspoken aversion to interaction" (1979), which contributed in the 1970s to the schism between social and psychological social-psychology and, in my view –although there was a stimulating debate on this topic (1988) - strengthened the new cognitivism. Productive, however, was the fact that this aversion led to a closer cooperation with other – and by no means so averse – linguists who were interested in perspectivation.

Serious consideration of the focus on the "other," not only for understanding language but also for producing language is, in any case, an essential theme for my collaboration with Theo Herrmann (cf. Graumann & Herrmann, 1984). It emerged in our reception of Karl Buehler's axiomatics and took shape above all in the research group that we established (since 1986) and the resulting Special Research Group 245 (funded by the German Science Foundation 1989–1996) on "Language and Situation" (Graumann & Herrmann, 1989a). This collaboration was based on our shared view that a bias of reception in the psychology of language has given rise to the obvious primacy of "perspective taking" and the resulting neglect of "perspective setting." The perspectivist structure and dynamic in dialogue also became the theme of a study group that I conducted with Ivana Markovà and Klaus Foppa (1988–1993) at the Werner-Reimer Foundation (cf. 1995). Basically, it is only the insight that perspectivity implies prospectivity as a temporal dimension that guaranties that this area of research will remain one of my leitmotifs.

At some point between completing my dissertation and my work on my "habilitation" project - my postdoctoral qualification monograph – I came to see clearly – in part through the collaboration with the Utrecht psychologist F. J. J. Buytendijk, in our work on Merleau Ponty and French phenomenology – that perspectival cognition corresponds in principle to the fundamental situatedness of human existence as bodily or physically conceived subjects. Whenever we speak of an individual or person, we are thinking in terms of person-in-situation. In understanding the human being as a situated being we are not only fulfilling a methodological postulate of phenomenological thinking, but also preventing the many pseudo-problems that arise as a result of the decontextualizations (of consciousness, behaviour, feeling, cognition, etc.) that are so common in psychology (Graumann, 2000).

Language

Language, both as a problem and as a means of psychological cognition, played a significant role even in my dissertation. On the one hand, there was no scientific construct and no theory of the "aha experience" that I could take seriously and engage with in my approach. On the other hand, the lexicon for matters of intuition (in the languages accessible to me), while extensive, consistently exhibited one basic pattern. Namely, although almost all of the verbs used for cognitive activity are formed with a personal subject and follow the *ego cogito* pattern, the intuitive thinking is usually expressed in the impersonal form following the pattern of *aliquid mihi incidet* (*occurrit*). These grammatical and other linguistic differences incited me to use language as a guide for developing a "preliminary concept" of the "aha experience" using the factors of *subitum*, experience, impersonality, and a-volitionality. When I approached other psychological problems in a similar way, developing a solution by starting with colloquial language, I was doing so in the conviction – one shared with phenomenologists like Binswanger and Schütz – that our human experience was and always has been a linguistically explainable and communicable phenomenon. Whether our interests as psychologists are focused on consciousness, experience, behavior, or on emotions or cognitions, we cannot jump over the shadow of language. And even though the language used to characterize psychological matters might be fraught with analogies, it is still the only language we have. We need only remain aware that, with our choice of a category, we are determining the perspective on the phenomenon we are considering.

Staying focused mainly on a descriptive approach, I attempted to explain and describe the occurrence of the "aha experience" in terms of the interplay between the fixedness of viewpoint that hinders the "aha moment" of insight and intuition and the unfixedness that facilitates the "aha experience." The high degree of fixedness resulting from a dominant monoperspective later led me to develop the hypothesis that the capacity to change perspectives or to achieve multiperspectivity is a prerequisite both for productive thinking and (where interpersonal connections are concerned) for tolerance (Graumann, 1996).

While in my work on the "aha experience," I turned to language as a guiding theme because of a *dearth* of theory on the topic, I made the same turn on a later project – the one dealing with qualities – because of an *overabundance* of theories. When Hans Thomae asked me whether I would write the encyclopedia article on personality traits for his *Handbuch der Psychologie*, my first reaction was: I do not care at all about the study of traits. Thomae responded that that was the best qualification for writing a critical contribution on the topic. The characterizing of traits is both an everyday practice and one for which entire theoretical models have been developed, and I set out to study the use of adjectives and the development of concepts of personality traits. By pursuing the differentiation between a verbal, an adverbially trait description, and an adjectival and substantive mode of "assigning traits," I was able to reconstruct both the theoretically meaningful difference of qualities as (predominantly descriptive) "consistent modes of behavior" and as (predominantly interpretive) "consistent personality aspects" and also the methodologically important step from the "intervening variable" to the "hypothetical construct" and to go on to clarify the resulting knowledge-claims. Yet the fact that this contribution became for years part of the recommended reading list for doctoral candidates can only be explained by the competition, still relevant in the 1960s, between charactero-logical and personality-theory concepts of traits (cf. Graumann, 1960).

In the years that followed, language became more and more important to me as both a means and a problem for the psychologist. This is evident in the way my teaching of general psychology came to include language and in my efforts to pursue and encourage psycholinguistic research at the end of the 1960s. It was also evident in my organization of a psycholinguistics symposium at Lehen Castle in the summer of 1969, attended by representative colleagues in the field, and also in the creation of a psycholinguistics department with the help of Hannelore Grimm, who completed the first doctoral thesis in that field at Heidelberg, and of Margret Wintermantel, who was later to make such essential contributions to the creation of the Heidelberg-Mannheim research group in "Speaking and Verbal Understanding in the Social Context" and the special research area "Language in the Social Context" (Special Research Group 245) that grew out of that initial group.

The differentiation between an adjectival and a substantive mode of designating traits became significant when we began to use language for a closer investigation of "linguistic discrimination" – thus social discrimination. To designate someone as "lazy" or "gluttonous" or to characterize that person as a "lazybones" or a "greedy guts" are socially and psychologically two different things, as is generally the case with the generalization of a trait (or property) into a type, that amounts to assigning the person to a stereotype. When in 1989 I collaborated with Margret Wintermantel to present a "functional approach" for the analysis of discriminatory discourse, we were making a first attempt to determine the social function of cognitive-emotional processes and their linguistic manifestation. In doing this, we differentiated a separating, a distancing function that accentuated difference – a pejorative and a fixing function – from the categorical ignoring of others. We carried out subsequent examinations of these terminological differentiations in this special research program project on "Discriminatory Discourse" with the help of facet theory ("Facettentheorie"), whereby only the intuitively evident differentiation between separation and distancing proved to be unreproducible (Graumann, 1994). However, the actual empirical problems emerge when we deal with implicit (inferential) – that is, only the more grammatically and lexically identifiable and highly context-dependent – forms of discrimination (Graumann, 1995). A means of insinuating discrimination that has yet to be subject to extensive systematic investigation is (the elimination of) ambiguity. A woman participating in one seminar brought an advertisement from a travel agency that read: "Why don't you let your wife decide where you'll go on your next vacation?" The fact that this text is (subtly) sexist was not immediately clear to everyone; some of the male participants thought it was emancipatory. They had not noticed the use of the verb "let/allow" ("lassen"). Yet our experience has shown in general that subtle racism or sexism does not have to be consciously registered by either the "sender" or the "receiver" to have an effect.

The Ecological Perspective

By introducing the ecological perspective in 1974 we were, with a liberal adaptation of Ernst Haeckel's ideas (1866), directing the attention of psychologists toward the interaction of living beings and their environment. Despite all other historical and methodological differences, this approach converges with the phenomenological focus on the situatedness of the subject. In spite of the traditional preference of the human sciences to limit the situation to the social milieu, it is simply a fact that we live in a world of material things that, with or without the active involvement of others, affect our experience and behavior in manifold ways. Yet although psychology long ago became a behavioral science and although there is hardly any behavior that is not related to things, the material world plays no role in this science; it is, as Linde said of sociology in 1972, "sachblind", blind for material reality. Proceeding from this neglect of the material world, I set out in the early 1970s to work out theoretically (cf. Graumann, 1974) – and also later in research projects with various coworkers (starting with Lenelis Kruse and Gerd Schneider) – psychology's formulation of the "ecological question in psychology" (1975), which then, with L. Kruse and E. D. Lantermann, resulted in a 1990 handbook on ecological psychology. The main themes of this work were the conceptions of appropriation of space, of the "life space" (1995) and of space in general (2003), and of nature (2004). The empirical focal point of this study was an investigation, carried out with Gert Schneider in several German cities and three French cities or neighbourhoods, of place identity – a topic that we had originally developed as a result of our collaboration with the City University of New York (Graumann/Kruse, 1993; Schneider, 1992). This involved a mix of quantitative (and in part experimental) and qualitative (phenomenological) methods – including a "linguistically based content-analyzing system for understanding the referential and predicative meaning of verbal data" that was specially developed by my coworkers Werner Kany and Gerd Schneider. This project involved an ambitiously complex and demanding mix of activities and approaches. But it not only enabled the relatively free "self-presentation" of the individuals surveyed but also made it possible for us to test and refute traditional assumptions – for example, the preference for a cognitive representation and the impression of an anomia on the part of urban residents.

We had resolved back in the early 1970s not to let the ecological perspective turn into merely another "hyphenated-psychology" (cf. Graumann, 1978) and we tried to uphold that resolve by applying the ecological perspective in a variety of areas of general psychology (for example that of memory, cf. Graumann, 1997) and of age research (children, the elderly, cf. 1998). Despite that, we were neither able nor inclined to hinder the trend toward a “territorially” independent “environmental psychology” that facilitated profiling (cf. Graumann, 2002b).

Historicity: History and Histories

While at the outset of this account I asserted with good reason that I was never the adherent of any particular school of psychology, and I nevertheless did encounter in my first semesters the assumption that I was a follower of the “syneidesiological” tradition. That was the term that Maria Krudewig, my “female doctor-father” – that was the term she used herself – employed to describe her (or should I say “his,” since she was my “doctor-father”?) psychology of consciousness: it was a later (and thus more mature) version of the Würzburg-Külpe School passed on by Johannes Lindworsky. Such attributions early on made me aware of the changes occurring in the terms designating psychology as an area with specific disciplines: the shifting emphasis from psychology, by way of consciousness and experience, to behavior.

The fact that what I once took to be the “language games of the psychologists” (1984) finds expression not only in the terminological fashions but also in changes in the basic understanding of psychology or of its very identity is evident only to the critical historiographer (Danziger, 1987). Of no minor importance here is the answer to a complex question that I posed in collaboration with Serge Moscovici in the course of a series of multidisciplinary symposia on the “historical development of social psychology” (1986/87), namely: is it the phenomena (such as masses or crowds, leadership, conspiracy) that change with time, is it only our conceptions and theories about them, or is it the interplay of those two factors? Without doubt the changing trends in theory as well as the “historical dimensions of psychological discourse” (Graumann & Gergen, 1996) belong under the heading of the historiography of science. Yet this theme is consigned to the category of “theory dynamics” of the theory of science. Accordingly, a theory that I put forward at the Mainz Conference of the German Psychological Society found agreement and approval, namely: that both the construction of theory and the construction of history exhibit an ultimately complementary lack, that is: to the lack of theory in the construction of history there is a corresponding lack of historicity in the construction of theory (Graumann, 1983).

Like most disciplines, psychology has left the construction of its identity primarily to the historiography carried out within its own discipline. Most monographs and textbook chapters offer a predominantly linear history of psychology with an almost stereotypical sequence of “pioneers” and “schools” – and generally without so much as a sidelong glance at adjacent or related sciences or disciplines. With any historical presentation of social psychology, it is immediately obvious whether it was written by a psychologist, a sociologist, or a psychoanalyst; a blinkered, if not mutually ignorant approach is the rule. The example of social psychology, with its representation by sociology as well as by psychology, shows the importance of institutes for the identity of a discipline: institutes, curricula, examination procedures, technical literature, and, last but not least, congresses and conferences. When the executive of the German Society for Psychology, of which I was the chair from 1968 to 1970, decided to produce every two years a "Report on the State of Psychology" – a task that I was the first to take on (Graumann, 1972b) – I was given the opportunity to choose a format that could offer a threefold reflection of psychology: as a science, discipline, and profession. Although this format has been preserved for several years now, I would be inclined today, as a result of negative experiences, to add an outside perspective, namely: psychology from a public perspective, for ultimately the identity of both individuals and groups includes the degree of coherence and continuity in the way they are perceived and evaluated from outside (cf. Graumann/Guski-Leinwand, 2004).

Only the more recent research on the social study of science has been able to show that it is the interplay of "cognitive" and "social" structures that occurs within the research groups of the "scientific community" (as understood by Weingart, 2005) that creates the identity of a science. But that has its price. We prefer consistency, coherence, and continuity to the absence or negation of those qualities, and when we set out to narrate and write our own histories or those of others or of a discipline, we are often, without actually being aware of it, dictated by narrative structures that are inherent in the mode of presentation itself. The narrative of our own or other people's lives and experiences is given the appearance of having direction and sense when "one thing leads to another," and that is more satisfying than merely having to observe how "one damned thing after another" happens (Gergen & Gergen, 1988).

I have often tried to show, when dealing with both figures and (supposed) developments, that activities that even simply foster - let alone create - the sense of coherence and continuity make many an occurrence appear to be history, while perhaps it is only "mythology" (Graumann, 1991, 1993, 1999, 2007). And so it is clear to me that others can reveal in my own autobiography – which almost unavoidably has turned into a "presentation of the self" – errors in consistency and coherence, which would once again confirm the communicative character of an autobiographical text.

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The Autobiography of a Marginal Psychologist: As Much as I Like Bob

Robert W. Rieber

In my youth I decided to become a scholar so…

I studied *Language* and it made me realize it was not communication.

I studied *History* and it made me realize how few people actually remembered it.

I studied *Law* and it made me distrust language.

I studied Politics and it made me angry at the irresponsibility of its leaders.

I studied *The Media* and it helped me understand the insidious power of misinformation.

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I observed people *Dying* and it helped me understand the importance of purposeful living.

I studied *Ethics* and it helped me understand how important it was to let right be done.

So then I studied and became a Psychologist only to find out how difficult it was to understand myself.

Call me Max.

I know that that's an unusual way of introducing myself since I have not changed my name. I was born Robert Rieber and as Robert Rieber I will die. But we can have other names, too, that we adopt because of the personal meaning that inheres in them. In my case the name comes from a famous short story by Somerset Maugham entitled *Mr. Know-It-All*. The story takes place on an ocean liner making its way from San Francisco to Yokohama (through Eaden on the gulf coast) in the 1920s. Because of shortage of accommodations, the unnamed narrator of the story is obliged to share a cabin with a man named Max Kelada, the Mr. Know-It-All of the title. The narrator makes no secret of his disdain for his roommate who, we learn, is "dark-skinned, with a fleshy hooked nose." Max seems to know about everything under the sun.

The narrator and Max are seated together at mealtimes, sharing the same table with an American diplomat named Ramsay and his pretty wife. The couple, we learn, is rarely together; she stays in New York, while he carries out his diplomatic responsibilities in Kobe, Japan. The subject of cultured pearls comes up one evening over dinner. Max is an expert; he buys and sells cultured pearls for a living. To test him, Ramsey asks Max to judge the value of his wife's pearl necklace. With his practiced eye Max declares that it is worth at least £30,000, maybe more. Ramsay scoffs; Max, he says, is mistaken because he knows for a fact that his wife bought the necklace for £18 at a New York department store. They make a wager of £100. Ramsay removes the necklace for closer inspection. But Max has no doubt; the pearls are quite genuine, and the necklace is worth a small fortune. "He was about to speak," the narrator recounts, referring to Max. "Suddenly he caught sight of Mrs. Ramsay's face. It was so white that she looked as though she were about to faint. She was staring at him with wide and terrified eyes. They held a desperate appeal; it was so clear that I wondered why her husband did not see it."

Max intuits that the pearls were a gift from a lover and that to reveal the truth would wreak havoc in their marriage. So he lies and admits that he has lost the bet; the necklace could not possibly be worth more than £18, he says, handing Ramsay £100. Everyone on board hears about the incident and thinks that Max has gotten his comeuppance. But the narrator knows the truth. Grudgingly, he has to acknowledge to him that Max is not quite as bad as he had originally thought. In doing so, he says to him, "Mr. Kelada, you are an honorable man," and Kelada responds, "Call me Max."

All my life I have identified with outsiders, and it could even be said that I have lived life on the margins; yet, paradoxically, I have managed to be in the thick of a heated interdisciplinary debate about the origins of language and human consciousness, so I suppose that also makes me something of an insider, too. Like Max, I am an insider–outsider.

The Evolution of an Outsider

I grew up in Philadelphia in the thirties and forties, the youngest son of hardworking Jewish immigrants who came from what was then part of the Austro-Hungarian Empire. Like most new immigrants, my parents were looking for a piece of the American dream. After much hardship, they started a hairdressing salon, which proved a fortuitous choice. The business flourished even during the depths of the Depression. Because my parents, as well as my parents' family, did not trust banks and steered clear of the stock market, they did not lose their savings in the crash as so many others did. They were even able to maintain a large house over the store and to hire a maid who acted, more or less, in the capacity of a nanny.

Because my siblings – one brother (who died in a veteran's hospital after World War II) and two sisters – were all considerably older, I was raised almost as if I were an only child. My older sister, Francis, was extremely important in helping in my early development, particularly toward my intellectual career. She not only gave me my first book and was a kind of role model for my future career (she was a history teacher), but she also was extremely important in assisting my mother in bringing me up when my father died after I turned twelve. One of my grade school teachers, who was quite fond of me, Miss Titus, told my mother that I was "rotten." Mother apologized to her, saying she knew she spoiled her little boy, but Miss Titus responded, "Nonsense, Mrs. Rieber, he's not spoiled at all, and he's just plain rotten." Subsequently, my mother found this so amusing that she repeated the line at every opportunity, especially to her family when they criticized her for spoiling me.

I explored every nook and cranny of Philadelphia, although even as a child I looked on Philly as a small town masquerading as a big city. No part of the city was off limits to me; I grew up playing with black kids as well as with white kids: it made absolutely no difference to me. I discovered that I could get along with practically anyone even if I did not really belong to any particular group or clique.

I was first introduced to psychology in high school by one of the greatest students and explicators of the human mind: Mr. William Shakespeare. I became obsessed by Hamlet – on the page and on the screen as played brilliantly by Lawrence Olivier. Even the movie was not enough to satisfy my curiosity about the hesitant Dane; I wanted more. That led me to Ernest Jones' *A Psychological Introduction to Hamlet*. Many years later I would be drawn to the works of the influential Russian child psychologist Lev Vygotsky. It turned out that he, too, considered Shakespeare's *Hamlet* as one of his most important intellectual influences; and so as I later discovered, like Hamlet, the history of the profession of psychology has become the story of a profession that could not make up its mind.

When I started out at the Pennsylvania State University, I decided to major in history. This was definitely influenced by the fact that my sister had introduced me to that subject. Then I quickly realized that it was the wrong choice. I flunked my exams with monotonous regularity. The reason was undoubtedly that my history teacher at the time was an abomination. I decided that I should change my major to a subject closer to my heart. There was a clinic at Penn State that specialized in speech pathology. That seemed to me like a promising avenue of research to investigate.

The work I did at the clinic was under the direction of a prominent speech pathologist by the name of Eugene McDonald. Later at Temple University, I studied under one of Kurt Lewin's students, James Jenkins. This education set the stage for the teaching and writing I would do in the future. I went on to pursue my master's degree in speech pathology as a graduate assistant in the clinic at Temple University. My sister's husband Arthur Krosnick, who was resident at Temple University Medical School, helped me considerably in guiding me to the appropriate personnel to obtain this position. Later on I took a large amount of graduate coursework in psychology and had the well-known professor James Page as my advisor at Temple, one of the most successful writers of books on abnormal psychology at the time. He recommended me to become a member of the American Psychological Association in 1958. However, I was growing restless. I felt constrained by Philadelphia. I had always had my eye on what I considered the big prize: New York.

As I was growing up I had seen New York any number of times – but usually just from the window of a train. Each summer my parents visited my aunt and uncle who had a cousin of my age and a house on Long Island; to get there we would take the train out of North Philadelphia Station. The journey took us past Manhattan, so I would be sure of catching a tantalizing glimpse of the magnificent skyline. I seldom got closer to Manhattan. I was frustrated – I wanted to be part of the city. I resolved to move to New York as soon as I could and do whatever I had to in order to make the move possible. What especially attracted me about New York was its cultural and ethnic diversity, and its rich cosmopolitan atmosphere.

In the summer of 1957, just before I was ready to make the move, I had a chance to attend summer school at the University of Vienna. In some way, I had been to Europe long before I crossed the Atlantic. The Europe of my imagination was created from spending countless hours in movie theaters. To the classic Sir Carol Reed film *The Third Man*, I owe my own version of Vienna, a film-noire city full of intrigue, shady spies, and treacherous women. I cannot say that my experience in the University of Vienna resembled that of the movie's mysterious Harry Lime, but it was an eye-opening one nonetheless, and I had a ball. By exposing me for the first time to smart and ambitious people from all over the world, this experience proved an invaluable prelude to New York. And for a budding psychologist, what could be a more romantic or appropriate destination than Vienna? There was the city's ineluctable Freudian mystique. It truly made the foundations for me to become an international psychologist. As one of the founding fathers of the international division of psychology of the APA, I have become a prominent contributor to meetings that I have attended throughout the world.

Meetings with Fromm and Other Neo-Freudians

Even as early as 1957, I could sense the ferment brewing just below the placid surface. The signs of the coming Cultural Revolution were there; you just had to know how to read them. One of the signs was a book by Eric Fromm entitled *The Sane Society* (Fromm, 1954). It was among the most influential books that

I had read until then, and later it influenced me to write a paper. In his book, Fromm set forth the idea that society was sick; it seems like a cliché now, but it was a startling notion then. Fromm identified symptoms of social distress that I would later have an occasion to investigate in my own work at a time when, if anything, society had become even sicker.

As a student I had always admired Fromm, actually I still have a tape recording of the author giving an interview in 1960 on NBC, in which he discussed the problems he discerned in the culture. Not long after I arrived in New York, I heard him speak at the Red Brick Church. I later had a chance to meet him on a number of occasions, although we never became close. Most of what I know of him comes from mutual friends, such as Herbert Spiegel and Maurice Green. Ted Schwartz, a psychological anthropologist and a student of Margaret Mead, also informed me of his relationship with Fromm. Schwartz had spent time in Mexico doing research for Margaret Mead, and she recommended that he also work with Fromm in Mexico. You have to keep in mind that Fromm had an authoritative Teutonic personality. He dismissed Ted's data out of hand when it became apparent that it was not compatible with Fromm's theory. This came as a painful blow to Ted since he had just spent three years collecting the data. He was so discouraged by Fromm's response that he never published his findings. I found out that this was not an aberrant incident; but it was a disturbing story, nonetheless, because if nothing else it showed that Fromm did not practice what he preached very well. All the same, I have heard different reports from other people that they got along rather well with Fromm.

All these stories about Fromm offered a complicated, and not always pretty, portrait of the man. A man of many moods, he was difficult toward some, while open to others. He could become truculent if you did not support his ideas. For what it is worth, let me add an experience that my friend Abram Kardiner had with Fromm. Fromm called him one day and said that he would like to get together. Kardiner assumed that Fromm wanted to talk to him about his book *The Individual in Society*. When they met, Fromm tried to convince him that he, Kardiner, was poaching on his field of research, and he should basically cease and desist. Now you have to take this story with a grain of salt; it is likely that Kardiner was exaggerating. But I could believe that Fromm would accuse a psychiatrist – such as Kardiner – of poaching on his territory. That is one of the difficulties you encounter in interdisciplinary research when each participating department has its own turf to protect. Neither Fromm nor Kardiner were easy men with whom to work. My friends Maurice Green and Herbert Spiegel, especially, have told me many stories that verify the fact that even great men have significant frailties in their character, and both Fromm and Kardiner fit that picture.

The First Job

Once in New York I found a job working at the children's clinic at New York University's Rusk Institute. I was hired by then Martha Taylor, now Martha Taylor-Sarno, and we have remained very good friends ever since. It was a clinic of last resort because it took in children with conditions that other institutions were unable to treat – and in some cases even to diagnose. The patients represented the most extraordinary personality types. There were several children, born prematurely, who spent the first few days or weeks of life in incubators. In those years, doctors were unaware that oxygen deprivation caused severe brain damage. One patient who stands out in my mind was an 8-year-old feral child who had been raised in a Nebraska convent. This girl had literally been found on a doorstep. The nuns regarded her as a miracle child and apparently never disciplined her or educated her. She was incapable of speech; she could only make strange sounds.

The inability to communicate was a common problem at Rusk. One of my patients was a teenage boy who did not talk at all. After ruling out a hearing disability, I tried to figure out whether he was suffering from brain damage or psychosis. I was stumped. His family was of little help. Finally in frustration, I decided to seek advice from the doctor who had treated him previously. That doctor happened to be the eminent psychiatrist and speech pathologist Dr. Emil Froeschels. Until the Nazis took power in Austria, Froeschels had been a full professor at the University of Vienna. He had successfully treated the great Russian bass Feodor Chaliapin when he temporarily lost his voice. In 1939, he went into exile, settling like so many émigrés in New York. When he received my letter requesting help, he wrote back to say that he never comments on patients by mail and would prefer to talk to me in person. Well, this was better than I would have dared hoped for. I was in awe of his reputation and delighted to have the opportunity to meet him.

As it turned out, he had been just as baffled by the teenager's condition as I was. "I don't make the diagnosis until I've cured the patient," he said. He was only half-joking. In the boy's case, he had not cured the boy so he was not sure what the diagnosis was. All the same we hit it off. Our meeting turned out to be the first of many. It was Froeschels, in fact, who was responsible for getting me admitted to the Adler Institute as a trainee.

By this point, Alfred Adler was long dead, and the Institute was headed by his daughter, Alexandra and son, Kurt, neither of whom was as smart as the father. They were difficult to work with and they imposed stupid rules; Alexandra, for example, expected people to stand up when she walked into the room. The two were intent on trying to poison your mind against Freud. Freud and Adler had a famously rancorous break. Freud was so disenchanted with Adler that he could not bring himself to express any remorse over Adler's death, which occurred suddenly at a conference in Aberdeen, Scotland. In a letter to Stephen Zweig, Freud wrote that having abandoned both religion and psychoanalysis Adler deserved to die far from home. (I later learned from David Bakan that dying far from one's family was considered an ignominious fate, almost a curse.) The antipathy in which Adler's children held Freud had the paradoxical result of actually making me more appreciative of the father of psychoanalysis, even though I was never prepared to embrace his theories. I think that Freud himself, no less than Adler, had substituted psychotherapy for religion. Leaving aside their ideological rift with Freudians, the Adler heirs proved to inept administrators as well. Although it still survives, the Institute has lost its luster in recent years.

In 1960, I left the Rusk Institute behind and, after a brief stint at Rutgers University as an instructor in speech pathology, I moved on to Pace University as an assistant professor in speech and hearing sciences and psychology. I would remain at Pace for nine years.

When I arrived at Pace, I had yet to obtain my Ph.D. My adviser had died before I could make much progress on my dissertation. I was a bit at loose ends as to how I was going to find another advisor. However, a solution would soon present itself as a result of a fortuitous encounter.

In 1962, I was invited to an international conference in Padua, Italy, which featured Wendell Johnson, a major figure in the field in stuttering. While at the conference, I fell into conversation with Dennis Fry, the director of the Department of Phonetics (which incorporated psycholinguistics) at the University of London. The conversation we began in Padua continued on a boat excursion to Venice. Along the way I told him about how I had been left in the lurch by the death of my advisor. Dennis at once invited me to apply to the University of London and study under him. My only obligation was to write my dissertation. It was a golden opportunity. The psycholinguistics lab there, directed by Frieda Goldman-Eisler, had an extraordinary pedigree. The first chair of the department was Alexander Graham Bell's father (the inventor of the phone also played a role in the development of the lab); the second chair, Daniel Jones, was the model for Professor Henry Higgins, the protagonist of George Bernard Shaw's *Pygmalion*. Dennis was only the third director in the department's history. Everyone associated with the lab, Dennis said, referred to themselves as "relatives of Higgins."

Pace allowed me to take a year off to go to London, though it was not entirely an altruistic gesture: the university benefited when more of its faculty members had Ph.D.'s. In any case, Dennis' invitation turned what might have been a routine enterprise into a marvelous year in London.

An offer I could not refuse in 1970 accounted for my next move, to John Jay College of Criminal Justice as an adjunct assistant professor of psychology. In 1972, I was offered a full-time position at John Jay College, which I accepted. The New York City University system had just been unionized, and the package that they could offer was so attractive that I suffered no misgivings about leaving Pace. My new offices were located in a building on West 56th Street, which had previously been occupied by Twentieth Century Fox. The behavioral science department at the time had subdivisions for psychology, anthropology, and sociology. They would very soon go their own way like the Baby Bells and become full-fledged departments in their own right.

Only a few years after I had joined John Jay, though, a crisis erupted that left me – and my colleagues – wondering whether we would have a school to teach in anymore. A decision had been made in the upper echelons of the City University system to shut down the college for budgetary reasons. John Jay was seen as an easy target because it lacked a "community" to support it – unlike Brooklyn College, for instance – and no organized alumni to raise a stink about the closure. Unknown to us, our president Donald Riddle had reached a deal with the chancellor to shut down John Jay after he had taken the precaution of finding a position in

Chicago, something we knew nothing about at the time. In public, though, he assured faculty members and students that he was fighting to keep the school open – phooey of course.

To John Jay insiders the crisis became known as "The Holocaust." Contrary to what Riddle and the chancellor had expected, the threat to shut down the school provoked a huge outcry. The protest was spearheaded by the police – many officers were graduates – and they demanded to know why John Jay was being marked for elimination at a time when crime rates were soaring and we were the only criminal justice college in the system. Unexpected allies popped out of the woodwork. One of them was a neighbor of John Jay named Tony Schwartz, who happened to be an advertising genius. (He was responsible for the famous "sunflower" spot that aired during the Johnson-Goldwater election campaign; it is still considered the most influential campaign ad ever broadcast.) Tony volunteered his services, free of charge, to keep it from shutting its doors. When it was all over, Tony wrote a book entitled *Media as the Second God*, in which one chapter was devoted to how he single-handedly saved John Jay through his media expertise. Consequently, other contributors to John Jay's cause explained that, in reality, it was a truly collective enterprise. Another welcome ally was Margaret Mead, whom I had gotten to know through my work. She asked me how many minority students attended the college. At the time it was about 40%, a much larger minority representation than at most city colleges. In her view, saving John Jay and protecting minority rights could be joined in the same cause. Margaret said she would write an op ed piece for *The New York Times* arguing for the preservation of John Jay. She was as good as her word.

In the end, the chancellor backed down. Not only did John Jay survive, but it would soon undergo an expansion. And I still had a job to go to.

Communicating Thoughts on Communication

My first book (edited with R. S. Brubaker, a former teacher and eventual friend) came about as an act of bravado on my part. I was in The Netherlands on a visit in the mid-1960s when the thought occurred to me that I might find a compatible home for a book I had been developing about speech pathology. I approached North-Holland Publishers (which much later turned into the publishing behemoth Reid-Elsevier). As it happened, North-Holland had already had some success with a book on phonetics, and so the editors were interested in bringing out another book about speech. They did not know who I was but they assumed that I must be somebody of importance, an impression that I reinforced by mentioning my association with Emil Froeschels. The book, entitled simply *Speech Pathology* is described in the catalogue as one which presents "the first comprehensive survey on a world-wide basis of the current trends in Speech Pathology, and is of great interest to those working in the fields of Speech and Hearing, Psychology and Education."

The good news was that I would soon see my first book in print. It was published in 1966. The bad news was that the royalty rate was pathetic – two percent!

When North-Holland asked us to write a second book we balked. We had no interest in investing so much time and work on a project with so little return. However, I had a counter-proposal for them. Would North-Holland have an interest in putting out a journal on communication disorders instead? For the publisher, the prospect of a periodical with a guaranteed base of subscribers was far more exciting than simply bringing out a book. Thus was born *The Journal of Communication Disorders*, which I ran for 25 years, an unprecedented record for Reed-Elsevier, which had a policy of rotating editors after ten years. The journal, with a circulation of about 1,000, made its debut as a quarterly and later became a bimonthly. In 1970, I founded the *Journal of Psycholinguistic Research*. This journal focused on the relationship of language and mind. It included physiological and psychological aspects as well. Then in 1993, I became the editor of yet a third periodical, *The Journal of Social Distress and the Homeless*, which I eventually took over entirely in its thirteenth year of publication when the publisher decided to discontinue it.

One of the most distinguished members of the board of directors of *The Journal of Social Distress* – and one of the most enigmatic – was the anthropologist Ashley Montague. I had first met Ashley in the early eighties at a conference of social scientists who were against the Cold War; it was held at the City University of New York. I asked Ashley to participate in another seminar about this topic, which he was very keen to do. I had always admired his work on race and found his views on cultural dissent provocative. I remember that as a student in the 1950s, I had attended a symposium in Philadelphia where he and Jack Kerouac debated each other on the subject of the Beat Generation. Montague took issue with Kerouac's claim that freedom should be seen as the ultimate goal in life. Freedom to do what you wanted seemed to be the animating principle of the Beat Generation; at least that is how Montague saw it. But in his view, Kerouac had not properly understood what freedom meant; it was not the freedom to do whatever you wanted. Freedom, Montague said, should be defined as the freedom to do what you *ought* to do. Kerouac, however, seemed incapable of understanding Montague's position. He reminded me of those people misguided enough to think that psychopaths are exercising "freedom" when they do whatever they want to do. Montague was right; that is not freedom at all. On the contrary, psychopaths are really in bondage because they do not have the freedom to do whatever they ought to do. (I do not mean to suggest that Kerouac was a psychopath, although it turned out that he was definitely self-destructive.)

Once I got to know him I realized that Montague was an obsessive–compulsive. He would act like a strict teacher and treat me like one of his adolescent students. When he asked me how I was and I would reply, "Good," he would reprove me, "You mean *well*, don't you?" In spite of his idiosyncrasies, we got along well. I sensed that he felt himself to be an outcast who had never won acceptance by the anthropological community. At the same time, he realized that he had to make a living and did so very skillfully, too, by turning out a number of highly popular books on anthropology. At the same time, he wrote some very serious ones as well. Nonetheless, he felt that his popular success made him suspect among anthropological circles. I remember once asking him to write a recommendation so that I could obtain a travel grant to represent John Jay at an international conference. Montague demurred, explaining that any letter of recommendation from him would probably kill any chance of receiving the grant. It was not that he was trying to get out of writing the letter – not at all. It was just that he really thought that his name would not mean anything to the grant-givers. On the one hand, I do not know if he was right or not; it depended, I suppose, on who read the letter. On the other hand, no one could deny that he had acquired many enemies in his career.

In spite of our friendship, he was always an enigma to me. I was not aware of just how contradictory he could be until a ruckus arose as a result of a special issue of *The Journal of Social Distress* we put out on political correctness. Toward the end of his life, I never saw Montague, although he continued to stay active on the board of the journal. Generally our communications were cordial – but not this time. Montague became incensed over one article, which he considered inappropriate. He fired off a letter to me demanding to know how I possibly could have published such a scurrilous article, having apparently overlooked the fact that another article in the same issue refuted it. Was he senile? Would he have made the same accusation ten years before? I do not know, but it seemed that at the end of his life he was giving in to the kind of intolerant world view that he had spent his entire career resisting.

The Interdisciplinary Approach

Throughout the 1970s, I began to see that both students and professionals alike suffered from a lot of misinformation and misunderstandings about the warring factions in the debate over language and communication. On the one hand, there were those who believed in the views set forth by the Skinnerians and the learning theorists, toxicologists, and environmentally oriented determinists. On the other, there were the adherents of Chomsky's position who maintained that the capacity for language was innate and not learned, and that syntax was hard-wired and only required normal development to manifest itself. The issue then and now was, of course, how to determine the way in which language capacity emerges over time and develops in the human organism.

I have always felt that, if you were going to really grapple with issues as complex and contentious as the origin and development of language or communication disorders, the best way to do this was by bringing together authorities from a variety of related disciplines. It is my belief that the best chance of success in making progress in most fields is through an interdisciplinary approach. The *Natural History of an Interview*, which was conceived by Gregory Bateson and others, sometime in the 1950s but never published, constituted a very valuable paradigm for approaching this problem. Instead of relying on a different body of data drawn from groups of subjects chosen by each researcher, they all used the same set of data. It was a marvelous paradigm for research that to the best of my knowledge had never been used before. In the typical paradigm for research, if you have a problem, you go out and recruit your subjects, and then give them the tests and conduct the analysis of the data. Optimally, the process should yield the same results regardless of the individual researcher but in fact if the researchers are using different sets of data the results are all too often contradictory.

The interdisciplinary approach sometimes works in theory better than in practice. Obviously its success depends in large measure on the individuals involved. If you put psychologists, who do not necessarily see themselves as psychologists, in the same room you are not likely to achieve especially fruitful results.

As I have said, from boyhood on, I have always had an aptitude for getting along with people who do not agree with each other but who see me as sympathetic to their position. Take, for example, B.F. Skinner and Henry A. Murray. I admired Skinner for his sense of political responsibility, even though I disagreed with Skinner's thinking about behaviorism. He once told me and a group of other distinguished colleagues at a reception held in my apartment after a New York Academy of Sciences conference that if we only lived by the principles of behaviorism the world would not suffer from violence and war. A nice notion to be sure, but I could not quite hide my skepticism. Harry Murray, the eminent psychologist and Melville scholar, did not agree with Skinner either, but while they were cordial to each other, they were hardly what you would call buddies. At the same NYAS conference, I ended up at dinner, sitting between them, listening to them reminisce about Whitehead and other colleagues they knew at Harvard. Each man in his own way believed me to be on his side, while at the same time suspecting me of, in effect, sleeping with the enemy. They both taught at Harvard and whenever I went to Boston I would make sure to see them both. They did not live far from each other. As soon as I saw Harry he would ask me if I had seen my "friend," meaning Skinner. Then when I had walked over to see Skinner, he would look out the window in the direction of Harry's house and ask me if I happened to have seen Harry lately. Although both men were very confused about exactly what my point of view was, I do believe that Murray somehow knew I was on his side of most of the issues.

It was my ability to maintain friendships with so many strong egos, in spite of their differences, that would serve me so well in gathering them together in the same room.

I had the opportunity to initiate the kind of interdisciplinary meetings that I had envisioned in 1974 when I received an appointment as a member of the Psychology and the Linguistic Sections of the Advisory Board of the New York Academy of Sciences. With the Academy as sponsor, I coordinated a number of conferences focusing on such subjects as aphasia, autism, and normal/abnormal language skills and brought together some of the leading authorities in the fields of linguistics and psychology. These conferences had the added benefit of providing me with a convenient forum to meet and get to know people whose work I admired (even if I did not always agree with their positions), including Skinner, Murray, Noam Chomsky, and Charles Osgood. It also helped that the Academy was well funded in those days and could spend a good deal of money on these events. The conferences were so well attended that we could barely accommodate all those who showed up; anticipating about 800 people for our first conference we ended up with 1,200. Our second conference, the one where I shared dinner with Skinner and Murray, focused on the history of psychology and was equally successful (Rieber & Salzinger, 1977). The Academic Press subsequently published a revised book version of the papers of these conferences (Rieber & Salzinger, 1980). Although I no longer find the Academy of much value we certainly had a good time together – nearly fifteen years.

Dialogues

In the early 1980s, I decided to see if it would not be possible to use a similar interdisciplinary approach in a book by inviting several leading experts to address essential questions about the psychology of language and thought. In this way, we could produce what I hoped would be a lively dialogue about the biological and physiological basis of language. The result was *Dialogues in the Psychology of Language and Thought* (in collaboration with G. Voyat), published by Plenum Press in 1983 (Rieber & Voyat, 1983). Among those I asked to contribute were Chomsky, Osgood, Piaget, George Miller, Jerry Bruner, and Roger Brown. Most accepted my invitation; however, because of circumstances beyond their control, Bruner and Brown were not able to finish the interviews. George Miller sent me an interesting letter, saying that he thought it was a fascinating idea and all the questions were certainly fine. He then added a witty – and plausible – refusal, however, explaining that the exercise reminded him of the questions he had had to answer on his preliminary examination to pass the doctorate and that were so anxiety-provoking that he could not do it. He was obviously joking, but in retrospect I would say that George's reluctance to partake in this project was based on his feeling that the book would bog down in a debate over what we called the "monkey business affair" with psychologists arguing back and forth whether apes could be taught to use language in any meaningful way. In George's view such a debate was nonsensical and a nonstarter. I personally believe that he was right, and still is.

I think that the book was a success in the sense that you could find virtually all of the major questions about language development discussed under one cover. It tackled some of the core issues that people were most interested in. Students have told me that they have found it particularly helpful because it clearly lays out the differences in the positions taken by the contributors. An updated version should probably be undertaken because Chomsky continues to change his theory periodically, though certainly not so significantly that it makes the book obsolete.

I have always been impressed by Chomsky's pioneering work, especially after having read *Language and Mind*. At last, I thought, someone had mounted a cogent attack on Skinner's position, which I have never had much time for. Also, I already greatly admired Chomsky's position in politics and social criticism. But I had not yet fully appreciated the complexity of Chomsky's position on the biologically determined origin of language. He was using empirical evidence, but basing his theory on purely rational grounds. Nonetheless, that is still science, because that is what physicists do. They start with theory that is based on a logical premise and you then gather empirical evidence. Admittedly, Noam was not doing much in the way of experimental research himself, but there were plenty of students who continued his work experimentally. Certainly there was no denying that his interpretation of their results was a plausible one, although that does not necessarily mean that it was the correct or only one.

Needless to say, Chomsky has a brilliant mind; he thinks faster than a speeding bullet. No one should be so foolish as to think they would ever win a debate with him. Yet I was sitting there with the temerity to start a dialogue with him about the basis of language. Well, it turned out to be a wonderful dialogue in person, and after he had edited it, it became even more of a true dialogue. He liked that mode of expression. That is part of his personality. The dialogue also helped me understand what he was driving at. I remember that until that point I had not understood what he had meant by psychological reality before we collaborated on this project. In Chomsky's view, I realized, psychological reality has little to do with an interpersonal experience. It was rational theory. Of course, one always starts with a sense of what we call intuition or the intuitive sense, but psychological reality had to be subject to a rational explanation, which Chomsky has always striven for in his writings. I admire him among the top scholars in the field today for his commitment to his personal philosophy of life and the political stands he has taken, although they have embroiled him in a great deal of controversy. He has always had the courage of his convictions. Although you would not know it from reading his work, he is also personable and cordial. Even people who hear his talks do not realize how gracious he can be; it is only when you talk to him that you really find out who he is.

If Chomsky's way of expressing himself was particularly well suited to the dialogue, the same could not be said for the eminent child psychologist, Jean Piaget. I came to know Piaget through Gilbert Voyat, who was a very close friend and colleague of mine and also acted as Piaget's right-hand man. Every time Piaget came to the States he stayed with Gilbert. I met Piaget on several occasions, but he preferred to speak in French, and it was rare to hear him utter more than a few words in English. So most of the time when we spoke, it was through an interpreter. Even the dialogue with Piaget in the book really was not his dialogue anymore than, for example, the debate between Piaget and Chomsky at the Abbaye d'Royaumont conference in 1975 (Piattelli-Palmarini, 1980), where apparently the two men never even met each other. And the format was not much to his liking either. Piaget did not feel that he was engaging in a true dialogue but was merely responding to the interviewer's questions. In spite of the language barrier, though, his charismatic personality came through. He was always fascinating to watch and to be around.

Charles Osgood took another approach to his contribution: I happened to get to know Osgood through my close friendship with O. Hobart Mowrer, who had brought Charlie to the University of Illinois from Yale many years before. Mowrer had began the *Psychology of language* based on the learning theory. Osgood was designated as heir apparent to continue Mowrer's psycholinguistic enterprise, in his own style, to its logical conclusion. So the dialogue was a real opportunity for Osgood. One day, while I was visiting Hobart at his home in Urbana, Osgood called to invite us for dinner at his house. In addition to the humans at dinner – Charlie, Hobert, and myself – there was another guest: Osgood's pet dog. The dog was trained to do almost anything Charlie needed him to do. It reminded me of what Julien Offray de La Mettrie (1950) suggested in his *Man and Machine*, such that it would someday be possible to train chimpanzees to act as our servants. Charlie went one step further and found a lower form of life to fulfill the role. It was quite astonishing to watch this tiny dog running to and fro, fetching things for Charlie at his command. The dog even sat at the table with us at dinnertime; like a well-disciplined child, it displayed perfect manners. Charlie was also a jazz aficionado, and he had even constructed a complete stereo system just so that he could play his jazz albums. He was a very talented amateur jazz musician himself, who performed gigs in small clubs from time to time on the side.

Osgood was also very meticulous. Everything had to be just right. He was just as much a perfectionist when it came to the dialogue he produced for me. He had a very complicated theory. Although he was a good writer and editor, he ended up editing his contribution to death. He made his position clear – that was not the problem – but as a dialogue it lacked something. Actually, he turned it into a monologue more than anything else, though not quite to the degree that Piaget managed to do.

Mowrer was very different from Osgood, and he was a wonderful, talented, and creative scholar. He was also past president of the American Psychological Association. At that time, presidents were chosen for their influence in the field of research in psychology. Mowrer had more citations in the literature than any other scholar at the time. All the same at a later date, he could not get his book on language published. This was at the height of the Chomsky revolution and Mowrer's theories were no longer in fashion. I did not take sides – nor would it have been appropriate; as the editor of *The Journal of Psycholinguistics*, I wanted to make certain that the Chomsky revolution in psycholinguistics was given a fair treatment in its pages, both pro and con. Although I had my own personal beliefs, I felt I could not very well take personal stands on any of these issues, if I was going to continue to publish the journal.

One day Hobart mentioned the tough time he was having getting his book in print. No publisher seemed interested. I told him to put his material together and send it to me. Maybe, I said, we could make something happen. At first I had a difficult time pulling the material together. Eventually, though, I did succeed in getting my publisher, Plenum Publishing Company, to agree to publish it in my book series. Of course, with Chomsky getting the lion's share of attention from scholars and critics, notice of Mowrer's book was zilch. Still, I was gratified that I had a hand in getting it published.

Hobart came to a sad end. He took his own life. I believe he was about eighty. His wife had died. His kids were away, and he was all alone in a big empty house. He had told me any number of times that he had done everything that he had wanted to do in his life. He had been suffering from various ailments that no one was able to diagnose. He decided that if he could not have the quality of life that he wanted then he should have the right to end it.

The Three Faces of the Scholar

I have always held that a scholar has three faces: one is what he writes, one is what he says publicly, and one is what he says in private. In many instances – certainly when the person is dead – we only have one or at the most two faces. Generally speaking, there is no way of knowing what William James meant, for instance, except through his writing. We are not privy to what he said to his friends or to what he thought, so we have to extrapolate from the words he left behind on the page.

But a scholar's writings do not always reveal the truth about what was really going through his mind. Here is a wonderful example of what I am referring to: in the 1980s, I attended a conference of the International Society for Cross-Cultural Psychology in Istanbul. One of the issues that came up at the conference (and has been resurfacing ever since without any resolution) is the question of whether there is such a thing as an indigenous psychology. That is to ask, is there a psychology peculiar to a culture or is there a pan-cultural psychology that is the standard for all places and all cultures? Having worked on a book with several colleagues about this subject in relation to Asian psychology, I was particularly interested in a lecture that was to be given by a Nigerian psychologist on that particular topic. Since many of those in attendance were inclined to favor the pan-cultural approach, I was sure that an African would embrace a position in favor of an indigenous psychology. To my surprise, though, the professor gave such a passionate presentation supporting pan-psychology that it shocked me and many others in the audience as well.

Although I did not have a chance to discuss his position with him after the talk, I assumed that he must have been heavily influenced by a British psychologist and that was why he had taken the stance that he had. Fifteen years later, though, I discovered that I was mistaken. I was attending another conference of The International Society for Cross-Cultural Psychology, this time in Warsaw, when I was approached by a Nigerian student who had heard I was from New York and wanted to find out how to obtain financial support so he could study there. I was especially interested in learning whether the same senior professor was working in Nigeria. The student had no idea but said he had been happy to introduce me to his professor who might be able to help me. It was the same man! At first I failed to recognize him. He was 15 years older, for one thing, and in Istanbul he had worn a colorful native costume. He acknowledged that he had given the address on the question of indigenous psychology. I then asked him the question that had been on my mind since. Did he really reject the notion of indigenous psychology? He smiled and admitted that of course, he had never believed in the notion of a pan-cultural psychology in the first place. However, the international organization had wanted him to be elected president of the society and to give a lecture advocating pan-cultural psychology because it was politically expedient to do so. Therefore, he could only give a lecture that pleased them, that was the African way.

It is as a result of experiences like that that I believe that you can not necessarily place your trust in what a scholar says in public or what he publishes, without some context to judge what he really thinks.

However, if you are clever and with a little bit of detective work, it is sometimes possible to find something in the work of an author, a clue about a scholar's thoughts or feelings, that he chose not to reveal – at least not explicitly. My work and publications in history of psychology have been greatly aided by my personal library.

The Bibliomania in Me

It is because of all of this that I am a book collector. I became interested in collecting books when I was a child. I started with the Katzenjammer Kids comic books. Collecting books became a passionate avocation, a healthy addiction. After a while I began to buy books from John Gach, who is the most important dealer of rare books on psychology and psychiatry in the country. I must have several thousand books by now, and I have published a bibliography of my collection (Rieber & Gach, 2006). Like many book collectors, I am always on the lookout for what I call sleepers – those books where the bookstore owners do not know the value of what they are selling. One of my most treasured "sleepers" happens to be the first U.S. edition of Freud's *Interpretation of Dreams*, published in 1915, which I picked up for $15, a veritable steal!

I have many other books by Freud, several first editions in the original German. But one of the most intriguing is a bound copy of several issues of a scholarly journal to which he contributed an article. The article later formed the basis of *Moses and Monotheism*. What is perhaps most intriguing about this particular article is not what is on the page, but what is missing – specifically the author's name. We know that Freud was obsessed by Moses. When he visited Rome he sat in front of Michelangelo's Moses for an entire day. (The photograph of the statue is included in the book.) But when he wrote *Moses and Monotheism* he initially called it a novel. This disclosure lent additional weight to my conviction that Freud had never managed to resolve his own ambivalent feelings about Judaism.

Detective work also entails hunting down sources. In this case, if I could not go directly to the source – namely Freud – I decided to do the next best thing and see his famous daughter Anna Freud in London. This was arranged by Kurt Eissler. When I arrived I discovered much to my disappointment that she was too ill to receive visitors because she was suffering from a stroke and aphasia. (In fact, she would die not long afterwards.) I did get a chance to speak to the housemaid, Paula Fiehtl. She had worked for Freud in Vienna, and so naturally I asked her what she remembered best about the master. Oh, she said, Sigmund was a practical joker. Her remark haunted me. It occurred to me that Freud was not above playing practical jokes in his writings and that many of the books that he wrote after World War I were to a greater or lesser degree written with tongue and cheek, a necessity to make sufficient funds to live on. I have no proof, of course, but I offer this as an illustration of how detective work can lead you down unexpected and occasionally profitable paths.

Investigations of Language and Culture

Language is a reflection of culture and culture is a reflection of language (Hoijer, 1954). As Hoijer wrote in his marvelous book about language and culture, "as we think, so we speak; as we speak, so we think."

During the time we were organizing the Psycholinguistics and Communication Disorders Conference in the 1970s, I began to investigate the connection between language and culture on the one hand and brain damage on the other. Language is based on noun structures and if you can not get the noun, you are kind of floating in the backwash of nowhere where the person with brain damage ends up searching for the noun in order to be able to get the verb. I had always been interested in the language of primitive cultures and had written a paper on it. So I was familiar with the contributions of a number of philosophical anthropologists who had worked with Margaret Mead and George Devereux, along with psychoanalysts who specialized in mental disturbances.

But what if not every language is centered on the noun? What if there were other cultures whose language structure was based on the verb? It is the case with the Navajos in North America and the Trobriand islanders in the South Pacific. So the impertinent question is this (to borrow a phrase from Gregory Bateson): What if we found a Navajo Indian who had a stroke and had aphasia? It would follow that in this instance the Navajo would not suffer from a loss of the capacity to find nouns but from an inability to find verbs (averbia) because their language is centered around the verb and not the noun. This issue, of course, goes to the heart of how culture might affect our innate structure of language. If the culture did have a pronounced impact then it would suggest that if you suffered brain damage in the relevant regions of the brain you would manifest different symptoms – different in the sense that a Navajo, for example, would have more averbia. If we could prove that a different culture could affect how language developed then it would be little short of revolutionary. We had been pulling the rug out from a lot of the assumptions that language theorists accepted practically as gospel.

To answer this impertinent question I would need to find someone who had done work with the Navajos. Why not get in touch with the best-known anthropologist of all – Margaret Mead? After all, she was living in New York and working at the American Museum of Natural History.

I had known who Margaret Mead was as an undergraduate, but I did not actually meet her until my first year in New York when I was working as at NYU Medical Center. I had a friend who was studying with Margaret as a graduate student at Columbia, and was lucky enough to be invited by him to her annual end-of-the-semester party. At the time, though, our relationship was a distant one since I never was actually one of her students.

In the 1970s, shortly after I had begun teaching at John Jay, I ran into one of Margaret's students, Paul Byers, who also happened to double as her photographer. It was through Paul that I established an even stronger relationship with Margaret because he was doing his doctoral work with her at the time.

When later I came to know her, I realized what an amazing woman she was. If she liked you, she could be extraordinarily helpful. I will never forget her willingness to help out when John Jay's existence was in peril. But if she did not like you, you had better steer clear of her. Once at an anthropology conference she made a dramatic entrance, carrying an intimidating-looking staff. She could have just wiped you away with it. She did not need to wield a big stick, though, to make her views known. On another occasion I was talking to her when a woman came up to her who was obviously an anthropologist of importance, but who acted like a groupie in the presence of a rock star. "Oh Dr. Mead," she gushed, interrupting us, "if I could only say how much I admire your work…" Before she could get out another word Margaret turned around and glaring at the woman, snapped, "How dare you interrupt me when I am talking about an important scientific matter?" The poor anthropologist quickly beat a retreat. That was when I realized that Margaret had another side to her. Seldom have I met anyone with a sharper tongue.

But we had always gotten on well, so I knew that when I brought my Navajo question to her she would be quick to respond, as indeed she was. When I reached her at her office in the Museum of Natural History, I asked her whether she knew of any primitive culture whose language was based on the verb, in which a case of aphasia had occurred.

Her response was that she did not know of a single case like the one I had described. I thought that was a little curious because I knew that cases of aphasia did occur in these cultures. Perhaps, she suggested, the families of those affected were ashamed and did not want to bring the patients to the attention of caregivers or else the patients died early because of inadequate medical care.

She advised me to call George Devereux and see if he could help. I did as she suggested and we arranged to meet at his apartment on the Upper East Side. A gracious host, he offered me a brandy and we sat down to talk. I had heard him once several years ago when I was in Philadelphia. Because he trafficked in ideas derived from psychoanalysis as easily as he did anthropology, I really did not know what world I was in when I was talking to him. He was a fascinating man and I enjoyed my interaction with him very much. He had two suggestions: one was to get hold of a book published in 1939 that he thought might be relevant. He also suggested that I get in touch with Kilton Stewart, who had studied primitive cultures. He told me to watch out, though, because Kilton "was a very strange guy." I thought that must make him very strange indeed, since George Deveraux was a pretty strange guy himself. Nonetheless, I called Kilton and explained my problem. Kilton told me to drop by the following Thursday night at seven o'clock. I walked over to his building and rang the bell. A huge woman came to the door. It turned out to be his wife. A very pleasant person, she invited me in and asked me to have a seat. As I waited the room began to fill up; soon nine or ten people were gathered, none of whom I had ever met before. Then I spotted my host who immediately caught my attention because of the conspicuously long pipe hanging from his lips. As Kilton Stewart addressed the group, I suddenly remembered that he wrote the book *Pygmies and Dream Giants*, which recounted his studies of pygmies in Malaysia. In the book, Stewart describes how every morning he would sit down at breakfast with different

Pygmy families. They had the habit of discussing their dreams of the previous night. The eldest in the family would analyze the dreams and advise the family members how to go about dreaming the dream the right way which was supposed to help other members of the family straighten out their problems. Kilton said that he had employed the same technique in his own therapeutic practice. I had not any idea that he had led such an amazing life. At no time, though, did he bring up the question about language that had brought me to his home. Suddenly I realized I was in a group therapy session. After a while the other guests departed, at which point Kilton turned to me and, shaking my hand, informed me that if I came back the next week he would charge me five dollars for the session. George was right: he was a strange guy. It was obvious that he was soliciting people to join his psychotherapy group, and I never found out anything about what I was interested in, but to say the least it was one of the most extraordinary experiences that I had in those days.

Margaret was also responsible for introducing me to her husband Gregory Bateson. When we met he was involved in the study of language universals, that is, principles of language that were applicable across all cultures and all times. This was at a time when I was working on a book that looked at facial expressions in various cultures. We were using the Osgood measurement called the semantic differential, which incorporated three major factors: evaluation, potency, and activity (e.p.a.) to assess the emotional meaning, which conveyed the same message in different language cultures. Bateson became interested in this project. We tested icons and graphic representations of ancient pre-Columbian culture. The subjects in this study were drawn from a variety of cultures, including the Spanish and Japanese, to try to determine the message embedded in them. Bateson had a succinct answer to the question. “The message,” he said, “is play.”

Unlike other marginal people who broke from the received view of the times, Gregory showed remarkable tolerance toward the Neurolinguistic Programming people in therapy, who subsequently distorted his ideas for their own purpose. Gregory felt that they should be allowed to do their thing. He did not seem to make any move to discourage them. He was always polite even when provoked, at least with colleagues. He did admit that it was tougher when his professional and personal worlds collided. He told me that he and Margaret had difficulties working together. Many of their fights occurred when they were about to investigate a new culture; then arguments would break out about who was going to research which area. Sometimes as soon as he decided that he wanted a particular area she announced that she wanted to take it. Finally, they came to the sensible conclusion that they would not work directly together as they used to.

Sybil: The Riddle of Multiple Personalities

It was not long after taking up my teaching duties at John Jay in the early 1970s that I came into contact with Flora Schrieber, who was then head of public relations for the school. A clever, talented writer, with a pop book on child development and language under her belt, she inspired admiration, envy, and resentment. She could put over the charm but she could be a vicious bitch when she lost her temper. A frequent contributor to *Science Digest* and other magazines, she was an endless self-promoter. In her *vita*, which she made sure to send to anyone who might be of help to her, she claimed that she had been “a friend of every president since FDR and most of their families.”

In 1972, Schrieber approached me with some cassette tapes. The tapes, she said, were recordings of therapeutic sessions. She was hoping to use them for some research she was pursuing. At the time I was studying the connections between mental illness and on-off speech patterns at the Columbia New York State Psychiatric Institute. The objective of our research was to see whether the vocalization of a person measured by computer could be used to determine the state of his or her mental health. She described it as an ideal research project for me. If I found something of interest in the tapes, she hoped, I would write it up and get it published in a research journal. I had no inkling that she was writing a book. Rather than analyzing what was said on the tapes we were simply going to analyze speech patterns. The experiment never got off the ground. When I played the tapes I could barely understand what was being said, because of static and background noise. She never asked for the tapes back, saying that she had already transcribed them all. As far as I know, up to that point no important and serious journal had ever published an article based on them. Subsequently, there were some publications in journals that were not the top journals in the field to say the least. After that I forgot all about the tapes.

Every couple of weeks Herb Spiegel, a good friend and distinguished psychiatrist, and I get together for lunch. We generally talk about our work and colleagues. But one particular lunch – in May 1997 – sticks out in my mind. Spiegel asked me if I would mind taking a look at the proofs of an interview he had given to a writer for *The New York Review of Books*. The interview focused on Spiegel’s association with Sybil, the most famous multiple personality disorder case of all time. This came as a shock to me. At no time in the course of our friendship had Spiegel ever even hinted to me that he knew Sybil. “That was because you never asked,” Spiegel said. There was another connection: How could I forget Flora Schrieber? It was then that I remembered the tapes Schrieber had given me long ago.

As it turned out, I still had two of them.

I wondered whether these newly discovered tapes would cast light on a case that had set in motion a cascade of events that would forever change the social fabric of America. Or to put it another way: the tapes, if they were what I thought they were, could prove a bombshell.

As I listened to the tapes a quarter of a century later I realized that they were not recordings of therapeutic sessions at all, as I had assumed. Instead they were a recording of a rambling conversation between Schrieber and Sybil’s psychiatrist Cornelia Wilbur about a book they were working on – a book that would later become the bestseller *Sybil*.

Sybil not only made the phenomenon of multiple personalities fashionable; it also made it – at least for a time – respectable. But is there really such a thing as a multiple personality? Before 1973 there were fewer than 50 known cases of the syndrome. By 1995 over 40,000 cases had been diagnosed. Some therapists contended that there were at least two million more. Were thousands of multiples (as they are known) wandering around, undiagnosed, and untreated until 1973? And what happened in the 1980s to produce such a bewildering number of cases (Rieber, 2006)?

What happened was *Sibyl*. When it appeared, some called it "a psychological masterpiece." *The American Journal of Psychiatry* declared that it was "destined to stand as a significant landmark both in psychiatry and in literature." It became a landmark all right, but not exactly the kind that the *Journal* anticipated. *Sybil*, which appeared in 1973, was an immediate sensation – and quickly moved to the top of *Time's* best-seller list. Paperback rights were acquired for $300,000, an enormous sum at the time. The TV movie shown two years later, with Sally Field as Sybil and Joanne Woodward as Wilbur, brought the story to millions more.

In the first tape, Schrieber and Wilbur talk about what they want to establish in the readers' minds. How will they construct the book? Recounting one of the initial sessions with Sybil, Wilbur says, "She introduced me to all the personalities…," and there was Wilbur explaining how she had dealt with all the personalities. "Well, excepting sometimes… I mean I would say to whoever was talking to me…well, who are you? Well, I am talking for, you know, and they had name three of four. And I would say, what does Peggy think about that? What does Vicky think about this? And I would say, Can I talk to Vicky? … I could summon them all."

The story – at least in the book – has a happy ending. In 1965, Wilbur declares Sybil cured, having "integrated" all her personalities. According to the book, she went on to get her college degree, became a successful artist and stopped having blank spells and memory lapses. The subject of the book pronounced herself eminently satisfied with the result. "Every emotion is true," she said. Wilbur agreed: "Every psychiatric fact is accurately represented."

Readers embraced the book with unrestrained passion. Women especially identified with its protagonist. One wrote, "I always wonder where she is now, and if she ever married. My sincere wish for Sybil is that she has peace of mind and a happy life."

From what I could discover, I concluded that the three women – Wilbur, Schrieber, and Sybil – were responsible for shaping the modern myth of multiple personality disorder. A psychological oddity, so bizarre and rare that it did not merit much publicity in most textbooks before 1973, multiple personality disorder had acquired a sudden acceptability. And it was not a disorder that was just limited to America. "MPD is being exported from the US as effectively as Diet Coke and the Gap," wrote social critic Elaine Showalter in the London daily, *The Observer*.

In 1980, MPD advocates successfully waged a battle to get the syndrome into the psychiatric bible, the DSM (*Diagnostic and Statistical Manual of Mental Disorders*), as an important disorder. Soon the number of cases and therapists specializing in the treatment escalated quickly. And the number of personalities that victims claimed grew in a similar fashion. Indeed, a leading American MPD therapist, Richard Kluft, maintained that he identified over 1,000 personalities in one individual. Nor were the newly uncovered personalities (or alters) always necessarily human. Some were identified as cats, dogs, stuffed animals, and, in one case, a lobster. By the late 1980s, MPD had become a staple of daytime TV talk shows. More recently the disorder has gained a foothold on the Internet where support groups have sprung up on sites with names like Divided Hearts, Shattered Selves, and Crazy People Incorporated.

Sybil, it became obvious, just did not make multiple personality disorder a fashionable illness in North America and abroad. With its emphasis on childhood sexual abuse, it also spawned two other related obsessive phenomena: one was the belief that people were being poisoned by buried memories, and the other was that only by reawakening those memories through hypnosis was recovery possible. Together, the three phenomena constitute what I term "a trinity of affinity" (Rieber, 1999).

In 1994, MPD was renamed as Dissociative Identity Disorder (DID). (Dissociation refers to a disruption in the various parts of mental functioning that constitute consciousness: forming and holding memories, assimilating sensory impressions, making sense of them, and maintaining a sense of one's identity.) By stressing the dissociation experienced by the person rather than the splitting of personality, the name change in the DSM reflected a groundswell of critical response to the whole idea of multiple personality disorder. Longtime opponents of the MPD movement – for that was what it had become – termed the phenomenon "a psychiatric craze." But if it was merely a fad it was a dangerous one. People who sought treatment found to their dismay that the "cures" were in many ways far worse the "disease" they were supposed to have. It appeared that in many instances, these MPD specialists were actually making troubled people sicker so that they could continue to treat them. Therapists influenced by *Sybil* are "unconscious con artists," in Spiegel's words, working at "memory mills," diagnosing MPD in patients and producing "phony memories." They are taking highly malleable, suggestible persons who might have a dissociative disorder and molding them into acting out a thesis that they are putting upon them. In my opinion, the book and film versions of *Sybil* need to be understood as symptoms of social distress and psychopathy of everyday life, a subject that I later elaborated on in a subsequent book, *Manufacturing Social Distress* (Rieber, 1997).

In my view, the Sybil case is an example of how phony facts create phony problems that in turn create phony solutions. *Sybil* is a triggering mechanism in the natural evolutionary development of reported false memory, child abuse and the misuse of hypnosis in the treatment of the mentally ill. It is little surprise then that the myth of *Sybil* began in the tumultuous years of the late sixties when revolution was in the air and the prevailing orthodoxy was under attack from every quarter. Writers like Thomas Szasz questioned the very definition of an illness, dismissing it as a myth. R.D. Laing questioned the nature of insanity. It was society that was sick, he asserted, not the person labeled mentally ill.

But what was going on in the minds of Schrieber and Wilbur, when they were putting the book together? Did they believe their story? Did they perpetrate a hoax on the American public, or did they actually buy into their own tale? Was the answer to be found on the tapes that Schrieber had given to me? I had to listen to those tapes repeatedly, because you can not really digest them at first listening. To put it in context of the history, it was not a boom, I did not have a eureka moment. All the same I knew I had something very important.

In August 1998, I presented my analysis of the tapes at the annual meeting of the American Psychological Association in San Francisco. (The talk was later published in the journal, *History of Psychiatry*.) After beginning my presentation with a brief rundown of the literature relating to MPD and discussing the current theories about hypnosis, I dropped the bombshell. As the rapt audience listened to the taped conversations, I proceeded to analyze what Wilbur and Schrieber were saying. It was Wilbur, I contended, who had labeled Sybil a multiple. The therapist was not finding the personalities inside of Sybil - she was planting them under hypnosis. With her patient hypnotized, Wilbur was manufacturing memories and concocting the primal scene - the grand illusion of an explanatory principle based on a sexual episode between her parents that she allegedly witnessed as a child. The idea was to make the punishment fit the crime, to give a justification for why Sybil was so fragmented. When Sybil became confused about the role each personality had in her life, it was Wilbur who came to the rescue and invented the lineup of personalities, explaining their connection to one another. And once Sybil was made to recognize the cause of her condition - sexual abuse at the hands of her mother - Wilbur then had to teach her to "hate" her mother. The primal scene also had another advantage. It would make the book sensational and sexy – and very salable.

Wilbur and Schrieber, in my opinion, were "not totally unaware" that the story that they told was wrong. Nonetheless, I said I would prefer to believe that there was as much self-deception as deception of others. Once you start making up a story to suit your own needs, it can take on a life of its own. Schrieber might have repressed the memory of how the story began, and then once it became a success there was no turning back.

I had no idea just how much interest his talk had generated. I called my answering machine. I had 25 messages from almost every imaginable source, from TV outlets to *People*. Koppel and Dateline were on my machine. When I reached San Diego a team from the TV program "Extra" insisted on shooting me for a segment. For the next several months the calls kept on coming. I almost felt like a stage or screen celebrity.

There were still some mysteries to clear up, though, questions that had not been answered by the tapes. I obtained access to some of Schrieber's files at John Jay Library. (There is also a secret file, which practically no one can look at.) At the bottom of one document, I came across a provocative statement scrawled in her hand. "I am now working on the most extraordinary case ever to hit the psychoanalytic literature," she had written. Then she added: "Who is Sylvia and what is she?" She had crossed out a name and substituted Sylvia. Sylvia was the name that Schrieber and Wilbur had used in their conversations before, for whatever reason; they had finally settled on the more evocative Sybil. But what was the name that Schrieber had crossed out? I assumed it was the real name of the patient. At that point her identity was still a mystery.

Another mystery was more easily resolved. When I finally got around to rereading *Sybil*, I had been struck by the number of personalities Wilbur had ascribed to her patient. But why sixteen? Why not fourteen or eight or three or three hundred? There was something about the number sixteen that kept gnawing at me. Then it hit me. I knew that Schrieber must have read *The Mask of Sanity* by Hervey Cleckley (1976). [Cleckley had also been the coauthor of *The Three Faces of Eve* (Cleckley & Thigpen, 1990).] In *The Mask of Sanity* Cleckley had described several distinguishing characteristics of a psychopath. There were, I recalled, sixteen in all. I felt sure I had the answer.

From time to time, I would run into a historian named Peter Swales who calls himself "an archaeologist of knowledge" and has made a name for himself as a debunker of Freud. More recently he turned his attention to unmasking the identity of Sybil. Reasoning that there must be some connection between "the true facts and a fabrication," as he put it, Swales used the book as a guide. His search finally led him in late 1998 to a small conservative Midwestern town called Dodge Center in Minnesota. Sybil, Swales discovered, was really named Shirley Ardell Mason. Unfortunately, the fact was known much earlier but the newspaper article revealed Shirley's name was forgotten. We will not chastise Peter; we will let the scholars discuss why he did not find the article. But he was too late to meet her. She had died peacefully at home on February 26, 1998 of breast cancer. She was seventy-five.

The only child of Mattie and Walter Mason, a hardware-store clerk and carpenter; Mason was raised as a strictly observant Seventh-Day Adventist. Residents of Dodge Center recall a somewhat withdrawn, slender girl with a talent for painting. Although her mother was known to display bizarre behavior, no one in the town knew of any instances of the sexual and physical abuse ascribed to her in the book. Until 1945, there was no indication that Mason was in trouble. In 1945, however, she suffered a breakdown and experienced severe anorexia. She met Cornelia Wilbur in Omaha in the early Fifties. After her mother's death, she moved to New York where Wilbur was practicing. Though the treatment lasted for 11 years, the relationship between the two continued afterward. When Wilbur left New York to take a teaching job at the University of Kentucky in Lexington Mason felt adrift. She neither married nor had children. The book's success, however, gave her financial freedom, allowing her to move to Lexington to be near Wilbur. In 1992, Wilbur died at the age of 88, leaving Mason $25,000 and all of her royalties from the book. After Wilbur's death, Mason became even more reclusive, spending time taking care of her cats, gardening, and painting, until arthritis made it too difficult to hold a brush. When she fell ill with cancer she refused medical treatment, averring that she had had "enough trauma" in her life.

I ran into Swales not long after Mason's identity had come to light. As we fell into conversation Swales offhandedly mentioned that when Mason first moved to New York she had lived for a year in a six-story walkup apartment on York Avenue between 78th and 79th Street. I stared at him "That's uncanny! I know that building," I said, "I was living there the same year she was. We could have passed each other a hundred times." When I looked at a recently published photo of Mason, I realized that it was possible that I might have seen a woman who looked just like her when I lived in the same apartment building. The conditions surrounding my ability to expose this case were entirely serendipitous. Had it not been for a personal friendship with Flora Schrieber and Herb Spiegel none of this material would have surfaced. More specifically, the tapes that Schrieber had given me would have never been looked at again if not for my subsequent conversations with Dr. Spiegel.

As to the question of whether or not the Sybil case was an out and out fraud; that of course depends upon your personal definition of that term. No matter what you wish to call it, it was a conscious misrepresentation of the facts. A fine line between self-deception and the deception of others is an important issue here. Unquestionably, Schrieber and Wilbur wanted to make Sybil a multiple personality case no matter what. This is clear when you examine their response to Dr. Spiegel that the publishers wanted a book on multiple personality when Herb Spiegel had already informed them that she was simply a case of hysteria.

After my exposure of Sybil came out, I often met people who wanted to know whether I believed that there were multiple personalities or was the whole thing phooey. I said that it was not quite so clear-cut, that first you had to explain epistemologically how an individual presenting such a disorder actually became sick and then determine ontologically why the sickness expressed itself in the form of MPD. Some of those I spoke to would shake their heads since it seemed to them that I was contradicting myself, holding out the possibility that multiples might exist while at the same time insisting that those people who declared that they were multiples were suffering from other forms of mental disturbances. "You sound like you have three personalities yourself, Bob," they would say.

And I would answer, of course, "Yeh, yeh, and yeh, but which one?"

Manufacturing Social Distress

Ever since I had read Eric Fromm's *The Sane Society* I had been preoccupied by the way in which society can become sick and how the symptoms of the disease actually manifest themselves; and I decided to write a book that would focus on "the psychology of malefaction." I was convinced that the problems that Fromm had identified in his book paled in comparison to those that were bedeviling society four decades later. The problems had changed a great deal in the interim and I believed that a different approach was required to address them. One of the phenomena I was anxious to explore was the prominent role that the psychopath played in society and how psychopaths were depicted, even glorified in pop culture. Could a person actually be evil? I recall meeting Harry Murray while I was exploring the phenomenon of the psychopath. He had an interest in the subject himself and told me that if I wanted to find out more about psychopaths I should read Whittaker Chambers' *Witness*. He said it was the best book on psychopathy. Harry should know since he had served as an expert witness in the Alger Hiss case in which Chambers was the most prominent witness for the prosecution. The jury did not pay any attention to Harry's testimony, though. At first I could not quite understand why Harry wanted me to read Chambers' book. When Murray had said he read *Witness* by Chambers, I did not realize why. Then I understood: Whittaker Chambers was not just writing about psychopaths; he *was* a psychopath and if I wanted to gain a good understanding of what goes on in the mind of one there were few better examples. I also realized that the reason that Harry wanted me to consider the Chambers book was because he was the expert witness for Alger Hiss and as we all know Harry lost the case and Chambers won. My book, which was the study of social-psychopathology in world culture, *Manufacturing Social Distress*, appeared in 1997. I later wrote another book, which was based on the original *Manufacturing Social Distress,* entitled *Psychopaths in Everyday Life* (Rieber, 2004). In the conclusion, I wrote, "Normalized psychopathology in high places, in my view, is largely the result of social distress as it has become institutionalized in the emerging world culture. Put simply, the psychopathology of everyday life will continue to prevail until we cease to be proud of those things of which we should be ashamed."

I am afraid that those words remain as true today as they were when I first started to work on the project eight years ago. For further views regarding this matter, I refer the reader to Rieber (2004).

Rieber's Gang

Rieber's "gang" is a phrase that Jerry Bruner branded us with back in the 1980s when I started an informal gathering of a group of old friends and colleagues to meet for dinner at Columbia University club to discuss our mutual interest. From time to time we would ask a guest to join us, Jerry was one of our first guests, and after the meeting ended he referred to us affectionately as Rieber's "gang." I suppose unconsciously, I had in mind a group that had existed in the late nineteenth century (Menand, 2001). This group referred to itself ironically and half-defiantly, as the metaphysical club. Among the members were Oliver Wendel Holmes Jr., John Fisk, Charles S. Peirce, Chauncy Wright, and William James. A similar group also existed in England approximately at the same time. These groups tend to last for about ten years and then gradually fade away; our group had the same fate. But during its active and fruitful lifetime, it provided a most stimulating and rewarding floor for all of us. The regular members were myself, Herbert Spiegel, Joe Jaffe, Tom Langner, Zvi Lothane, Jason Brown, Maurice Green, and William Stewart. We had many more irregulars who tended to show up much less frequently. Our meetings averaged once a month and for the most part consisted of enjoying a wonderful buffet dinner at which time we exchanged lot of information about ourselves, our work as well as the current rumors that were going around about the politics of professional and personal lives.

Joe Jaffe

One of the first members of the metaphysical club whom I had met was Joe Jaffe, around 1969. In a conversation with George Miller, he mentioned the fact that I ought to contact Joe Jaffe at Columbia's New York State Psychiatric Institute (PI). The reason for this was at that time I was trying to finish up my dissertation at the

University of London, in England. The connection with Jaffe is that he had just developed an early computerized technique to measure the pauses in speech that occur in natural communication situations. Frieda Goldman-Eisler, with whom I was working in London, was probably one of the first to use this technique manually rather than through computerized analyses. I proposed using this technique to supplement her findings, since it would take a fraction of the time to get the analysis done by computer rather than using the Goldman-Eisler methods. Jaffe was more then interested to assist me in this project by helping me to learn how to use the AFTA, which was an acronym for his device to perform this task. Our ability to get along during this period as well as our mutual interest and compatible personalities resulted in a mutual friendship and working relationship at Columbia up to the present date. Joe Jaffe and Sam Anderson, who was hired at PI (about the time I arrived), wrote several papers together during this early period. Sam who as a student of Jerry Bruner at Harvard also became a college and friend up to the present date. Subsequently, I moved *The Journal of Communication Disorders* to Columbia University Psychiatric Institute and as time passed I was able to expand the *Journal* into a bimonthly publication of approximately seven hundred pages a year. The scope of the journal was also expanded to include speech and language disturbances in mental illness i.e., the psychopathology of language and thought.

Herbert Spiegel

Herb Spiegel was the oldest member of our group, at the time I am writing this he is celebrating his ninetieth birthday. I had met Herb in the early seventies, when I stated my affiliation with Columbia and the New York State Psychiatric institute. Joe Jaffe who was instrumental in getting my appointment in Columbia introduced me to Herb one day and referred to him as our resident expert in hypnosis. At the time I thought I knew something about hypnosis but in actuality all I knew was the garden variety misinformation that most textbooks and misinformed professionals propagate. It was not until the early 1980s that I had the opportunity to actually learn how important Herb was in the field of psychiatry and how little I knew about hypnosis and related altered states of consciousness. It turned out that Herb was a neighbor who lived around the corner from me, so we occasionally had dinner at a delicatessen, which we both frequented. It was then I learned Spiegel was deeply was involved in forensic psychiatry as an expert witness. In those years, John Jay College of Criminal Justice was fortunate to have funds to sponsor all kinds of events particularly conferences that dealt with subject matter directly related to the mission of the college. I was very active in the New York Academy of Science in those days and suggested to my department that we propose a conference to the academy on the subject matter of forensic psychiatry. I obtained the interest of two of my colleagues at the time, mainly Fred Wright and Charles Bahn. The conference was held June 20th 1980 and published as separate volumes of the annals of the academy (Wright, Bahn, & Rieber, 1980). Herb delivered a major paper entitled, *Hypnosis and Evidence: Help or Hindrance*. It was that paper that eventfully stimulated the creation of a course of forensic hypnosis at John Jay that Herb and I taught together for many years. We also organized, several years later, the first conference on the use of forensic hypnosis in psychology and psychiatry. I cannot overestimate how powerful Herb Spiegel's influence was upon me. This was especially true when we taught a course together at John Jay on forensic hypnosis. All that I have learned about the nature of hypnosis and its applications I owe to Herb.

Tom Langner and Other Members of the Gang

Tom was an old friend of Joe's and was also one of the principle investigators and authors of the Manhattan study on mental illness in New York City. Tom and I spent many hours talking about everything under the sun, as well as some of the projects he was working on that later turned into publications. Zvi Lothane was brought to the group by Jason Brown, a neurologist colleague of mine, who came periodically but did not attend regularly. Zvi and I frequently traveled to Europe together. We were both Europe freaks. He was born in Poland, went to Russia during the Second World War, and then migrated to Israel. He then came to the United States in the 1960s and was trained as a psychiatrist. Maurice Greene, who was a senior member of our group and personal friend of Herb Spiegel and Joe Jaffe, became quite friendly with me during the 1980s and we wrote several things together relating to the history of psychoanalysis. Maurice had worked with Eric Fromm and Harry Stack-Sullivan as did Herb Spiegel so that we all got some personal views of these famous characters from Herb and Maurice. Robert Kelly, whom I met at John Jay in the 1980s, was teaching in the doctoral program in criminal justice at John Jay. There we taught courses together and became close friends and later wrote several things on social distress and organized crime, and traveled with one another to various conferences in Europe and other countries over the years. Howard Gruber also occasionally attended the Metaphysical club from time to time, but more importantly was one of the major figures with David Bakan, John Broughten, and me in creating the PATH (publications in the theory of history and psychology) series of publications. This series is still in existence and has produced more than 25 volumes. I would like to think that it was a major factor in creating a greater interest in facilitating publications in the history of the theories of psychology. Howard Gruber and I became good friends and often met for walks in the park, and had coffee at one of his favorite coffee houses on the Upper West Side. He was one of the foremost scholars of Piaget in America, and was chosen by Piaget to write his autobiography. Unfortunately, he became ill and was not able to finish the work. He recently died in a nursing home in New York City.

Another very close friend and colleague also recently died in a nursing home in Toronto. David Bakan was as about as close a friend and colleague as I can imagine. Even though we were not living in the same city, we shared our ideas and views about almost everything. We talked at least a few times a month over the telephone about everything under the sun. I knew most of David's family so well, including his wife Milly; I felt as if I was a part of his family.

We often "argued the world" not just about psychology but about politics and life in general. To me David was an extraordinary human being, a man of all seasons, but most important of all his integrity was not a passing incident in his character, but rather a basic pattern of his life. Something of David's own career is recounted in his autobiography in this book. David also wrote a wonderful forward to my book, Manufacturing Social Distress (Rieber, 1997). During the 1980s I often attended the International Society for Cross-Cultural Psychology. In a meeting held in Aberdeen, Scotland, I was fortunate to meet an Indian psychologist by the name of Anand Paranjpe, and David Ho, a Chinese psychologist at University of Hong Kong. They were both interested in putting together a book on Asian psychology, and wanted to know whether I would be part of the project. After much discussion, we agreed to do a book, which was published under the title of *Asian Contributions to Psychology* (Paranjpe et al., 1988). Anand and I became friendly and often participated in international meetings during the 1980s and 1990s especially at the *International Society for Theoretical Psychology*. This association was originated by Canadian, Dutch, and American psychologists that met every other year, alternately in Europe and America. These meetings turned out to be very attractive and I tried to attend as many as I could. I made many friends there that helped me better understand the nature of psychology in other countries throughout the world. Individuals such as Leo Mos, from Alberta, whom I had met through Joseph Royce in the 1970s. He became a very good friend and colleague. Royce had established an institute for advanced study at the University of Alberta in the 1970s and was kind enough to invite me on several occasions to participate in their activities. Unfortunately, this institute ceased to operate after the Alberta oil industry had stopped flourishing in the late eighties. Joe unfortunately died shortly after. Leo and I remained good friends through our work at the ISTP.

I met Carl Graumann in the USA at an APA meeting during the mid 1970s. As I recall, it was in the presence of Klaus Riegel and Frank Hardesty. Afterwards we met quite often in Europe and America and became friends as the years passed. Our encounters have been much too infrequent, and I regret that we did not have more time to spend with each other over the last 20 years. Carl's work will be summarized through his own autobiography in this book, but he has attempted to address many issues I have been interested in over the years; for example, the psychology of language and thought as well as cultural psychology in general. Over the years we have both been very appreciative of L. S. Vygotsky, Kurt Lewin, and Karl Buhler. Other colleagues I met through that organization were such prominent individuals as Hans Van Rappart from Amsterdam, Kenneth Gergen from the United States, Wolfgang Maiers from Berlin, and too many others to mention.

International Psychology and Otto Klineberg

My work in international psychology was something that had started quite early with my first trip to Europe as a young student; however, it was greatly increased after I had become a professional, especially during the 1970s and 1980s. More about these societies a bit later. Here I should mention that my friendship with Otto Klineberg began with his return from Paris in 1980. He had spent over 20 years as the head of UNESCO in Paris. Upon returning to the States, although he was an emeritus professor at Columbia University, he became affiliated with the City of New York Graduate Center through Herald Proshansky, who was at that time the President of the Graduate Center. Otto came to meetings I had organized at John Jay with Robert Jay Lifton on various aspects of political psychology and violence in world culture. I had some years before been responsible for bringing Bob Lifton to John Jay from Yale, where he organized a Center for the Study of Violence and Human Survival. Otto Klineberg's attendance at these meetings resulted in our walking back home together from John Jay while talking about everything under the sun, but mostly about his career. He is best known for having done the first study of Negro intelligence under Franz Boas at Columbia University in the 1920s. One day to my surprise he asked me to do the Columbia oral history of his life, which I naturally answered in the affirmative. Although I did not know anything about the Columbia oral history project, I soon found out and completed the oral history some years later. We were both quite pleased with it, although in retrospect, Otto left out most of the juicy information he privately told me. Somehow he did not understand that it was perfectly appropriate to discuss this material in the oral history. I later found out from both his son and his wife, this was inevitable, because that was the way Otto would do things, always "with dignity and grace avoiding arsenic and old lace."

Over the years, I have regularly participated in the activities of several of my favorite small professional societies. These groups, mainly Cheiron in America and (what used to be called the European Cheiron, now under the name of the International Society for Human Sciences), and last but not least the International Society for Theoretical Psychology. My participation in these organizations has enabled me to become close with many important international psychologists, for Example, Graham Richards, Jim Good, Leo Mos, and many others.

Vygotsky

The Russian psychologist Lev Seminovich Vygotsky has long been one of my intellectual heroes. A pioneering figure in postrevolutionary Soviet psychology, he worked at the Moscow's Institute of Psychology throughout the 1920s and early 1930s where he investigated the role society and culture played in the development of consciousness in childhood. His work on signs and their relationship to language influenced eminent psychologists like A.R. Luria, Michael Cole, Jerome Bruner, Seymour Sarason, Jean Piaget, and many other friends and colleagues. His radical approach to the study of consciousness represents a contribution as significant as that of William James. He succumbed to TB at the age of 38, cutting short a brilliant career. The only consolation we can take is that if he had lived a few years longer he would have certainly have been killed by Stalin, who disdained his work.

For decades the whereabouts of Vygotsky's papers were unknown and I was aware that many scholars had sought them to no avail. As it happened, though, my editor at Plenum Press had some important literary connections in the former Soviet Republic of Georgia. One day, after he returned from a visit there, he met me for lunch and announced, "I have just secured the rights to the Vygotsky papers." I was astonished – and incredulous. He had the contract with him. As it was written all in Russian, I had to take his word for it. I was to be the editor, he said, which meant that I would have to find skilled translators to render the voluminous collection into English. (I wanted English speakers fluent in Russian rather than the other way around.) The project was a consuming one that took many years to complete; the first volume appeared in 1987 with the fifth making its debut only in 1999. Later I was asked by the publisher to put together a book based on the six-volume collected works entitled *The Essential Vygostky* (Rieber & Robinson, 2004). Nonetheless, the effort paid off. Vygotsky, I have always felt, deserved much wider recognition in the West than he had received. After all, this was a man who was called by Stephen Toulmin (1978) "the Mozart of psychology." I would have preferred to call him the "Shakespeare of psychology." He was an anti-reductionist in that he did not try to describe consciousness or human behavior in terms of a few simple principles. He was interested in context – the history of our history. That is to say, an individual cannot be considered in terms of his or her problems or symptoms; you have to know about the time in which the individual is living and what influences to which he or she is subject to. I have no doubt that had he lived, he would oppose the direction in which psychology is headed these days because of its reductionist approach and its lack of historical context. That is why I identified with him so much. Stalin would have certainly murdered him if he had not prematurely died of tuberculosis. In his own way, Vygotsky, too, was an insider-outsider, influencing and commenting on the state of psychology without being quite a part of it. Like him, I am a marginalist, standing outside the mainstream of psychology, but still an active participant in psychology. The profession has changed considerably since I entered it many years ago and has broken up into too many subspecialties, which has fractionalized the discipline. The rapid and profitable expansion of the American Psychological Association has culminated in a number of benefits, but the organization still seems dominated by its many flaws. The result of this condition within its current headquarters in Washington, DC has become more like a Tower of Babel, where one specialty does not seem to speak the language of the other. I have elaborated upon this in a lengthy article dealing with the crisis in psychology. If I had to identify myself in any particular interest group it would be with one that embraced a nondeterministic, humanistic point of view that recognized the importance of the distinction between what the German philosophers called *Naturwissenschaften* versus *Geisteswissenschaften*, that is, natural sciences versus human sciences (for my *Weltanschauung* see Rieber (1997).

So if you happen to run into me you know how to greet me.

Call me Max.

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In Search and Proof of Human Beings, Not Machines

Joseph F. Rychlak

The Challenge

What was to become a career challenge fell on me "like a ton of bricks" one morning in November of 1957. I had taken the doctoral degree in Clinical Psychology at The Ohio State University that spring and was having a marvelous time in my first job as an Assistant Professor at Florida State University. The incident to which I am referring nicely captures the core of my life's work, and therefore I would like to

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present it before moving on to other matters. It occurred while I was teaching an applied course in family and child therapy. There were four male graduate students enrolled, and I had scheduled a case-review session in which other clinical staff and a consulting psychiatrist were present. I was infuriated to find my students sitting through the review of their cases without saying one word regarding the dynamic aspects of their clients, mentioning only statistical data like social class level or number of appointments met. No hypothetical interpretations were proffered, such as possible motivations of the child or suggestive dynamics of the parents. I tried repeatedly to bring them out with hints and questions but, much to my embarrassment, my students did not cooperate. The shame was so great that I could not make eye contact with my colleagues or the psychiatrist.

After the meeting had concluded I asked my students to remain and vented my irritation with their performance. What was going on here? I quickly learned that these young men had a rationale for their behavior. It seems that they had been learning in another class, where they were instructed in Skinnerian conditioning, that it is pointless to speculate theoretically on dynamics in the "old-fashioned" manner I espoused because there is no sound way in which to prove any of this. What we should be doing is learning how to shape behavior through scientific studies, beginning with lower organisms like white rats, and working our way up to human beings. This was not a theoretical matter, but strictly one of observable and thus provable manipulation. I had experienced a touch of this sort of mechanistic thinking while a graduate student at Ohio State, but no one was naive enough in that academic setting to deny that we always take on theoretical positions in our scientific work. I argued in this vein, noting that to follow Skinner's assumptions was to greatly alter the human image – a theoretical alteration if ever there was one. But all of this fell on deaf ears. My frustration was only increased.

It became obvious that a realm of instruction other than psychotherapy per se was called for, and that it was up to me as the teacher to provide it. The traditional profession of psychology seemed under threat. These students were committed to their cause and "took no prisoners" in the debate. Rationalism was conceding to mechanism. I could not understand how such intelligent young men could be so dense, and longed for the intellectual stimulation of my graduate days. Of course, I also realized that I was pretty dense when it came to a full understanding of what was taking place in my chosen profession. This deeply frightened me and briefly prompted me to wonder why I did not go to law school in the first place. I ended our session and headed home for lunch. After another hour of venting, through which my poor wife had to suffer, I left for the school library. I clearly remember taking down the first volume of the *American Psychologist*, a journal that traced the development of the profession of psychology in the twentieth century. The aim here was to discover what sort of profession I had gotten myself into. I thereby initiated what was to become a career-long effort to answer my student's objections to the practice of psychology in which people could be thought of as self-directing organisms, living dynamically meaningful lives, rather than as mechanical robots in need of controlled manipulation by others.

The Early Years

I was born on December 17, 1928, in a cold-water flat on Pulaski Avenue in the small town of Cudahy (6,000 or so residents at the time), a suburban community of Milwaukee, Wisconsin. This was on the south side of town, which was heavily populated by Polish families who had immigrated to America and spoke predominantly in their native tongue. Casimer Pulaski was a hero of the American Revolution. My parents were born in America, offspring of immigrants and hence fluent in the Polish language. They were both around 20 years of age at the time of their marriage, and in short order found themselves struggling through the Great Depression that began in 1929. Dad had to find work where it was available, at one point taking a job of building automobile tires in Detroit, Michigan. Mother was fortunate to land a job working as a seamstress in Milwaukee and continued working until her retirement at age 65. She lived into her 97th year. Dad eventually went to a "barber college," and opened his own business. In time he became interested in public affairs and for several years served as an Alderman. Then, for the last 15 years of his work life he was elected to the office of City Clerk in Cudahy. He passed away in 1982. My father had extremely high ethical standards, and I was always somewhat fearful that I would tarnish his reputation by some stupid thing that I did during adolescence. Neither of my parents finished high school. My mother was moderately concerned with my schoolwork, but my father was very much so. My childhood was pretty much a matter of doing such things as playing sports, camping, flying kites, building model airplanes, fishing, bike riding, and so on. I also liked to read and did a lot of it. Two of my aunts provided me with appropriate reading material. I had my cousin, Antone, to play with throughout childhood. We were almost identical in age. Antone was the son of my father's brother and lived just across the street from me. We were like brothers, and confided in each other completely. We had a wonderful childhood, with large fields to play in, and never really thought of the harsh 1930s as economically depressed. We were just "not rich."

Dad was a real pal to me. He taught me how to swing a bat, toss a football, and to box. One of my uncles was a professional boxer, and I was going to both professional and amateur "fights" with Dad by the time I was seven years old. I did a fair amount of boxing up through my high school years. We also went to football and baseball games. There were two elementary schools in south Cudahy, situated across the street from each other. These schools shared the same playground area. One was a public school named Kosciuszko (after the Pole who was also a hero in the American Revolution) and the other was Holy Family, a Roman Catholic school. As it turned out, I spent my first three grades at Kosciuszko, the next two at Holy Family, the sixth grade back at Kosciuszko, and the last two grades at Holy Family. That comes to four years at each school, and I had an additional kindergarten year at Kosciuszko.

Why did this shifting back and forth occur? Because of a running difference of opinion between my parents over where I should attend: the first move to Holy Family was inevitable, because I had to take my First Holy Communion at about that time. I will not go into any of the other arguments advanced for choice of a school. Dad got me into Kosciuszko for the sixth grade but mom pulled me back to Holy Family for the final two years, using the assistance of the parish pastor to do so. Dad was apparently "surrounded." Now, it does not take an expert to suggest that such shifting back and forth played havoc with my education. The two schools were not teaching courses on the same schedule. I somehow missed the beginning instruction on diagramming English sentences so that my grammar and composition grades suffered. But my reading grades were always excellent, and I made good use of this ability in the study of history and geography. Actually, my father had taught me to read by the time I entered kindergarten. I always liked school and was a pretty good but not outstanding student.

But there was another important outcome of this shifting back and forth from one school to the other. I got to see and understand alternative points of view, which is a capacity that is not insignificant to the profession of a psychologist. The school children at these two locations were not exactly friendly. There was great competition going on continuously, and this turned into ridicule and name calling on the playground, and occasionally resulted in physical combat among the boys. I was, quite naturally, expected to take the side of whichever school I was attending. But I knew and had friends on *both* sides of the street. I could not give my complete allegiance to either group. I also believe that this experience helped solidify what I consider my "loner" tendencies. That is, I was perfectly willing and, indeed, preferred relying on myself rather than "the gang" to choose anything for me. I guess there is a streak of selfishness in me for I would rather part company and go my own way than tag along with others who are out to do something that does not really interest me. I found that it did not bother me at age ten to go to movies alone, or spend an afternoon at the library. I did not need other kids around, except maybe for Antone during critical periods of maturation. Even though I realize that people think you are unfriendly or "stuck up" when you act like this, I could never bring myself to be insincere in seeking approval from others. I am almost too free and open with my personal views. People have told me that I readily show my emotions in facial expressions. I am sure they are correct. I once worked on a faculty, which had a professor who was a Polish citizen. I found out later that some of my faculty colleagues commented on my loner tendencies. He shrugged and replied "That's just Polish!"

When I was 13-years old my mother gave birth to my brother, Donald. My mother was only too happy to teach me how to change diapers, prepare formula, and all of those things that babies demand. Somehow, Dad got out of such duties, but Mom had me properly trained in no time. Actually, I have always enjoyed children. My own children have been the joy of my life. I got a million laughs looking after my brother. Of course, there were times when I had made plans to have some fun with my pals only to learn that I was on emergency baby-sitting duty because Mom had some unexpected development demanding her presence elsewhere. But in the main I had few regrets. My only regret is that my brother was to die unexpectedly when he was only 39-years old, leaving a beautiful family and a big hole in my life. An interesting aspect of this positive attitude for children is that I have always found it exhausting conducting psychotherapy with a child who is less than about ten years of age. I can do the job, but cannot shake off any emotional turmoil at the end of the day. I do not "take work home" like this when my client is an adult. Therefore, it should be no surprise to learn that the great majority of my clinical work has been with adults.

Growing Up

World War II was declared on December 8th, 1941. The war had a tremendous impact on me. I followed all of the military actions, kept up with the designs of American airplanes, and built a model of my favorite. I also built a crystal set that actually worked, and from there developed an interest in radio. I pictured myself a radioman/gunner on some huge bomber in the European campaign. I loved airplanes but never actually thought of myself as a pilot. Gunner, radioman, or even a flight mechanic suited me. I enrolled in Cudahy Public High School in the fall of 1942. High school found Antone and me together every school day – walking to school and back, lockers side by side, trying out for football together, ogling the girls, and so on. My reading ability and capacity to understand what I read made course work fairly manageable. But my life in high school revolved around football. I was able to earn my first football letter as a sophomore, the only player to do so. I played every position in the backfield, ending up as the quarterback in my senior year. Unfortunately, the best I could achieve was an "honorable mention" in the all-conference voting of my senior year. I was given two awards at graduation, one for being the outstanding graduating athlete and another for having the highest grade-point average for an athlete.

I was still pretty much a "kid," having a great time in high school with my share of girlfriends but never a "steady." I often fantasized about getting out of Cudahy one day to see the world. College was something we heard about but almost none of my graduating class had such plans for the future. My folks could not send me. They were still putting away money in hopes of building their dream home one day. I had some notions about maybe receiving a football scholarship. It was 1946, the war had ended in the previous year and plenty of slightly older football prospects had returned to swell the ranks of college football teams. There was no room here for honorable-mention quarterbacks. I had landed a job at the local drop forge, working in the shipping room. Was this to be my future? After much soul searching, I decided the thing to do would be to go into military service and earn the GI Bill, so that I might one day have the money to go to college. Quite honestly, this was a vague goal at the time. I primarily just wanted to get out of the small town and "see the world." Antone liked the idea, but his folks would not sign for him. We were only 17 years old and needed their signatures to enlist. So, I had to go it alone after convincing my parents to sign for me. That was pretty much the end of our close relationship, for we went separate ways in life (and he died much too soon).

I enlisted in what was then called the Army Air Corps on August 27, 1946, and was honorably discharged with the rank of sergeant on June 29, 1949. Enlistees were sent to Lackland Air Force Base, San Antonio, Texas, for basic training. San Antonio was an interesting city, and I had some riotous Saturday evenings there with my buddies. I had visions of flying all over the world following basic training – preferably as a tail gunner on a flying fortress. Instead, I was lumped in with about 40 other guys who scored reasonably well on the Army General Classification Test and, following basic training, was sent to Barksdale Air Force Base, Shreveport, Louisiana, where I spent the rest of my hitch coding morning reports in the Statistical Control Section of the Air Training Command. We tracked the movements of all the military personnel involved in various training programs – machinists, pilots, cooks, and so on. It was an unromantic, dull, desk job.

The day I stepped into our basement office at Barksdale was one of the low points of my life. I felt as though I had been sent to prison. After a few days, though I had said nothing derogatory on the job (my complaining reached its high point at the base beer garden), the captain in charge of our section could see my desperate mood and ordered me to report to him after regular working hours. This involved a fatherly chat in which I unburdened my soul and he listened with great understanding. He then offered me a deal: If I would just stick it out, he would see to it that when he was rotated out in one year I would be transferred overseas as per my wishes. I agreed to straighten-up and play the "good soldier" for one year. This meant primarily to get the disgusted look off my face.

Yet by the time that year rolled around, I had changed quite a bit. What happened at Barksdale had already begun during basic training at Lackland. For the first time in my life I became truly aware of "class" differences. I found myself saluting other guys, many of whom were just a few years older than me but who had spent some time in college. As a result, they wore little gold or silver bars on their shirt collars, and I had to salute them because they were supposedly "officers and gentlemen," whereas I remained a "dog face" (enlisted man). It did not take a genius to figure out that if I wanted to get ahead in life like they had done I would have to really and truly attend college. It was no longer acceptable to vaguely talk about "maybe" or "probably" going to college. I now *really wanted* to go and felt agitated about the fact that I was way behind in my academic preparation. High school was fun and football, but somewhere along the way I missed out on something called an education. I had to catch up.

My plan was to use the rest of my enlistment as a kind of "prep school" for college. This amounted to a lot of reading. I got a copy of the Harvard List of Great Books, and began reading any that I could lay my hands on. I stumbled upon a collection of books called the Delphian Course in a used bookstore. This was a kind of adult education series of ten volumes (for which I paid ten dollars) covering ancient history, Greek mythology, and early literary writings. I studied words. I spent a lot of time at the Barksdale library, which was a good one and even was given a part-time job there. Unfortunately, this lasted only a few months but it was long enough for me to get to know the place like the back of my hand. I took extensive notes in my studies and made use of these in later years (such as my notes on Darwin). I studied some philosophy (e. g., Plato, Socrates, Aristotle, Descartes), but only a little psychology. I also had my enjoyable diversions, such as playing on a semi-pro baseball team, traveling to St. Louis as a member of the base tennis team, or golfing regularly. I took in a Mardi Gras in New Orleans on a three-day pass. We were allowed to take round-trip air hops on weekends if a scheduled plane had room for us. I had such memorable trips to Los Angeles and New York City. I was seeing a bit of the world after all.

My captain was true to his word. After about a year, he was given orders to ship out and looked into an overseas assignment for me. None was available, but he did find a slot needing filling in Alaska. There was no overseas pay differential for assignments in Alaska. And Alaska is not Paris. I thought it over for a day and decided that I would live out my enlistment at Barksdale. My work was lousy, but my living conditions in what is called the "permanent army" duty were excellent. We stood almost no inspections. We did not have to march. We could wear civilian clothes after work hours and on weekends. And I was growing intellectually. So, I declined the captain's offer while expressing my profound gratitude to him for thinking of me. I should also mention at this point that, thanks to meeting a guy on base who had attended college as a psychology major, I learned a lot about what it would mean to enter this profession. I began writing to universities and pretty well settled on this as a career goal. The only other careers I gave some consideration to were philosophy and law. I had now reached adulthood, knew what I wanted, and had the financial resources and personal motivation to get it.

College and Graduate School

I went to the University of Wisconsin as an undergraduate, taking a double major in psychology and philosophy. The first two years were spent studying at what was then known as the Milwaukee Extension Division of the University of Wisconsin, and the remaining two years at the parent campus in Madison. Initially, my thought was to earn a master's degree in psychology, and then find a job as an industrial psychologist. But I soon learned that it required a Doctor of Philosophy degree to do interesting work in the field. I then set my sights on a career in the mushrooming field of clinical psychology. I did well in undergraduate school, earning A grades in all but four of my courses and was elected to Phi Beta Kappa. I graduated with senior high honors as well as departmental honors. With the help of my advisor, Emmett Baughmann, I made application to a half-dozen graduate programs. I was admitted to Illinois and Stanford, but decided on Ohio State based on what Professor Baughmann had to say. In the fall of 1953, I drove my 1941 Chevrolet down to Columbus, Ohio, found a room, and turned yet another page of my life story.

I was very lucky to do my graduate work in clinical psychology at Ohio State during the mid 1950s, because both Jules Rotter (1954) and George Kelly (1955) were developing and about to publish their theoretical works. Rotter was a neo-Hullian, tempered by a strong attraction to Adlerian theory, which gave his Social Learning Theory a quality that did not quite fit Hullian theory. That is, Adler was a teleologist, viewing people as intentional beings who are responsible for their behavior.

Adler would not agree with theoretical explanations that relied on the shaping of human behavior without personal intention. Hull was a behaviorist who explained people's actions as due to associative shaping rather than intention. An intention was merely a mediating cue of some sort that had been input in the past and now played a role in directing behavior. I had Rotter as my clinical supervisor at the VA Clinic in Columbus for one year and I can say with confidence that he dealt with the intentional side of client behavior. His views took on a more mechanistic tinge when he put them down on paper. Rotter wanted to base his theoretical views on empirical evidence, and there is just something about this scientific framework that turns people from intentional to mechanical organisms.

Kelly theorized in the phenomenological tradition. His Personal Construct Theory was clearly teleological, and it encompassed dialectical reasoning, as when he defined *a construct as* two referents sharing one end of a bipolar meaning while a third was oppositionally negated. For example, the construct of gentleness would be reflected in saying "Mary and Alice are gentle; Jane is not" (Kelly, 1955, p. 111). I had stumbled onto the important role played by dialectical oppositionality in human reasoning as far back as my studies in the Air Force. Initially this arose in my struggles with Marxian and Hegelian formulations. It struck me from the very first that this sort of understanding by opposites permeated Freudian and Jungian explanations of behavior. I reasoned that if a robot input the Ten Commandments it would have ten possible actions for cognizance and enactment. However, a human being would have at least 20 for there is no way to give a command without implying its opposite meaning. Commanded to honor father and mother implies that they could alternatively be dishonored. Mechanical robots cannot reason in this *dialectical* fashion. They reason exclusively on the basis of what Aristotle called *demonstrative* reasoning. In the latter case, what is being considered mentally is presumed to be "primary and true," taken as the only alternative possible because meanings opposite to what is being processed are never suggested, implied, hinted, provoked, and so forth.

As I took my coursework and clinical supervision from Rotter and Kelly, I studied their thinking and actual clinical practices in terms of the images of humanity that their formal approaches relied on. In time, I was to name the contrasting images as follows: Rotter relied on a Lockean model of the person (after John Locke) and Kelly relied on a Kantian model of the person (after Immanuel Kant). The predominant image in psychology since its inception as a science has been the Lockean model. Rotter tried to follow this model in his formal theorizing. Behaviorism and all related formulations (which include computer modeling) have put all their eggs in the Lockean basket. On the one hand, Locke followed British philosophy, which formulated human behavior in a third person or, as I called it, *extraspective* fashion. People were pictured as being moved about by billiard-ball contacts, which were intentionless and lacking in free will or human agency. Just as gravity "shapes" events in the physical world, so do associations between cognitive signals "shape" human behavior. The theory of causation here is termed *efficient causation* (via Aristotle).

On the other hand, in the Kantian model, people are described from a first-person or *introspective* perspective. Kant distinguished between a noumenal and a phenomenal realm of meaning. He demonstrated that we all rely on a priori phenomenal patterns of meaning that influence what we can know *from birth*. Our mind is not *tabula rasa* (a blank tablet) at birth, as Locke believed, waiting for initial inputs to somehow coalesce and thereby mediate our reasoning process as we mature. Kant held that from the very beginning of our lived experience, we have the phenomenal ability to frame what we know, to influence our life's course as if we were looking through a pair of conceptual glasses that framed things in either a rose colored or a darkened fashion. To know what a person is being "shaped" to do, we must have an understanding of what he or she is bringing to bear in this conceptual process. This is how I found Kelly picturing people, and in doing so he was relying on formal and final causes (after Aristotle). A formal cause is any sort of pattern, which can involve plans and selections or choices made according to a scheme. This is what prompts an intention and the final cause is essentially the resultant reason for doing anything, trying to attain some end or goal. *Telos* is a Greek word meaning "end," and hence *a teleology* is any theory attempting to explain events (including behavior) as taking place for the sake of (or intention to reach) some such end. Kelly made it clear that his theory did not object to teleology as did the vast majority of psychological formulations of his day. I realized at this point in my development that I was a Kantian and a teleologist.

Given the similarities in our views, one might expect that I would have taken the doctorate with Kelly. But I did not. I took it with Rotter (my master's degree was with Paul H. Mussen). As I have always given Rotter's Social Learning Theory a teleological interpretation (its basic concept is that of an "expectancy," which could be an intention) and it did not bother me that he tried to follow Hull in writing formulae for predicting behavior in quasi-mechanical fashion. I liked the clarity of Rotter's approach and the direct tie it has to laboratory experimentation. Kelly's approach is also experimental, but because he employed a type of assessment scale (i.e., the *Role Construct Repertory Test)*, a different style of empirical investigation – one that I disliked – has been pursued. Although Kelly thought of people idiographically (i. e., one person at a time), too many of his followers today are turning his theory into a nomothetic (i. e., groups of people) study. Rotter's approach allowed me to do a doctoral study in which I showed that the context in terms of which a person framed activity influenced the conditioning that followed. I got nice findings and a publication in the much respected *Journal of Experimental Psychology* (Rychlak, 1958). I think the only reason this happened is because the editor of this journal sat on my doctoral committee. I was never accepted by this journal again, though I submitted dozens of experimental reports to it over the years. There is a reason for this rejection, of course, succinctly captured by: "Teleologists need not apply."

While working as an intern, I developed a rather severe depression over a client I was seeing. This was a child molester, a man who had been having sexual relations with his stepdaughter for many years. I found him a disgusting excuse for a human being. I then began to fear that my moral standards would not allow me to be an effective clinician. As Kelly was consulting me at the Veteran's Administration Clinic, I took the occasion one afternoon to tell him about my self-doubts. Kelly, in a most sensitive and insightful manner, quickly grasped the depth of, and reasons for, my depression. I spent several sessions with him going over my problem and he helped me immensely. He essentially became my psychotherapist. I think that my subsequent desire to earn accreditation from the *American Board of Examiners in Professional Psychology* at the earliest possible date stemmed from this period of professional self-doubt. I was greatly bolstered by the fact that I got the "ABEPP" certification on the first try, which was hard to do in 1962. It was kind of like making the first-string football team.

On a happier note, one of the major events of my life took place in Ohio. I was married! Lenora (Smith) was an undergraduate student at Ohio State University, whose parents lived in Columbus. We were married on June 16, 1956, and Lenora continued her education, although by December of that year she was pregnant. Lenora and I have had a traditional marriage. Our family came first in our lives. I always went where the job opportunities for growth could be found (salary also played a role, of course), and this required our moving around the country. Lenora backed me up without complaint. This meant we had to forego the help of a close relationship with our parental families. But the very fact of having to go it alone in raising our children made us into a very close-knit family of parents and offspring. In time, Lenora would take a formal role in my work.

I completed my graduate studies and my Veterans Administration training in four years, graduating in the spring of 1957. The summer before taking up my first teaching position at Florida State, I began what would become a 25-year alliance with Douglas W. Bray in the Management Progress Study he was conducting in the American Telephone and Telegraph System. This longitudinal study followed young men (later, young women as well) through their careers to see who were successful, what the ingredients of a successful career amounted to, and so on. Data were collected during the summer months at assessment centers set up at certain hotels around the country. In 1957, the location was Washington DC. I worked as a personal interviewer in this study. Later I devised a "life themes" scoring system that allowed us to put the contents of the personal interviews into numerical analyses. I published a book on some of these data (Rychlak, 1982). But a major benefit to me and my family was the travel opportunities that this consulting position made possible. We spent later summers working in New York City, Detroit, Philadelphia, Minneapolis, and Denver. Also, I benefited personally from my friendship with Doug in that he was an excellent model of the sophisticated professional. I have never quite reached sophistication, but I was able to copy aspects of his style in giving talks and managing complex situations. I also learned a bit about wines, but still prefer beer with my meals.

Florida State University

This brings me back to the incident of the case review session at Florida State. The core argument that flared up repeatedly in my debate with the students had to do with the fact that psychology is a science, and therefore we should not be speculating on dynamics with our clients, but rather applying empirically proven techniques of manipulation to cure them of their maladjustments. I will have something to say later in this chapter on the findings of manipulation in conditioning experiments. But for now I want to begin a practice that I will follow for the rest of this chapter, and that is to put my developing ideas concerning psychology before the reader as they evolved in my life story. With the emphasis on science, it seemed only sensible to look into the philosophy of science. Fortunately, a book had just made its appearance written by the eminent philosopher of science Philipp G. Frank (1957). Frank had associated with the philosophers of the *Vienna Circle* and eventually established his own presence in the field as one of its deepest thinkers. He not only taught me much in this book but gave me additional leads concerning the rapidly growing field of the philosophy of science.

What amazed and delighted me was Frank's statement that leading thinkers believed science was "certainly under the strong influence of Kantian philosophy" (Ibid., p. 306). Science investigates the relations between symbols to form a logical component that is contributed by the scientist *qua* human being. I viewed this as recognition of formal causation in theory construction, a pattern that cannot simply be reduced to underlying efficient causes, but that can serve as a framework guiding ongoing events (i.e., final causes). Behaviorism might interpret behavior as a series of efficiently-caused hook-ups or S-R bonds, but such extraspective characterizations are unquestionably contributed to by (introspective) assumptions of the scientist as thinker. They are the patterned frames of reference that endow knowledge with a certain meaning. I considered this an appreciation of the role played by human teleology in science as well as in the daily life of everyone. Newton's Laws of Motion were wonderful contributions to physical science, but are they to be directly applied to human behavior? Frank argued that we cannot decide which of two scientific theories is to be accepted by judging them within the purview of these sciences. He concluded: "we see that the validity of a scientific theory cannot be judged unless we ascribe a certain purpose to that theory" (p. 359). So, there remains wide latitude for the scientist to try out many different theories to account for the same empirical findings. Such writing gave me confidence that a study of human beings in teleological terms was not beneath the level of good science. If eminent scientists could look at people as intentional beings, why not psychologists?

One of the more important philosophical convictions that I found in reading Frank and others was that the observed "facts" do not always (if ever) speak for themselves. Frank used as an example of this truism a disagreement between two quantum-theory scientists. They both looked at the empirical data in detail, but had conflicting understandings of what they "saw." Frank concluded that there was no contradiction between these colleagues on the observed physical reality but they "interpret the same scientific theory by different analogies taken from common-sense experience" (p. 247). Looking back at this quote, I am delighted to see the use of analogy in the explanation, because this became a basic concept in my own theory. But, more important for the present is the fact that, thanks to Frank, I developed a great appreciation for the distinction suggested in all of this between what I would say is *a theory* and what is *a method*. I came to think of a theory as "a series of two or more schematic labels (words, visual images that we name, etc.) that have been hypothesized, presumed, or even factually demonstrated to bear a meaningful relationship, one with the others" (Rychlak, 1994, p. 322). A method is "the means or manner of determining whether a theoretical construct or statement is true or false" (p. 317). I was to refine this distinction further in later years.

It is not uncommon for empirical researchers to equate the method they are using to gather facts with the theory they are using to explain such facts. Another famous philosopher of science, E. A. Burtt (1955), made this very point at a grander level when he observed that a physical scientist is under strong and constant temptation to "make a metaphysics out of his method" (p. 229). I think mechanistic theoreticians in psychology are especially prone to do this. They set up experiments based on efficient-cause manipulations and then immediately equate these observations with their efficient-cause theory. I find even today that it is difficult separating the method used from the theory claimed in many of the so-called rigorous experimental reports published by mechanistic psychologists. The experimental manipulation as called for by the method is presented, the data are statistically tested, and then the findings are framed in the same efficient-cause mode that the method has relied on – all in the name of rigorous empiricism that does not admit to alternative (e.g., teleological) theoretical explanations of the data. Who needs an alternative when the "hard facts" are unquestionably "seen" to support the mechanistic theory in every way. They not only support it they *are* it!

I should at this point give a little background concerning the job at Florida State. My friend, John Neff, helped me to get this position. He had moved down from Columbus to Tallahassee the previous year. As Lenora's obstetrician was in Columbus, she stayed on after spending the summer of 1957 with me in Washington D.C., where I worked on the Management Progress Study. On September 23, 1957, Lenora gave birth to our son, Ronald Joseph, as I "sweated it out" with John's support. One month later she joined me. We had a very nice winter in Tallahassee. I was again offered a job by Doug Bray to help out with data collection in the summer of 1958, based this time in New York City. We sublet an apartment in Brooklyn Heights and had a great time over the summer, with only one low point when our son developed his first illness – an ear infection that had me in a dither. Luckily I had not signed my contract to return to Florida State because after arriving in New York I was contacted by Jim Elder, Department Chairman at Washington State University in Pullman, Washington. He needed someone in clinical and wondered if I would be interested. I was. He arranged to come out to interview me and I was subsequently hired. Now the problem was how to move from New York to Pullman by way of Tallahassee. Somehow, we managed, and oh how I miss those exciting times. I love to drive and here was an adventure made for me.

Washington State University

The job at Washington State involved running a small community clinic (Human Relations Center) and teaching one course each semester. I was worried about too much time committed to clinical administration, but things worked out beautifully.

I had my own facility, separated from the Department of Psychology, and my own secretary. Running the clinic was no problem for me, and I enjoyed the psychotherapy this allowed me to carry out. I had a few really interesting cases to deal with, including a phantom limb problem and one long-term case that was diagnosed by another clinician as schizophrenia. I disagreed. I was able to follow up the latter client years later (and all was well!). Lenora and I found a nice little house for rent, a few blocks from my clinic. I had enough time to squeeze in research projects, which in those days involved experiments on the group Rorschach, dreams, free association, and various personality measures. I had students working with me and gave my first advanced degrees there.

Our life in Pullman was pleasant enough, but we were far removed from family ties in the Midwest with no real financial capacity to fly back and forth. It was too far to drive more than once a year. The summer jobs with the Management Progress Study were a godsend, for they covered our expenses as we motored to Philadelphia in 1959 and Minneapolis in 1960. In April 1961, I was contacted by Saint Louis University concerning a position. I believe that Rotter had recommended me. As I had a good opinion of St. Louis dating from the tennis tournament experience while in the Air Force, I decided to look the job over. I was to fly out in mid-May, and Lenora's obstetrician said that she could accompany me. The baby was due in late June. Fortunately, she decided not to make the trip. That is, no sooner had I checked into my hotel in St. Louis than I learned that Lenora had gone into labor. My mother flew to Pullman that very day to help out and I continued my job interview. I was fortunate to be hired and almost made it back home for the birth of my daughter, Stephanie Dianne, on May 16, 1961. I have since kidded Stephanie about this because the name Rychlak can be anglicized as "Early." But now, this meant I was not present when either of my children were born. Lenora has said that, considering my tendency to be a "worry wart," this was probably a good thing. In any case, the fall of 1961 found us living in St. Louis.

As for my intellectual development at Washington State, I published my first theoretical paper in which the distinction was drawn between *procedural* and *validating* evidence (Rychlak, 1959). The point I was making is the Kantian one that human beings naturally believe what makes common sense to them, thereby allowing them to "proceed" from where they are to where they hope to get – as when a scientist designs an experiment based on the assumption that he can control circumstances, predict to a preselected criterion, and then draw conclusions from the subsequent empirically observed evidence that are "true." The purpose of experimental design is to keep the scientist's expectations and biases from exerting an influence on the data. Validating evidence has been defined as proving something through the act of "control and prediction" preliminary to observation of what takes place. I was, in this paper, being critical of those in my profession who fail to see that procedural is not quite the same thing as validating evidence: "Problems result when the clinician [e. g., a psychotherapist] takes the client's behavior, made on the basis of the client's personal procedural evidence as *necessarily* validating the clinical interpretation" (p. 648) [italics in original]. Procedural evidence is fundamental to all thought but such conceptual tests are never free of possible manipulation by the person who is using plausibility to make and defend a point of view. Experiments are designed (via procedural evidence) to take the individual's intentional manipulations out of circulation as much as possible. One puts up and then shuts up as the empirical findings roll in, hopefully as predicted. I do not think it is necessary, and in fact it is impossible, to validate every idea one puts forward based on procedural plausibility.

Note that, unlike some other critics of psychology, I have never given up on the necessity of moving to empirical validation from initial reliance on procedural evidence. This involves designing appropriate experiments (validating evidence) to put our theories (based on procedural evidence) to test. If we rely 100% on procedural evidence (plausibility, conviction, rational dialogue, emotional certainty, etc.), as many critics of psychology want us to do, I do not see how we can hope to be recognized as a legitimate science. Besides, as I will show in the pages to follow, it is *not* necessary to change the rules for evidence in psychology to prove that people are purposive organisms with an ability to choose the grounds for the sake of which they behave. Note further that procedural evidence is what we mean by theoretical evidence. On strictly rational grounds there are, in principle, N (i.e., *unlimited*) possible theoretical explanations of any fact pattern. But these explanations are cooked up after the empirical data are registered. The trick is to set up circumstances that might or might not support your theory and then let the empirically observed chips fall where they may. Experiments that are well designed play no favorites. And those that are biased can be shown (through procedural analysis) to be unacceptable. So, even as data continue to roll in, we shall – or should – have ongoing interpretations and reinterpretations of what it all means. Procedural and validating evidence are, therefore, both sides of the same coin. They complement each other and must be given equal weight in what we profess to accomplish as scientists. Of course, if we deny that psychology is a science then all bets are off. I would like to keep psychology a science, required to validate its theories whenever possible, even as I realize that my philosophical analyses rest entirely on procedural evidence.

Rather than just sit around and criticize mechanistic or associationist psychology, I decided that it was up to me to find some way of showing that people influence what takes place in their life in *pro forma* fashion, and to do so without changing the method of scientific proof that I had been philosophizing about. My theory held that people are not manipulated (efficiently caused) so much as they are Kantian-like framers of their experience (formal/final causes). Could I prove this empirically? I found a fascinating study in the literature by Irwin A. Berg (1957) in which he proved that when people are asked to look at various abstract designs and rate them along a dimension of "like to dislike," abnormal people (i.e., psychotics) reflected a different preference than normal people. Indeed, the abnormals seemed to like what normals disliked and vice versa. I considered this to be a freely selected evaluation based on an innate judgment capacity. At first I called it *reinforcement value,* but in time switched to *affective assessment* because of the mechanistic connotations of the word "reinforcement."

A typical association theorist would, of course, ascribe this judgmental difference to some kind of biological (inborn, etc.) tendency and not to a truly free decision rendered by the person. I was to spend years meeting such challenges. But at this point in time, I settled for the Berg procedure and had college students – as experimental volunteers – rate for likeability what used to be called "nonsense syllables" but are now called "consonant-vowel-consonant (CVC)" trigrams. Some examples of such trigrams would be: RIB, LAT, COS, and RAX. They are assembled by simply aligning *all* of the letters in the alphabet in the different CVC combinations possible. My experimental participants would rate each of these trigrams on a four-point dimension of "like much, like slightly, dislike slightly, and dislike much." To ensure judgmental reliability, I had the participants rate the trigrams on two occasions, with one week intervening, and then classify those as *liked* only if rated positively on both occasions or *disliked* only if rated negatively on both occasions. This enabled me to make up unique lists of liked and disliked trigrams for my participants. A few weeks later, I had them look at a series of such prerated trigrams and then try to recall as many as possible (*see* Rychlak, 1994, pp. 192–194). Later I used actual words like grass, door, rain, sugar, etc., in such a format.

As predicted, my participants recalled more of their liked than their disliked verbal items. No big surprise. Traditional association theory would say that this was due to the fact that some of these truncated, word-like sounds of the CVC trigrams were culturally shaped to bear a positive while others bore a negative value. To meet this criticism, I identified participants who had rated the *same* list of trigrams in opposite directions – some liking what the others disliked and vice versa. These participants learned according to their personal judgments of likes and dislikes. The traditional associationist still argues that it is the unique past shaping of participants that matters, so that although they do not follow some common cultural probability of preference, people only do so because of their unique past history of shaping (i.e., external rewards and punishments). One wonders how an explanation like this can ever be put to test or disproved. I found such claims too slick and simple-minded, but they had to be met, so I rolled up my sleeves.

Saint Louis University

St. Louis University is a Jesuit institution, and I was reared a Roman Catholic. I am loyal to these roots, even though I cannot be considered a religious zealot by any means. I have had to pay a price for my religious identity as a psychologist, for it has tended to undermine my teleological theoretical position concerning the description of human behavior. Some of my colleagues have dismissed my views on free will as simply a manifestation of my formal religious education – my past shaping – which is kind of funny when you consider that it has amounted to just four years in an elementary school. I am pretty sure that I believed in what is called "free will" when I was attending kindergarten at the public school, in the sense that I considered myself responsible for many of my personal choices. This is an assumption that people affirm based on common sense (procedural evidence), and must then be bullied out of by eager empiricists who cling to simplistic conceptions of human nature. Here is where you find the zealots.

A problem that I have had to face in my theoretical–philosophical writings is that in order to convey my ideas, it is necessary to introduce a number of concepts with which psychology is not familiar. In an effort to correct this limitation, I wrote my first book, entitled *A Philosophy of Science for Personality Theory* (Rychlak, 1968), and thanks to Bob Rooney had it published by Houghton Mifflin. This book was reasonably successful, went through a revision, and has sold out every copy printed. But it did not really solve the problem I had of "falling between the cracks" in psychological theorizing. I had reviewers of my research submissions say that they could understand the experimental design, but not the proffered theoretical explanations. Well, my reply was that this was due to their poor academic preparation. Philosophy was historically close to psychology. The so-called father of psychology, Wilhelm Wundt, held academic appointments in both disciplines and did not want to separate the two in the education of psychologists (*see* Boring, 1950, p. 325). On the brighter side of my career development, Bob Rooney saw to it that Houghton Mifflin offered me a contract to do an introductory personality and psychotherapy text. My goal was to do something substantial because in the late 1960s the books in this area were, in my opinion, either too "Mickey Mouse" or fraught with misunderstandings of the classical theorists.

I spent one year studying Freud's works, the next year studying Jung's works, six months on Adler, and so on, until after four years I had a manuscript. I took detailed notes during my studies and have used them in my work ever since. Unfortunately, Bob had left Houghton Mifflin by this time. He was the sort of person who did not put the "bottom line" above the pursuit of excellence and the development of talent. My introductory textbook appeared when I reached Purdue under the title *Introduction to Personality and Psychotherapy: A Theory-Construction Approach* (1973). It has been a successful text, but to my disappointment the philosophically oriented theory-construction chapters are not what sell the book. It is the in-depth coverage of the personality theories that sell it, and, it is unfortunately considered by many to be too difficult for today's undergraduates to grasp. It tends to be used in graduate coursework, which is OK by me. But I can not help wondering why the complicated biological, computing, and statistical courses offered today are never judged to be too difficult for undergraduates. Publishing a book on personality fixed my image as a "personality psychologist." Of course, I was very interested in personality, but my efforts to examine and trace the influence of theories on our conception of human nature is what I am really after. I was also beginning to get deeper into human learning, covering such topics as cognition and memory. In other words, I was attempting to study and write at an abstract level, one that would equally subsume the works of Freud, Jung, Rogers, Skinner, Hull, and so forth. I call my approach Logical *Learning Theory* (LLT).

Logos has Greek roots meaning *reason* (discourse, definition, etc.), or the word by which inward thought is framed and expressed. As I was attempting to understand learning *from* an introspective perspective, and believed that people predicated their understanding in this manner, the use of "logos" in "logical" worked out perfectly.

I began to direct Master's and Doctoral studies relating to LLT. It was an exciting period of my career in which the experimental results were very encouraging. My students and I found that subjects with an abnormal diagnosis (psychotic or neurotic) failed to learn their liked words or trigrams more easily than their disliked. Indeed, they quite often actually learned disliked items statistically significantly faster, and with better recall, than liked items (Rychlak et al., 1971). Those who come at life with a generally normal, positive predication, extend such meanings more readily than negative meanings. They expect life to be pleasant most of the time, cooperate with others, and apply constructive effort to correct their errors. Those who predicate life as generally negative extend such predications more readily than positive meanings. They hold grudges, rationalize their misconduct, or consider themselves victims of the misdeeds that others have supposedly sent their way. We later were able to demonstrate similar negative learning styles among people who have low levels of self-confidence (August & Rychlak, 1978). We then showed this influence more clearly by asking high school students to perform in a learning task they either liked or disliked. As predicted, we found that those who liked the learning task showed the typical advantage for liked trigrams, and those students who disliked the task showed the reverse facility (Rychlak & Marceil, 1986). All ratings of trigrams were done *idiographically,* that is, by the individual person who was recruited to perform in the learning task.

We had to face the usual criticism from colleagues who contended that my concept of *affective* assessment was merely another form of cognitive association. Great care was taken to frame experiments proving that *affective* assessments of learnable materials could be demonstrated when frequency of contact - or strength of association – was held constant across experimental conditions. Several studies had participants rate trigrams and real words for both affective preference and for assorted frequency instructions such as whether the items were word-like, familiar, usable in a sentence, easy or hard to learn, pronounceable, and so on (Rychlak & Nguyen, 1979; Tenbrunsel et al., 1968). In other words, participants were asked to rate the same list of trigrams or words for affective preference on one day, and then on a second day they rated these trigrams for word quality, or judgments of whether they were easy or hard to learn (this order was counterbalanced over the two ratings). In study after study, it was made ever clearer that an affective assessment is not identical to a judgment of the strength of association of learnable items. Indeed, in cross-validating factor analyses, the affective instruction (like vs. dislike) was shown to be independent of the association measures (familiarity, pronounceability, etc.). In fact, these opposing instructions were found to be essentially orthogonal (Rychlak, Flynn, & Burger, 1979). Such findings surely argued against any effort to "reduce" affective assessment to association. I began now to introduce specific names to the theoretical concepts of LLT.

I felt we needed a way of expressing formal and final causation that is different from the efficient causation of traditional association theory. Therefore, I began referring to a telosponsive process instead of the older *association process*. To telospond is to behave for the sake of a *precedent (pree-see-dent)* meaning that has been affirmed and now acts as a Kantian frame of reference. This reference frame extends meaning in a *sequacious (see-qway-shus)* manner. The concept of sequacious extension captures the fact that precedent meanings are *necessarily* extended, so that what goes on in human cognition is not an associated "effect" but rather a framing "cause." People do not "respond" as the efficient-cause concept has it, people "telospond." We see this precedent-sequacious flow of telosponsivity summarized in the old saying: "If it looks like a duck, walks like a duck, and quacks like a duck, *it's a duck!"* The precedent meanings force the inference, which is where human intention springs from as well. Over the years I have come to use *predication* and *telosponsivity* interchangeably. What is important is to realize that we have here an abstract intentional process to contrast with the abstract stimulus-response process underwriting classical association theories. It is *Stimulus-Response* vs. *Precedent-Sequacious.*

Lenora and I bought our first house in St. Louis. It was a huge, three-story Georgian colonial ten foot ceilings, red brick, slate roof, copper rain gutters, on a beautiful but declining boulevard. It was also in need of repairs, which except for a new furnace were not very serious. The house was quite reasonable but it emptied our pocketbook anyhow. For eight years, we painted, refinished floors, landscaped, shopped at garage and estate sales for carpets and furniture, and turned that place into what was for me a mansion if not a palace. For a guy reared in a cold-water flat to wind up in that home was like winning the lottery. In fact, I used to dream about it regularly once we had left St. Louis. Lenora finally commissioned an artist to sketch a picture of it for me. It hangs on our wall at the present time. The kids had a great time in that house too, hiding in the three-stories of rooms or sliding down the banister. But when we sold it we lost several thousand dollars. Well-spent loss, I would say.

I have great memories of St. Louis, where my family settled into a real home and my career began to crystallize. My children were very successful in their school work. Ron began to show the remarkable athletic ability that he possessed. I did some coaching with him, especially in baseball and boxing, but this was nothing like the training that my father gave me. Ron began playing in little league baseball. Stephanie was always at the top of her class and began showing the leadership qualities she would carry on into adulthood. Her organizing skills and speaking ability are outstanding. Lenora held a state-level PTA office and received an award for her work for a political party. We obviously had a nice, traditional family life. There was the sorrow of losing Lenora's father in 1969.

The Department of Psychology was young and dynamic, under the excellent leadership of Don Kausler. I attracted several graduate students, who, as noted above, helped me carry LLT forward. But I began to see that gradually I was moving from a full-fledged clinical psychologist to more of a general theorist subsuming this and several other specialties in psychology. I was becoming increasingly aware that to accomplish what I wanted I would have to spend less time in the clinic and more in the laboratory. To be honest, I was finding clinical work repetitious and therefore just a little boring. It had the old glamour only when I would come upon a very interesting client. I had passed my ABEPP exam in 1962, but except for validating me as a clinical psychologist it did nothing for me. I have no regrets.

My self-confidence was raised, and it was in this year that I also began jogging, a healthy practice that I have kept up to the very present. But there was still a nagging irritation that we could go on making a living as clinicians, spinning our theories of a teleological nature, yet the "lab guys" in their white jackets would just ignore us and say "Basic science proves that human beings are mechanical robots" (or words to that effect).

Well, I did not (and do not) believe that basic science proves that people are robots. My studies of the philosophy of science were convincing me that it was the theories of the mechanists that provide this "proof" by way of interpretation (procedural evidence) and not properly understood experimental research. I was sure that my telic interpretation could be equally substantiated in a rigorous experimental context. When a job opportunity came up in 1969, enabling me to shift from clinical into personality at Purdue University, I was only too glad to move on. Mark Stephens, a friend from Ohio State, who had been working at Purdue for some years, recommended me for this position. So, we sold my beloved mansion, packed up the furniture in a huge rented truck, and made our way to Lafayette, Indiana.

Purdue University

We had difficulty finding a suitable house in West Lafayette, where Purdue is located.

There were very few places for sale in 1969. However, we did find a marvelous house about five miles south of Lafayette, the sister city of West Lafayette. It was a garrison colonial on an acre of land, and it captured Lenora's fancy as deeply as the St. Louis house had captured mine. Our children loved the place too. They attended good schools and excelled as usual. In addition to a dog and a cat, we got a second car, a red Volkswagen camper that became my personal treasure as Lenora gleefully took over the Mercedes.

Gasoline was a major item in our budget as the kids moved through grade and high school.

Joining a major psychology department as a full professor was a distinction that I did not take lightly. I worked all the harder, enjoying every moment of it. I was very impressed by the efficiency of the Department of Psychological Sciences, as it was to be known. Jim Naylor was the Head (actual title), and I found him to be an excellent administrator, with high standards and a sense of fair play. The plan was for Donn Byrne – who joined the faculty the same year I did – and me to form the nucleus of a new personality program in the department. And so we did. In time, I was asked to sit on the faculty of clinical psychology as well – which I agreed to do. Despite our theoretical differences, Donn and I got along very well, with mutual respect and support.

Fortunately, I had a number of students take an interest in LLT and the empirical work mushroomed. Even though our studies were well designed and had important findings, I could not interest any granting agency in supporting the work. There was a stretch of ten years in which I submitted extensive research applications to governmental agencies only to be denied. There was usually one judge of the submissions panel who thought my work was a "breath of fresh air," but the rest simply could not grasp the need for such theorizing. Stimulus-response, input output, cause-effect, independent-dependent variables – all the *same* efficient cause concept ruled the day! I would just go ahead with the proposed work anyhow and get good results. Publishing was not easy, but I managed to get my work into print through persistence. I have had some explosive exchanges with editors. It once took me five years to publish a certain paper, but I did eventually wear down the editorial staff.

I conducted experiments with college students, mentally ill patients, and students in the school systems of West Lafayette, Lafayette, and Indianapolis. This early work as well as a preliminary statement of LLT was published in a book entitled *The Psychology of Rigorous Humanism* (Rychlak, 1977). I was beginning to get some visibility in academic circles, receiving invitations to speak at departments and various conventions. Whenever possible, I tried to take my family with me on such events. We traveled to Europe a couple of times while I was at Purdue. Lenora and I took a memorable speaking tour around the Los Angeles area during a sabbatical leave in 1977, and I spoke at four or five departments of psychology. I also wrote the manuscript of a book entitled *Discovering Free Will and Personal Responsibility* (1979) during this semester's hiatus. Actually, I had begun to write widely on various topics relating to human agency. These were not research reports, even though they sometimes contained references to my empirical work. As it turned out, my reputation in psychology developed into that of a philosopher-theoretician, and the experimental side of my career was not widely understood or appreciated, although I did have some followers who knew what I was trying to accomplish.

Our children continued doing well. Stephanie graduated from high school as the valedictorian. She was also a leader, participating in school government, as well as homecoming queen, cheerleading captain, actress, and orator. Ronald was a school leader, a Merit Scholar Semi-Finalist, and an exceptional athlete, earning not only all-conference recognition in baseball, football, and wrestling, but ranking at the state level in all these sports as well. This naturally pleased – and even amazed the "ex jock" in me. He was offered college scholarships in football, but did not have to take that route as he earned a full academic scholarship to Wabash College, where he was eventually to become student body president. Stephanie was awarded merit scholarship assistance at DePauw University. She graduated summa cum laude. Ron earned a degree in law at Vanderbilt and, after working in corporation law for a time, assumed a position as a law professor at The University of Mississippi (Ole Miss) where he is currently an Associate Dean of the Law School. Stephanie worked in business for a few years, and then returned to earn the Ph. D. degree in developmental psychology at Loyola University Chicago. Both Ron (to Claire Lindsey) and Stephanie (to Todd Stilson) were married and have turned Lenora and me into proud grandparents. Stephanie opted to spend most of her time rearing her two young sons, and is now the Education Specialist at Junior Achievement of Central Indiana. Ron has five daughters and one son to keep him busy, but his writings have brought him scholarly visibility that is truly distinguished.

I spent 18 unexpected months as interim Head of the Department of Psychological Sciences at Purdue (from June 1979 to December 1980). I had never aspired to an administrative position, and only took this assignment because the department was under some strain internally. Jim Naylor had stepped down and a replacement could not be agreed upon from the standing faculty. A search for the new Head was called for, and that would take time. Possibly it was my lack of desire for such work that got me the interim assignment. Fortunately, I had an excellent Dean (Bob Ringel) who helped me over the rough spots and was always "there" for counsel. I found this interlude in my strictly academic career to be an education. I could see how that kind of work could be interesting as well as challenging. I admired my fellow department Heads for their abilities to manage. One of the more eye-opening things I learned is that not all problems are solvable because the people concerned really do not want to solve them. There is always a political aspect to any problem solution, and I was amazed to learn how political academics actually are. I did what had to be done to run the department in the mornings and devoted the afternoons to revising my personality-psychotherapy textbook. There were some tough spots, as when following a building extension I had to decide on new space allotments for the various programs in the department. Talk about your cat and dog fights! But, all in all, I think a grade of "B" was appropriate for my performance as interim Head.

As for developments at Purdue in the empirical study of affective assessment, I will just give a selective overview of the 100 or so investigations that were conducted. Most of these studies were done as Master's or Doctoral degrees. We found that in a learning sequence, proceeding from the memorizing of disliked to liked trigrams resulted in a highly significant nonspecific *positive transfer,* whereas taking the other direction had no such facilitative effect (Rychlak & Tobin, 1971). The implication here is that, faced with homework including both liked and disliked subject matter, it would seem wise to undertake one's disliked subjects first. Then the following study of liked subjects would benefit from the lift of completing something disliked. Affective assessment effects were found to play a role in memorizing the names of other people and also recognizing their faces under brief (tachistoscopic) exposure (Rychlak, Galster, & McFarland, 1972). A surprising finding, but one that has been replicated several times, is that black participants have larger "like-dislike" affective differences in their learning of trigrams and words than white participants (Rychlak, Hewitt, & Hewitt, 1973; Rychlak, 1975). An interesting combination of affective assessment with personality test scores was undertaken. Subjects of certain personality characteristics were required to learn words that were consistent or inconsistent with their personality test scores. They first rated these words for likeability. To give one example, it was found that when a participant with *a submissive* personality is asked to memorize words with an *ascendant* meaning (e. g., *competitive, persistent*) an affective reversal takes place so that more of the disliked ascendant words are remembered than the liked ascendant words (Rychlak et al., 1973). No such affective reversal was noted when submissive participants were given submissive words to memorize that were either liked or disliked.

We found evidence that affective learning styles are not uniformly "one way." That is, if a person has a problem or negative attitude in some area of life, words taken from this meaningful realm will show the "negative over positive" influence on memory. But if the life area is not a problem area, the typical "positive over negative" recall is found (Rychlak, Carlsen, & Dunning, l974). For example, if a person hates baseball but loves tennis, we are likely to find more negative than positive word meanings recalled in the former realm and positive over negative word meanings in the latter realm. Once again, keep in mind that it is the person who does this affective rating (idiographically), *not* some controlling environment. We tried to show a certain self-fulfilling prophecy tendency in human behavior. We argued that personality test scaling "worked" because such measurements tapped the person's ongoing life predications. To test this, we had a number of choice points designed so that it was possible to predict the selection a person would make based on a high personality score. Thus, on the one hand, an introverted person would be expected to select an alternative in which he or she would say little at a birthday party in preference for enjoying the discourse of others. An extraverted person, on the other hand, takes the opposite approach and speaks "right up." We found this to be true only when the person *liked* being introverted or extraverted. If there was some reason for the person to dislike his or her personality style, this prediction did not hold up (Gruba-McCallister & Rychlak, 1981). Such self-evaluations obviously play an ongoing role in the actions of people.

We then conducted a few studies relating to brain-lateralization (Rychlak & Slife, 1984) and cognitive processing (Rychlak & Williams, 1984). I have skipped over several other research projects focusing on affective assessment, especially those which proved that it was not possible to "reduce" affective assessment to an association measure.

Nevertheless, whenever we submitted one of our studies to the "leading" experimental or cognitive journals, we were routinely rejected on the basis that "maybe" there were other factors accounting for the findings. This gave me chronic tension and some resultant physical symptoms. Thank God for jogging. Rather than tie myself in a knot I could always go out for a relaxing jog – never fast or too long, but always helpful to the mind and soul.

The early years of the 1980s were traumatic ones, as a number of loved ones passed away during a two-year period – including my father, my brother Don, and cousin Antone. At about the same time, an old friend from Saint Louis University, Father Dan O'Connell, contacted me. He was now on the psychology faculty of Loyola University Chicago, and they were having a chair funded in humanistic psychology that he felt I might like to occupy. I resisted at first. Stephanie had one more year to go at DePauw, and we wanted to be close to her. However, Jeanne Foley, chairperson of psychology at Loyola, was very understanding of my circumstances, and things were worked out so that I could assume the chair in the fall of 1983 instead of 1982. Moving to Chicago amidst the funerals detracted from the pleasure of a new career challenge for me. And I know that Lenora found it very difficult giving up her home in the country. But, the kids were now on their own, and a shift to the Chicago area would not only put us closer to my now widowed mother (who still lived in Cudahy), but also provide some excitement to our lives. So, once again, we packed up and moved on.

Loyola University Chicago

I assumed the Maude C. Clarke Chair in Humanistic Psychology at Loyola University Chicago in the fall of 1983. Maude was living when we arrived. She attended my inaugural address, and Lenora subsequently had a dinner party in her honor. A marvelous human being, a down-to-earth, sincere kind of person, she was an ex-nurse who held the rank of Lieutenant Colonel during World War II. I believe that my appointment was the first chair in humanistic psychology to be given in the United States. The appointment was actually dual, in that I was also made a professor of philosophy, although I never actually performed in that formal capacity. I am not a humanistic psychologist in the sense that many of my colleagues use this term – as someone interested in studying only "the higher" experience of human beings, refusing to "objectify" people in experimentation, and so on. The term *teleological psychologist* would suit me better. As noted earlier, I am a bit put off by humanistic theorists who demean the traditional forms of experimental validation in favor of a more discursive proof based on analytical argumentation or "dialogue" that relies exclusively on procedural evidence. I sometimes feel a little guilty about this state of affairs in psychology because my writings have helped to spell out the limitations on certainty that the logic of scientific proof entails. In my opinion, too many colleagues have taken these limitations to mean that science either proves nothing or proves anything that a scientist wants it to. I sometimes fear that we are approaching nihilism in certain circles of psychology today. The tendency to confuse "theorizing" with "proving" is rampant. I have tried to keep these two sides of the scientific enterprise clear and distinct in my work. Lenora has always given me editorial assistance in my writings. I do my own typing, even on final copy, but she is the chief editor of the work. It seemed a good idea in the new environment for her to actually attend the job with me as an executive assistant. We were not seeking additional salary, of course, but I wanted her to have an office at the school. The administration found adjoining offices for us, set outside the Department of Psychology proper, but in good proximity to the clinical faculty. Both offices were on the tenth floor overlooking beautiful Lake Michigan. Actually, my chair was not limited to clinical psychology, although this was my primary contact and I attended this subfaculty's meetings. But I had graduate students taking degrees with me in all four of the areas offered at Loyola – clinical, experimental, social, and developmental. This is as it should be, because I have come to think of myself as the author of a theory that is highly abstract and which can therefore subsume theories in many specialties. I deal in the image of humanity in all its manifestations.

Lenora looked after the budget for the chair, made all of the travel arrangements, handled supplies, carried out editorial and library duties, and made sure my two graduate student assistants were paid their salaries regularly. Lenora is more gregarious than I am, and therefore presented a fine image for the Clarke Chair. She seemed to know everyone in the ten story building we worked in. What I especially liked is that she accompanied me on most of my travels to give papers (we paid her expenses, not the chair). I usually gave five or six papers a year, a couple out of the country. Over the 16 years during which I held the Clarke Chair we visited many countries all over the world. As for our personal residence, we moved into a large, vintage condominium near Lake Michigan in Evanston, a suburban community of Chicago. The condo is only about three miles from the campus, and occasionally we would walk to work when the weather was nice. I have kept up my jogging, doing three miles at a moderate pace several times a week.

As I was deliberating whether to take the Clarke Chair, I considered what this could or should mean to my career. Obviously, it would be the last academic appointment I would fill. I was about 55 years of age at the time. After much soul searching, I decided to consider this an opportunity for me to change research directions from the exclusive emphasis on affective assessment to a more in-depth analysis of the predication process per se. I would then wind it all up by writing a book detailing LLT and the evidence pro and con. This was the game plan and I am pleased to say that it was pretty well fulfilled. I continued writing many theoretical articles between 1983 and the year of my retirement, 1999, when I was also given the honor of emeritus status at Loyola. [In 2003, we moved to Carmel, Indiana, a suburb of Indianapolis where Stephanie's family is located.] A basic theme in these many articles was that modem psychology has followed the lead of British philosophy, which dropped dialectical cognitive processing from consideration, and consequently lost an opportunity to describe human agency. Psychology was beginning to herald a "new era" of cognitive theory in the 1960s, where computers were taking over and the older learning theories were supposedly being tossed into the trash can. I strongly disagreed with this fairy tale. The names had changed but the content had remained the same. Thus, the "stimulus" of the old theories became the "input" of the computer models, and the "response" became the "output." Efficient causation was not altered here one iota, even with the addition of a "feedback" concept. Association and mediation were still what shaped people, now pictured as robots rather than telephone switchboards. I was eventually able to pull my criticisms of computer modeling together into a volume entitled *Artificial Intelligence and Human Reason: A Teleological Critique* (1991). I received an "award" from the computer lobby for this book's contents. They actually ridiculed my critique but did not address my arguments. I did not feel any shame from such treatment, which I had gotten used to in dealing with psychologists. I was actually tickled by it because I had clearly "rattled their cage."

In LLT, the assumption is made that the bipolarity of dialectical reasoning is as important to cognition as unipolarity, and is the ultimate source of human agency or free will. Humans do not first learn "hot" as a singular term and then learn "cold" singularly before the two terms are associated together. They necessarily grasp *both* meanings of this bipolarity at the same time insofar as they *really learn* either side of the opposed meanings. They also can learn to link an antonym to a word (e.g., *approve* linked to *reject*) *as* readily as a synonym (e.g., *decline* linked to *reject*) (Rychlak, Barnard, Williams, & Wollman, 1989). This line of empirical research also proved that people rely on oppositionality in their learning as often as they rely on nonoppositionality. Participants who are given instructions to look for either opposite or nonopposite sentence meanings can make oppositional decisions with equal or greater speed than nonoppositional decisions. Evidence was found supporting our prediction that so-called "depth of processing" was facilitated by unrecognized oppositionality in the experimental words used (Rychlak & Barnard, 1993). A frequent claim made by mechanistic theorists is that it is past frequency of contact with anything reasonably contiguous, including word-meanings, which determines how well learning proceeds. People learn through such ongoing associations, which shape their behavior. We proved that impression formation can be influenced by opposite meanings even when frequency and contiguity are removed from consideration (i.e., held constant) (Bugaj & Rychlak, 1989). I took such findings to mean that oppositionality is not simply another form of past environmental shaping. People are able to take such shaping in a direction opposite to that which is intended by the "shaper." Here is where affective assessment would play a role. As agents, people sometimes do what they like (or "want") to do no matter who demands otherwise. Any parent can attest to this basic human capacity.

The final area of research I will mention presents some of the most interesting evidence for the validity of predication. We did several experiments to support the claim of LLT that predication is a logical process whereby meaning is extended from a broader context to a narrower, targeted context of meaning. This meaning extension is presumed to be independent of linguistic syntax or the passage of time. It relies on formal causation and not efficient causation. An efficient cause must, by definition, function over the passage of time – as when one billiard ball collides with another, sending it along in some direction. Formal causes rely on the meaningful organization of varying patterns. According to LLT, the meaning flow in cognition is from a predicate to a target. In practice, this organization would be seen in a sentence like "John is reliable." Here, the predicate term (reliable) is extended to the targeted subject term (John). As a process we see the predicate term assuming a wider range of meaning than the subject term. This is not in the word per se. Words are mere *contents* in the predicational *process*. That is, we could say "Reliability is John" in which case the predicating meaning continues to bear a wider realm of significance, taking John's nature as a framing realm that includes reliability, presumably along with other such characterizations. This line of theorizing presents the cognitive process, not as pushed along from antecedents to consequents via efficient causation, but patterned into meaning via formal causation as metaphors, analogies, and so forth. And when the person now behaves "for the sake of" this framing (or patterning), we would add a final cause (intention) to our description.

We deduced from this line of theorizing that when unrecalled sentences are cued with their predicate word meanings, there should be greater memory retrieval than when such unrecalled sentences are cued with their targeted subject word meanings. Thus, if we presented "John is reliable" to our research participants along with many other such sentences and then later asked them to recall as many of these sentences as possible we would find that only a small percentage could be recalled. Assuming that "John is reliable" was one of the unrecalled sentences, what would be the best strategy for facilitating its recall – to cue our participants with – the word "John" or with the word "reliable?" Association theory really does not provide grounds for such a prediction, unless one wanted to say that the first word in a sentence would necessarily be the (efficient) "cause" and hence should trigger recall better than the word that comes second as an "effect." This line of thought would predict that "John" would be the better cue of the two possibilities. Logical learning theory would say that predication begins with the broader predicate meaning in the process extending or "flowing" to its specific target. Hence, "reliable" is the better cue. Four experiments were conducted to test the LLT prediction in various ways, and the data consistently arrayed in its support (Rychlak, Stilson, & Rychlak, 1993).

Thinking of people as predicators seems to have considerable validity. I worked hard to present this all in a tightly developed defense of my views, resulting in what I jokingly call my "magnum opus." I refer here to my book entitled *Logical Learning Theory: A Human Teleology and its Empirical Support* (1994). I subsequently took LLT into a more broadly conceived topic in my book *In Defense of Human Consciousness* (1997). I interpreted unconsciousness as a form of *unipredication in* which the person is unable to predicate alternatives, or draw inferences as in the act of what I called *transpredication* (see Rychlak & Barnard, 1996). In 2003, *The Human Image in Postmodern America was* released, in which my emphasis on predication takes center stage in a survey of modern times. All of these writings construe people as agents, as responsible for much of their life outcomes by way of the framing predications they intentionally affirmed. So, as happens with many academics, retirement has not really ended my efforts to get psychology to accept a more human image of people. In the closing section of this chapter, I will give an overview of what psychology would be like had it taken the path I have followed. This implies that I had, at best, a very slight influence on psychology and actually have been generally unsuccessful in my career efforts. This is essentially true, but *I am not deeply saddened!* I arrogantly consider it "their fault" and not mine. I ran a good race. Most of it was fun and I would do it all over again with only minor changes. Besides, I honestly believe that one day psychology will be forced to adopt terminology similar to mine to accurately capture human nature. There is still too much naiveté around in which an effort is made to "account for" teleology by reducing this formal and final cause concept to material and efficient causation. One day it will be realized that **this can't be done!**

A Different Psychology?

This book presents alternative approaches to psychology by different psychologists who are – as we might say in terms used by a popular television show in the United States – "*not ready for prime time!*" At least a certain number are not ready – including me, of course. For one reason or another, the image of psychology advanced by some of the contributors to this volume did not have a major impact on the direction taken by the field. I have written extensively on the historical factors that went into the decision to avoid certain alternatives in psychology (see especially *The Psychology of Rigorous Humanism*, 1977). I do not wish to go over these factors in the present context. Instead, I will consider certain aspects of the kind of psychology that my approach to the study of human behavior would generate.

First of all, psychology would retain its status as a science. Science begins with a reliance on procedural evidence in the framing of its theories (hunches, etc.). If at all possible, and most of the time, it then turns to validating evidence as it designs empirical steps to test what the theory predicts. The distinction between theory and method would be primary here. This distinction would clarify the fact that the scientific method has no control over the theories put to it, nor can it dictate what single theory accounts for the data observed. Alternative accounts of empirical data will always be possible, and hence there is an important role being played by procedural evidence. However, this does not diminish the importance of validating evidence. When validation goes against a theory advanced by the scientist, major problems arise. This is what I think is happening with the mechanical explanations of traditional conditioning theory. The brilliant survey of empirically validated research carried on by Brewer (1974) entitled “There is no convincing evidence for operant or classical conditioning in adult humans” initiated a slide into obscurity for conditioning theories that has not yet abated. Brewer demonstrated convincingly that so-called behavioral conditioning occurs only when the person under such shaping is conscious of what is being suggested in the experimental procedure, and is intentionally willing to go along with this suggestion. Despite many efforts to salvage the traditional view of conditioning as a “blind manipulation,” the Brewer findings stand as highly convincing proof of human teleology. But it is still dispiriting to see how few psychologists really grasp what this implies.

The human image that my approach advocates would surely generate some trouble in today’s culture, for LLT has continually validated an image of humanity in which purpose, choice, and personal responsibility are fundamental. One of behaviorism’s greatest achievements was to convince everyone that “positive” reinforcement was the ultimate goal in human relations. In manipulating behavior, it is the aim of behaviorists to avoid any “negative” reinforcement of behavior. Skinner used to love to point out that “A person is not an originating agent” and can take no credit for accomplishments nor blame for failures (1974, p. 168). But in a teleological theory of human behavior, it becomes necessary to take such credit and blame. According to the mechanistic school of thought, a person might say “I take full responsibility for my actions, which were destructive, and due ultimately to my previous environmental shaping.” The focus of responsibility shifts from the person’s intentional acts to the environment, which renders the admission of responsibility meaningless. I find this line of thought prevalent today, so that to hold people responsible for their failures is to punish or negatively reinforce them. As a result, the teleologist is likely to be accused of “blaming the victim.” The suggestion here being that a past life filled with negative reinforcements has injured the person who therefore is incapable today of ceasing to misbehave through personal intention and effort. Self-control and responsibility is a mirage.

This denigration of the individual’s capacity to evaluate, choose, and intentionally carry out a plan would be terminated in my psychology. This does not mean that I would outlaw mechanistic explanations of behavior. Let us simply have two schools of thought on the matter of what makes humans “human.” Time will tell which one is most instructive in capturing the human image. I have not found such willingness on the part of mechanists to allow teleological theoreticians to flourish in psychology. We all know who gets the grant money, the publications in so-called leading journals, and the "second class citizenship" in the field. There is another factor involved here, and that is the continuing – and even growing – emphasis that psychology is placing today on biological explanations in all aspects of human life. But as I have shown in my writings, biology does not do away with the human's capacity to behave as an agent over and above the demands of physiology on the body (*see especially* Rychlak (1991), Rychlak (1997)). I find too many of my colleagues ready to concede the field exclusively to biological explanations. This would not happen in a psychology of my choosing.

In closing it is only honest to point out that when we place greater reliance on people as agents we must inevitably expect more from them - which is why our critics paint us as reactionaries, putting blame on victims, expecting more from people than they can deliver, possibly trying to bring religious biases into the picture, and so forth. But in a time like today, when there is great concern expressed over the fear of terrorist attacks, etc., I find that many people are actually discovering such untapped personal resources. And even if they are not finding such inner strength, they are at least looking for it. People are putting questions to themselves that have rarely been asked before. I do not believe they will find these answers in the behavioristic theoretical language of yesteryear. We need a new language of description and analysis, one that places greater emphasis on humanity while retaining its scientifically established validity. This is the human image that psychology as I envision it would offer for today and tomorrow.

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