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

* David Bakan died on October 18, 2004, at the age of 83, at Mount Sinai Hospital in Toronto.

D. Bakan
York University, Toronto, Ontario, Canada

L.P. Mos (ed.), *History of Psychology in Autobiography*, Path in Psychology,
DOI: 10.1007/978-0-387-88499-8_2, © Springer Science + Business Media, LLC 2009

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."

Selected Bibliography

Bakan, D. (1949). The relationship between alcoholism and birth rank. *Quarterly Journal of Studies on Alcohol*, *10*, 434–440.

Bakan, D. (1952). The exponential growth function in Herbart and Hull. *American Journal of Psychology*, *65*, 307–308.

Bakan, D. (1953). Learning and the scientific enterprise. *Psychological Review*, *60*, 45–59.

Bakan, D. (1953). Learning and the principle of inverse probability. *Psychological Review*, *60*, 360–370.
Bakan, D. (1954). A reconsideration of the problem of introspection. *Psychological Bulletin*, *51*, 105–118.
Bakan, D. (1954). Freud's Jewishness and his psychoanalysis. *Judaism*, *3*, 1–7.
Bakan, D. (1955). The general and the aggregate. *Perceptual and Motor Skills*, *5*, 211–212.
Bakan, D. (1956). Clinical psychology and logic. *American Psychologist*, *11*, 655–662.
Bakan, D. (1958). *Sigmund Freud and the Jewish mystical tradition*. Princeton, NJ: Van Nostrand.
Bakan, D. (1961). Idolatry in religion and science. *The Christian Scholar*, *44,* 223–230.
Bakan, D. (1962). Suicide and the method of introspection. *Journal of Existential Psychiatry*, *2*, 313–322.
Bakan, D. (1965). Some thoughts on reading Augustine's *Confessions. Journal of the Scientific Study of Religion*, *5*, 149–152.
Bakan, D. (1965). The mystery–mastery complex in contemporary psychology. *American Psychologist*, 20, 186–191.
Bakan, D. (1966). Behaviorism and American urbanization. *Journal of the History of the Behavioral Sciences*, 2, 200–220.
Bakan, D. (1966). The test of significance in psychological research. *Psychological Bulletin*, 66, 423–437.
Bakan, D. (1966). T*he duality of human existence:* Isolation and communication in Western man. Boston, MA: Beacon Press.
Bakan, D. (1967). Infanticide and sacrifice in the biblical mind. *Midway*, 8(1), 37–47.
Bakan, D. (1967). *On method: toward a reconstruction of psychological investigation.* San Francisco, CA: Jossey-Bass, Inc.
Bakan, D. (1968). *Disease, pain, and sacrifice: Toward a psychology of suffering*. Chicago, IL: University of Chicago Press.
Bakan, D. (1971). The effect of corporal punishment in school. *Journal of the Ontario Association of Children's Aid Societies*, *November,* 10–15.
Bakan, D. (1971). *Slaughter of the innocents: A study of the battered child phenomenon*. Toronto: CBC. [San Francisco, CA: Jossey-Bass, Inc., 1971.]
Bakan, D. (1972). Should would-be change agents enter psychology? *Journal of Applied Behavioral Science*, 8, 363–367.
Bakan, D. (1973). Slaughter of the innocents. *Journal of Clinical Child Psychology*, *2*(3), 10–12.
Bakan, D. (1974). Mind, matter, and the separate reality of information. *Philosophy and the Social Sciences*, 4, 1–15.
Bakan, D. (1975). Speculation in psychology. *Journal of Humanistic Psychology*, *15*(1), 17–25.
Bakan, D. (1979). And they took themselves wives: On the emergence of patriarchy in Western civilization. New York, NY: Harper and Row.
Bakan, D. (xxxx). Politics in American psychology. In R. Rieber & P. Salzinger (Eds.), *Body and mind: Past, present, and future* (pp. 117–129). New York, NY: Academic.
Bakan, D. (1982). On evil as a collective phenomenon. *Journal of Humanistic Psychology*, 22, 91–92.
Bakan, D. (1991). *Maimonides and prophecy*. Northvale, NJ: Aronson.

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.