The Journey From The Bronx to Stanford to Abu Ghraib [1]

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In the Beginning Was The Bronx

Poverty is a relative thing; it is easier if you have relatives around to count on, and if there are others who are poorer than you. Downward social comparison was a fact of life for us generations before it became a published process. I was fortunate to have both conditions in effect while I was growing up. While affluence buys rich folks the luxury of creating physical distances from neighbors as well as selective exposure to others, for poor folks others are always "in your face." That's great if you're a kid living in a crowded urban area. For me it meant that there were always other kids available for play, day and night, right outside your house on the streets and stoops. It also meant that there were always new social learning opportunities lurking out there in the real world when those others were not your friends but enemies.

The other thing about growing up poor, which helped me to become a social psychologist, is that it encourages situational breeding, since you want to blame the situations and not the persons for all the failures you see around you. The economically advantaged prefer to rely on dispositional attributions to account for their favored status in life, since they want to believe that their radiance comes from inherent natural differences favoring them and their kind.

I learned first hand many of the lessons of social psychology, on a personal experience basis. Prejudice? I was chased and beaten daily for weeks by the neighborhood toughs until one day my mother asked the janitor's son to take me to

church on Sunday and he admitted that he and his buddies were making my life miserable because they thought I was a "dirty Jew boy," big nose, slim, blue eyes, fragile. I was six years old and sickly. Group initiation rituals? To join the East 151St Gang, first I had to fight the last kid who was admitted to the gang. I did that reluctantly, since I was so scrawny and did not like to be hurt, and did not want to hurt anybody. The bloodthirsty kids formed a circular boxing ring, screaming constantly and urging us to hit harder. The fight officially ended when the older kid gave up or the new kid got a bloody nose, which I did as soon as possible. Next, I had to climb to the top branches of a tall corner tree and bring down my sneaker that had been thrown up there by the gang leader, "Popeye, The Armenian." Scary, but not as much as having to crawl through the transom of the fruit store late at night and steal a bag full of fruit to be eaten by the gang. Finally came the strangest ritual of all to a six-year old. Around the corner was The Stocking Man's Store, a small shop selling women's stockings and undergarments. In front of the store were his goods laid out on a platform resting on orange crates and saw horses -- and also a street grill of iron letting air and light into the basement below the store. The final initiation task was to break into the basement and then look up the women's dresses as they shopped above, thereafter to tell the tales of what you had seen to the assembled gang. You were notified in advance that you could not come back up until you saw someone who had no panties; we called them "bloomers," and could regale the masses with the forbidden sexual sights that you had witnessed.

These childish urban initiation rituals seem to tap into some of the same basic aspects of masculine identity, as do adult cultural rituals reported in anthropological accounts of "primitive" tribes in exotic places. Ingratiation tactics? If you were frail,

your survival depended on learning and effectively utilizing finely honed ingratiation tactics to ward off attacks and exploitation by the big, bad kids, to get some of them to take you under their mentoring wings.

The general level of poverty in the many neighborhoods I lived in (we had moved 19 times before I commuted to college at age 18) also meant that play always revolved around group-centered, "people-initiated games," and not commercial toys or TV or solitary activities. And there was no overlap between the world of children and that of adults. They never intruded upon our world in the streets except to curtail it for dinner calls and daily taps. There was no Little League or organized soccer; nothing was organized by or watched by adults. We owned the streets; they owned their small tenement apartments. That meant bargaining, negotiating, and conflict resolution strategies were learned and refined "on the job" by us without interference by our parents. To that extent then, my earliest informal training as a naive social psychologist came bottom up, directly from the streets in this neighborhood overflowing with diversity.

In those days of the late 30's and early 40's, New York could be characterized as having many side-by-side mini ghettos, where most people living on one street were Irish; around the corner, they were all Jewish; across the street were Italians; down the corner were mostly Blacks. Often a corner candy store or grocery store or bar was the central meeting place where these ethnic divisions would blend in the quest for that particular service. My friends were an amalgam of the whole American melting pot. World War II changed everything. Poor people had jobs and made money, since the

demand for workers was high and there was not much available on which to spend your money.

Shortly after the war, four simultaneous events changed the nature of the South Bronx from a poor, but family-oriented, low-serious crime neighborhood where I loved living, into a chaotic, burned-down place to avoid. Those who had saved money during the war were able to move up the ladder and out of the old neighborhood -- mostly, these were the Jews who moved north to new housing developments. Into their space vacuum came Puerto Ricans migrating to the land of plenty, many from rural areas and farms into the heart of an urban inner city. They were in conflict with Blacks for the bottom rung of the economic ladder and new tensions ran high and often exploded into violence. Returning soldiers and Mafia contributed drugs to the South Bronx, and drugs created a new life style for local gangs, so turf meant a business domain that was guarded by guns, threats and action. Finally, as violence escalated and gangs took over, many of the other old timers also moved to safer places, leaving some vacant apartments and tenements behind. Gangs torched the buildings to get rid of the remaining tenants so they could take them over as clubhouses. Landlords, who were not making any money on their rent-controlled, dilapidated buildings also arranged to have them torched to collect the insurance. The South Bronx became a symbol of urban blight, resembling bombed out European cities.

These dramatic ecological and sociological changes were exciting for me to observe first hand. I was eager to go beyond mere personal concern to collecting data as these events were unfolding.

As a high school senior at James Monroe High School, I discussed some of these situational upheavals and their consequences with one of my classmates, a very smart, skinny kid, Stanley Milgram. He came from a more affluent neighborhood in that school district; I attended "his" high school by falsely representing my address as being other than the South Bronx. But it was clear to me that I wanted to be either a journalist or a psychologist. I was cured of both desires in freshman year at Brooklyn College by struggling to do well in English composition and by getting a C grade in Introductory Psychology (from Evelyn Raskin). That C was an unexpected, alien thorn in my academic career -- I ultimately graduated Summa, with that one C blemish. The super star psychologist on our campus, Abraham Maslow, who floated around with an ever-present entourage, was on his way to Brandeis University (to inspire Elliot Aronson), so would not be around to make up for the boring psychology texts, useless lectures, and silly little psychology experiments. I switched to Sociology and dual majored in Sociology and Anthropology, where the professors were asking big questions about the ethics of the Atomic Bomb, the nature of mass movements, and the differences between bottom-up and top-down revolutions. I glommed on to a wonderful Polish sociologist, Felix Gross, a former colleague of Bronislav Malinowski, who took me under his wing after I had taken more than 15 units of credits with him. He took me camping and always had a story about life in Academia in Europe and the need to understand the deeper structure of social phenomena and not settle for the surface appearances. After helping to review his book on European Mass Movements, Felix gave me a citation of recognition in the preface and that was so exciting to be "in print."

I also was attracted to Charles Lawrence, a sociologist of enormous talent, who specialized in race relations, and the Negro family. Charlie's infectious smile and wit were a lovely counterpoint to Felix's serious demeanor. He also encouraged me to join the NAACP and to become more socially conscious, which I made efforts to do, but was more interested in Varsity Track and Fraternity socializing.

The sociological frame enabled me to focus my neighborhood observations into several interesting studies. The first examined the dynamics of prejudice between Puerto Ricans and Negroes in the South Bronx, using interviews and surveys. It was published in a sociological journal during my junior year. The second was an observational and archive data collection of the appeal of the political parties to the minority vote in the South Bronx during the 1952 national election. My third undergraduate foray into field research emerged from observing that despite the norm of tolerance and integration at Brooklyn College, a decidedly socialist stronghold in the 1950's (called by some "The Little Red School House"), self-segregation was apparent in the student cafeteria. I set out to make systematic observations of the seating patterns of Whites and Blacks at each table across the term and over all hours– to reveal that indeed there were some race exclusive tables. No White ever sat at certain tables even when they were empty and the same was true of Blacks not sitting at "White" tables; although, of course, they were not marked as such. I replicated that study at CCNY ten years later, since Brown vs. The Supreme Court had intervened between my undergraduate experiences and later experiences as an NYU professor. The pattern of self-segregation by race was as evident in 1963 as it had been in 1952.

In my senior year, my buddy Gerry Platt, a psychology major and fraternity brother, talked me into pairing with him in Experimental Psychology. Although reluctant to get involved with psychology at first, I was soon smitten by the precision of answering specific hypotheses with "hard data." Sociologists asked the big questions but never quite had good enough answers, while it became evident that Psychologists were asking low-level questions but were good at methodology and analysis. I liked that and realized it was up to me to pose more interesting questions and maybe to do so by wedding my broad interests in the sociology of institutions with the psychology of individuals. After that course, I switched my major to Psychology. Although I was a psychology major for only a short while, the major influence on me came from Harold Proshansky, recently out of Ann Arbor, Michigan, and teaching Personality Theory. His intellectual enemies were something he called "Rat Behaviorists" at Yale. Later in the year, when I was accepted for graduate study at Yale, Hal was distressed since he wanted me to go either to Michigan or Minnesota. But he gave me valuable fatherly advice to help my transition, first about not letting those narrow S-R ideas get into my head, and then to consider changing the way I dressed since those Yalies would not appreciate the essence of my New York Ghetto sartorial style, and might reject me. Of course I said that I would not change, that they would have to adjust to me since the clothes were part of my basic self expression– the blue suede shoes, the Billy Eckstein rolled collar shirts, string ties, peg pants, and of course my Phi Beta Kappa key hanging proudly from my knee length key chain.

On Almost Being the First Black Graduate Student at Yale

Jump ahead to 1959. I have graduated Yale and am in Bonn, Germany at the International Congress of Psychology presenting my first big time paper on differentiating between the Freudian version of the concepts of fear and anxiety using Schachter's affiliation paradigm. While talking to Harold Kelley, who had been one of my teachers during my first year of study at Yale before he moved on west to UCLA, I mentioned how difficult it seemed for our Jewish colleagues to deal with being in Germany and relating to Germans, since the wounds of the Holocaust were still open. Hal floored me with his rejoinder, something like, "well it's probably similar to how you felt at Yale when the faculty assumed you were Negro." Say what? He then went on to recite the battery of circumstantial evidence that led to that reasonable assumption and a major split in the Yale Psychology Department faculty over whether I should be accepted given my record or rejected given my record. In fact, they did neither. I never received "the accept", "the reject", or "the wait list" letter from them—I got nothing at all.

On April 14, 1954, I had prepared my letter of acceptance to the University of Minnesota to work in the famous social psychology laboratory under the direction of Stanley Schachter, who called me to say that he liked my interests in race relations and group dynamics and would encourage me to develop them. That night I got a call from a Yale professor asking if I were still interested in Yale graduate school because he was coming down to New York City the next day for the EPA convention and would like to interview me for a possible position as his paid research assistant. He asked me to hold off mailing my letter to Schachter until we had a chance to talk at the bar in the Hotel New Yorker, 10 A.M. sharp. I was excited because Yale was my first choice, since it

was close to my home so I could visit often. And it was after all the Ivy League, and Yale and Harvard were the big ITS in the Bronx.

After he had two double martinis and I pretended to drink mine, and making small talk, Professor K. C. Montgomery said that he was doing research on exploratory behavior in rats and needed a good research assistant to help him carry out the many studies for which he had just received a big NSF grant. Did I know anything about "running rats?" "Yes sir" (we ran them out of our apartment regularly and deftly). Could I build equipment? "Certainly, sir as long as there is a diagram to follow (my father could be recruited to build anything with Renaissance eloquence, even rat cages, if need be). " OK, then you've got the job, free tuition and a $1700 stipend for 20 hours of research assistance. Read these reprints of mine, and come up to the lab before the term starts so we can begin breeding and building the cages." "Sure thing, you won't be sorry you chose me. I will be a good worker." I don't recall if he said, "See you later, Boy," Maybe it was my imagination.

When I got to those hallowed halls of Ivy, I quickly became a Rat Runner of the first degree. I bred hundreds of rats, nursed them, fed them, watered them, cleaned their shit and cages, after building literally untold number of special cages, by hand, some to deprive them of behavioral freedom, others to encourage it in free environment rearing, and still others to deprive them of both behavioral and sensory stimulation. We graduate student rat runners worked around the clock, during holidays (we traded care taking and running subjects duties to go home either Easter or Christmas). At first I felt like I was a slave laborer, working my RA butt off up to 40 hours a week in addition to my studies (where I did not excel since I had a weak undergraduate psychology background). I

complained to the chair, Claude Buxton, but to no avail. I called my mother just before Christmas to say I was going to quit and come home for good. Wisely she said I could do so but not until the summer since my sister was using my old bedroom and it would not be right to disrupt her in the middle of her studies (by summer I was "cutting the mustard" and had no thoughts of leaving ugly New Haven). Montgomery would give me a long To-Do list and then disappear. What they concealed was that he was suffering from clinical depression and was in and out of local mental hospitals. The next year he committed suicide.

I'm not sure if it was guilt over Montgomery's death (caused in part by not getting tenure), or the dissonance of persuading myself that living my life in the animal basement wing of the Institute of Human Relations was what I really wanted and not Human Relations which I thought I wanted when I was a know-nothing undergraduate in that Gestalt stronghold at Brooklyn College. But whichever it was, I then got totally committed to my rats, their data, and publishing our findings. I convinced someone at NSF to sign over the remaining two years and $38,000 of Montgomery's grant to me, with Professor Fred Sheffield serving as ex officio. I wrote and published four articles on this research enterprise, and on the side a few more of my own. One was with Neal Miller, whom I think of as my Behaviorist-Experimentalist Mentor Supreme, the other was with another graduate student, Herbert Barry. We shocked our faculty by getting an article accepted in Science -- that related the effects of two drugs (the new hot medicine, chlorpromazine, versus caffeine) on inhibiting or enhancing sexual behavior in male rats. And Herb and I did it all without faculty involvement. (Incidentally, I have just published an article honoring the memory and contributions of K.C.Montgomery (with Allan

Kalueff from NIMH). But here I was as a relatively new graduate student with an admittedly deficient background in psychology, barely admitted to the Yale Psychology Department, arguably the best in the nation at that time, with my own large animal laboratory, in charge of a major NSF grant, and four publications including being lead author in Science. (No, mom, I think I will stay around here a bit longer.)

I was in my third year, feeling like hot stuff, doing some re-analyses of our Science article data in the calculator room, when a faculty member, Bob Cohen, asked me what I was doing. I went into great detail about the merits of this rigorous experimental protocol. He then stopped me to ask if I would do him a favor and look out the window across to the street in front of the medical school and tell me what I saw. I did so assuming he wanted to know if his beautiful wife, Barbara, was there waiting for him. I said no one was there. He said really, no one? I then told him there were a bunch of people in one group, a couple in another, to which he asked me to try to figure out what the couple were discussing. I examined their body language and made some inferences, with the caveat that I could not be at all sure of the accuracy of my interpretation. Bob then threw the solar plexus punch. "Don't you think that it would be more interesting to spend your career as a psychologist trying to figure out what people mean by their behavior than what white laboratory rats do?" Needless to say, I was furious over being duped into this rather obvious "soft side" psychology trap. But when that emotion subsided, it made me think. I had betrayed my origins by giving up my love for observing people and trying to understand the complexities of human interactions for the ready accessibility of rat psychology.

The next term, Bob Cohen and Jack Brehm co-taught a new course in advanced social psychology that I took and persuaded my roommate, Gordon Bower, to join me. I had taken basic social psychology as an independent reading course, under the guidance of Leonard Doob, that focused on the classics but stopped sharply at 1950. The main readings of the Cohen-Brehm course were typed copies of Leon Festinger's manuscript entitled, A Theory of Cognitive Dissonance. Brehm had been Festinger's student and his thesis was one of the first experiments on dissonance theory. Cohen, who was a student of Michigan's Alvin Zander, was less a methodologist than Brehm and took more of a holistic approach to social psychology, even admitting personality interactions and promoted ideas such as "needs for cognitive clarity." Together, they were a dynamic-duo who were delightful to study under and to work with.

I was entranced by Festinger's chutzpah of drawing such wide-ranging derivations from such simple assumptions and premises. But more than that was fascinating to my peers and me. Dissonance theory went directly against the very rational, systematic, bottom-up empirical approach dominant in the Yale Attitude Change Program and even in much of the animal behaviorist research since the heady theoretical days of Clark Hull, a few years before my arrival at Yale. We got caught up in the appeal of those non-obvious predictions that challenged the validity of "Bubba-Psychology," in which everyone's grandmother can predict the outcome of any psychological study described to them. For example, in Hovland's Attitude Change course, one of our assignments was to construct a table of all existing results in that area by coding them first according to the categories of input/ mediating/ output variables, then by whether they were stimulus (communication) factors, audience factors, media or channel factors,

and then according to processes borrowed from Hovland's earlier training as Hull's student; message learning or encoding, motivation to accept or resist, message retention, and action consequences. He believed that a comprehensive theory of communication, persuasion and attitude change could be developed from such a taxonomic approach. But faced with Festinger's daring style of theory formation, this static approach immediately lost its appeal to many of us. However, I felt like a bit of a traitor since Hovland had been my first social psychology mentor and I learned much from this genius. I had worked with him on issues of judgmental distortion, did some research that was published on semantic ambiguity, and wrote my major area paper reviewing the literature on traditional psychophysical judgment and social psychological judgment. "Mr. Hovland," as everyone reverently called him, told me that he and Muzafer Sherif, his visiting collaborator, found some of it to be useful in their new formulation of latitudes of acceptance and rejection. My doctoral dissertation, jointly sponsored by Cohen and Brehm, pitted predictions from their rational formulation against dissonance theory's rationalizing formulation – and dissonance carried the day and my Ph.D. degree.

I withheld turning in my dissertation until next year in 1959, to avoid the military draft, which I could escape by turning 26 years old. It helped also to be working at the West Haven Veteran's Hospital as social psychology postdoctoral trainee, under the supervision of Aaron Hershkowitz, who was steeped in the social ecological approach of Barker and his teachers at the University of Kansas. It was different from anything I had ever studied, focusing on how aspects of the physical environment influenced individual and group responding. I benefited more, though, from the opportunity to wander the wards, talk with patients and attend clinical staff meetings. I had developed an interest in

psychopathology from taking a fabulous course, taught by Irving Janis, which met for a full day a week at the Middletown State Mental Hospital. Janis' real genius was less in experimental social psychology than in experimental psychopathology. He would interview a patient before the class; generate hypotheses about his or her behavior in response to further stimuli, which were then invariably proved to be correct. We each were assigned our "own" patient on whom we did a complete psychological work up. Although I was auditing the course, I wrote a 60-page report that I later used as course material in my introductory class as well as in my textbook, Psychology and Life.

That interest in psychopathology was encouraged by my contact with Irving Sarnoff, a wonderfully creative clinician, also just off that post-WW II train from Michigan to New Haven. He was a rare breed at Yale since he actually believed in Freudian theory and set out to show that some of Freud's ideas could be translated into ingeniously testable laboratory experiments. Together we did an elegant study to show that Schachter's association of high anxiety to social affiliation was not accurate since he was confusing anxiety with fear. We reasoned, following Freud, that fear as the reaction to an objectively valid, external threat would increase affiliation with others similarly aroused, but that anxiety as an irrational evaluation of an objectively harmless stimulus would lead instead to the desire for social isolation– which we found in an interaction between two levels of fear and anxiety.

After presenting that study at the International Congress of Psychology, using a variety of colorful slides to depict the experimental setting and the research procedure, in addition to the usual convention of presenting slides or overheads of only results, Ned Jones complimented me graciously and recommended we submit it to the Journal of

Personality, which we did. It was published in 1961. I was feeling a professional “high,” when Hal Kelley and I had that exchange about my nearly not being admitted to Yale because I was thought to be Black. Seymour Sarason later validated Kelley’s recollection of this strange tale in his memoirs. (I worked with Seymour for several years co-directing his anxiety in children project as he began to move off into community psychology). So here is the gist of that story.

Hal said that my graduate school application was “tabled” because there was a split among the faculty on how to deal with it, with me. He went on to tell me that was the case because some were sure I was a Black ghetto kid, or Mulatto, while others were less sure, but depending on that diagnosis it would change the way they interpreted my grades, recommendations, and test scores. Once the circumstantial evidence in my file was framed as coming from a minority city kid, then everything seemed to fall into place naturally. "For instance?"

Let me now briefly summarize that evidence contained in my Yale application: Interests -- listening to Modern Jazz, Charlie Parker, Lester Young, Dizzy, Miles, Lady Day; Favorite reading -- Downbeat magazine; Activities -- Captain of the Track Team; Major: Sociology-Anthropology (and also Psychology) with top grades in The Negro Family In the U.S.; and Race Relations; Extra-curricula activities -- Secretary of the local NAACP chapter; Primary recommender -- Charles Bradford Lawrence, well-known Negro sociologist, who happened to send his letter on NAACP stationary because he was out of college stationary at home and his letter was late; Research evidence: two studies enclosed, one on a publication on the dynamics of intergroup prejudice between Puerto Ricans and Negroes in the Bronx, the other was patterns of racial self segregation in a

college dining facility; My Italian name -- Roy Campanella, famous Brooklyn Dodger catcher at that time was surely Negro with an Italian name. And so it went. Even the GRE scores fit the stereotype, low math relative to good verbal scores.

It was not unreasonable for the faculty to assume this dude is Black. But wait. In those days there was also a required photograph glued to the application and that cemented the false identification. To save money on the cost of sending out many photos with all my applications, I had one of my graduation photos duplicated cheaply, ten for a dollar, and they were dark and grainy cheap copies with more contrast than the other applicant's and showed off a skinny dark young man with a pencil moustache wearing some high style Bronx clothes that were not sold at the J. Press men's clothing shop, or the Yale Coop.

Expert psychological reasoning from a false, if not unreasonable premise, went like this: Good letters of recommendation need discounting since they obviously reflected reverse biases. This young man will have difficulties adjusting to life at Yale since there are none of his kind in the department or in the university so it would be a disservice to him to admit him. Some faculty may have difficulty adjusting to him and his lifestyle, especially those from the South (like the professor who interviewed me in New York City.) But the liberals in attendance argued that it would be good to take one, even if a token one, since this one was not too bad. But if he failed and had to be kicked out, how would that look for the department? In any case, indecision ruled the day and my application was literally shelved, with an intention of getting back to me later. On next to the last day of the student acceptances, Gordon Bower, the top admit, deferred to get a Masters Degree in Philosophy of Science at University of Minnesota, and that Southern

professor with the grant and the drive to explore was suddenly without an R.A. Maybe he called those on the wait list who either did not want to run rats or by that late date had made a prior commitment elsewhere. That left only me in the null category. Montgomery calls; I say I am eligible and eager to come to Yale. But curiously he does not offer me the position over the phone. Instead he comes to New York, asks a few simple questions to which I lie, he sizes me up and offers me the job on the spot. After Hal Kelley told me this surprising tale, I thought back to that April 15, 1954 day in the bar at the New Yorker Hotel to wonder why Montgomery had to interview me in the flesh, since I could have answered those same questions during his phone call? In those days, no admitted student went through an interview process, so why did he have to see me before he could offer me a research assistantship on his grant money and thereby admission to Yale?

Upon my arrival at Yale, some of the faculty were indeed sorry to see that I seemed White when they had hoped I would be their first Black. But I don't think my boss man saw it that way. In fact, I now think that had he seen me as Black, he might have informed me that a "more qualified" applicant had already taken the job just that morning before the interview. Maybe, however, I am going too far beyond the data. But the data that I can add in conclusion are that my mixed message application and transcript also included the line items that I was Summa Cum Laude, Junior year Phi Beta Kappa, fellowship winner, Fraternity President, Varsity Athlete with a Presidential award for distinguished scholarship, and some other goodies thrown in for good measure. All of that was not sufficient to get me a seat on the first run bus to New Haven, maybe because it was negated by all that circumstantial evidence which triggered negative stereotypical

thinking even among some of the most brilliant scholars and honored psychological researchers in the land.

Although I was nearly the first Black graduate student in the Psychology Department at Yale, James Jones was heir to that claim many years later. Jim has gone on to make important contributions to the study of racism and prejudice that mark him as one of Yale's important native sons.

Learning What Matters

It should be evident from this personal travelogue that I learned early many of the most vital lessons in social psychology. From my ghetto experiences, I learned that Situations Matter and also that Culture Matters in shaping human behavior and interpersonal relationships. I learned further that Content Matters, on the basis of my student days as a sociology student and my field observations of prejudice in action and the persuasive appeals made (only) by the American Labor Party to get the minority vote in the 1952 election. What I learned at Yale were two more things that mattered a great deal to me and influenced the rest of my research career: Methodology Matters and Behavior Matters.

The latter are the two residuals of Yale Behaviorism that are not given enough credit when we dismiss the rest of the Behaviorist Manifesto: Learning via principles of reinforcement follows species universal principles that transcend content, situation, and of course, culture. It was that misguided arrogant ideology which sidetracked much of psychology for decades, just as Benton Underwood's mindless study of nonsense syllable learning of memory without content delayed the study of meaningful dynamics of memory for narrative and personally significant events. What I learned as an apprentice

to Neal Miller and Carl Hovland is the importance of conducting research that was rigorous, operationally as precise as possible, with sufficient pre-experimental observation and considerable pre-testing to assure you understood the phenomenon under investigation and how to demonstrate the causal connections your hypothesis advances.

What I got from my years of studying rats was observing and recording their behavior, rather than inferring what was going on inside their furry little bodies and small brains. So much of my research since then has focused on dependent variables that were observable gross behavior patterns and not just check marks on scales or elicited predictions about how research respondents imagine they would behave in a given situation. But what I learned from Bob Cohen is the message repeated over the years by my colleagues is that People Matter the most. Awareness of that axiom tempers the austerity of any social behaviorism with a compassion for human fragility, a respect for human dignity, and an appreciation of the complexities of the human mind. It has helped me to try to design research that is characterized by both style and substance, and an eye for applying what I have learned to improve some aspect of human functioning. I think that the research I reported in my 1969 book, The Cognitive Control of Motivation, is the best example of my attempt to wed the rigorous methodology of my Yale behaviorist training with the rich texture of social-cognitive constructs.

The Yale Candy Store

The Yale Psychology Department in those golden days of the late fifties was an enormously overstocked candy store for a kid from the Bronx, and all the candy was free for the asking. Hovland brought to the social area a horde of social psychologists that we students could learn from and do research with: Irving Janis, Hal Kelley, Bob Abelson,

Jack Brehm, Bob Cohen, Bill McGuire, Milt Rosenberg, along with guest lecturers like Don Campbell, Herb Kelman, and others, like Muzafer Sherif. And some of us got to visit with Hovland the new social psych lab he helped to create at Bell Labs in NJ, headed by Mort Deutsch and Hal Gerard. I also got to work with Seymour Sarason on his test anxiety in school children research, eventually taking over as co-director when he moved into the area of community psychology in 1959. I was fascinated by the work of Irving Sarnoff, as I noted, and we did a lovely study comparing Freudian distinctions between fear and anxiety within a paradigm developed by Stanley Schachter for studying affiliation processes. Then there was also on the faculty John Dollard, Mark May, Leonard Doob, Irving Child, Frank Logan, Bill Kessen, Ed Zigler, Norman Miller and more oldies but goodies to learn from and work with.

We had a lot of hotshot graduate students as well: Gordon Bower, my roomie and for whom I was later best man at his wedding the next year; Roger Shepard, Dave Sears, Jon Freedman, Dean Pruitt, Arnie Lyman, Tim Brock, Lyman Porter, Buzz Hunt, to name a few who come readily to mind.

I got my first taste of teaching psychology in 1957 when after taking a course in how to teach by Claude Buxton, the Chair, I was the first graduate student to be allowed to teach a full course in Introductory Psychology to the hallowed blues of Yale Men. (A professor became sick just before term time and I was the default value). I loved every moment of it and from then to now, and continue my unabashed love affair with teaching tempered by my equal passion for doing research.

I deferred my graduation for a year because our government was still drafting men to age 26 from the Korean War conscription. I got a postdoc fellowship to work at

the West Haven V.A. for a year while also continuing part-time teaching at Yale and part-time co-directing Sarason's anxiety project and publishing with him. Again as with Janis' abnormal course, I found clinical work really interesting and spent a lot of time just talking with patients and trying to understand how they had become so mentally disturbed.

I had hoped to stay at Yale as a part-time instructor for a few more years, but Miller recommended me to his buddy, Howard Kendler, for a job opening at NYU in the Bronx, and how could I resist going home again? Little did I realize that the short trip from Yale to NYU in the Bronx would be downhill a very long way. That will be our next installment.

NYU (1960-67)

When interviewing for the job at NYU, Chairman Kendler asked me what was more important to me, fame or money. "It depends," I answered, on how much of each was at stake, but in general I would prefer fame to money. He replied that he had anticipated that answer given the choices of being an Instructor for $6,500 a year versus only $6,000 as an Assistant Professor -- the fame option! He then noted that I liked to teach and he would see to it that I got as much of that pleasure as possible.

My teaching load was extraordinary -- five semester courses each term, most large lecture courses, plus two summer school courses to add some money to my fame. And even that was not sufficient to pay the high cost of living in New York, so one year I taught a 13th course at Yale one afternoon a week, a Masters course in the Education School on learning, and another year I moonlighted by teaching social psychology at Barnard College. My love of teaching turned into an addiction overload, at least 3 hours a

day in class, new preparations and grading at night, not to mention office hours, advising majors, being on the medical and dental school advisory committee, starting and leading a Psi Chi chapter. My teaching was mostly at the uptown Bronx campus but I also taught graduate classes at the Greenwich Village campus of NYU, and went weekly to the evening departmental colloquia, and found time for training and research consulting in experimental and clinical hypnosis at the Morton Prince Clinic for Hypnotherapy. And in my spare time I designed and built a new research laboratory, with funds from a new NSF grant so I could continue with my other passion, and if successful might escape the NYU dungeon.

Love Affair with Teaching

Even my horrendous load at NYU did not diminish the joy of teaching psychology that had been nurtured at Yale. Once I was at the lectern, all the daily stresses vanished; I was able to create a sense of personal flow, a total immersion into the 50-minute moment, again and again. Teaching was both my calling and my salvation. I taught large introductory psychology courses fall, winter and summer, and followed them with large social psychology courses, along with a variety of higher-level courses. That meant I could gather a bunch of students who “majored in me,” taking as many as five sequential courses. Barry Schwartz, Shep Siegel, Ken Fink, Ebbe Ebbesen, and Steve Maier were part of those golden undergraduate teaching times, as well as Ellen Langer and Allen Schatzberg, all of them now respected professors in psychology and psychiatry. I won my first distinguished teaching award and realized that to continue teaching and research I had to use my teaching as a source of research ideas and then recycle into teaching some of my research to create a synergy between these twin

passions. I was also blessed with some great graduate teaching assistants, most notably, Scott Fraser.

Research Programs

There were five lines of research that I somehow carried out during these hectic years: a) the psychology of affiliation, following up some ideas based on Stanley Schachter's work; b) developing powerful demonstrations of Festinger's theory of cognitive dissonance viewed as the cognitive control of motivation; c) conjugate reinforcement as a technique for quantifying subjective states, based on Ogden Lindsey's operant conditioning methodology; d) continuing some of my Yale work on persuasion and attitude change, and e) the newest, most exciting research based on an idea from Golding's novel, Lord of the Flies -- deindividuation as facilitating anti-social behavior. It was all within a tradition of experimental social psychology, primarily laboratory research, but with some field studies, such as vandalism of automobiles that I had put on Bronx Streets and later those in Palo Alto (that influenced the authors of the Broken Window Theory). I had a great research team for several years and lived in our little lab much of the day and evening when I was not teaching -- Ebbe Ebbesen, Scott Fraser, Matty Weisenberg and others were dedicated research assistants. My NSF research grant helped make it all possible. I also relied on many talented undergraduate research assistants, as I continued to do in my later research at Stanford.

Activism, Political and Social

I was jarred by the 1962 Cuban Missile Crises, and later the horrors of the Vietnam War. My secretary, Anne Zeidberg, shamed me into activism, since I am a non-political person whose overextended professional life left no time for personal

indulgences let alone political involvement. But she made me aware of my role in academia and the need to voice opposition to our mistake of entering and continuing in the war in Vietnam. I organized one of the nation's first all night teach-ins in 1965 and led a walk-out of students and parents at an NYU graduation (1966) against giving Robert McNamara, Secretary of Defense, an honorary degree, and took active parts in other protest movements in NYC and DC, against this mindless war and for the new civil rights movement for African Americans. I carried that sense of activism with me to Stanford where I continued to be energized against the war through a variety of activities. And I am now more fervent than ever in opposing war by our nation, especially the Bush Doctrine of pre-emptive war based on the intuition of some brain trust that another country might pose a danger to U.S. security.

Having been energized as a social change agent, I helped organize The Harlem Summer Project in 1965 that enlisted student volunteers from NYU and City College to work in a Harlem church schoolyard all summer in one of several programs I designed. We had a kind of Head Start program teaching young children the basics of reading, writing and arithmetic in small personalized settings using the newest technologies; introducing high school students to the joys of college by having them attend special lectures by top professors at both colleges, visiting dorms, varsity team practices, and meeting with admissions counselors; and a Black Pride program for teenagers that took them around the city to events that featured Black performers, such as Ella Fitzgerald in practice for a concert, photography exhibits and more. We financed the project with gifts from fund raising parties and a small grant of $2000 from the city which was mostly for free lunches.

Transformative Experiences

Several experiences that had a profound impact on my career and thinking about psychology began with my summer school teaching at Stanford in 1963 where I got to be around top-level students and colleagues of the caliber that I had grown accustomed to at Yale, and made me aware of the standards necessary to function at that high level. Sitting in on Festinger's weekly research seminars at his home on the campus was an intellectual treat.

Similarly, summer school teaching in Leuven, Belgium, in 1966 changed my lowly self-image, by making it as "the young kid on the continent" along with a remarkable team of social psychologists from the U.S. as part of the first summer school of the European Association of Experimental Social Psychology. The U.S. team included Hal Kelley, Bob Zajonc, and Hal Gerard, along with European faculty of Josef Nuttin, Joseph Jaspers, Ragnor Rommetviet, and Jaap Rabbie. We each had a small group of advanced European graduate students to lead in designing, executing, analyzing and writing a publishable research project in six weeks. The intensive collaboration, with none of my usual endless distractions, was intoxicating and I flourished. Our group did a study on deindividuation using Belgium military soldiers as our participants, and we were the first team to meet the goals of the summer school. On the way to Leuven, I stopped off in Paris at the invitation of French social psychologists Serge Moscovici and Claude Faucheux to lecture to their graduate students at the Sorbonne for a week. There were wonderfully lively exchanges with the students and my new French colleagues that broadened my perspectives on my work and the significance of psychology. Claude also introduced me to the joys of fine wine and French cuisine that we enjoyed for a week in

Paris, the most memorable of which for a poor Bronx boy was wild boar in a Madeira-chestnut sauce.

A Quantum Leap in National Visibility

I needed to get an early promotion in order to increase my lowly assistant professor salary so my family could survive. When I asked the Head of the NYU Psychology Department, Ray Katzell, if he would put me forward for this accelerated step up a year early, he made clear two things. First, some older faculty thought I was too brash and needed time to mellow (reactions against my anti-war activities), and also that although my career was moving along, I needed to demonstrate a "quantum leap in national visibility" for him to endorse early promotion to associate professor. That meant significantly increasing my publications and invited lectures. I took the bait and then worked even harder to accomplish that ambiguous, open-ended goal. I relinquished any ties to present hedonism and became an over-the-top future oriented workaholic.

Visiting at Columbia University (1967-68)

I would hang out at the Columbia Social Psychology program after my biweekly moonlight teaching at Barnard College (which is across the street) because so many fine social psychologists were there. When Bill McGuire took a leave to check out a job at U.C. San Diego, I was invited to replace him for the year, which I jumped at. But before I could start, my friend, Stan Schachter, also took leave to get married, and Bibb Latané left to work with John Darley on bystander intervention research. Before I could feel sorry for being abandoned, I met two remarkable graduate students, Lee Ross and Judy Rodin, who made my year. They were so smart, so professional beyond their years, and so creative that I loved working with them in class, in the lab and in our regular coffee

shop lunches. We published one of the first studies on attribution theory, and later I helped Lee get a job at Stanford and Judy to take my job at NYU. Lee Ross and I are still buddies at Stanford and Judy went on to have an amazing career at Yale then as President of the University of Pennsylvania, and now as head of the Rockefeller Foundation.

I am not sure whether my colleagues viewed being at Columbia to be a sign of my increased national visibility, but it must have helped in some ways to get me a new job at Stanford. One day in December, out of the blue, the Chair of Stanford's Psychology Department, Al Hastorf, called to inform me that I had been selected by the senior faculty to join them as a Full Professor with tenure! To replace my idol, Leon Festinger! To be part of the best psychology department in the world! And not to have even applied for the position! I must say immodestly that I was able to tell the NYU Psychology Department Head that perhaps this transition from untenured Assistant Professor at NYU-in-the-Bronx to tenured Full Professor at Stanford-in-Paradise might define a quantum leap in national visibility. And no thanks; I would not consider staying on, even if someone else did the windows instead of me.

However, it was hard to leave my Bronx family knowing that I might never return home again. My father had been working for me at NYU as my lab technician, a position he expanded after I left. But I most missed my wonderful mother and kid brother, Don. My wife Rose Abdelnour-Zimbardo and I decided we would separate and eventually divorce after she and our son Adam spent the 1968 year at Stanford. Although I would call Adam every week and visit with him in New York or at Stanford during holidays and the summer, it was heartbreaking not to be there for him on a daily basis, to be part of his youth. Fortunately, he came to Stanford for his undergraduate studies, and has stayed on

living in San Francisco. We are tight buddies, sharing coffee regularly near his home at Jumping Java. He is now a fine therapist with an MFT degree and a master's specialization in human sexuality.

The Stanford Decades (1968-Forever, Almost)

I was there and I was scared. I had been a biggish fish in a little pond, now I was the minnow in the tank of sharks. Hilgard, Atkinson, Bandura, Bower, Maccoby, Mischel, Shepard, Flavell, Pribram, Thompson, Sears, and many more brilliant, creative, productive colleagues abounded there. But freed from the daily grind of survival at NYU and the stressful living in NYC, I felt liberated just to have so much time to think, to plan, to develop ideas, to get feedback from faculty and students whose input was stimulating. With my new “normal teaching load,” I could focus on perfecting my teaching and have time to develop teacher training workshops, seminars and teaching manuals. Being liberated from my excessive teaching load to having only a few courses each quarter meant I could put more gusto in each of them and still have more time left over for a little fun on the side. No more hours-long commute, I could bike to work from the faculty residence in the student dormitory (Cedro) where I lived for free.

The productive juices flowed, and in short order, I wrote: an entirely new 8th edition of the textbook, Psychology and Life (with Floyd Ruch), as well as the Instructor’s Manual, Student Study Guide, Reader, and also the brief edition of that text. I then knocked out Influencing Attitudes and Changing Behavior (with Ebbe Ebbesen); Canvassing for Peace (with Bob Abelson), as well as a Nebraska Symposium on Motivation chapter, with some of my best writing, and on the side many professional articles -- in my first three years on the new job.

Psych and Life was a big hit, selling more than 100,000 copies, and made a lot of money. So for the very first time in my life, I no longer felt poor. I traded my borrowed bike for a 1955 Mercedes Benz 350-SL silver bullet convertible, the most beautiful car in the world. And I bought some new suits and joined a spa to exercise and get in shape. I played center field again with the Psychology Department's grad student/ faculty team that won the Stanford intramural league that summer. Went to my first rock concert at the Fillmore in San Francisco and at Stanford's Frost theatre as well as free concerts in Golden Gate Park. Also discovered fine wine in Napa/Sonoma Valley that changed my drinking habits permanently. I clearly was beginning to blend some hedonism with my excessive future orientation to experience more pleasure in life than my solo work act. But in Stanford's paradise, work comes first and my eternal struggle between work and play continues to this day.

Applying Social Psychology Wisely and Well Also Matters

In my own research I have tried to move back and forth between studying real world phenomena in field and laboratory settings and illustrating the applicability of my own research findings and those of my colleagues in a variety of ways. It is my strong belief that short of research done to test the conceptual adequacy of some theoretical formulation, social psychologists have an obligation to contribute to the enhancement of the human condition through research that applies what we know in sensitive and effective ways – as Elliot Aronson has shown us with the use of his Jigsaw Classroom technique for promoting cooperation among school children, an intervention that integrates minority children into the mainstream of class activity better than any other available educational tactic. I have used my various research programs as vehicles for

promoting prison reform and judicial legislation, to reducing urban vandalism, and to overcoming shyness (in my popular writing, media appearances and by establishing a shyness clinic in the community to treat shyness in adults and adolescents). For me, a new mantra emerged: Application Matters.

The Stanford Prison Experiment (SPE)

My classes usually involve students in experiential projects, either solo, in pairs, or teams. In spring 1971, I invited students in my new course, Social Psychology in Action to select from among a set of ten projects that combined sociological and psychological features (back to my undergraduate training at Brooklyn College). Teams of 10-15 students could choose a topic and then working with a graduate TA to study it in depth, and either present their project themselves in class or give me the materials to present for them. Many of the projects involved the person in an institutional setting, like elderly going to homes for the aged or people becoming prisoners or guards.

The Prison Project Team decided to do a mock prison in their dormitory with half being prisoners, the rest role- playing guards over a weekend. On Monday during class, their presentation became explosive as they described the intense emotions that experience elicited in them. Some said they could no longer be friends with others who were guards because they felt that showed their true selves. Some were in tears. After class we had an intensive debriefing, but I realized something powerful had happened in that mock prison. However, we could not separate the personal reasons these particular students chose this topic (dispositional factors) from the situational forces acting on them. Only a controlled experiment could do that. Voilá, the idea for the SPE was born after

discussions with my graduate research team of Craig Haney and Curt Banks, and the undergraduate who headed up that class project, David Jaffe.

By now, I can say the rest is history and a history lesson told on our web site, www.PrisonExp.org, developed by former graduate student, Scott Plous, and his student, Mike Lestik. A Google search of "experiment" reveals it to be the top site out of more than 300 million results worldwide. The SPE and Milgram's obedience research are the bookends of social psychological research that demonstrates the power of situational forces over individual will to resist. Again, curious that little Stanley and I were classmates senior year at James Monroe High School in the Bronx and ended up doing comparable research on situational power.

The SPE has had a profound effect on much of my thinking about power and evil and even about better ways to teach in a more truly democratic fashion. It also changed the lives of some of the participants, graduate student Craig Haney, and former graduate student, heroine of the SPE who forced its early termination, Christina Maslach (now Mrs. Zimbardo), and our consultant, Carlo Prescott, formerly incarcerated for 17 years.

After nearly 35 years, I have finally gotten around to do a serious write up of the SPE in a detailed daily chronology with a whole chapter devoted to each of the six days in that basement dungeon and other chapters focused on the first day of the police arrests and the parole board meetings. I wrote it all in a cinematic style, present tense, first person narrative, with minimal psychological intrusions. Several subsequent chapters deal with its meaning, ethics and the many extensions and variations it spawned.

The Abu Ghraib Little Shop of Horrors

The reason for finally writing such a book about the SPE is the confluence of its messages and visual images with those of the abuses and tortures by American Military Police prison guards at Iraq's Abu Ghraib prison. The scandal broke on national news in April, 2004 when the set of horror images of "digitally documented depravity" surfaced from the cameras of the MPs doing those dirty deeds to national and world wide media by the heroic whistle blowing of army reserve Specialist Joe Darby. The images of naked men, of prisoners with bags on their heads, of sexual humiliating poses all brought back a rush of ugly SPE memories long stored away in the crevices of my cortex.

Military leaders and Bush administration leaders all madly rushed to the usual dispositional tactic of blaming the grunts, the perpetrators of evil as the "few bad apples." Such an attribution immediately centers the focus on the characteristics of those "rogue soldiers" and takes the System off the attributional hook for contributing to such violations against humanity. I immediately countered with the banner of social psychology proclaiming that it was probably a "Bad Barrel" -- the situation -- that had corrupted these formerly good American men and women soldiers.

Thus emerged the idea of a new book that sought to understand how such abuses could happen based on the analytical tools of social psychology and particularly what I had learned from ye olde Stanford Prison Experiment. I became more like an investigative reporter than a social psychologist researcher by first becoming an expert witness for one of those MP guards, Sgt. Chip Frederick, in charge of the night shift on the prison tier where these abuses were conducted. As such, I had access to all the digital images, all the investigative reports, to this soldier, and then I got to know him, his family and others who had been at that prison. I now know a great deal more than most other

experts about that situation and the Military and Civilian Command personnel responsible for creating a system that created an impossibly chaotic situation for the soldiers serving as reserve army guards there. In my book, I end by putting each of them on trial for their systemic complicity in these abuses -- thus assuming a new persona as Zimbardo, the Prosecutor.

My book is titled, "The Lucifer Effect: Understanding How Good People Turn Evil," published by Random House (March, 2007). It represents my personal journey through the hells of these two prisons, the mock one in the basement of the Stanford Psychology Department and the all too real on Tier 1-A, Abu Ghraib. I really poured my heart, soul and brain into writing this book over the past two years. I am too close to it to be objective, so forgive my immodesty in believing it will become an enduring best seller and the capstone of my career.

Lessons for Students from My Journeys

So my meandering path through social psychology finds me now studying both situational and dispositional variables and their interaction in research on the psychology of time perspective, the cognitive and social bases of the origins of psychopathology, and the effects of technology on shyness, along with investigating the development of prosocial and antisocial behavior among school children, the role of personality factors in political behavior, the psychological foundations of terrorism, and more. It has been my life long passion that those cuddly white rats could never quite fulfill, although it was easier to predict their behavior and publish their data than it is dealing with capricious people and the editors of the Journal of Personality and Social Psychology.

My message to the next generation of psychology students, those blessed to be social psychologists, and the others who ought to be, is very sexy. Do It with Love, Passion, Arousal, Devotion and Pride -- or go on a different journey.

Love being a psychologist because it is the most wonderful way to spend your life figuring out the mysteries of human nature. And once you have solved a few of its puzzles you are in a position to determine ways in which you can make life better through your teaching, research, clinical practice, or applying psychological knowledge across many domains.

Don't just do psychology in a nice gentle way; do it with all out, unbridled passion, going over the top, being charismatic, not just a good psychologist. Passion means at the moment you engage psychology of any kind, get into the magical moment of flow, with no past or future, just the totally engaging here and now. Become psychology.

Get excited about what you do as a psychologist; be aroused by learning new things, by communicating new ideas, by helping enhance the quality of mental life of individuals and even nations.

Become devoted to a life-long journey of discovery, of curiosity about the workings of the brain, the mind, and behavior. Remember what matters: content, context, culture, good methodology, applying our knowledge to improve the human condition, and have respect for people, animals, and nature.

Be prideful about your major, your career, and your profession -- as a psychologist. It is the most complex, compelling, demanding, changing, expanding, deepening field there is. Own it, make it yours. Do it, be it, all the time.

Finally, the difference between me -- now a celebrated psychologist -- and my two grandfathers, one a shoemaker and the other, my namesake, a barber, is only one thing -- Education. I was blessed, given the gift of being allowed to be educated and they were not. They had to work with their hands and not with their minds. They shined maybe hundreds or thousands of shoes, and shaved as many faces, in their noble professions. But they were not able to touch the minds of their customers as I have been privileged to do as a teacher for 50 years to many thousands of students. So please never take your education for granted; take it as special gift from your parents, your society, and your teachers that you treasure every day and find new ways to repay them all with your joy in learning and your accomplishments using what you have learned to enhance the human condition. Dear students of psychology, it is now time now for Your Journey. Go in peace and joy.

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1. This essay appeared in **Journeys in Social Psychology: Looking Back to Inspire the Future** (2008). Edited by Robert Levine, Aroldo Rodriguez, & Lynnette Zelenzny. New York: Psychology Press (Taylor & Francis Group)

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