A Personal Perspective on the Development of Social Psychology in the Twentieth Century

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Reflecting on his career as a social psychologist, Morton Deutsch guides us through a remarkable number of significant events that have shaped the field. He begins with his experienced under the leadership of Kurt Lewin and the impact of the intellectual atmosphere that prevailed at the Research Center for Group Dynamics, which shaped not only his dissertation but his entire value orientation as a social psychologist. He tells of his later work within the more applied atmosphere of the National Training Laboratory led by Ron Lippitt, describing his own particular research and many of the indelible contributions he has made to the field. Deutsch observes that his career as a social psychologist has centered on two continuing themes: cooperation, competition, and conflict on the one hand and distributive justice on the other. He concludes his reflections with the hope that future social psychologists will achieve a successful integration of three of the intellectual heroes of his youth: Freud, Marx, and Lewin.

My life almost spans the existence of modern social psychology. My commentary on social psychology will be from the personal perspective of a reflection on my career as a social psychologist and the factors, social and personal, that influenced its development. However, I shall precede my autobiographical reflection with a brief commentary on the development of social psychology prior to my exposure to it.

Although modern social psychology was born in the first decades of the twenty century, its ancestry in social philosophy can be traced back to ancient times. (For an excellent review of the precursors of modern social psychology, see Allport, 1954a). It is a child of psychology and sociology, having been conceived in the ambivalent mood of optimism and despair that has characterized the scieentific age. The rapidly expanding knowledge, the increasing confidence in scientific methods, the ever quickening technological change with its resulting opportunities and social problems, the development of new social organizations and of social planning, the social turmoil, the repeated disruption of communities and social traditions – all these helped to create both the need for social psychology and the awareness of the possibility that scientific methods might be applied to the understanding of social behavior.

Charles Darwin's theory of evolution dominated the intellectual atmosphere of the time, and it became a model for early theorists in social psychology, who also set as their goal the achievement of a broad, encompassing theory of social behavior. The programmatic statements of theorists such as Charles Colley (1902), Gabriel Tarde (1903), William McDougall (1908), and Edward Ross (1908) were grandly ambitious in scope but meager in detail. Many of the initial explanations of social behavior were made in terms of such processes as sympathy, imitation, and suggestion, which, in turn, were thought to be instinctually determined. The "herd instinct," the "instinct of submission," the "parental instinct," and a host of other instincts were invoked as innate, evolutionary derived causes of behavior.

The instinctual doctrines, however, did not last long. By the middle of the 1920s, they were in retreat. The prestige of the empirical methods in the physical sciences, the point of view of social determinism advanced by Karl Marx and various sociological theorists, and the findings of cultural anthropologists all contributed to their downfall. The two emphases in the rebellion against the instinctivist position, the rejection of the notion of instinctually caused behavior and the methodological stress on empirical procedures, still color contemporary social psychology. Empiricism is an inheritance from psychology; environmentalism is a legacy of sociology.

Opposition to the doctrine of instincts and, along with it, the minimization of genetic as compared to environmental influences upon social behavior led to many studies that illustrated the effects of social factors on individual psychological processes. (Bartlett's [1932] "Social Factors in Recall," Sherif's [1936] "Group Influences Upon the Formation of Norms and Attitudes," and Piaget's [1948] "Social Factors in Moral Judgment" are classic studies of this genre.) In consonance with the rapid social changes so characteristic of the modern period, investigations by social psychologists challenged long-held views about the fixity of human nature and about the innate superiority or inferiority of any social class, national group, or race. Social psychologists were not initially unsympathetic to J.B. Watson's (1930) extravagant assertion that "there is no such thing as inheritance of capacity, talent, temperament, mental constitution, and characteristics." More recently, there has been recognition that any full explanation of the development of human behavior must take into account the genetically determined biological equipment with which individuals confront their environment; even more lately, the emergence of "evolutionary social psychology” reflects this emphasis. Yet almost all social psychologists still reject the view of innate superiority-inferiority and the notion that social behavior is “fixed” by instinct. The rejection of abstract theorizing about social behavior in favor of empirical investigation provided the stimulus for the development of a variety of methods for studying social behavior; systematic interviews to obtain information about the motivations underlying behavior; controlled observational procedures to describe and classify behavior in social situations: methods of content analysis to analyze speeches, documents, and newspapers: sociometric techniques to study the social bonds and patterns of social interaction within a community; projective instruments of the study of personality patterns; and so forth. These methods have been extensively applied in public opinion polling, consumer research, studies of morale, investigations of prejudice and discrimination, personnel selection, and the like.

This revolt against armchair theorizing led many social psychologists not only to leave their armchairs but also to stop theorizing. Or perhaps it is more accurate to say that social psychologists who began to engage in empirical research in the 1920s and early 1930s did little to connect their research with theoretical ideas. During this same period, the psychoanalysts and also the early theorist abandoned their armchair mainly for the lecture podium.

Toward the end of the 1930s, under the enthusiastic but gentle leadership of Kurt Lewin, modern experimental social psychology began to flourish. Lewin and his students demonstrated that it is possible to create and study groups in the experimental laboratory that have important features in common with real-life groups. In doing so, they stimulated an interest in social psychological experimentation and attracted many experimentalists to work in this area.

Autobiography: Presocial Psychology

I was born, prematurely, in 1920 into a Jewish middle-class family in New York City, the last of four sons. I was always eager to catch up with my older brothers, feeling like an underdog, so I skipped through elementary and high school and entered the City College of New York (CCNY) in 1935 at the age of fifteen: two and a half years younger than most students.

I started off as a pre-med major with the idea of becoming a psychiatrist, having been intrigued by the writings of Sigmund Freud, some of which I read before college. I was drawn to psychoanalysis undoubtedly because it appeared to be so relevant to the personal issues with which I was struggling, and also because it was so radical and rebellious (it seemed to be so in the early and mid-1930s). During my adolescence, I was also politically radical and somewhat rebellious toward authority, helping to organize a student strike against the terrible food in the high school lunchroom and, later, a strike against the summer resort owners who were exploiting the college student waiters, of whom I was one.

The 1930s were a turbulent period, internationally as well as domestically. The economic depression; labor unrest; the rise of Nazism and other forms of totalitarianism; the Spanish civil war; the ideas of Marx, Freud, and Albert Einstein; as well as the impending second world war were shaping the intellectual atmosphere that affected psychology. Several members of the psychology facility at CCNY were active in creating the Psychologist League, the precursor to the Society for the Psychological Study of Social Issues. Thus when I became disenchanted with the idea of being a pre-med student after dissecting a pig in a biology lab, I was happy to switch to a psychology major: It was a simpatico faculty. Psychology was a part of the Department of Philosophy at CCNY when I started my major in it. Morris Raphael Cohen, the distinguished philosopher of science, was the leading intellectual figure at CCNY, and his influence permeated the atmosphere.

At CCNY Max Hertzman introduced me to the ideas of Kurt Lewin and other Gestalt theorists. And under Walter Scott Neff's direction, I conducted my first laboratory experiment, a variation on Sherif's study of social norms, employing the autokinetic effect. As I now recall, in it I introduced a stooge who constantly judged the stationary speck of light in a dark room as having moved a substantial distance in one direction. (Most subjects see the light as moving a small difference in one direction.) The stooge has a considerable impact on the judgments made by the naïve, majority of subjects. The findings of this pilot study anticipated later research by Serge Moscovici on minority influence.

My first exposure to Lewin's writings was in two undergraduate courses, taken simultaneously: social psychology and personality and motivation. In the social psychology course, one of our textbooks was J.F. Brown's *Psychology and the Social Order* (1936). This was an ambitious, challenging, and curious text that tried to apply to the major social issues of the 1930s Lewinian and Marxian ideas, with a sprinkling of the Riemanian geometry employed by Einstein in his theory of relativity. To a naïve seventeen-year-old undergraduate student like me, it was a very impressive and inspiring book showing how social science could shed light on the urgent problems of our time.

In the personality and motivation course, I read Lewin's *Dynamic Theory of Personality* (1935) and *Principle of Topological Psychology* (1936). I also read his *Conceptual Representation and Measurement of Psychological Forces* (1938) as an undergraduate, but I cannot recall when. I and others experienced great intellectual excitement on reading these books more than fifty years ago. *A Dynamic Theory of* Personality consisted of a collection of independent articles, previously published in the early 1930s, whereas the other books made a brilliant but flawed attempt to articulate the foundations of a scientific psychology with the aid of topology. They were mind openers. These books are permeated by a view of the nature of psychological science different from what was then traditional. The new view was characterized by Lewin as the "Galilean mode of though," which was contrasted with the classical "Aristotelian mode." In my writings on field theory (Deutsch, 1968), I have characterized in some detail Lewin's approach to psychological theorizing , his metatheory.

Although I was impressed by Lewin's writings, my career aspirations in psychology were still focused on becoming a psychoanalytic psychologist as I decided to do graduate work in psychology. My undergraduate experiences, in as well as outside the classroom, led me to believe that an integration of psychoanalysis, Marxism, and scientific method, as exemplified by Lewin-s work, could be achieved. In the 1930s such influential figures as Wilhelm Reich, Erich Fromm, Max Horkheimer, Theodor Adorno; and Else Frenkel-Brunswik, as well as many others, were trying to develop an integration of psychoanalysis and Marxism. Also at this time, some psychoanalytic theorists such as David Rappaport were intrigued by the idea that research conducted by Lewin and his students on tension systems could be viewed as a form of experimental psychoanalysis.

I am not sure why I was advised to go to the University of Pennsylvania to take my master's degree. Possibly it was because it had a well-established psychological clinic and two faculty members, Frances Irwin and Malcolm Preston, who were sympathetic to Lewin's ideas. I had some interesting clinical experiences there working with children, largely without supervision, but the course work seemed dull and antiquated in comparison with my undergraduate courses at CCNY. I earned the reputation of being a radical by challenging what I considered to be racist statements about Negro intelligence in a course on psychological measurement given by Morris Viteles.

After obtaining my M.A. degree in 1940, I started a rotating clinical internship at three New York state institutions: one was for the feebleminded (Letchworth Village), another for delinquent boys (Warwick), and a third for psychotic children as well as adults (Rockland State Hospital). During my internship I became skilled in diagnostic testing and clinical interventions with a considerable variety of inmates, more widely read in psychoanalysis, and more aware o f how some capable inmates were unjustly retained in the institution because of the valuable services they performed for it or its staff.

I also had the good fortune to meet Clark Hull (the famous learning theorist) while he was visiting a former doctoral students of his, a staff psychologist at Letchworth Village. He was a remarkably generous and tolerant person. We had several long discussions, one related to his recently published book developing a hypothetico-deductive system for rote learning. I had read the book and was somewhat critical of it from two perspectives: the perspective of Gestalt psychology and of Morris Cohen and Ernst Nagel's book on scientific method, both of which I had been thoroughly indoctrinated in while I was an undergraduate at CCNY. Hull seemed genuinely interested in what I had to say even though I was an overly brash twenty-year-old pipsqueak. We had another interesting discussion in which he gave me advice on how to seduce a woman. He told me that, on a date, I should carry a handkerchief permeated with perspiration. He explained that sweat and sexual feelings were associated together because of their joint occurrence during sexual intercourse and that sweat would arouse sexual feelings. In retrospect, I realize that he must have been joking since his suggestion never worked for me.

When Pearl Harbor occurred in December 1941, I was still in my psychology internship. Shortly thereafter, I joined the air force. My first assignment was to a psychological research unit at Maxwell Field in Alabama, which did psychological testing of aviation cadets to classify them for training as pilots, navigators, or bombardiers. I soon became bored with testing and wanted to participate directly in action against the Nazis. I became a cadet and was trained as a navigator. To get to our combat base in England, our crew flew to and stopped at bases in such exotic sport as Trinidad: Fortaleza and Belém in Brazil; Dakar and Marrakech in Africa; and Scotland. What an eye-opening cross-cultural experience; I had never been outside the Northeastern part of the United States before joining the air force.

I flew in thirty bombing missions against the Germans. During combat I saw many of our planes as well as German planes shot down, and I also saw the massive damage inflicted by our bombs and those of the Royal Air Force on occupied Europe and Germany. Moreover, being stationed in England, I saw the great destruction wreaked by the German air raids and felt the common apprehensions while sitting in air-raid shelters during German bombings. Although I had no doubt of the justness of the war against the Nazis, I was appalled by its destructiveness.

After my combat tour of duty was completed, I returned to the United States and was assigned as a clinical psychologist to an Air Force Convalescent Hospital and served as such until shortly after V-E Day. I was demobilized early as the result of being one of the few nonpatients at the hospital who had been in combat and had amassed a substantial number of demobilization points.

After my demobilization, I contacted some psychology faculty members I knew at CCNY to ask for advice with regard to resuming graduate work in psychology, I discussed with them my somewhat confused interests in getting clinical training, in studying with Lewin because of his work on democratic and autocratic leadership, and in doing psychological research. As a result of these conversations, I decided to apply for admission to the doctoral programs at the University of Chicago (where Carl Rogers and L.L. Thurstone were the leading lights), at Yale University (where Donald Marquis was chairman and where Clark Hull was the major attraction), and at MIT (where Kurt Lewin had established a new graduate program and the Research Center for Group Dynamics). As one of the first of the returning soldiers, I had no trouble in getting interviews or admission at all three schools,. I was most impressed by Kurt Lewin and his vision of his newly established research center and so decided to take my Ph.D. at MIT.

My Autobiography as a Social Psychologist

I date the start of my career as a social psychologist to my first meeting with Lewin, in which I was enthralled by him and committed myself to studying at his center. He had arranged for me to meet him for breakfast at a midtown hotel in New York in August 1945. Even though it was very hot, I dressed formally – with jacket and tie – to meet with this distinguished professor. Our meeting time was 8:30 A.M., but he did not appear until about 9:00 A.M. He came bustling in, cheerfully looking around for me, his face bright pink from a recent sunburn. He was not wearing a jacket or a tie, and his manner was quite informal. I recognized him from a picture that I had seen and introduced myself, and we set off for the hotel's dining room. But they would not admit us because he had no jacket or tie (how things have changed). We then went to a nearby coffee shop. I do not remember much about the conversation other than that I described my education, experience, and interests, and he described his plans for the new center. I was experiencing a trancelike sensation of intellectual illumination with new insights constantly bubbling forth from this brilliant, enthusiastic, effervescent, youthful, middle-aged man. He spoke a colloquial American, often with malapropisms, and he was both endearing and charming. I left the interview with no doubt that I wanted to study with Lewin. I also left in a dazed sense of enlightenment, but I could not specifically identify what I was enlightened about when I later tried to pin it down for myself.

I had a similar experience a month later when I went to MIT to study and work with Lewin. He discussed with me some work he was then doing with the Commission on Community Interrelations of the American Jewish Congress (a commission he helped to establish) to reduce anti-Semitism and other forms of prejudice. His discussion of the issues was intensely illuminating when I was with him, but I could not define it afterwards when I was alone. At the end of our meeting, he asked me to prepare a review of the essence of the literature on prejudice, and he indicated that it should be brief and that he needed it in three days. I felt good. I was being treated as a serious professional and was given a responsible and challenging task. Lewin's treatment of me was, I believe, typical of his relations with his colleagues and students. He would discuss a topic with great enthusiasm and insight, he would ignite one's interest, and he would encourage one to get involved in a task that was intellectually challenging, giving complete freedom for one to work on it as one saw fit.

Shortly after arriving at MIT; I noticed a very attractive young woman, named Lydia Shapiro, who would occasionally pop into the center. She was working under Lewin's direction as an interviewee for a study on self-hatred among Jews. We started to get to know one another over cherry Cokes and jelly donuts. Being supported on the GI bill, I was a cheapskate, and she did like jelly donuts. I don't recall the specifics, but somehow I was assigned to supervise her work. After learning that she spent much of her supposed work time sunning herself on the banks of the Charles River, I fired her. About a year and a half later, on June 1, 1947, we got married. Stan Schachter and Al Pepitone, with whom I was sharing an apartment, were my best men at the wedding. In moments of marital tension, I have accused Lydia of marrying me to get even, but she asserts it was pure masochism on her part. In our fifty years of marriage, I have had splendid opportunities to study conflict as a participant observer.

Immediately after our honey moon in Quebec, we went to Bethel in Maine for the first National Training Laboratory (NTL). I served on its research staff with other students from the RGGD at MIT and from the Harvard Department of Social Relations. Lydia and another woman were the rumrunners for the workshop; Bethel was a dry town, and they had to drive 20 miles to buy the liquor to keep the workshop staff and participants well lubricated.

The first NTL was a natural follow-up of the Connecticut Workshop on Intergroup Relations held during the summer of 1946. As I now recall it, the training staff consisted of Ron Lippitt, Ken Benne, and Lee Bradford, and the research staff consisted of Murray Horowitz, Mef Seeman, and myself. One evening, following a lengthy workshop day, Lewin, the workshop participants, the trainers, and the researchers were all sitting around a conference table when one of the participants turned to the researchers and asked us what we were doing. We said that we were keeping track of the patterns of interaction among the group. He then asked us to describe what we had noted; Lewin suggested that would be an interesting thing to do. We summarized our impressions, and this lead to a lively, insightful, learning experience. This was the embryo of the T-group and sensitivity training that was given birth at the first NTL in 1947.

I would now say that the researchers at the first NTL did not fully appreciate the importance of the new procedures and new movement being developed. The evangelical tone of some of the trainers appalled many of us, with the result that there was considerable unhappiness among the researchers that summer of 1947. Today many of us recognize that NTL as the birthplace of much of applied social psychology, especially in the area of organizational psychology.

The Research Center for Group Dynamics

Lewin assembled a remarkable group of faculty and students to compose the Research Center for Group Dynamics at MIT. For the faculty, he initially recruited Dorwin Cartwright, Leon Festinger, Ronald Lippitt, and Marian Radke (now Radke-Yarrow). Jack French and Alvin Zander were to join later. The small group of twelve students included Kurt Back, Alex Bavelas, David Emery, Gordon Hearn, Murray Horowitz, David Jenkins, Albert Pepitone, Stanley Schachter, Richard Snyder, John Thibaut, Ben Willerman, and myself. These initial faculty and students were extraordinarily productive, and they played a pivotal role in developing modern social psychology in its applied as well as its basic aspects. As I write these last two sentences, it strikes me that all of the students and the key faculty members were male. This was quite a change for Lewin; in Berlin most of his students were female (e.g. Bluma Zeigarnik, Tamara Dembo, Eugenia Hanfmann, Maria Ovsiankina, Anitra Karsten). It is interesting to speculate how modern social psychology's development might have differed if the student group included a substantial number of women.

Lewin died suddenly on February 11, 1947, of a heart attack. The RCGD had been functioning for considerably less than two years when he died. Yet in this brief period of time he had established an institution that would strongly influence the development of modern social psychology. Let me offer some thoughts about why the Research Center for Group Dynamics was so remarkably productive.

Reasons for the Center's Effectiveness

First, Lewin was an unusually effective scientific "tribal leader" (to borrow a phrase from Donald Campbell). As I have indicated in describing my personal contacts with him, he was enthusiastic, inspiring, and persuasive. He led those working with him to feel they were involved in an important, promising enterprise that could have valuable consequences for both social science and society. He treated his faculty and students as colleagues: giving them autonomy and responsibility and a sense of being actively involved, individually and collectively, in creating the new field of group dynamics. He also encouraged open and vigorous conflict about ideas and methods among his faculty and students in the never ceasing attempt to get to a deeper understanding of the issues involved.

This was most evident in the loosely organized research seminars, named the Quasselstrippe (or winding string), that he led for the faculty and students. In the Quasselstrippe a faculty member of student would typically present some research or some theoretical issue that he or she was involved in, and a lively controversy would erupt. Sometimes the controversy was related to the presentation, but frequently the discussion wandered off into other issues. Not infrequently the most heated exchanges took place between Leon Festinger and Ronald Lippitt, who had rather different views of the nature of science and research. During these vigorous disputes, Lewin would be smiling benignly as he watched his intellectual offspring squabble. Almost invariably at the end of these wandering, disputatious research seminars he would emerge from his role as an observer, and in an active way he would offer a deeper, integrating perspective that would provide a basis for synthesizing the conflicting viewpoints.

It was not only Lewin's leadership style but also his ideas that contributed to the productivity of the RCGD. Very much influenced by Ernst Cassirer, the German philosopher of science, he thought "the taboo against believing in the existence of a social entity is probably most effectively broken by handling this entity experimentally" (Lewin, 1951, p. 193). The concept of "group," as well as other concepts relating to social psychological phenomena, had little scientific status among psychologist in the 1930s and 1940s when Lewin was first turning his attention to social psychology. He believed the "reality" of these concepts would be established only by "doing something with them." So at the center there was strong pressure to do something with the concepts related to groups and not merely to talk about these ideas. And, of course, the faculty and students did many experiments to demonstrate that one could, in a sense, capture for science such phenomena as “styles of group leadership,” “social influence,” “cooperation and competition,” “group cohesiveness,” “pressure for uniformity,” “trust and suspicion,” “social comparison,” and so on. The pressure to do something with the concepts was directed not only toward experimentation but also toward application, namely, to show that these concepts could be employed to change exiting social reality – to improve group functioning, to reduce prejudice, or to train more effective leaders.

Lewin’s metatheory, his conceptual language, as well as his specific theoretical ideas were also important influences on the members of the center while they were at MIT. More than thirty years later, in the spring of 1978, there was a reunion at Columbia University of almost all of the surviving RCGD members. The participants included Kurt Bach, Dorwin Cartwright, Leon Festinger, Jack French, Gordon Hearn, Harold Kelley, Ronald Lippitt (via tape), Albert Pepitone, Stanley Schachter, and myself. At that reunion the participants were asked to indicate Lewin’s effect on their work. From the discussion, it was evident that all of us had been very much influenced by Lewin’s way of thinking about science and by his general orientation to psychology. Elsewhere I have described the key elements of Lewin’s metatheory – in other words, his field-theoretical approach to psychology. This is what had most impact on the participants. Few were still involved in Lewin’s conceptual language or terminology, with topological and vectorial psychology. Some had been stimulated to do work that related to Lewin’s specific theoretical ideas, particularly those relevant to tension system, level of aspiration theory, social interdependence, group leadership, group decisionmaking, changing individual attitudes, and quasi-stationary equilibria. And several were stimulated by Lewin to be concerned with articulating the connection between social psychology theory and change in social practice.

Nevertheless, the common thread that linked our group of past RCGD members together was a Lewinian way of thinking. It emphasized the importance of theory; the value of experimentation for clarifying and testing ideas; the interrelatedness between the person and the environment; the interdependence of cognitive structures and motivation; the importance of understanding the individual in his or her social (group, cultural) context; the usefulness of theory for social practice; and the value of trying to change reality for the development of theory. These emphases are not unique to the Lewinian way of thinking; they characterize good social science and good social practice. But Lewin was the one who introduced them to social psychology.

The RCGD fostered a sense of pioneering elitism among its members. We felt we were working on the frontiers of social psychological knowledge, creating new research methods, and capturing new phenomena for science. This fostered a narcissistic arrogance in many of us that permitted us to venture on untrodden paths and to feel rather superior to the work being done by our friends and neighbors in Harvard's Social Relations Department as well as elsewhere.

In addition, of course, the center had a critical mass of active researchers among its faculty and students, so that the publications of this group dominated the early work in experimental and applied social psychology. Alfred Marrow (1969), in his biography of Kurt Lewin (*The Practical Theorist*), listed over 100 publications and dissertations connected with the RCGD during the period of 1945-1950. In a sense, apart from whatever merits we had, we were so influential because we were lucky enough to be active early in the development of modern social psychology, when there were comparatively few others who were doing research and publishing in this field.

Lewin recruited a very able and congenial group of mature students who, for the most part, had done previous graduate work in psychology and had served in the armed forces in World War II. They were prepared to take responsibility and to work with the faculty as colleagues. The relatively young faculty were unusually accessible and open to collaborative working relations with the students. As students, we were quickly involved in the design and execution of experiments and research on training workshops; some of us were also rapidly thrust into the role of conducting training workshops on group processes and group leadership. The students comprised a small, cohesive group that provided much mutual support even as we had intense intellectual discussions about the new ideas and techniques that were being developed.

Lewin also recruited a remarkably gifted younger faculty. I assume that he purposefully created a faculty that had some tension as well as some unifying elements within it, a faculty within which there would be productive tension in theory, research, and application. As suggested earlier, Festinger and Lippitt had fundamental disagreements, and while he lived, Lewin served as an integrating force, intellectually as well as administratively. After his death, Cartwright maintained administrative integration, but there was little intellectual common ground between the disparate perspectives of Festinger and Lippitt. For many students, Festinger became a symbol of the tough-minded, theory-oriented, pure experimental scientist, whereas Lippitt became a symbol of the fuzzy-minded, do-gooder, practitioner of applied social psychology. These were unfortunate caricatures of both Festinger and Lippitt. Such distortions were, I believe, one of the contributing causes to the estrangement between basic and applied social psychology in the United States during the 1950s and early 1960s. I doubt that these caricatures would have developed if Lewin had lived longer. As my earlier quotation from him indicated, he saw an intimate, two-directional link between the development of theory and practice.

My career in social psychology has been greatly affected by Kurt Lewin and my experiences at the Research Center for Group Dynamics.[1] First, I probably would not have been a social psychologist were it not for the inspiring interview with him in the summer of 1945. Second, the intellectual atmosphere created by Lewin at the

RCGD strongly shaped my dissertation and my value orientation as a social psychologist. Lewin was not only an original, tough-minded theorist and researcher with a profound interest in the philosophy and methodology of science, but he was also a tenderhearted psychologist who was deeply involved with developing psychological knowledge that would be relevant to important human concerns. Lewin was both tough-minded and tenderhearted; he provided a scientific role model that I have tried to emulate. Like Lewin, I have wanted my theory and research to be relevant to important social issues, but I also wanted my work to be scientifically rigorous and tough-minded. As a student, I was drawn to both the tough-mindedness of Festinger's work and to the direct social relevance of Lippitt's approach and did not feel the need to identify with one derogate the other.

My Dissertation Study

My dissertation started off with an interest in issues of war and peace (atomic bombs had been dropped on Hiroshima and Nagasaki shortly before I resumed my graduate studies) and with an image of the possible ways that the nations composing the newly formed United Nations Security Council would interact. The atmosphere at the center, still persisting after Lewin's premature death, led me to turn this social concern about the risk of nuclear war into a theoretically oriented, experimental investigation of the effects of cooperative and competitive processes. The specific problem that I was first interested in took on a more generalized form. It had been transformed into an attempt to understand the fundamental features of cooperative and competitive relations and the consequences of these different types of interdependencies in a way that would be generally applicable to the relations among individuals, groups, or nations. The problem had become a theoretical one, with the broad scientific goal of attempting to interrelate and give insight into a variety of phenomena through several fundamental concepts and basic propositions. The intellectual atmosphere at the center pushed its students to theory building. Lewin's favorite slogan was, "There is nothing so practical as a good theory."

As I reflect back on the intellectual roots of my dissertation, I see it was influenced not only by Lewin's theoretical interest in social interdependence but also by the Marxist concern with two different systems of distributive justice: a cooperative egalitarian and a competitive, meritocratic one. In addition, the writings of George Herbert Mead, affected my way of thinking about cooperation and its importance to civilized life.

This study,[2] in addition to being the takeoff point for much of my subsequent work, has helped to stimulate the development of a movement toward cooperative learning in the schools under the leadership of David and Roger Johnson. Although cooperative learning has many ancestors and can be traced back for at least 2,000 years, my dissertation helped to initiate the development of a systematic theoretical and research base for cooperative learning. Hundreds of research studies have since been done on the relative impact of cooperative, competitive, and individualistic learning (see Johnson & Johnson, 1989). These various studies are quite consistent with one another and with my initial theoretical work and research on the effects of cooperation-competition (Deutsch, 1949a, 1949b) in indicating favorable effects upon students. Through cooperative learning, students develop a considerably greater commitment, helpfulness, and caring for one another regardless of differences in ability level, ethnic background, gender, social class, and physical ability. They develop more skill in taking the perspective of others, emotionally as well as cognitively. They develop greater self-esteem and a greater sense of being valued by their classmates. They develop more positive attitudes toward learning, school, and their teachers. They usually learn more in the subjects they are studying by cooperative learning, and they also acquire more of the skills and attitudes that are conducive to effective collaboration with others.

The Research Center for Human Relations

After obtaining my Ph.D. from MIT in the summer of 1948, I joined the Research Center for Human Relations (then at the New School) headed by Stuart Cook. The war against Nazism had stimulated a considerable interest among psychologists in understanding prejudice and how to overcome it, and financial support for research in this area was available form Jewish organizations such as the American Jewish Congress as well as from federal agencies. Among the many groups receiving funding for work in this area were members of the Berkeley Public Opinion Study and the former Frankfurt Institute of Social Research, who produced *The Authoritarian Personality* (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950); Lewin's MIT Center, which developed not only the first workshop for reducing prejudice and improving intergroup relations but also action research "to help social agencies that were developing programs aimed at reducing prejudice and discrimination"; and the Harvard group working with G.W. Allport (1954b) on creating an integrated overview of the nature of prejudice and ways of reducing it.

The Research Center for Human Relations was in 1948 also mainly funded by agencies interested in reducing prejudice. As soon as I joined, I became involved in a study of interracial housing that I conducted with Mary Evan Collins. We started with an "experience survey" of knowledgeable public housing officials to identify the important factors affecting interracial relations in housing projects. On the basis of this survey, we decided that the residential pattern – whether the races were segregated or integrated with in the housing project – was a critical determinant. We then set out to identify housing projects that were otherwise similar but differed in terms of whether black and white residents lived in separate buildings or were integrated within each building. We were able to identify biracial segregated public housing developments in Newark, New Jersey, and racially integrated ones in New York City that were roughly similar. We then did an extensive interview and a small observational study in the projects, and by the use of various controls we created a quasi-ex post facto experiment. Despite the obvious methodological limitations of such a study, it was clear that the two types of projects differed profoundly in terms of the kinds of contacts between the two races and the attitudes that they developed toward each other.

This study (Deutsch & Collins, 1951) had important social consequences. As the executive director of the Newark Public Housing Authority stated in a postscript to our book, *Interracial Housing*, "The partial segregation which has characterized public housing in Newark will no longer obtain. In large measure, this change in fundamental policy reflects the impact of the study reported in this book. The study has served as a catalyst to the re-examination of our basic interracial policies in housing and a s a stimulus to this change." It also led me to become active on a Society for the Psychological Study of Social Issues (SPSSI) committee concerned with intergroup relations. Over the next several years, this committee gave talks before policy-oriented groups as well as helped lawyers who were challenging racial segregation in various suits brought before federal courts. The committee also contributed material to the legal brief that was cited in the 1954 Supreme Court decision *Brown v. the Board of Education*, which outlawed racial segregation in schools and other publicly supported facilities.

In 1949 the Research Center for Human Relations moved to New York University (NYU), and I became a member of its graduate faculty in psychology. Here I worked collaboratively with Marie Jahoda and Stuart Cook on an SPSSI-sponsored textbook, Research Methods in Social Relations (Jahoda, Deutsch, & Cook, 1951), one of the earliest - if not the earliest - of its kind. To help me overcome my Kafkaesque, Germanic style of writing, Mitzi pinned in my wall a slogan that stated, "You don't have to write complex sentences to be profound." It was a good reminder as well as a subtle way of deflating my pompous persona of theorist-basic researcher with which I had emerged from my graduate studies.

At NYU I also worked collaboratively with Harold Gerard on a laboratory study of normative and informational influence on individual judgment (Deutsch & Gerard, 1955) and a study of decisionmaking among high-level air force officers. In addition, with support from the Office of Naval Research, I was able to start a program of research on factors affecting the initiation of cooperation. Hal had introduced me to Howard Raiffa, who in turn introduced me to the Prisoner's Dilemma (PD), which I soon turned into a useful research format for investigating trust and suspicion (Deutsch, 1959, 1962a, 1973). I was probably the first psychologist to use the PD game in research. Unfortunately, the PD game (like the Asch situation and the Skinner box) became an easy format for conducting experimental studies, and as a result a torrent of studies followed – most of which had no theoretical significance.

I added to my busy schedule by undertaking training as a psychoanalyst at the Postgraduate Center for Mental Health, which had an eclectic orientation rather than being committed to one or another school of psychoanalysis. It involved not only my own analysis (three times per week) but also six to nine hours of classes, twenty hours of doing psychoanalytic psychotherapy, and two to three hours of supervision per week. It was hectic, but I was young. It was an extremely valuable supplement to my work as an experimental social psychologist, which gives perspectives only on very narrow cross-sections of people's lives. Psychoanalysis provided a longitudinal, developmental view in addition to glimpses into the internal psychodynamics underlying a person's behavior in conflict situations. My psychoanalytic work stimulated my research interest in such topics as trust and suspicion and conflict. It has been a two-way street. My social psychological work on conflict, negotiation, and mediation has affected my therapeutic approach to the conflicts experienced by patients as well as my approach to marital therapy. I continued a small private practice until about ten years ago, when I wanted to have more freedom to travel. The practice was personally rewarding. I helped a number of people, it enabled me to stay in touch with my own inner life, and it provided a welcome supplement to my academic salary.

During my tenure at New York University, most of my salary was paid out of soft money, from research grants or other monies from outside sources. As McCarthyism developed increasing strength in the early 1950s, social science and social scientists became targets of attack, being labeled as "radical," "fellow travelers," "communist sympathizers," and the like. If your personal library contained books by Karl Marx, if you had participated in interracial groups challenging segregation, if a friend was or had been a member of the Communist Party, and so on, you were suspect and might be purged from your position. During the height of the McCarthy period, many funding agencies no longer were willing to support research dealing with prejudice or interracial relations, and there was much talk of reducing federal support for social science research. Thus I was happy to accept when Carl Hovland, in 1956,, invited me to help establish a new basic research group in psychology at the Bell Telephone Laboratories. Bell Labs had an excellent reputation for its support for basic research, and this is what I wanted to do, without the constant problem of raising money.

Much to my surprise, even during the worst part of McCarthyism I never had any problems, nor did my funding from the Office of Naval Research or the air force stop. Although never a communist, I had many of the characteristics of the "usual suspect." Possibly, I was not harassed because I had received a security clearance from the air force before doing research on decisionmaking in the early 1950s.

The Bell Laboratories

Bell Labs was, by academic standards, a luxurious place to work. I received a good salary and had no trouble getting research assistants, equipment, secretarial help, and travel money as well as much freedom to do what I wanted. I was able to hire Bob Krauss and Norah Rosenau, then graduate students at NYU, to work as my research assistants. I was also able to add Hal Gerard and Sy Rosenberg to our research staff. It was a productive group. At Bell Labs, Bob Krauss and I developed and conducted research with the Acme-Bolt Trucking game; we also started on our book, *Theories in Social Psychology* (Deutsch & Krauss, 1965). I did various other studies including “The Interpretation of Praise and Criticism” (Deutsch, 1961), “Dissonance or Defensiveness” (Deutsch, Krauss, & Rosenau, 1962), and “The Effects of Group Size and Task Structure Upon Group Process and Performance (Deutsch & Rosenau, 1963). This last was a fine study that was never written up for publication because of Norah Rosenau’s premature death and my change of interests as I moved to Teachers College in 1963.

In addition, while at the Bell Labs, I was its unofficial peacenik, criticizing the strategic thinking among establishment intellectuals and coediting the book Preventing World War III (Wright, Evan, & Deutsch, 1962). During this period I was quite active in SPSSI, articulating some of the social psychological assumptions underlying our national policy and even becoming its president.

Although Bell Labs was in many respects a fine place to work, it had its problems. Compared to a university, it was a stiff organization: It had a clear hierarchical structure; it had fairly set hours of work and vacation (from which I was a tolerated deviant); the lab had no small, offbeat, informal eating places that served wine of beer; there were few students and little ethnic and racial diversity.

In addition, there were specific problems related to our psychological research unit. Although it was located in the Bell Labs in Murray Hill, New Jersey, the Personnel Research Group at AT&T had been instrumental in getting the unit established and thought that we should be primarily working closely with them on problems with which they needed help. None of us who had come to Bell Labs at Carl Hovland’s urging had this view, nor apparently did Carl. The administrative head of our unit was a former member of the AT&T Personnel Group. An uncomfortable power struggle developed about what we should be doing, which Bell Labs ultimately won. But because of the dispute and also because we were the oddballs of the Bell Labs (which was composed mainly of physical scientists and mathematicians), we were the constant object of high-level attention. We had visits from the president of AT&T, the president of Western Electric, the presidents of various Bell Telephone Companies, and do on, and at each visit our group would have to put on a show, lasting one or two days, in which we would demonstrate our research. During one of these visits, when a committee came in order to make a recommendation about the future of our group, we received word that Bob and I had just been awarded the American Association for the Advancement of Science (AAAS) sociopsychology prize for the research we had done at the Bell Labs with the Acme-Bold Trucking game (Deutsch & Krauss, 1962). This apparently laid to rest the doubts about our group.

In addition to the people I recruited for my research group on interpersonal processes, Alex Bavelas, another key staff member selected by Hovland, recruited Herbert Jenkins, a Skinnerian who did his research on learning using pigeons.

Herb must have had several people a day ask him, jokingly, “Going to replace the telephone with pigeons, eh?” After a year or so, Bavelas quit the labs, feeling that it was not a receptive environment for what he wanted to do. Jenkins then recruited Roger Shepard, who started his brilliant work on multidimensional scaling there.

While at the labs, I was consulted by its administration on problems such as how to improve the creativity of their researchers, how to apply social science knowledge to improve the functioning of the various telephone companies, and how to improve race relations. As I recall, I gave many potentially useful suggestions, none of which were implemented. I also suggested that they hire Henry Riecken to establish a social science developpement group to develop existing social science knowledge for use in the Bell system. Although Bell interviewed Riecken, they did not implement this idea either.

Hovland died in 1961, and about a year later I started to think about leaving the labs. I was getting tired of commuting from New York City to Murray Hill: I missed working with graduate students as well as the looser, less hierarchical atmosphere of a university; and I was bored by the special attention that our group was receiving. My memory of the specifics are unclear, but around this time I was approached by Teachers College to consider an appointment to replace Goodwin Watson, who was retiring, and to head its doctoral program in social psychology. Teachers College was attractive to me because Lydia and I were determined to continue living in New York, I would have freedom to create a new social psychology program, and I was interested in education. I received other feelers from nearby institutions (the Department of Management at Yale University and the Department of Psychiatry at the Albert Einstein College of Medicine) that would have provided higher salaries and more affluent settings, but they did not have the lure of shaping a social psychology program.

Teachers College

When I joined Teachers College in September 1963, I had a strong view of what I wanted the new social psychology program to be like. I wanted it to attract students and turn out graduates who would be tough-minded and tenderhearted, who would be as knowledgeable and expert in theory and research as the best of the “pure,” experimental social psychologists and also socially concerned with developing and applying social psychological knowledge to the urgent and important social problems of our time. In other words, I wanted to develop a program that would overcome the split that had developed between the laboratory and applied social psychology during the 1950s and the early 1960s. As I have indicated earlier, the differences between the sharp-minded and sharp-tongued Festinger and the evangelical, unsystematic Lippitt were precursors of this split, which widened into a chasm in the decade after Lewin’s death (see Deutsch, 1975 for a more extensive discussion of this rift).

Although the split was understandable in terms of the insecurities of both sides in a young discipline, it was harmful and stupid from my perspective. It polluted the atmosphere of social psychology. When I left Bell Labs (a tough-minded institution) to join Teachers College (a tenderhearted one), I thought that my experimental colleagues would consider this to be a loss of status for me and that my new colleagues would be concerned that I would be overly critical and scientistic (rather than scientific) as well as out of touch with practical realities. However, by the time I came to Teachers College, I felt sufficiently secure in my own identity as a social psychologist not to be concerned by colleagues who would deprecate either tenderheartedness or tough-mindedness.

I was fortunate when I came to Teachers College in several respects. First, although Teachers College, like most schools of education, has relatively little money for research by its faculty or stipends for its graduate students, I was able to bring in outside funding to get the social psychology program off to a good start: The National Science Foundation (NSF) gave funds to build a well-equipped social psychology laboratory, the Office of Naval Research (ONR) supported my research, and the National Institute of Mental Health (NIMH) provided a training grant that would support most of our graduate students. Second, we were able to attract many excellent students who fit our criteria of being tough-minded and tenderhearted, including Harvey Hornstein, David Johnson, Jeffrey Rubin, Roy Lewicki, Barbara Bunker, Madeleine Heilman, Kenneth Kressel, Charles Judd Jr., Janice Steil, Michelle Fine, Ivan Lansberg, Louis Medvene, Susan Boardman, Sandra Horowitz, Susan Opotow, Even Weitzman, Martha Gephart, and Adrienne Asch. Third, our program was initially small enough for us to be a very cohesive group that mainly worked cooperatively on interrelated research projects under my direction. We could have frequent informal lunches together during which we discussed politics, diets, Jackie Ferguson (our fascinating secretary who mothered us all), and research and theory. Many good ideas emerged from these lunches. Finally, the change from Bell Labs to Teachers College accelerated a shift in focus and labeling of my research. At the Bell Labs, I and others came to view the Acme-Bolt Trucking game as a bargaining game, so I began to think of studies that employed it as bargaining or negotiation and more generally as conflict studies. This was a shift away from labeling them as studies of the condition affecting the initiation of cooperation.

With a change in labeling, I began to reframe the question underlying much of my research from, “What are the conditions that give rise to cooperation rather than competition?” to “What are the conditions that give rise to constructive rather than destructive processes of resolving conflict?” At a conceptual level, the two questions are very similar. Nevertheless, the latter phrasing is much sexier; it resonates directly to many aspects of life and to the other social sciences as well as psychology. And it is also directly connected to many of the social issues with which I was concerned: war and peace, intergroup relations, class conflict, and family conflict.

It was a productive reframing that led to much research in our social psychology laboratory by my students and myself. My book The Resolution of Conflict: Constructive and Destructive Processes, published in 1973, summarizes much of this research and had a considerable impact in the social sciences. It helped to provide a new way of thinking about conflict and broadened the focus of the field to include constructive conflicts as well as destructive ones.

Our research into the question central to *The Resolution of Conflict* started off with the assumption that if the parties involved in a conflict situation had a cooperative rather than competitive orientation toward one another, they would be more likely to engage in a constructive process of conflict resolution. In my earlier research on the effects of cooperation and competition upon group process, I had demonstrated that a cooperative process was more productive than a competitive process in dealing with a problem that a group faces. I reasoned that the same would be true in a mixed-motive situation of conflict. A conflict could be viewed as a mutual problem facing the conflicting parties. Our initial research on trust and suspicion employing the Prisoner's Dilemma game strongly supported my reasoning, as did subsequent research employing other experimental formats. I believe that this is a very important result that has considerable theoretical and practical significance.

At a theoretical level, it enabled me to link my prior characterization of cooperation and competitive social processes to the nature of the processes of conflict resolution that would typically give rise to constructive or destructive outcomes. That is, I had found a way to characterize the central features of constructive and destructive *processes* of conflict resolution; doing so represented a major advance beyond the characterization of *outcomes* as constructive and destructive. This not only was important in itself, but it also opened up a new possibility: that we would be able to develop insight into the conditions that initiated or stimulated the development of cooperative-constructive versus competitive-destructive processes of conflict. Much of the research my students and I have done has been addresses to developing this insight.

Much of our early research on the conditions affecting the course of conflict was done on an ad hoc basis. We selected independent variables to manipulate based on our intuitive sense of what would give rise to a cooperative or competitive process. We did experiments with quite a number of variables: motivational orientation, communication facilities, perceived similarity of opinions and beliefs, size of conflict, availability of threats and weapons, power differences, third-party interventions, strategies and tactics of game playing by experimental stooges, the payoff structure of the game, personality characteristics, and so on. The results of these studies fell into a pattern that I slowly began to grasp.

All of these studies seemed explainable by the assumption, which I have labeled "Deutsch's crude law of social relations," *that the characteristic processes and effects elicited by a given type of social relationship (cooperative or competitive) also tend to elicit that type of social relationship*. Thus cooperation induces and is induced by a perceived similarity in beliefs and attitudes, a readiness to be helpful, openness in communication, trusting and friendly attitudes, sensitivity to common interests and deemphasis of opposed interests, an orientation toward enhancing mutual power rather than power differences, and on on. Similarly, competition induces and is induces by the use of tactics of coercion, threat, or deception; attempts to enhance the power differences between oneself and the other; poor communication; minimization of the awareness of similarities in values and increased sensitivity to opposed interests; suspicion and hostile attitudes: the importance, rigidity, and size of the issues in conflict, and so on.

In other words, if one has systematic knowledge of the effects of cooperation and competitive processes, one will have systematic knowledge of the conditions that typically give rise to such processes and, by extension, to the conditions that affect whether a conflict will take a constructive or destructive course. My early theory of cooperation and competition is a theory of the *effects* of cooperative and competitive processes. Hence from the crude law of social relations stated earlier, it follows that this theory provides insight into the conditions that give rise to cooperative and competitive processes.

The crude law is *crude*. It expresses surface similarities between effects and causes; the basic relationships are genotypical rather than phenotypical. The crude law is crude, but it can be improved. Its improvement requires a linkage with other areas in social psychology, particularly social cognition and social perception. Such a linkage would enable us to view phenotypes in their social environments in such a way as to lead us to perceive correctly the underlying genotypes. We would then be able to know under what conditions “perceived similarity” or “threat” will be experienced as having an underlying genotype different from the one that is usually associated with its phenotype.

Although the gaming conflicts in the laboratory during this period (1963-1973) were relatively benign, the conflicts in the outside world were not. During this period the cold war escalated; the Berlin crisis occurred; the brothers John and Robert Kennedy and Martin Luther King Jr. were assassinated; the United States was increasingly involved in the Vietnam War; there were teach-ins, campus upheavals, race riots, Woodstock, love-ins, communes, the emergence of the new left, and so on. I was not immune to the effects of these events, personally or professionally.

Professionally, as a result of *Preventing World War III* (of which I was coeditor), my activities in SPSSI, my various speeches, and our conflict studies, I became identified as one of the psychologists (along with Ralph White, Charles Osgood, Irving Janis, Jerome Frank, and Herbert Kelman) concerned with war and peace issues. I was invited to participate in meetings on the Berlin crisis, arms control, deterrence, Soviet-U.S. relations, and so on. Some involved high-level diplomats, others involved people in the defense establishment, others were at the UN, and still others were with citizen groups or social scientists. During the 1960s I was also trying to get more of my fellow psychologists involved in these issues. I took the opportunity of several addresses to speak to these issues: My 1960 SPSSI presidential address was “Psychological Alternatives to War” (Deutsch, 1960); my 1966 New York State Psychological Association talk was “Vietnam and the Start of World War III: Some Psychological Parallels” (Deutsch, 1966); my 1968 Eastern Psychological Association presentation was “Socially Relevant Science” (Deutsch 1969b); and my Kurt Lewin Memorial Award address was “Conflicts: Productive and Destructive”[3] (Deutsch, 1969a).

About the time I was finishing the manuscript for my conflict book, in May 1972, I received from Melvin J. Lerner, then at the University of Waterloo, an invitation to participate in a conference entitled “Contributions to a Just Society.” Mel had been an NYU social psychology student who had worked with Isadore Chein but had taken some courses with me. Shortly after the conference, he asked me to contribute to the *Journal of Social Issues* volume on the justice motive that he was editing. The two papers I wrote as a result of his urgings were “Awakening the Sense of Injustice” (Deutsch, 1974) and “Equity, Equality, and Need: What Determines Which Value Will Be Used as the Basis of Distributive Justice?” (Deutsch, 1975). In preparing these papers, I reviewed the existing work on the social psychology of justice an became quite dissatisfied with the dominant approach to this area: equity theory. My dissatisfaction led me to write an extensive critique of equity theory in 1977 (Deutsch, 1978, 1979) and, with the support of the National Science Foundation, to embark on a program of research on the social psychology of distribute justice. This program was, without my full recognition, something I had been engaged in for many years. Like Molière’s bourgeois gentleman, I had been “speaking justice” all the time without being aware of it. My dissertation study could be thought of as a study of two different systems of distribute justice, cooperative-egalitarian and competitive-meritocratic. Our research on bargaining and conflict had direct relevance to a central question in the social psychology of justice, namely, What are the conditions that facilitate the establishment of a stable system of justice among interactants that they will consider to be fair?

Our research program had three main components: (1) experimental studies of the effects of different systems of distributive justice, (2) research into the determinants of the choice of distributive systems, and (3) investigations into the sense of injustice. The theory and research that emanated from this program has been presented mainly in my 1985 book, *Distributive Justice*. I believe the it is an important extension of the work I had done on conflict.[4]. The book received extremely favorable reviews, but I was disappointed that it did not create as much of the stir as I had hoped, despite some of its interesting ideas and provocative research findings. Possibly this was due to my having included in the book many theoretical papers that had been published earlier.

The year 1982 was particularly outstanding for me. I made two important addresses. In one, my presidential address to the International Society of Political Psychology, I developed the concept of “malignant conflict” and described the processes involved in such conflicts and used this discussion as a basis for analyzing the cold war between the United States and the Soviet Union (Deutsch 1983, 1985). The reaction of the audience was very gratifying. In various follow-ups (e.g., interviews, talks, conferences, pamphlets) it received considerable attention.

The second address was my inaugural lecture as the E.L. Thorndike Professor of Psychology and Education at Teachers College. I admired Thorndike both as a psychologist and as a person (after reading an extensive biography of him), but I felt his views about race reflected the ignorance and bigotry prevalent in his time. In my opening remarks, I expressed my admiration for Thorndike but dissociated myself from his statements about racial and ethnic groups. My address was essentially a review of my work in social psychology. However, in a concluding section, I indicated my intention to help to further develop the educational implications and applications of my work on cooperation an d conflict resolution. To this end, I proposed establishing a center at Teachers College that would foster cooperative learning and constructive conflict resolution in the schools. At that time I vainly hoped that I might be able to induce a former student of mine to direct, administer , and raise funds for such a center; I never liked administrative work or raising funds, even though I had been reasonably successful in doing so during my career. In 1986, with the aid of a small grant from President Michael Timpane ($9,600), I started the center that I later ambitiously name the International Center for Cooperation and Conflict Resolution (ICCCR).

In 1982 I also published a paper, “Interdependence and Psychological Orientation,” that integrated several strands in my work. Mike Wish and I (while Mike was on the faculty at Teachers College) did some initial work on characterizing the fundamental dimensions of interpersonal relations. This work grew out of some research that my students and I were doing on marital conflict; we felt it would be useful to go beyond personality descriptions of the individual spouses so that we would be able to characterize the couple as a couple in terms of their relations to one another. Using various data-collection procedures and multidimensional scaling methods, we (Wish, Kaplan, & Deutsch, 1976) came up with five dimensions: cooperation-competition, power distribution, task-oriented versus social-emotional, formal versus informal, and intensity of the relationship.

Previously, I had done much to characterize the social psychological properties of the first dimension, cooperation-competition. Now I sought to do this for the others. Undoubtedly influenced by the popularity of the cognitive approach, I labeled my first attempt “modes of thought.” But this title did not seem to be sufficiently inclusive. It appeared to me evident that cognitive processes differ in types of social relations, and I wanted to sketch the nature of some of these differences. However, I also thought that the psychological differences among the types of social relations were not confined to the cognitive processes: Various motivational and moral dispositions were involved as well. It had been customary to consider these latter predispositions as more enduring characteristics of the individual and to label them “personality traits” or “character orientations.” Since my emphasis is on the situationally influenced nature and, hence temporariness of such predispositions, these labels did not seem fitting either. Thus I settled on the term "psychological orientation" to capture the basic theme of this paper, namely, that people orient themselves differently to different types of social relations and that these orientations reflect and are reflected in various cognitive processes, motivational tendencies, and moral dispositions.

At the time I was not doing research in cognitive social psychology, but I was sympathetic to it for two reasons. First, as someone greatly influenced by the Gestalt psychologists as well as by Lewin and Fritz Heider, I felt perceptual and cognitive processes were very important. Second, I felt it was a healthy reaction to the antimentalist views of B.F. Skinner and his followers, which were quite popular in psychology in the 1960s and 1970s. My sympathies for the cognitive approach possibly unconsciously led me to suppress the significant differences between it and my emphasis on psychological orientations. Psychological orientations involve the cognitive but also motivational and moral orientations. In the 1980s, cognitive social psychologists neglected both the motivational and moral aspects of people's orientations to social relations.

More recently, there has been increasing recognition of the importance of motivation, even belated recognition of the relevance of Lewin's approach, which integrates cognition and motivation. However, psychologists have not yet acknowledged that there is a moral, normative feature to every type of social relation and that any reasonably full characterization of the psychological orientation associated with a social interaction (or its perception) will include the person's moral orientation as well as his or her cognitive and motivational orientation. My work in the area of justice, of course, has helped to sensitize me to the importance of moral norms in social situations. I speculate that the neglect of the moral component of psychological orientation is linked o the fact that the study of justice has not been central in the social psychological research literature. The flurry of interest in equity theory died down in the late 1970s with the decrease of interest in dissonance theory. The dissonance component of equity theory was its most interesting psychological feature.

After publishing *Distributive Justice* in 1985, I sought funding from NSF for a program of basic research related to some of the ideas in my paper "Interdependence and Psychological Orientation." Unfortunately, my proposal was not funded. By this time our NIMH-supported, predoctoral training program was no longer in existence; NIMH's interest had turned toward postdoctoral training. Teachers College provided no funds for research or for graduate research assistants and little secretarial support or money for travel or equipment. It was also a period in which academic appointments became scarce. The consequence was that our doctoral students increasingly became part-time students who often had full-time jobs. In addition, they became more interested in nonacademic positions and more frequently decided to specialize in the organizational rather than in the social psychology component of our doctoral program in social and organizational psychology.

In this context I discontinued my basic research, which had been primarily conducted in the laboratory. From 1985 on, I continued to write and publish papers mainly for small conferences related to conflict or justice, several as award addresses for honors I was receiving and a number by invitation of editors of books or special journal issues. Among the thirty articles I have published since 1985, several titles stand out: “On Negotiating the Non-Negotiable”; “Psychological Consequences of Different Forms of Social Organization”; “The Psychological Roots of Moral Exclusion”; “Sixty Years of Conflict”; “Equality and Economic Efficiency: Is There a Trade-Off?” “Kurt Lewin: The Tough-Minded and Tender-Hearted Scientist”; “Educating for a Peaceful World”; “The Effects of Training in Cooperative Learning and Conflict Resolution in an Alternative High School”; “Constructive Conflict Resolution: Theory, Research, and Practice”; (with Peter Coleman) “The Mediation of Interethnic Conflict”; “William James: The First Peace Psychologist”; and “Constructive Conflict Management for the World Today” (see citations in the References).

The International Center for Cooperation and Conflict Resolution

In 1986 I started the center that I promised in my Thorndike inaugural address. Our first activity was a workshop to which I invited the superintendents of school districts n and around New York City as well as representatives of several foundations who might become interested in financing the activities or our center. In addition to introductory remarks made by the president of Teachers College and myself, the workshop consisted of a series of miniseminars chosen to reflect the kinds of activities in which our center would engage: cooperative learning, the constructive use of controversy in teaching, conflict resolution training in schools, the training of student mediators, and research evaluation of programs. Each seminar was conducted by a leading expert (e.g., David and Roger Johnson led the seminars on cooperative learning and the constructive use of controversy).

As the result of this workshop, one of the superintendents invited us to develop a program of cooperative learning in his wealthy, suburban school district and to evaluate the program. We sought without success to broaden the program to include conflict resolution training. However, the superintendent was helpful in arranging for us to meet with the superintendent of a nearby, comparable school district that would serve as a control. We approached several foundations for funds but were rejected until I noticed in a publication that Hank Riecken was on the board of the W.T. Grant Foundation. I contacted Hank and told him of our plans and hopes, and he arranged for me to meet with the president and himself. Both were enthusiastic about our plans, which called for support for five years at a level of $200,000 per year, and they asked me to write a detailed proposal for submission to the board. The board approved the project for three years and indicated that after the first year we should obtain half our funds from other sources. At the time I did not realize that this was a customary but nasty policy of many foundations – forcing one to remain continuously in a fund-raising mode.

We began the project with a preliminary workshop in which David Johnson got a group of senior, influential teachers involved in cooperative learning. They became enthusiastic supporters. Our next step, which proved to be fatal, was to introduce the questionnaires, observational measures, and other recorded data we wished to obtain. We needed permissions from the school board as well as from the school personnel and parents of the students. When the school board learned that we were not only interested in academic achievements but also in measuring social skills, social relations, and psychological adjustment, they were horrified and canceled permission to do the study in their district. As the superintendent regretfully explained, the political attitudes of the board members were to the right of Attila the Hun, and they thought of mental health as a dangerous, explosive topic.

At this point I was sorry that I had left the social psychology laboratory to do research in field settings. However, Ellen Raider, who had joined our center as training director after we were funded, came up with the center-saving suggestion that we move our project to an inner-city, alternative high school where she knew the principal and associate principal. Luckily, the foundation was happy to approve the move; they preferred that our research be done with inner-city youth.

I shall not describe the many headaches and heartaches we had in carrying out our research other than to indicate that we were training overworked and fatigued but dedicated teachers, most of whose students lived in poor and difficult circumstances and often did not have the reading or writing skills necessary for successful work as high school students. Also, to put it bluntly, the physical conditions of the school and neighborhood were horrible. Many aspects of the project were not executed as well as we had planned: the training of the teachers; the measurement of the effects on students; the duration of the study; the records kept by the school on student attendance, dropouts, disruptions, and so on. By the standards of a laboratory experiment, it was very unsatisfactory research. Yet I must say that I came out of this study with a great deal of appreciation of those researchers who are foolhardy enough to leave the laboratory. They must have the kind of administrative and social skills, flexibility, ingenuity, statistical wizardry, and frustration tolerance rarely required in laboratory studies.

Despite our problems, much to our surprise, we were able to demonstrate that our training had important and significant effects on the students. In brief, the data showed that as students improved in managing their conflicts (whether or not because of the training in conflict resolution and cooperative learning), they experienced increased social support and less victimization from others. This improvement in their relations with others led to greater self-esteem as well as a fewer feelings of anxiety and depression and more frequent positive feelings of well-being. The higher self-esteem, in turn, produced a greater sense of personal control over their own fates. The increases in their sense of personal control and in their positive feelings of well-being led to higher academic performances. There is also indirect evidence that the work readiness and actual work performance of students were also improved. Our data further indicated that students, teachers, and administrators had generally positive views about the training and its results.

This study was the first longitudinal study of the effects of cooperative learning and conflict resolution training conducted in a very difficult school environment. It was also the first to go beyond the measurement of consumer satisfaction. Its positive results were consistent with our theoretical model and with results obtained in smaller, brief studies in experimental classrooms. In part because the study was conducted in the New York City school system, the city’s board of education made a contract with ICCCR in 1992-1994. The contract specified that ICCCR would train two key faculty of staff people from every high school in New York City so that one would become sufficiently expert to be able to train students, teachers, and parents in constructive conflict resolution and the other would become sufficiently expert in mediation to be able to establish and administer an effective mediation center at the school, with students functioning as mediators.

Ellen Raider and her staff conducted the training, which took place for fifty hours over ten sessions, for a total of 300 people in cohorts over a year and a half. The training methods were based on a model and manuals developed by Ellen Raider and Susan Coleman. The principals of the various high schools also received training in conflict resolution and mediation in three-day workshops, abbreviated versions of the larger sessions.

Although ICCCR was not provided with funds to conduct a research evaluation of its training, the research division of the board of education and the Dispute Resolution Center of John Jay College were able to conduct some relevant research. The research indicated that within two years of training almost all of the more than 150 high schools who participated had established mediation centers in their schools (fewer than 5 percent had not). In addition, most of the schools had introduced into their curriculum education in constructive conflict resolution, and thousands of students had exposure to such education. All participants in the research believed that the program had a positive impact on personal relationships and school climate overall. Cited were improvements in the way students dealt with anger and resolved conflicts, heightened respect for differences, better communication skills, and increased understanding of students’ needs on the part of the school staff. Some people noted that the school atmosphere was calmer and more collaborative. Peer mediators, disputants, and students who had participated in lessons in cooperative negotiation all commented on positive changes in their own interactions with others, both within and outside of school. Most telling, perhaps, was that disputants had enthusiastically recommended peer mediation to their friends, and curriculum students believed that all students should be required to take lessons in conflict resolution.

ICCCR continues to do conflict resolution training in various school systems and in other contexts, such as the United Nations. More recently, as a prelude to offering graduate studies in conflict resolution at Teachers College, Ellen Raider conducted workshops on conflict resolution with various members of the faculty. The graduate studies now exist as one of the concentrations in the degree programs in social and organizational psychology as well as a certificate program for nondegree students. I have continued to teach a theory course entitled “Fundamentals of Cooperation, Conflict Resolution, and Mediation.” Ellen and her staff have been conducting our various practica courses in this area.

I have also been the organizer for a faculty seminar on conflict resolution from which a book is now in preparation, “The Handbook of Constructive Conflict Resolution: Theory and Practice,” to be published by Jossey-Bass in 2000. I have written four chapters for it, and I am serving as its editor along with Peter Coleman, who is the new Director of ICCCR. As I have reduced my academic responsibilities (less teaching, no more faculty meetings, only one or two highly selected doctoral students whom I supervise), Lydia and I have been doing considerably more traveling and dining in superb restaurants.

Conclusion

As I look back upon my career, several things stand out for me.

Luck. I was lucky to go to CCNY, which had two young faculty members, Max Hertzman and Walter Scott Neff, who stimulated my interest in Lewin and in social psychological research. I was extremely lucky to be a student at the RCGD at MIT, where I was able to become part of a small, innovative group of faculty and students who had a major impact on the development of modern social psychology. Moreover, my career got off to a quick start largely as a result of the prodding of Stuart Cook, who had me involved in writing two books shortly after I obtained my Ph.D. Also, I was fortunate to be able to receive financial support for my research throughout most of my career. In addition, I have had the opportunity to work with many excellent, productive students who have stimulated me and contributed much to my research. Not least, I was lucky enough to marry a woman whose esthetic sensibility and practical skills helped to create a congenial and supportive home environment that enabled me to focus my attention on scholarly activities rather than on such household activities as fixing things (which I never could do anyway).

Continuing Themes. My work on social psychology has been dominated by two continuing themes with which I have been preoccupied throughout my career. One is my intellectual interest in cooperation and competition, which has

Been expressed in my theorizing and research in the effects of cooperation and competition, our studies of conflict processes, and our work on distributive justice. I have continued to believe that these foci are central to understanding social life and also that a “social” social psychology rather than an “individual” social psychology would have these as its fundamental concerns. The second continuing interrelated theme has been developing my work so that it has social relevance to key social problems. Sometimes images, derived from such social problems as war and peace, prejudice, marital conflict, and injustice, would be the starting point for the development of a theoretical analysis or an experimental study. At other times I would use theory and research (other social scientists’ as well as mine) in an attempt to shed light in important applications, particularly in the field of education, where I am considered to be one of the parents of cooperative learning and conflict resolution training.

Episodic Research. Occasionally, I strayed from the two themes just described, to do single studies that expressed my reservations about some of the fashionable theorizing and research. I took potshots at Solomon Asch’s neglect of group factors in his conformity studies, at Festinger’s omission of defensiveness in his dissonance theorizing, at equity theory’s assumptions of greater productivity when people are rewarded in proportion to their performance, at social perception studies that ignored the social and institutional context in which social acts are imbedded, and at Henry Tajfel’s initial assumption that the mere awareness of a difference among a collection of individuals will promote group formation. My straying was usually short-lived because my primary interests were in the two themes described above and I was not sufficiently energetic to take on additional themes.

Familial Context. As I look back on my career, I am impressed by how much its themes have been influenced by my experiences within my family as well as what was occurring in the broader society. Within my family, I was the youngest of four sons, and I felt a strong need to catch up with my next older brother (two and a half years older), believing that if I did not I would be excluded. In fact, one of my earliest memories focuses on injustice. I was about three and a half years old. We were all staying at a resort in the Catskills, and a counselor organized a game of softball for the older kids (the six- to eight-year-olds). I was excluded from it because I was too young and was asked to stay on the side. I was very mad, and when a foul ball was hit near me, I recall picking it up, running with it, and throwing it as far as I could in a direction away from the players. I trace my passionate feeling about injustice to such early experiences as this one. In my attempts to keep up with the older kids, as a child and youth I was quite competitive. However, it was a strain, and when I lost I felt injured and when I won, surpassing my older brother and his friends, I could feel their sense of hurt and shame. My questioning of the value of competition undoubtedly arose from these episodes. This questioning was reinforced by the favorable attitude toward socialism that was held by my parents and many of their friends. My father became rabidly antiunion during my rebellious adolescence, and, perversely, this strengthened my favorable view of unions, cooperation, and socialism.

The Social Context. I grew up in a time when, as a Jew, I experienced many instances of prejudice, blatant as well as subtle, and could observe the gross acts of injustice being suffered by blacks. In my youth and adolescence, there was the economic depression, union organizing, the Spanish civil war, and the emergence of fascism, Nazism, and Stalinism. I was politically engaged – contributing lunch money to the Spanish loyalists, organizing strikes in high school and in a summer resort, participating in a sit-in against the fascist ambassador, and so forth. It is no wonder that I was attracted to Lewin, who I saw as taking psychology in a direction that would enable it to contribute to the development of a democratic cooperative society that was free of prejudice.

The activist theme in my career as a social psychologist undoubtedly reflects the social context of my youth. The social context also helps to explain why I did not become a political activist or union organizer. In my family, among my fellow (mostly Jewish) students, and in my high school and college, there was a strong emphasis on ideas and intellectual achievement. Our heroes were those who contributed to the world through their ideas – Darwin, Marx, Freud, and Einstein. They had exemplified Lewin's dictum, recalled earlier, that "there is nothing so practical as a good theory." This has been the second theme of my career.

One final note: Every society has its own implicit assumptions of which its members are usually not aware. We live in a highly individualistic society. Its ethos is that of the lone, self-reliant, enterprising individual who has escaped from the restraints of an oppressive community so as to be free to pursue his or her destiny in an environment that offers ever expanding opportunity to those who are fittest. I think this image has influenced much of American social psychology, which has been too focused on what goes on in the isolated head of the subject, with a corresponding neglect of the social reality in which the subject is participating.

The socialist ethos incorporates the view that the human being is a social animal whose nature is determined by the way people are related to one another in their productive activities in any given community. Its vision is of social beings free to cooperate with one another toward common objectives because they jointly control the means of production and share the rewards of their collective labor. This vision is a useful supplement to the dominant emphasis in American social psychology. However, it is neglectful of the characteristics of individual persons – characteristics that are determined mainly in the course of interaction between the biological person and his or her social environment.

I conclude with the hope that future social psychologists will be more concerned than we have been with characterizing the socially relevant properties of individuals and the psychologically relevant attributes of social structures. To oversimplify it, I hope that they will provide a successful integration of the orientations of three of the intellectual heroes of my youth: Freud, Marx, and Lewin.

Notes

1. Lewin was widely admired by other psychologists. In the summer of 1947, after his death, there was a meeting of the Topological Circle at Smith College. At this meeting there were such eminent psychologists as Fritz Heider (the host), Edward Chace Tolman, and David Rappaport, as well as many of the faculty and students of the RCGD. At that meeting Heider presented the ideas that are the core of his subsequently published book. Heider was a shy and somewhat inarticulate public speaker, but the profundity of his ideas gripped us all. The meeting also provided us the opportunity to have lively informal discussion with Tolman and Rappaport (who offered me a job at Austen Riggs).
2. One sour note in connection with my dissertation: For it, I had developed an observation schedule and manual describing the "function of participation" for characterizing the behavior of group members. It included a description and detailing of various task, group, and individual functions. I also used this material in analyzing observational data in connection with the research done on the first NTL. Much to my surprise, shortly before my dissertation defense in the summer of 1948, an article by Kenneth Benne and Paul Sheats entitled "The Functional Role of Group Members" appeared in the *Journal of Social Issues*. This article was mainly a reprint of my manual with some elaborations; my authorship received no acknowledgement. When I brought this to the attention of Benne and Sheats, they acknowledged that their article was based on my manual, but since it did not have my name on it, they thought it was some impersonal product of NTL. They apologized for their error, but when the article was widely reprinted in books, there was no attempt to undo their error. When I published my dissertation, I included a footnote indicating that some of my dissertation material had been published in "The Functional Role of Group Members."
3. In 1968 I also gave this address at a meeting of social psychologists from the West (the United States and Western Europe) and from Eastern Europe. We met in Prague shortly after the Soviet Union had sent its troops into Czechoslovakia to squash an incipient rebellion against Soviet domination. Despite our misgivings, we came at the strong urging of our Czech colleagues who wanted to maintain their contacts with the West. My paper included a section on what strategies and tactics were available to "low-power" groups when confronting "high-power" groups. The Czechs loved it and widely circulated a tape recording they made of it.
   Leon Festinger, in contrast, asked me, "Is this science?" I replied, "Leon, you and I have a different conception of the nature of science." My conception, I believe, was more inclusive than his. Leon and his followers were always puzzled by me: They thought I did fine theoretical and experimental work, but they did not understand my willingness to apply the best available social science knowledge to important social issues even when that knowledge was not firmly rooted in experimental research.
   The meeting in Prague was sponsored by the Transnational Social Psychology Committee of the Social Science Research Council (SSRC). Leon was its chair, and under his leadership it did much to stimulate the development of social psychology in Western Europe.
   However, Leon was very much annoyed and harshly criticized Henry Tajfel for his

manuscript “Experiments in a Vacuum” and Serge Moscovici for his “Society and Theory in Social Psychology,” both of which were critical of American social psychology. This occurred during a committee meeting in West Germany in 1971.
The committee also exerted some efforts to develop social psychology in Latin America. We held a seminar in Chile for Latin American social psychologists during the tumultuous period just prior to Salvador Allende’s coming to power. After Leon resigned as the committee chairman, I was asked to take on this role. We had another East-West meeting in Hungary, in a small resort village about 20 miles from Budapest. We also held a conference in Majorca that led to the book Applying Social Psychology (Deutsch and Hornstein, 1975) About this time, SSRC decided to end its financial support for the committee (it had had a rather extended life by SSRC’s usual standards for committees). The committee, however, was not quite ready to quit. Martin Irle hosted a small meeting in Mannheim, Germany. I hosted an even smaller one in my beach house in East Hampton, New York, and Jujuji Misumi hosted an even smaller one in Japan.
This traveling committee, which met mainly outside the United States (so as to stimulate the development of social psychology elsewhere), included – at different times – such people as Leon Festinger, John Lanzetta, Stanley Schachter, Harold Kelley, Henry Riecken, and myself from the United States, as well as Serge Moscovici, Henry Tajfel, Jaap Kookebacker, Martin Irle, Ragnar Rommetveit, Jujuji Misumi, and Jaromir Janousek from other parts of the world. Throughout much of its existence, Jerome Singer was the committee’s witty and tolerant administrator for SSRC.
During much of the same time, there was another traveling committee funded by trhe Office of Naval Research, through Luigi Petrullo, which met to discuss research on conflict. About half of its members were from the United States and the other half from Western Europe. Its U.S. members included Harold Kelley, Gerald Shure, John Thibaut, John Lanzetta, Dean Pruitt, and myself. Among the European were Serge Moscovici, Henry Tajfel, Claude Faucheux, Claude Flament, and Josef Nuttin Jr. We met about twice a year, alternating locales between Europoe and the United States. We had many good discussions, excellent wine and food, and formed some lasting friendships. We also did a cross-national experiment and bargaining that has rarely been cited. It was a wonderful period to be a social psychologist.

4. Among the many students who contributed directly to this book were Rebecca Curtis, Michelle Find, Sandra Horowitz, Ivan Lansberg, Brian Maruffi, Louis Medvene, Dolores Mei, Marilyn Seiler, Janice Steil, Bruce Tuchman, Janet Weinglass, William Wench Jr., and Cilio Ziviani. Other students in my work groups on justice who have contributed indirectly to this volume include Lorinda Arella, Adrienne Asch, Susan Boardman, Ellen Brickman, Ellen Fagenson, Martha Gephart, Cheryl Koopman, Jay Kantor, Eric Marcus, Susan Opotow, Jorge da Silva Ribeiro, Rony Rinat, Shula Shichman, and Rachel Solomon.

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