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Continuity and Change: My Life as a Social Psychologist

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When you reach a certain age, you become increasingly interested in reflecting on your life and writing autobiographically about your career and your ideas, and others become increasingly interested in hearing "your stories" (to quote Mica Estrada-Hollenbeck, one of my students, to whom collectively these remarks are dedicated). We all know, of course, that the interest in telling these stories and in listening to them is bolstered by social norms that legitimize older people's reminiscences and mandate younger people's polite attention. I am quite happy, however, to take advantage of these norms and to indulge my autobiographical musings.

In a recent collection of essays by Holocaust refugees and survivors who subsequently became social scientists (Suedfeld, 2001), I had the opportunity to reflect on the impact of the Holocaust on four topics that have been central to my work over the years: conformity and obedience, nationalism and national identity, ethnic conflict and its resolution, and the ethics of social research (Kelman, 2001a). In an article that I am writing for *Political Psychology* (and which is characteristically late), I trace the different ways in which interactive problem solving—my approach to conflict resolution (Kelman, 1998a, 1998c; see also Kelman, 1972a), derived from the work of John Burton (1969, 1979)—reflects central themes of my earlier work. Recent papers reviewing my work on the Israeli–Palestinian conflict (Kelman, 1999) and on the concept of legitimacy (Kelman, 2001b) have a decidedly autobiographical flavor. Furthermore, several years ago, some of my students initiated an oral history project, in which I have had the opportunity to talk about and reflect on each of the problem-solving workshops and related programs—over 60 events by now, mostly (but not entirely) with Israeli and Palestinian participants—that I have been involved in over the years. The project is now being brought to completion by Cynthia Chataway and Reina Neufeldt, with the collaboration of Rebecca Edelson. Also, my colleague Michael Wessells has been conducting a series of interviews with me, which he will eventually write up, focusing on the origins and development of my work in peace research, conflict resolution, and the social psychology of international relations.

The present chapter gives me another and very special opportunity to reflect on my work during the past 55 years. The focus of these reflections is my particular way of doing social psychology over these years—my way of expressing the core of my professional identity as a social psychologist. The background of these reflections, very appropriately, is the work of my students as exemplified in the chapters and comments in the preceding pages.

On Being the Subject of a *Festschrift*

I have always felt that the greatest tribute that can be paid to a scholar is to issue a *Festschrift* in her or his honor. The present *Festschrift*, therefore, is a gift that has profound meaning for me and that I value immensely. It validates my work over the years and gives me the sense that what I have tried to do has had an impact on others, that it has reverberated in what they chose to study and how they chose to study it, and that it is a link in that endless chain of efforts to understand and improve our world. I am deeply grateful to all who played a role in this enterprise—in planning, arranging, speaking at, and participating in the *Festschrift* conference in August 2000, in editing this volume, and in writing, presenting, reviewing, and editing the chapters and comments.

When Alice Eagly first spoke to me about the people to be asked to present papers and prepare chapters for the *Festschrift*, and later about the list of people to be specifically invited to participate in the August 2000 conference, I was very clear about one principle: I wanted my students and their work to be the primary focus of the enterprise. There is no necessary reason for a *Festschrift* to focus on the subject's students. It would be quite appropriate for the contributors to be nonstudent collaborators or even colleagues who neither studied nor collaborated with the subject but were influenced by his or her work. Indeed, in the present case, the conference invitation list included not only my students, but also my closest colleagues and collaborators over the years.[1] Still, it was my students (many of whom, of course, have also been and continue to be my close collaborators) whom I wanted to be the contributors to the *Festschrift* itself.

My criteria for claiming people as "my students" may be a bit expansive (or should I say expansionist?), as can be judged from the three lists included in Appendix A. The first list is not controversial. It includes, in chronological order, the 33 doctoral candidates for whom I served as the primary thesis adviser (or *Doktorvater*, to use the German designation that I find appealing). I was pleased to note that both the first and the last person on this list, Peter Lenrow and Rebecca Wolfe, respectively, were at the conference. Further analysis of this data set reveals that the median position on this list is held by Lee

[1] I was delighted to welcome at the conference current collaborators, like Lenore Martin, and close colleagues from earlier periods, like Arthur Gladstone (going back to the late 1940s and 1950s), William and Zelda Gamson (going back to the 1960s), and Gordon Bermant (going back to the 1970s), as well as Ai-Li Chin, the widow of Robert Chin, a close friend and colleague over many years. I would have been equally delighted to welcome other close collaborators from different periods of my life—such as John Burton, Stephen Cohen, Ronald Fisher, Jerome Frank, Harry Lerner, Christopher Mitchell, Morris Parloff, Thomas Pettigrew, Harold Saunders, Charlotte Schwartz, Brewster Smith, Michael Wessells, and Ralph White—who, regrettably, were not able to make the event.

Hamilton, who (among numerous other achievements) coined the term *crimes of obedience*, which made both of us famous (Kelman & Hamilton, 1989). Of the 33 individuals on this list, 26 received their PhDs from Harvard University, having worked with me either during my first five-year term (1957–1962) as Lecturer on Social Psychology or during my return engagement (1968–1999) as Richard Clarke Cabot Professor of Social Ethics. Six individuals received their degrees from the University of Michigan between 1965 and 1969, during my tenure there (1962–1969). (For the benefit of careful readers, I should note that in the academic year of 1968–1969, I was a professor both at the University of Michigan and at Harvard, but teaching at neither—a coup that I attribute to my low-key negotiating style.) Nadim Rouhana received his PhD from Wayne State University, but had come to Harvard—with the blessings of his Wayne adviser, Kalman Kaplan (who himself can be found on the third list in Appendix A)—to work with me on his dissertation. I was appointed adjunct professor at Wayne (needless to say, without pay) to serve as Nadim's adviser.

The second list in Appendix A includes individuals for whom—at various points in their graduate training—I served as academic adviser, research/practice adviser, member of the thesis committee, and/or thesis reader. Most of the people on this list were graduate students in my department at Harvard or Michigan. However, the list also includes a dozen individuals who received their doctorates from schools other than my own[2] on whose doctoral committees I played an active role. Interestingly, all 12 of these people at some point took or audited my graduate seminar on International Conflict: Social Psychological Approaches. Also included on this list are people who have been actively associated with PICAR, my Program on International Conflict Analysis and Resolution at Harvard's Weatherhead Center for International Affairs. For many of these, the association began with their participation in my graduate seminar on international conflict—which was clearly a major recruiting ground as well as socialization experience for my graduate students in the 1980s and 1990s. List II is definitely not complete. I constructed it from memory, since I have not kept systematic track of all of my advising and thesis-reading assignments. Names appearing on this list belong to those advisees in whose training I played an active role and with many of whom I have maintained continuing contact.

The third list in Appendix A includes postdoctoral fellows, research associates, and visiting scholars who came to Harvard or the University of Michigan under my sponsorship. I do not include in this list names that already appear on lists I and II. Moreover, like list II, this list is not comprehensive; of the names included, some are individuals with whom I have collaborated closely on joint research projects, and all are individuals with whom I interacted closely on shared intellectual interests. Again, I have maintained continuing contact with many of the people on this list. Whether I have a right to claim them all as

[2] The Harvard Graduate School of Education (Ariela Bairey-Ben Ishay, Winnifred O'Toole, Sara Roy, Pamela Steiner), the Kennedy School of Government (Thomas Princen), the Fletcher School of Law and Diplomacy (Daniel Lieberfeld), MIT (Eileen Babbitt), Boston University (Maria Hadjipavlou), the City University of New York (Bethamie Horowitz, Lynn Ruggiero), the University of Maryland (Jay Rothman), and the University of Oslo (Daniel Heradstveit).

my students is open to debate. The designation is entirely appropriate for those who came specifically as postdoctoral fellows shortly after receiving their degrees. I felt it was also appropriate for those who came to work with me as research associates at Harvard and the University of Michigan early in their careers. Including on this list people who came as visiting scholars at a later stage in their careers may be an indicator of the expansionism I mentioned. I justify it by the fact that many of them have themselves described me as their "mentor," thus feeding my expansionist tendencies. The best case in point is the last name on list III, Jorje Zalles. Although we interacted intensively during his year as a visiting scholar at the Weatherhead Center, he had actually not come specifically under my sponsorship. But, when he took to calling me *maestro* (even in print), I felt justified in including him on my list.[3]

These three lists do not exhaust the categories of people whom I feel I could rightfully claim as my students. Omitted from these lists are the sizable number of undergraduates at Harvard whose honors theses I supervised, some of whom have gone on to become accomplished social psychologists. One of these undergraduates, as it happens, did make list I; I refer to none other than Alice Eagly, who produced a *summa cum laude* undergraduate thesis under my supervision. (Her thesis experiment, along with one of my experiments, was later published in a joint article; see Kelman & Eagly, 1965.) Alice went on to the University of Michigan, where I joined her a year later and eventually became her doctoral thesis adviser. One of my qualifications for that role, I am sure, was that I had learned early on that the best way to supervise Alice was not to interfere as she proceeded with great competence to do what needed to be done.

Also omitted from the three lists are my students in the various graduate seminars and undergraduate courses that I taught over the years, unless I played additional, active roles in their graduate education. It is always a special treat to meet or hear from former students in my classes—including some who

[3]I want to remember warmly and pay tribute to six people on these lists whom we lost to premature death. Margaret (Peggy) Hofeller was my student at Michigan and spent her career as a teacher and dean at Hofstra University; my frequent discussions with her about the concept of legitimacy, which was the focus of her doctoral thesis, greatly helped me in developing my own ideas on this topic. Stanley Milgram was already an advanced graduate student when I first came to Harvard, but I served on his thesis prospectus committee and as a careful reader of the final product; in later years, we interacted on various occasions around our shared interest in obedience to authority and the ethics of human experimentation. Donald Warwick was an advanced graduate student when I arrived at the University of Michigan, and I served on his doctoral committee; later he became one of my closest colleagues and best friends at Harvard, where we cotaught a course, coauthored several chapters, coedited a volume on *The Ethics of Social Intervention* with Gordon Bermant (Bermant, Kelman, & Warwick, 1978), and jointly participated in various projects relating to ethical issues in social science. I first met Earl Davis in Germany in 1960, and we interacted frequently around several shared interests until his death in Ireland, where he had spent a large part of his career; he was a Visiting Scholar at Harvard under my sponsorship in 1982–1983. Anita Mishler and I were both research assistants at the National Training Laboratory for Group Development in Bethel, Maine, in the summer of 1948; in the late 1950s and early 1960s she worked, along with Lotte Bailyn, as my research associate on a project dealing with the impact of a year in the United States on Scandinavian exchange students. Finally, I met Jeffrey Rubin shortly after he arrived at Tufts in the fall of 1969 and we became good friends and collaborated on a variety of projects until his tragic death in 1995; he also spent a year as a visiting scholar at Harvard under my sponsorship.

took one of my large undergraduate courses and whose names or faces I would not have recognized if they had not revealed themselves. I am delighted when these former students tell me about the special memory, insight, or standpoint that they took away from the course—particularly when they tell me how the course has changed their lives or their view of the world. (Needless to say, I assume that these reported changes have been of positive value to their lives and to the world at large.) Special mention should be made here of my seminar on international conflict, which I taught at Harvard 17 times (the first two times with Stephen Cohen and the last two times with Donna Hicks) between 1971 and 1999. For many of the students and active auditors in this course (a total of perhaps 400 over the years)—whether or not their names appear on my three lists—participation in this intensive seminar and its associated practicum did, in fact, have substantial impact on their subsequent professional careers.

Finally, I restrained myself from including on the lists some of my younger colleagues in different fields who—though they were never my students in the conventional sense of the term and never worked under my sponsorship—have described me as their mentor or role model. In according me this honor, these colleagues were often communicating not only that their own work was influenced by mine, but that their definition of their professional roles was encouraged and legitimized by my model: in stepping outside of traditional disciplinary boundaries, in combining research with practice, in addressing current social issues, in attending to the ethical implications of the professional enterprise. I happily claim these colleagues as my students, but I do not feel entitled to add their names to my "official" lists.

The contributors to this volume are a sample of my students over the years.[4] Most of the chapter authors are drawn from list I, although lists II and III are represented by two authors each. The six commentators (all of whom can be found on list II) were all, in one way or another, my current students at the time of the *Festschrift* conference. Since then, three of them (Jennifer Richeson, Erin Driver-Linn, and Rhoda Margesson) have completed their work and received their PhDs. When I describe the contributors as a sample of my students, I do not imply that they are a random sample. They were selected to represent different eras, different interests, different orientations, different spheres of activity, different disciplines, different nationalities. Differences aside, they are all individuals whose work and ideas I value and toward whom I feel great friendship and affection. Though they are not a random sample, they do represent the body of my students, in that many others could have been invited to contribute to the *Festschrift* and all, individually and collectively, are of great personal importance to me. Indeed, many others of my students participated in the *Festschrift* conference—in some cases, coming from long distances. (John Smetanka, whom we tried very hard to trace, and eventually located in Bangladesh, gets the prize for making the longest journey.) Some spoke from the floor, others made moving remarks at the dinner. Several told me how much they enjoyed meeting their "siblings" from earlier or later generations.

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[4]The one exception is Ann Locke Davidson, collaborator and coauthor of Janet Schofield. I have not met her personally, but am happy to welcome her to the family.

The astute reader will have noticed by now that my students, in all their categories and varieties—those who are listed and those who are unlisted, those whose contributions appear in the preceding pages, those who participated in the *Festschrift* conference and those who were unable to come (in some cases sending much appreciated messages of regret)—have, individually and collectively, occupied a central place in my life. I can only hope that I have added some meaning to their lives; I can say with assurance that they have given meaning to mine. This is hardly surprising, in view of the fact that the role of teacher was a central part of my identity during my 42 years of active faculty service at Harvard and Michigan—and, indeed, remains a central part of my identity more than 4 years into retirement, even though I no longer teach classes or (officially) take on new advisees. Many teachers develop a feeling of closeness to their students, especially graduate students with whom they work on their doctoral dissertations; it is no coincidence that familial terminology is often used to characterize the relationship. This feeling is particularly marked, however, for me and my wife, Rose, because we do not have children of our own. My students provide the richness and continuity that add meaning to our lives.

The Formative Years

Although I have spent most of my career in the teaching role, I did not begin serious teaching until 1957—10 years after starting graduate school and 6 years after receiving my PhD. Thus, I had a significant period of time in which I was able to develop my identity as a social psychologist before I even began to develop my identity as a teacher.

When I began my undergraduate studies at Brooklyn College in 1943, at age 16, I had only the vaguest career plans. I was still a member of the religious Zionist youth group that I had first joined in Vienna in 1938, after the Anschluss. The trajectory for members of this organization was to make *aliyah*—move to Palestine—and live in a *kibbutz*. I believe that, by the time I started college, I had pretty much decided that I was not going to follow that path, although I am not sure exactly when and how I had made that decision and dropped out of the group. Nevertheless, my expectation was that I would pursue a career somewhere within the domain of Jewish life—perhaps as an educator, community worker, journalist, or some combination thereof. Writing was always part of that package and so, in the absence of more precise career goals, I opted to major in English literature.

After the war, I became increasingly involved in the peace and civil rights movements. On the train back to New York from a conference in Chicago, organized by politically engaged pacifists—probably in the summer of 1945—I had a long conversation with Charles Bloomstein, a conscientious objector and editor of a thoughtful political newsletter during the war, which helped to crystallize my thinking about where to go next. He said that, if he were in college now, with my interests, he would study psychology or sociology, because the best ideas for work on peace and social change are likely to come from these fields. I followed his advice and, in my junior year, opted to become a psychology major.

(In the end, I graduated with a double major in English and psychology.) I picked psychology over sociology, in part, because I had a running start in psychology, having already taken the introductory course. In part, I believe, I was more comfortable with a psychological level of analysis because its focus on the individual brings it closer to both the observable data and the ultimate criteria for social policy.

My introductory course in social psychology, using Katz and Schank (1938) as the text, confirmed my interest in the field. I was particularly intrigued by the Lewin, Lippitt, and White (1939) work on group atmospheres and autocratic versus democratic leadership (see also Lippitt, 1940, and Lewin, 1948, chap. 5). The course instructor, Janet Kane—noting my performance in the course—strongly urged me to take more social psychology, and I followed suit. The course in advanced social psychology, taught by Daniel Katz (who was also department chair at the time), left me with the strong sense that this was the field for me. In the first half of the course, we read and discussed Floyd Allport's (1933) *Institutional Behavior* and Franz Oppenheimer's (1914/1975) *The State.* The second half was devoted to the detailed study of survey methodology—including questionnaire construction and interviewing—and each student actually designed and carried out a small survey. I found the combination particularly exciting; it persuaded me that social psychology—at least as practiced by Dan Katz—combined a focus on larger social and political issues with scientifically grounded empirical research. My laboratory course in experimental psychology gave me my first introduction to the autokinetic phenomenon (Sherif, 1936), which I later used in my first-year research project at Yale (Kelman, 1950a). For my course on personality, I wrote a term paper, titled "Towards an Explanation of Nazi Aggression," which drew heavily on the frustration-aggression hypothesis (Dollard, Doob, Miller, Mowrer, & Sears, 1939) and also used the work of Cantril (1941) and Fromm (1941). This paper foreshadowed my Lewin Memorial Address (Kelman, 1973) and my work with Lee Hamilton on crimes of obedience.

The Lewin address—given in response to receipt of the Kurt Lewin Memorial Award from the Society for the Psychological Study of Social Issues (SPSSI)—harks back to the Brooklyn College days in other ways as well. After taking Dan Katz's course, I repeatedly turned to him for advice about my future plans. On one occasion, he gave me some literature about SPSSI (of which he was secretary-treasurer at the time) and mentioned that it was an organization I might be interested in. Clearly I was and have been ever since; SPSSI epitomizes my reason for turning to social psychology. I joined in 1946, when I was still an undergraduate, and eventually became very active in it. When I received SPSSI's Lewin Award in 1973, it was—very appropriately—Dan Katz who presented it to me. Modesty notwithstanding, I cannot resist quoting two of Dan's comments in his presentation of the award. In commenting on my relationship to SPSSI, he described me as one of those "members who in their personalities reflect the total pattern of the objectives and practices of the organization" (Katz, 1973, p. 22). In comparing me to Kurt Lewin, he said that "Herb Kelman is in the pattern of Kurt Lewin in that he integrates the two roles [of social psychological researcher/theoretician and social actionist]. He utilizes theoretical analysis and research methods in his social action approach. The result of his work is both a better social world and a better social psychology" (Katz, 1973, pp. 21–22). There was no way I could have even dreamed in 1946, as I sat in Dan Katz's office, that I would receive such an award 27 years later. But it was precisely the possibility of integrating social action with social science that attracted me to social psychology—more precisely, to the kind of social psychology represented by Daniel Katz, Kurt Lewin, and SPSSI.

One highlight of this period was the appearance of Kurt Lewin on campus, giving a lecture on his group decision experiments. I found the work fascinating and concluded that this was the kind of work I would like to do. At the same time, I was worried about the ethical implications of using group-dynamics procedures to manipulate human behavior—an issue to which Lewin himself was by no means oblivious (Marrow, 1969, p. 179). Not surprisingly, when I decided in my senior year to apply to graduate programs in social psychology, my first choice was the Research Center for Group Dynamics, which had recently been established by Kurt Lewin at MIT. Unfortunately, Lewin died (at age 56) in February 1947 and the center suspended new graduate admissions, pending its move to the University of Michigan. Although I was not destined to study with Lewin, he and his tradition played an important role in my graduate training and my subsequent career, as will become apparent here and there in the coming pages. I am rather pleased, therefore, that Reuben Baron (chap. 1, this volume) calls me a Lewinian or neo-Lewinian. I have been told that before and have suspected it myself. But when Reuben tells it to me, I pay attention. Back in the early 1960s, when we worked together at the University of Michigan, he informed me that I was a functionalist (in the context of social psychological theory). He was right, of course, and I should have known it, particularly in view of Dan Katz's association with the functional approach (e.g., Katz, 1960). But my tendency has always been to draw ideas from wherever I found them without signing on to a theoretical school. Still, I was happy to declare myself a functionalist (e.g., Kelman & Baron, 1968, 1974) and to be so classified by chroniclers of the field (e.g., Himmelfarb & Eagly, 1974). To be called a Lewinian by Reuben Baron certainly feels right to me, as well as complimentary. He also calls me a "protodynamical systems theorist," which also sounds great, but I still need to figure out the implications of that designation.

Back to 1946: In my senior year in college, I had to decide what to do next. One option was to enter the Jewish Theological Seminary (JTS) for rabbinical studies—not because I wanted to become a pulpit rabbi, but because this seemed like the most appropriate training for a career in Jewish education or community work. I was well prepared for this option. While attending Brooklyn College, I also attended the Seminary College of Jewish Studies (affiliated with JTS) and indeed received a BHL (Bachelor of Hebrew Literature) degree from the college in 1947, at the same time as my BA. I proceeded with an application. At more or less the same time, I applied to several graduate programs in social psychology, recommended by Dan Katz. As it happened, I was accepted both by JTS and by the three programs—each with an interdisciplinary flavor—that I was most interested in once MIT dropped out of the picture: Yale, Harvard, and the University of Michigan. When I could no longer delay my decision, I knew that graduate school in social psychology was the way I wanted to go. Of my remaining three options, I eliminated Michigan, which had recently established an interdepartmental (sociology and psychology) doctoral program in social psychology that Dan Katz was to join in the fall of 1947, because they wanted me to take additional course work (notably in biology or physiological psychology) during the summer before entering graduate school, and I had other plans for the summer. I eliminated Harvard, which had recently established the interdisciplinary Department of Social Relations, because they initially offered me no financial aid, which I needed; later, in the summer, I was offered a scholarship, but by then I had already accepted at Yale. Yale offered me a research assistantship with Irvin Child, who was collaborating with anthropologist John Whiting on a cross-cultural study of the relationship between child-rearing practices and adult personality (Whiting & Child, 1953). The study utilized the ethnographies indexed in the Cross-Cultural File at Yale's Institute of Human Relations (later renamed the Human Relations Area Files) as its source of data. The work, the pay, and the interdisciplinary Institute of Human Relations—which I had already encountered in the volume on *Frustration and Aggression* (Dollard et al., 1939)—seemed to meet my needs and I accepted.

The decision to pursue graduate studies in social psychology did not mean that I had decided to become a social psychologist, any more than opting for JTS would have meant that I had decided to become a rabbi. But it certainly set me on a path toward adopting, shaping, and personalizing my identity as a social psychologist. My arrival in New Haven in the fall of 1947 began what I describe as my 10 formative years, in which I gradually defined my identity, not only as a social psychologist, but as the kind of psychologist that I remained for the rest of my life (so far, at least; I refuse to dismiss the possibility of change, even if the probability is very low). I shall try to describe the four phases of this formative period briefly, aiming not to be comprehensive, but to highlight the experiences that helped define my way of doing social psychology.

Yale

The Social Relations Department at Harvard or the Joint Doctoral Program in Social Psychology at the University of Michigan, it seems, would have been more natural training grounds for someone starting out with the interests that brought me to social psychology and ending up with the uses to which I ultimately put my training. Yale at the time appeared to be a bit too psychological in its social psychology, too behavioristic in its theoretical orientation, too exclusively experimental in its methodological tastes, too "basic science" in its agenda for someone like me. In fact, I considered switching to the Harvard program after my first few months at Yale—largely because I felt there was not enough social psychology in the department—and I had the opportunity to do so. In December 1947, Bennet Murdock and I went to Cambridge to explore options in the Social Relations Department. We met with Gordon Allport who, it turned out, was particularly interested in us because he felt that—with our Yale background—we could bring some needed strength in experimental psychology to the social psychology program. Shortly after our visit, he invited us to join the program, but, in the end, both of us decided to stay at Yale. In my own case, one consideration, no doubt, was the fact that I had made friends in the department and become integrated in the group of mostly unattached graduate students who spent most of their time at "the Institute" (i.e., the Institute of Human Relations, located in the Yale medical complex) where psychology was housed—along with anthropology, psychiatry, and child development. Most important, however, was that the prospects for social psychology at Yale began to look much brighter to me. First, at the urging of some of my fellow students and myself, Leonard Doob and Irvin Child agreed to offer a year-long graduate seminar in social psychology and personality. Second, Carl Hovland—the chair and leading presence in the department—received a Rockefeller grant to establish the Yale Communication Research Program (generally referred to as the "attitude-change project") and offered me a research assistantship in it. The invitation from Harvard gave me the opportunity to recommit myself to Yale—a decision that I have never had any reason to regret. Eventually, of course, I ended up teaching in the Social Relations Department at Harvard and the Joint Doctoral Program at Michigan, but fortified with my Yale training.

On balance, I found my Yale training more liberating than restrictive. To be sure, we had to take the department's dominant theoretical approach—Yale learning theory, derived from the work of Clark Hull (e.g., Hull, 1943)—as our point of departure and to become conversant in its language. But there was ample room for adapting the model to one's own needs and applying it to a broad range of problems. Indeed, the environment of the Institute of Human Relations encouraged many ambitious (if at times, perhaps, a bit reductionist) efforts to apply learning-theory concepts to the analysis of such diverse and socially relevant topics as frustration and aggression (Dollard et al., 1939—to which I have already referred), social learning and imitation (Miller & Dollard, 1941), personality and psychotherapy (Dollard & Miller, 1950), social attitudes (Doob, 1947), and even war and peace (May, 1943). As already mentioned, I was personally involved as a research assistant in two such enterprises: the research on child training and personality (Whiting & Child, 1953) during my first year in graduate school, and the research on communication and persuasion (Hovland, Janis, & Kelley, 1953) during the remaining three years. In keeping with the interdisciplinary flavor of much of the work at the institute, I had considerable exposure to other disciplines during my graduate training—a great deal to anthropology and psychoanalysis, less to sociology (in part because it was housed at the other end of the campus).

Carl Hovland, my mentor as of 1948 and my thesis adviser, played a critical role in allowing me to develop my own approach to the field. He was a first-rate theorist and experimentalist, but—though one of Hull's leading students and steeped in Hullian theory—he was more interested in addressing concrete problems than in testing theoretical systems. He was eclectic in his choice of theoretical concepts, as evidenced by his successful collaboration with such theoretically diverse colleagues as Irving Janis, Harold Kelley, and Muzafer Sherif. He often started with practical questions, such as those that the designers of a persuasive communication might raise: Would it be more effective to present both sides of the issue or only the side we are advocating? Would it be more effective to start out with our best arguments or to end up with them? To answer such questions, he would draw on relevant theoretical concepts, wherever he could find them, to develop complex hypotheses about the conditions under which different relationships hold, and then proceed to test these hypotheses with sophisticated experimental designs. This systematic way of defining the problem and designing the research that can address it is perhaps the most important lesson I learned from my association with Carl Hovland. As for selection of the problem to be addressed and the theoretical approach to be adopted, he always encouraged me to follow my own inclinations—of course, within the substantive and methodological framework of the attitude-change project. On the other hand, he had his ways of letting me know when he was not satisfied with the direction I was taking. As a result, it took three extensive tries before I came up with a mutually acceptable thesis proposal. At the time, I complained about Hovland's nondirective approach, but it soon became clear to me that his mentoring style, while clearly communicating his high standards, encouraged me to develop independent ideas and emphases in line with my own interests and concerns.

The emphasis in my Yale training on rigorous theoretical thinking, elegant experimental design, and sophisticated analysis was not only useful, but also congruent with my personal style. I was particularly captivated by analysis of variance and determined to use a Latin square design in my thesis even before I knew what the thesis would be about (and I followed through—see Kelman, 1953). Perhaps this training encouraged my bent toward linear thinking about a world that I have always known to be circular but, ultimately, it has given me tools to think systematically about complex issues, including interactive and dialectical processes. Yale training—at least in my days—also helped to anchor graduate students in the discipline of psychology as a whole, not only their specialty. Psychology at Yale emerged as a fairly unified field, largely because of the presence of an overarching theoretical framework that served as the point of departure for most (or at least the most influential) faculty members across the spectrum. The conflict between "hard" and "soft" psychologists that divided some other departments (leading, for example, to the partition of the Harvard Department of Psychology and the establishment of the Department of Social Relations in 1946) did not arise at Yale, since it was the "hard" psychologists themselves who chose to work on the "soft" issues. In this atmosphere, it was quite natural that I—though always committed to social psychology—would take my minor area exam in learning (based on an extensive yearlong course with Neal Miller) and would acquire a heavy dosage of clinical training (including a yearlong seminar and supervised practice in projective testing with Seymour Sarason and in psychotherapy with John Dollard, as well as regular attendance at psychiatric rounds).

I emerged from this training as a fairly well-rounded psychologist, a well-trained social psychologist, and a competent experimenter (as confirmed—I am happy to say—by Reuben Baron, chap. 1, this volume). In addition to its intrinsic value, this training gave me the firm ground from which to strike out in new directions and the credibility to do so. At the same time, the modeling and mentorship of my teachers at Yale, and particularly of Carl Hovland, provided validation and encouragement for social psychological work that starts with applied problems, that addresses larger social issues, and that takes an interdisciplinary orientation.

In assessing the impact of the Yale experience on my evolving identity as a social psychologist, I must stress that my theoretical training at Yale was not all S-R learning theory, and my social psychological training in those years did not all happen at Yale. We had a great deal of sympathetic exposure to psychoanalytic theory, with emphasis on the need to translate its propositions into empirically testable hypotheses—as was indeed done by several of our professors. I was particularly interested in Freud's papers on technique (Freud, 1924/1950, pp. 285–402), which I studied carefully and have drawn on in my later teaching of psychotherapy and practice of conflict resolution. Kurt Lewin's theory of personality also received extensive coverage in our course readings and directly influenced some of the work of Irvin Child and Neal Miller. I immersed myself in the writings of Lewin and his associates, in both personality theory and social psychology, and did papers and reports drawing on that literature. I even published a polemical paper (Kelman, 1950b) that contained a review of the research literature on group dynamics as of that date. I developed a reputation as the resident Lewinian in the department.

But I also used my summers well, to broaden my training in social psychology in areas that were not represented at Yale—and incidentally to become acquainted with many birthright Lewinians and their work. In the summer of 1948, after my first year at Yale, I participated in the Training Laboratory for Group Development at Bethel, Maine (original home of the T-group), as a research assistant and trainee. It all started when Ronald Lippitt gave a colloquium on this emerging enterprise at Yale. I raised a question about the potential for manipulative use of such group processes. In his response Lippitt told me that it is typical of New Englanders to raise this kind of question—a response that, as a Jew from Vienna and Brooklyn who had lived in New Haven for about half a year, I found rather amusing. Whatever ethical questions I may have had, I asked Lippitt how I could get to Bethel and he helped to arrange the assistantship that brought me there. I continued to have ethical questions about training groups, as well as methodological ones (I had trouble, for example, with the concept of a group whose sole task was to study itself), but I learned a great deal at Bethel that I found useful in my later work (including how to ride a bicycle). I also had the opportunity to get to know the faculty members from the Research Center for Group Dynamics who were at Bethel that summer (in addition to Ronald Lippitt): Dorwin Cartwright, Jack French, and Alvin Zander.

I spent the second summer (1949) of my graduate years at the University of Michigan, where I was a student in the summer institute on survey methods and a research assistant at the Survey Research Center—all of it made possible by Daniel Katz. I did intensive course work in basic survey techniques, survey design, sampling, and scaling. For my assistantship, I had the responsibility of planning and carrying out the analysis of data from one of the studies in the SRC's program on human relations in industry (which was under Katz's general direction at the time; the study director for my project was Eugene Jacobson). I spent much of my spare time at the Research Center for Group Dynamics, interacting intensively with members of the final cohort of Lewin's students who were there at the time—teaching, working on research projects, and/or finishing up their dissertations: Harold Kelley, John Thibaut, Kurt Back, Stanley Schachter, Albert Pepitone, Murray Horwitz, and Ben Willerman (who was actually at the SRC). I also found time to draft my first thesis proposal (perhaps as a course paper), outlining an experimental test of the effects of group decision on attitudes, couched in Hullian terminology (replete with fractional anticipatory goal responses). I presented my ideas to Leon Festinger, who had conducted one of the earlier group decision experiments in Lewin's program and who was also at Michigan at the time, but he could see no reason why I would want to work on this topic. In the end, my professors at Yale were also insufficiently enthusiastic and I dropped the idea.

My summer in Ann Arbor was a turning point in my self-definition. Up to that point I thought of myself as a graduate student in (social) psychology. But, being away from an environment in which I was defined by my student role, and situated in an environment in which I was functioning as a full-fledged (albeit young) professional and treated as such, I began to think of myself as a social psychologist. It is not that I was unaware of my continuing status as a student; I was certainly reminded of it when my first two thesis proposals failed to elicit clear support from my advisers. But I had now made a commitment to social psychology as an identity and a career. Increasingly, I acted as a young professional—and as one with his own perspective on the field. After my return from Ann Arbor, I gave a colloquium on the innovative approach to scaling developed by Clyde Coombs, with whom I had taken a course at the summer institute. I also reported to Carl Hovland on the as yet unpublished work on social communication that Leon Festinger and his associates were engaged in; Hovland later told me that my recommendation contributed to the decision to bring Harold Kelley to the department the following year. In my last year at Yale, I collaborated with Arthur Gladstone (with whom I had also collaborated earlier in establishing Walden House, the student cooperative house that was my home between 1948 and 1951) in two efforts. Early in 1951, we gave a joint psychology colloquium on the social implications of psychological research, in which I spoke about manipulation of human behavior as an ethical dilemma confronting many areas of research and practice in the field (remarks that, more than a dozen years later, became the basis of a symposium paper and article—see Kelman, 1965b). Around the same time, we published a letter in the *American Psychologist* (Gladstone & Kelman, 1951), in which we proposed that some of the basic assumptions of pacifist thinkers were consistent with psychological theories and findings and that it would be important to subject them to systematic research—a proposal that led to the establishment, in the following year, of the Research Exchange on the Prevention of War. These activities were concrete expressions of my interest in integrating my ethical and activist concerns with my professional work—which had led me to social psychology in the first place—and they set the pattern for the kind of social psychologist I was to become for the rest of my career.

My dissertation experiment used a fixed, persuasive communication, following the paradigm of the Yale attitude-change project (see Kelman, 1953). In the write-up, I freely mixed (without apology) S-R and Lewinian terminology and sources. My central concern—the relationship between overt conformity to social norms or social pressures and internalized change in attitude—was the starting point of my theoretical and empirical work for years to come. I decided that I would explore the internalization of attitudes in a real-life context as well as in the laboratory and—in view of my evolving interest in psychotherapy—I concluded that group therapy would be an ideal setting to pursue this interest. I therefore applied, successfully, for a postdoctoral fellowship from the Social Science Research Council to study group therapy—not as a clinician, but as a social psychologist interested in it as an intensive influence situation, potentially conducive to important changes in attitude and personality. I felt enormously validated when Hovland (who had become Carl at the end of my orals), commenting on the direction I planned to take, told me that he believed internalization was the most important topic to which the field needed to turn.

Johns Hopkins

The SSRC gave me carte blanche in selecting the site for my postdoctoral fellowship. I explored a number of options and boiled them down to a choice between Baltimore and the Boston-Cambridge area. I found active group therapy projects in four Boston hospitals and interesting research on group process—especially the work of Freed Bales—in the Department of Social Relations at Harvard. Bales extended a warm invitation to house my fellowship in his laboratory; the Boston Psychopathic Hospital (now Massachusetts Mental Health Center) was also ready to house me. The Boston area clearly offered a rich, stimulating environment for my fellowship. My only worry was that I would be overwhelmed by all the options, try to do everything for the first few months, and eventually settle on one program—having lost precious time in the process. Baltimore created no such worries. There was only one thing going on there that was relevant to my interests, but it was clearly of high quality and very congenial to me: the group psychotherapy research project at the Phipps Psychiatric Clinic, Johns Hopkins Hospital, under the direction of Jerome Frank. It was one of the earliest systematic and methodologically rigorous research programs on the evaluation of psychotherapy. I had read some of Frank's papers on group therapy when I began exploring that topic. Most important, however, I was familiar with his earlier work. Before going to medical school, Frank received a PhD in psychology from Harvard and went on to do postdoctoral work at Cornell with Kurt Lewin (with whom he had also worked earlier in Berlin). One of the products of this period was a series of studies on social pressure and resistance thereto (Frank, 1944a, 1944b)—anticipating some of the findings of Milgram's obedience research—which influenced my own dissertation.

I chose to go to Baltimore, which turned out to be a wise decision. In the end, I stayed at Johns Hopkins for three years. After completing my year as an SSRC fellow, I wanted to extend my stay—primarily because I had started a psychoanalysis, which I did not want to terminate prematurely. I was fortunate to receive a postdoctoral fellowship from the National Institute of Mental Health (NIMH) for 1952–1953, which later was renewed for an additional year. For the first year and a half of my time in Baltimore I was housed at the Phipps

Clinic; after that, I moved to the Homewood Campus, so I would have more time to pursue my own work. I should mention that my plans during that period were complicated by my resistance to the military draft. We were in the middle of the Korean War and, having finished my studies, I was called up for induction shortly after I came to Baltimore. I had registered as a conscientious objector, but my New Haven draft board denied me CO status (on the basis of a narrow interpretation of the religious criteria for that status). I lost my appeals, and, having exhausted my legal options, I chose to refuse induction. I was prepared to go to jail—knowing that the customary sentence for draft refusal was a year and a day—and I was making plans for using my prison time most productively. Fortunately, however, the grand jury that considered my case, on the recommendation of the district attorney, ruled in my favor. The draft board finally gave up on me, granted me the CO classification, and even agreed to designate my NIMH fellowship as the alternative service required of COs in those days.

The three postdoctoral years that I spent in Baltimore played a critical role in my personal and professional development. The activities I pursued and the ideas I formulated during that period laid the foundations for most of my subsequent work. What helped to make this such a fruitful period, I believe, is the fact that I was by then a fully credentialed, independent professional, no longer constrained by my student status, yet at the same time not tied down by the duties of a regular job. I thus had maximal freedom to pursue my own interests and define my own identity.

At the personal level, the most important foundational experience of those years is that Baltimore is where I met, courted, and married my life partner, Rose. This is clearly a foundation the two of us have built on over the years, having reached, in August 2003, the 50th anniversary of our marriage. Also, as already mentioned, I was in analysis throughout my three years in Baltimore. It was a fairly classical, Freudian analysis. Needless to say, it contributed a great deal to my understanding of the therapeutic process and relationship. It did not produce dramatic personal changes—no overarching new insights and no recovered childhood memories. It did not even break my lifelong habit of coming late (after a while, my analyst gave up trying to interpret it). What it did accomplish, I believe, is to make me more reflective about my goals and relationships and more accepting of myself—more tolerant of my limitations.

At the professional and intellectual level, I continue to draw and build on the ideas that I developed during those years. In many ways, my activities in Baltimore set the direction of my future work. It was at Hopkins that I worked out the distinction between the three processes of social influence and at Morgan State College in 1954 that I carried out the first experiment testing that model (Kelman, 1958). I started out with the distinction between compliance and internalization, supported by my dissertation. As I explored the literature on various real-life influence situations, I concluded that this dichotomy did not adequately capture some of the most interesting instances of social influence—particularly brainwashing and religious or political conversion (the phenomenon of the true believer), as well as certain aspects of childhood and adult socialization. I think Lee Hamilton (chap. 4, this volume) is right when she suggests that the process of identification—which I introduced to capture these diverse manifestations of influence—is the most complex and interesting of the three processes (and, I might add, the most uniquely social psychological).

The group therapy project, which originally brought me to Hopkins, provided many experiences and learning opportunities on which I have drawn and built in many ways. Through regular and extensive observation of therapy groups, participation in staff meetings, frequent conversations with colleagues (especially Morris Parloff and Jerome Frank himself), and active involvement in evaluation research (Kelman & Parloff, 1957; Parloff, Kelman, & Frank, 1954)—along with my personal psychoanalysis—I acquired a wealth of "anthropological" knowledge about the field of psychotherapy. Frank's commonsense approach and emphasis on the role of the therapist and the patient–therapist relationship in determining therapeutic outcome (see Frank, 1961) was particularly helpful in my subsequent teaching and writing (e.g., Kelman, 1963) about psychotherapy from a social psychological perspective. The experience in evaluation research was also relevant to my later work in evaluating the impact of international exchange programs (e.g., Kelman & Ezekiel, 1970). Finally, while in Baltimore, I continued my interest in group process (following up on my Bethel experience) and, together with Harry Lerner, edited an issue of the *Journal of Social Issues*, comparing group methods in psychotherapy, social work, and adult education (Lerner & Kelman, 1952). My exploration of group process in these different settings directly influenced my subsequent work with problem-solving workshops in conflict resolution (see, for example, Kelman 1991a, 1997a).

My work in peace research and the social psychology of international relations also has strong roots in this period. The letter that Arthur Gladstone and I published in the *American Psychologist* stimulated correspondence and meetings that led to the formation in 1952 of the Research Exchange on the Prevention of War—which, as far as I know, represented the first organized effort to promote the field of peace research (Kelman, 1991b). The Research Exchange published a *Bulletin*, edited by Arthur Gladstone (with myself as book review editor), in which I published several articles on my evolving views on the study of war and peace and the psychological aspects thereof. The Research Exchange also organized symposia (two of which were published) and discussion meetings at various professional conventions, as well as two summer workshops. (Rose and I attended the workshop at Fellowship Farm, Pennsylvania, in the summer of 1953, in lieu of our honeymoon—setting a pattern for the rest of our lives.)

Although my teaching career did not begin until 1957, I did have my first teaching experience at the Baltimore College of Commerce, where I twice taught a course on business psychology. I needed to supplement my meager fellowship income to pay for my four weekly analytic sessions. In the course, we used a text on business psychology, but my lectures dealt with basic topics in social psychology and personality. The course contained the seeds of the main undergraduate course that I was to teach—under different titles and with gradually changing content—throughout my teaching years.

I cannot end my account of the Baltimore years without mentioning that I played an instrumental role in founding a chapter of the Congress of Racial Equality (CORE) shortly after arriving in Baltimore and was an active participant in its successful nonviolent direct-action campaign to open dime-store lunch counters and other facilities to the Black population. Rose and I spent many a date on a picket line or sit-in at Woolworth's or Grant's. I was active in CORE and other civil rights activities both before and after my Baltimore years (serving as national field representative of CORE between 1954 and 1960), but the Baltimore period stands out in a number of ways. When I arrived in Baltimore, it was a completely segregated city, but one ready for change. It took a lot of dedicated work, skill, and coordination to produce the change, but it was exciting to be able to see our efforts make a real difference. Another feature of Baltimore CORE was the active involvement of members of the city's very vital Black community, including its labor union, church, and university sectors. We were very much part of this community, engaged in a joint effort to create social change. The experience taught me a great deal about social change, particularly the role of nonviolent direct action (see Kelman, 1968b, chap. 9) and the importance of combining it with other strategies, as we did in our CORE work: public education, negotiation with local store managers, and campaigns directed to the national headquarters of chain stores.

As my third fellowship year drew to a close, I had to think about finding a job. My search for an academic position was unsuccessful and I began negotiations for a research position at the National Institute of Mental Health. In the meantime, I received an invitation to join the initial group of fellows at the Center for Advanced Study in the Behavioral Sciences, newly established by the Ford Foundation on the Stanford campus. (It is probably no coincidence that Carl Hovland was a member of the board.) Some older colleagues advised me that it was time to get a real job. Dissatisfied with that advice, I turned to David Riesman, who was a visiting professor at Hopkins that semester; I was sitting in on his seminar and had gotten to know him fairly well by that time. He told me what I wanted to hear: that I will have other opportunities to get a job, but that the invitation to the center represented a rare opportunity. It was one of many bits of good advice that I received from David Riesman over the coming years.

Center for Advanced Study in the Behavioral Sciences

In my final analytic session, my analyst became uncharacteristically directive (we were sitting face to face in that session) and told me that the only way to go to California was to drive across the country. When I pointed out that I had no car and did not know how to drive, he told me to buy a car and take driving lessons and assured me that by the end of the trip I would know how to drive. He even told me how to handle mountain roads. Rose and I did buy a car and had a great time driving across the country.

When I arrived at the center, I found a very interesting and diverse group of colleagues. The distribution of fellows in that initial year was bimodal, including a sizable number of very senior people (such as Franz Alexander, Kenneth Boulding, Clyde Kluckhohn, Harold Lasswell, Paul Lazarsfeld) and a sizable number of quite junior people of whom, at age 27, I was one of the youngest. In part, this was by design: One of the early ideas for constituting a center class was to invite a number of senior scholars along with a group of younger satellites for each. That concept never took hold in that first year—in fact, a strong egalitarian atmosphere evolved, in which each fellow, regardless of age, was treated as a fully independent scholar—and it was soon dropped. Another reason for the bimodal distribution, I believe, was that recruitment for the first class started very late, so that the people who were free to accept the invitation were either senior enough to obtain a year's leave on short notice, or junior enough to have no stable job (or, like myself, no job at all) to take leave from.

I probably should have devoted this year of complete freedom to writing up my three-process model and my experimental test of it. But it seems that I preferred to take advantage of the rich array of intellectual pursuits that were represented at the center and to learn about the concepts and methods that colleagues from several disciplines were advancing. I participated in a wide variety of activities—ranging from a research project on psychological correlates of different somatic disturbances (see Kelman, Alexander, & Stein, 1958) to a study group on social movements in which I presented my own analysis of the Sabbatian movement (an influential Jewish messianic movement of the 17th century).

The year at the center did generate some concrete products in the peace research domain. Encouraged by the collegial atmosphere at the center, I called together a number of the fellows—including Kenneth Boulding and Anatol Rapoport—to talk with them about the Research Exchange on the Prevention of War and get their advice on how to move forward more rapidly (I was impatient in those days) on the development of a professional base for the organization and how to attract international relations specialists to this enterprise. These discussions led to the proposal to establish a new journal, which would replace and expand on the *Bulletin of the Research Exchange*. We decided to name the new publication *Journal of Conflict Resolution: A Quarterly for Research Related to War and Peace,* and to base it at the University of Michigan, since Boulding was there, Rapoport was about to move there, and William Barth and Robert Hefner—both Michigan graduate students at the time—were already producing the *Bulletin of the Research Exchange* there. The *Journal of Conflict Resolution* is now in its 47th year of publication. During the year at the center, I also completed work on an issue of the *Journal of Social Issues,* addressed to research on war and peace, that I coedited with Barth and Hefner (Kelman, Barth, & Hefner, 1955), including my closing article, which clearly reflected the interdisciplinary setting in which it was produced (Kelman, 1955).

The most important impact of my stay at the center was that it helped me define myself, at this early stage in my career, as part of an interdisciplinary community of behavioral and social scientists. I was, of course, strongly predisposed in this direction, but the year at the center provided ideas, contacts, and validation for interdisciplinary work and, above all, rewarding experiences of interaction across disciplinary lines. Thus, it set the pattern of my career as a social psychologist—firmly anchored in my mother discipline—who has always operated in interdisciplinary settings and in relation to colleagues from other fields, whether clinicians, ethicists, political scientists, international relations scholars, or Middle East specialists.

By the end of the year, I had not yet succeeded in locating a suitable academic position, despite strong support of my candidacy for an opening in the Department of Social Relations at Harvard from Clyde Kluckhohn, and despite the efforts of Ralph Tyler—the center's first director—to find an opening for me in the Committee on Human Development at the University of Chicago. I decided to resume negotiations with the National Institute of Mental Health and accepted a position in the Laboratory of Psychology, part of the NIMH intramural program, based at its Clinical Center in Bethesda.

National Institute of Mental Health

A good part of my first year at NIMH was taken up with fighting to hold on to my job. I was terminated (as was Rose, who had taken a position as social worker at the National Institute of Neurological Diseases and Blindness) because the Department of Health, Education, and Welfare's (HEW's) security office—established at the height of the McCarthy period and still very much in place in 1955—questioned my past political activities and my associations (see Kelman, 1957). After six months of struggle, with excellent legal help from Richard Schifter (whom I knew from his Yale law student days in New Haven, where he lived in one of our sister co-op houses and was active—with support from me, among others—in establishing an ACLU chapter, and who was later to become assistant secretary of state for human rights), and with moral and financial support from SPSSI and APA, we achieved a complete reversal of the termination action, including an apology from the Secretary of HEW. This successful outcome would not have been possible without the unwavering support of my superiors and colleagues in the Laboratory of Psychology and elsewhere in the NIMH system.

Because of the friendships that I formed with colleagues at NIMH, deepened by their stand on my behalf in the face of the political pressures of the day, I view my experience there as a positive contribution to my formative years, despite the obstacles that I had to overcome. The relationships with three colleagues in the Laboratory of Psychology stand out in particular. David Shakow, chief of the laboratory, became a valued mentor, who was very supportive of my ethical and social concerns and my approach to the scholarly enterprise. Morris Parloff, with whom I had collaborated closely at Johns Hopkins, was chief of my section at NIMH and instrumental in bringing me there—and continues to be a valued and respected friend to this day, more than half a century after I first met him. Donald (Mike) Boomer shared his office with me, as well as his wisdom and humor; among other things, he agreed to supervise me in short-term therapy with a patient, thus doubling my experience as a therapist and adding a Sullivanian model to the Freudian/behaviorist model that John Dollard provided in his supervision of my one previous venture into therapeutic practice.[5]

[5]It should also be mentioned that my relationship to NIMH as an institution over the years—both before and after my position on the staff—was very positive. In addition to the two years of postdoctoral fellowships at Johns Hopkins (1952–1954), NIMH granted me a Special Research Fellowship to spend a year (1960–1961) at the Institute for Social Research at Oslo. (The latter—not coincidentally—was offered to me after I had been denied a Fulbright grant for entirely political reasons, apparently based on incomplete information about my case at NIMH.) NIMH also supported my research program on social influence and behavior change with a series of research grants, as well as the International Conference on Social-Psychological Research in Developing Countries that I organized at the University of Ibadan in December 1966 to January 1967. In turn, I served on NIMH's Psychology Training Review Committee for several years, as well as other NIMH committees.

New research plans in the psychiatric wards of the Clinical Center—including a study that Charlotte Schwartz and I were hoping to conduct on an experimental program for psychotic patients and their parents—did not materialize. I did manage, however, to analyze and write up some earlier data and to work on some theoretical papers. My major—and not insignificant—achievement during this two-year period, however, was completion of a nearly 200-page manuscript, presenting my three-process model of social influence and the experimental evidence in support of it (Kelman, 1956). I submitted this manuscript (anonymously, as required) in successful competition for the Socio-Psychological Prize of the American Association for the Advancement of Science. The biggest mistake I made in my professional life was my failure to publish this manuscript at the time. I signed a contract with John Wiley & Sons, who were prepared to publish the manuscript with just the addition of an introductory chapter and virtually no other changes. But I felt it was not ready, wanting to do some additional experiments and some theoretical elaboration. I did conduct some further experiments and revise and elaborate some of the text, but in the meantime the literature grew, the task became more daunting, and I was distracted by a variety of other interests. As a result, although the ideas and some of the research have been partially presented in articles and other books, I have never produced that promised full statement of the model and detailed presentation of the data—at least so far: I have not entirely given up yet, and Erin Driver-Linn is proactively working with me in putting the old manuscript (as already revised) into a form and context that might make it interesting to contemporary readers.

Returning to 1957, it was clear to me (as well as to my colleagues) that—despite the rewarding features of my NIMH experience that I have described—I really belonged in a university, rather than a psychological laboratory based in a governmental medical facility, even one that allowed researchers as much autonomy as I had at NIMH. A university was obviously a more appropriate environment in which to pursue my interest in international relations, to comment on public issues, and to explore the relationship between social research and social action. Thus, when I was offered a faculty position in the Harvard Department of Social Relations, starting in the fall of 1957, I was delighted to accept.

The Teaching Years

My formal teaching career began with my first Harvard appointment in 1957. I had no teaching experience as a graduate student; teaching was never even an available option. The teaching I did at the Baltimore College of Commerce was a valuable experience and I certainly took it seriously, but it was a job rather than a central element of my identity. It was only in 1957 that my identity as a teacher began to take shape, but it soon became central to my personal identity and has remained so throughout the years. During my 42 official teaching years, starting in 1957 and ending with my retirement from teaching in 1999, I have held only three jobs in two universities—not counting over a dozen appointments, of varying lengths of time, as visiting professor, fellow, or scholar in different institutions in the United States and abroad.

Because of my poor planning and self-indulgence, the preceding section used so much of the generous amount of space made available to me that there is not enough space left to give the kind of detailed account of the 42 years covered in this section that I gave to the 14 years covered in the preceding section. At best, I figure that I have about a third as much space to cover three times as many years. I maintain, however, that this imbalance is quite appropriate to the focus of this *Festschrift* on my students and their work, for two reasons. First, my teaching years require less elaboration because they are well represented by the samples of my students' research and thinking that are offered in the preceding chapters. This is not so much because of a correspondence in the content of their work and mine (which applies more in some cases than in others) but because their work picks up, in one or another way, the kind of social psychology that I have practiced, taught, and stood for. Second, in a book in which and through which my students pay tribute to me, it is important that I, in turn, pay tribute to my teachers and mentors. I hope this is part of what the detailed account of my formative years conveys, explicitly and implicitly—in its references especially to my primary mentors, Daniel Katz and Carl Hovland, but also to others who have played an important mentoring role, such as Irvin Child, Leonard Doob, Jerome Frank, David Riesman, and David Shakow. Their most important contribution has been to encourage me to be and become myself, and I hope that I have played a comparable role in my relationship to my own students. More generally, the emphasis on my own formative years reminds us of the flow of influence across generations in the development of scholarly traditions. For the reasons given, then, I am content to limit myself (particularly since I have no other choice) to just a few general observations about my 42 teaching years.

(1) By the time I entered the teaching role, I had pretty much developed my identity as the kind of social psychologist that I was to remain—with some variations on the basic themes—for the rest of my career. As a consequence, most of my teaching from the beginning has been in the areas of my special concern, and my teaching and advising were nicely integrated with my interests in research, theory, and practice. Of course, over the years, I did my share of the teaching that had to be covered, including coteaching the undergraduate introductory course (albeit the semester that covered social psychology, personality, and psychopathology) and the proseminar in social psychology, as well as running general research seminars. Many of the undergraduate theses I supervised were in areas outside of my special interest; a large proportion of students I advised were in special concentrations (such as conflict studies), in joint concentrations between psychology and other disciplines (sociology, government, Far Eastern studies), or in Harvard's interdisciplinary social studies program. At the graduate level, too, I often took on students who were working on independent projects, unrelated to the research programs of any of the faculty members, and more often than not using nonexperimental methods. (Roger Brown was also known to take on students with diverse interests, not necessarily related to his own work; both of us, in this regard, were following in the footsteps of Gordon Allport.) I also spent a lot of time in careful editing of my students' work, as many of my advisees will testify. In short, I did not just use

my teaching and supervision in the single-minded pursuit of my own agenda, but I did find great synergy between my teaching or advising and my research. Many of my best ideas developed or became crystallized in the course of interactions with my graduate students, discussions in my seminars, or preparation of lectures.

(2) It is interesting that, in each of my three academic appointments, one of my "outside" interests—my exercises in reaching out to other fields, beyond the confines of social psychology: whether psychotherapy, international relations, or ethics—was a key factor in my selection. To be sure, my credentials as a bona fide social psychologist, including my Yale degree, my experimental work, and my theoretical contributions, were by no means irrelevant and indeed gave me the requisite "idiosyncrasy credits"—to use Edwin Hollander's (1958) concept. I know, for example, that the AAAS Socio-Psychological Prize contributed significantly to my invitation to Harvard in 1957. But my primary credentials for the particular position for which I was recruited that year derived from my work in psychotherapy.

The appointment was specifically in the clinical program within the Department of Social Relations and the initiative for it came from David McClelland, head of the clinical program at the time, who was interested in my social psychological perspective on psychotherapy and my analysis of it within a general framework of social influence and behavior change. In line with this interest, I developed and taught a yearlong seminar, required of all third-year clinical students, alongside of their practicum training in psychotherapy (which was, of course, supervised by a clinician). The first semester—which virtually all of the graduate students in social psychology took as well—focused on processes of social influence and covered the theoretical and experimental literature in that field (including, of course, the three processes) and various real-life influence situations *other than psychotherapy* (such as childhood and adult socialization, political and religious conversion, and assimilation). The second semester focused on theory and research in psychotherapy, with emphasis on the patient–therapist relationship and the therapeutic interaction (comparing, in particular, Freud's, Sullivan's, and Rogers's views on these matters).

I was appointed for a five-year term as Lecturer on Social Psychology, a title I preferred, because it both expressed my professional identity and communicated clearly that—though teaching about psychotherapy—I was not claiming clinical credentials. While based in the clinical program, I taught a middle-level course on Attitudes and Their Change, and had extensive contacts with colleagues, graduate students, and undergraduates in social psychology.

In 1962, when my five-year term at Harvard came to an end, I moved to the University of Michigan as Professor of Psychology and Research Psychologist at the Center for Research on Conflict Resolution. At the University of Michigan, my tenure and my academic duties were in the Department of Psychology. It was understood from the beginning that I would be centrally involved in the Joint Doctoral Program in Social Psychology, a collaborative enterprise between the Sociology and Psychology Departments. For a short time, in fact, I was chair of the program. I took on the assignment at a time when the program was about to collapse because of differences between the two departments in their size and operating style. My colleagues and I believed that, in view of my strong commitment to an interdisciplinary view of social psychology, I might be able to keep the program alive. Unfortunately, however, my strong commitment was not matched by sufficiently strong political skills and so—to my profound regret—I ended up presiding over the dissolution of this experiment.

My outreach beyond the confines of my own discipline, once again, played a significant role in my appointment at the University of Michigan, which was—as noted—a joint appointment between the Psychology Department and the Center for Research on Conflict Resolution. The center was an outgrowth of the *Journal of Conflict Resolution,* which, as I mentioned earlier, was based at the University of Michigan. The community that developed at the university around the editorial work on the journal decided to push the work forward through the establishment of an interdisciplinary research center in the field, and the idea gained support from the university administration. The desire to expand the number of faculty members with an interest in the center's interdisciplinary work, my continuing involvement with the *Journal of Conflict Resolution* as a founding member of its editorial board, and Dan Katz's key role both in the center and the Psychology Department all contributed to my invitation to come to the University of Michigan.

Another one of my "outside" interests—my concern with ethical issues—played a significant role in my invitation, in 1968, to return to Harvard as Richard Clarke Cabot Professor of Social Ethics. This chair was established in 1966 to commemorate Richard Clarke Cabot and the Department of Social Ethics, which he chaired (along with his professorship in the Medical School) between 1920 and 1931, when it was absorbed in a new Department of Sociology. The chair is not intended for a professional ethicist, but for a scholar in any department of the Faculty of Arts and Sciences who focuses on ethical questions confronting individuals in modern society.

According to the endowment, the incumbent "should deal with problems of practical ethics, should help students face ethical questions frankly and openly, and should help them relate themselves thoughtfully to the social issues of the day, so that they might at least envisage the possibility of careers in either social or public service" (Bentinck-Smith & Stouffer, 1991, p. 109). The first incumbent of the chair, very appropriately, was Gordon Allport, who had started his Harvard teaching career as an instructor in social ethics under Cabot. Allport, unfortunately, died in 1967, at age 70, within a year after the appointment. The Department of Social Relations—as the historical successor (via Sociology) of the Department of Social Ethics—was given the opportunity to search for the next incumbent and it chose to nominate me.

Clearly, the department would not have offered me a professorship had I lacked strong credentials in my own discipline. But it was my focus on ethical issues that provided the additional qualifications stipulated in the description of the chair: my work on the ethics of social research, on the psychology of social issues, on war and peace, and on justice and social change. My book, *A Time to Speak: On Human Values and Social Research* (Kelman, 1968b), which was in press at the time of the appointment, was probably one of the most important items in my bibliography when my candidacy was being considered.

Rose and I were reluctant to leave the University of Michigan, but the invitation from Harvard was hard to resist. One of the special attractions was the nature of the chair, which turned my "extracurricular" activities into part of the job description. Another was, of course, the special meaning of being named as Gordon Allport's successor, particularly since I had gotten to know him quite well during my first appointment at Harvard and he had been a source of encouragement and inspiration.

(3) Over the course of the years, the center of gravity of my work shifted from social influence to international conflict and its resolution. This is evident from my own writings and from the research of the students I supervised. The shift can be noted, for example, as one moves across the chapters in the present volume. Perhaps the best indicator of the shift is the topic of my trademark graduate seminar, which traditionally met on Wednesday evenings. In the earlier years, the title of my trademark seminar was Processes of Social Influence or some variant thereof, and it followed the format of the seminar described earlier that I first introduced in 1957. Needless to say, students heard and read a lot about my three processes of social influence, but the seminar covered the experimental literature on social influence and examined a number of real-life influence situations. In the later years, my graduate seminar on International Conflict: Social Psychological Approaches became my trademark Wednesday evening event. The seminar dealt with social psychological dimensions of international relations and approaches to the resolution of international/intercommunal conflicts, with special emphasis on *interactive problem solving*—the term I came to use to designate my own approach. The seminar used the Middle East conflict as its special illustrative case and included an intensive Israeli–Palestinian problem-solving workshop in which the seminar students participated as apprentice members of the third party. (In 1979, the illustrative case, to which the workshop was also devoted, was the Cyprus conflict.)

Despite the shift I have noted in the center of gravity of my work, I believe that there has been a remarkable degree of continuity over the years. It is true that a major turning point in my work occurred in the late 1960s and early 1970s, when I became acquainted (in 1966) with John Burton's work in conflict analysis and resolution, began to build on it theoretically and methodologically (e.g., Kelman, 1972a), made my first efforts to apply the approach in the Middle East, and finally committed myself (in 1973) to putting conflict resolution in the Middle East at the center of my professional agenda. However, my interest in the social psychology of international relations and in conflict resolution goes back to the very beginnings of my career, as I pointed out in the preceding section. This interest played an important role in my original selection of social psychology as a field of study, and it was reflected in much of my work in the 1950s and 1960s, including participation in the founding of the Research Exchange on the Prevention of War and the *Journal of Conflict Resolution*, editing of *International Behavior* (Kelman, 1965a), research on the impact of international educational and cultural exchanges (e.g., Kelman & Bailyn, 1962; Kelman & Ezekiel, 1970), and research on nationalism and the relation of the individual to the national system (e.g., DeLamater, Katz, & Kelman, 1969; Katz, Kelman, & Flacks, 1964; Kelman, 1969).

By the same token, social influence has remained a continuing theme, even as the center of gravity of my work shifted toward international conflict. My work with Lee Hamilton, culminating in *Crimes of Obedience* (Kelman & Hamilton, 1989), explores influence processes in hierarchical relationships. Furthermore, my conflict resolution work itself centers on a model of mutual influence in a conflict relationship and has drawn on my early interest in group processes as a source of significant attitude changes (see Kelman, 1997a). More generally, conflict as a multifaceted process of mutual influence is one of the key propositions in my analysis of the nature of international conflict (Kelman, 1997c), and influencing the other side is one of the key components of the macroprocess of negotiation (Kelman, 1996). In fact, I have tried to link the analysis of influence in international relations to my three processes of influence, and I believe I have come closest to doing so in my recent formulations of reconciliation, to which I shall return in the next section.

Finally, the continuity in my work over the years is provided by certain central themes that have characterized my work on social influence as well as on international conflict. In both areas, I have been particularly concerned with the depth and durability of change—whether in response to persuasive communications or to conflict resolution efforts; the role of individual change as a vehicle for change in the larger social system; the role of legitimacy in the relationship of individuals to hierarchical organizations and to the nation, the state, or other collectivities and institutions; and the moral dimension in human relations, including the ethical issues generated by the process and outcome of social scientists' own research and practice.

(4) In addition to my trademark graduate seminar, I have offered a trademark undergraduate course throughout my teaching career. The course had different titles at different times and its contents changed and evolved over the years, reflecting developments in the field (and in the world), new emphases in my own work, and differences in the definition of the overall theme of the course. But, many topics and illustrations survived over the years—including some of the jokes I used in my lectures, which I was reluctant to drop as long as they seemed to produce the desired response. In my first term at Harvard, the course was called Attitudes and Their Change. At the University of Michigan it became Attitudes and Social Behavior, a title already in the catalog. When I returned to Harvard as Cabot Professor of Social Ethics, I introduced a general education course entitled Human Values and Social Psychological Research to reflect the mission of my chair. I later moved the course, with appropriate modifications, into my department with the title Individual and Social Change. I also, during that period, included a weekend exercise as part of the course, using SIMSOC, an instructive simulation of the formation and functioning of a society developed by my colleague William Gamson (1978). On one occasion, we did a simulation of the Israeli–Palestinian conflict. In the fall of 1985, as Sterling McMurrin Distinguished Visiting Professor of Liberal Education at the University of Utah, I taught a course on Stability and Change: Recurrent Themes in Social and Political Psychology. On returning to Harvard, I restructured my trademark course and taught it under the title Stability and Change in Attitudes and Social Relations.

The last version of my trademark undergraduate course, which I taught five times in the 1990s, was a large-enrollment core curriculum course entitled Individual and Social Responsibility: A Social Psychological Perspective. Harvard's core curriculum identifies several different ways of knowing, not necessarily corresponding to established disciplines, to each of which students are expected to have some exposure. My course, which was part of the area of social analysis, was developed and originally taught with the assistance of Susan Korper, who has a superb level of knowledge and understanding of all strands of my work—including my work on attitudes, social influence, authority, ethics, conflict resolution, and the Middle East. She helped to devise an outline that somehow covered and integrated all of those domains, put together an appropriate reading list, and selected and edited a series of films illustrating central themes of the course. The course, using my own version of a rule-consequentialist approach to moral decision making, covered a wide range of topics in social psychology and related fields bearing on the question of how individuals—through personal and collective effort—determine and assume responsibility for their own actions and for public policies and practices. At various points throughout the course, I introduced "reflexive exercises," designed to turn our analysis of individual and social responsibility back on the behavior of social scientists themselves. This course gave me the opportunity to pull together virtually all of the themes that I had addressed over the years and to relate them to each other in a meaningful way. I found it particularly rewarding to present these ideas to a broad spectrum of students, most of whom concentrated in the natural sciences or humanities, and—in keeping with the terms of the Cabot chair—to offer them some of the tools for dealing with the ethical questions they would face in life and relating themselves to the social issues of the day.

A Social Psychological Perspective

The subtitle of my core curriculum course raises a question to which I address the remainder of this chapter. What do I mean by "a social psychological perspective"? Or, to reverse the question: What is my perspective on social psychology? I believe that the best answer to this question is provided by the preceding chapters in this volume. Despite their diversity—or, perhaps, in keeping with their diversity—they all illustrate, in one way or another, the particular perspective on social psychology that my work represents. Perhaps the best way I can even come close to integrating this rich set of papers is to offer a few observations about my particular perspective on the field that, I propose, they all share.

Definition of Social Psychology

Inside my copy of the classic text in social psychology by Krech and Crutchfield (1948), I found some pieces of paper with reactions to their introductory chapter that, from all indications, I had written close to the time the book was published—in other words, early in my graduate student years. I had some misgivings about their definition of social psychology as "the science of the behavior of the individual in society" (p. 7), especially their argument that person objects are similar to other objects, except for possessing certain special properties. In my notes, I argue that our reaction to other human beings cannot be compared, for example, with our reactions to wind or water, even though these share some of the properties of human beings, such as mobility and capriciousness. The notes grapple with the question of what precisely makes human objects unique for us.

I ultimately found my answer in the concept of social interaction, as developed primarily by sociologically based social psychologists. I remember feeling a sense of recognition in the summer of 1949, when I first heard Freed Bales (in a lecture at the University of Michigan, where he was teaching summer school) define social psychology as the study of social interaction. This definition goes beyond Krech and Crutchfield's in focusing on the behavior in society of *individuals in relation to one another.* Moreover, social interaction is more than behavioral interaction—more than action and reaction of individuals in one another's presence. It refers to the interaction between "minded" individuals, each of whom assumes that the other—just like the self—brings a set of expectations, intentions, and goals to the situation. Thus, participants in social interaction, in pursuit of their own needs and interests, engage in a continuing process of taking the other's role in order to assess and address the other's expectations, intentions, and goals. Social interaction is informed and guided by its societal and organizational context, which defines the nature of the situation in which the interaction takes place and the norms and rules that govern the interaction.

As my own conception of social psychology evolved, I brought the societal and organizational context of interaction explicitly into my definition of the field, while maintaining the focus on social interaction. This formulation corresponds to Shoshana Zuboff's (chap. 7, this volume) idea of social psychology's "middle kingdom" and to José Ramón Torregrosa's (chap. 2, this volume) call to give the sociological dimension the place it is due in our conception of social psychology. Thus, in a statement also cited by Torregrosa, I offer the following definition of the field:

> Social psychology—which is a sub-field of psychology as well as sociology—is concerned with the intersection between individual behavior and societal-institutional processes. It follows from this concern that the primary focus for social-psychological analysis is social interaction, which is, par excellence, the area in which individual and institutional processes intersect. Social interaction is thus the level of analysis that is most purely and most distinctly social-psychological. (Kelman, 1965a, p. 22)

A full analysis of social interaction requires simultaneous attention to variables at the level of the individual and of the social system as both inputs and outcomes of the interaction: How is the interaction shaped by what the individual participants bring to it and the societal/organizational context in which it occurs, and how does it, in turn, impact the subsequent functioning of the participants and of the larger social system (the group, organization, society, or collectivity) within which their interaction is an episode?

According to this definition, the subject matter of social psychology clearly includes the study of social interaction processes themselves, such as verbal and nonverbal communication, interpersonal relations, or small group dynamics. It also includes the functioning of individuals, as shaped by their direct or indirect interactions with other individuals, media, and institutions in negotiating their social environment, and as expressed in social attitudes, social roles, or collective identities; as well as the microprocesses of societal and organizational functioning, such as the social interactions through which leadership is exercised, decisions are made, or conflicts are managed. Most distinctively social psychological topics are those topics that explore relationships across the individual and social-system levels of analysis—i.e., the effects of societal/organizational inputs on the behavior of individuals, or the effects of individual inputs on the functioning of societies or organizations—with social interaction, explicitly or implicitly, as the mediating process. A good example of the former relationship is the process of socialization into a society, profession, or movement, whereby the rules, roles, and values of the particular social system are transmitted (through various socializing agents) to individual members and expressed in their attitudes, beliefs, and actions. A good example of the latter relationship is the process of social protest, whereby the motives and perceptions of members of a society are translated (through various forms of collective action) into changes in societal policies and practices.

Social psychology, as I define it, is particularly well suited to exploring the relationship between individual change and social change. Changes at these two levels can best be conceived as linked to each other in a continuous, circular fashion. Structural changes, by way of various processes of social interaction, produce changes at the level of individuals, which in turn, by way of another set of interaction processes, produce new changes at the system level, and so on. Thus, for example, the U.S. civil rights movement in the 1950s and 1960s was spurred on by structural changes in the United States and elsewhere—such as the rise of a Black urban middle class and the establishment of independent states in sub-Saharan Africa; the resulting group mobilization and mass action promoted psychological changes in the form of development of group consciousness and of a sense of entitlement and efficacy, which in turn encouraged the organized use of political influence conducive to civil rights legislation and to changes in occupational, educational, and political structures. My interest in the relationship between individual change and social change was a major factor in my initial choice of social psychology as my field of endeavor and it became increasingly central to the way I conceptualized my work. Thus, as I came to look at social influence in terms of the linkage between the individual and the social system—and at the three processes as representing different types and avenues of linkage (cf. Kelman, 1974; Kelman & Hamilton, 1989; Kelman & Warwick, 1973)—it became clear that changes in individuals' attitudes and behavior in response to social influence may have consequences for the social system within which the influence relationship takes place. In my later work, I have stressed that my approach to conflict resolution—interactive problem solving—and its operationalization in problem-solving workshops are quintessentially social psychological in that they seek to induce changes in individuals, through interaction in small-group settings, as vehicles for change in the larger system: in the official policies and the political cultures of the conflicting parties.

The definition of social psychology that I have outlined here seems to cover the work presented in all of the preceding chapters. I am not suggesting that all of the authors would necessarily subscribe to my definition, but what all of the chapters have in common—despite the breadth and diversity of the topics they address—is embeddedness in a social psychology that explicitly assigns a central role to the societal and organizational context of the behavior and interaction of individuals. This view of the field, corresponding to Thomas Pettigrew's (1991) concept of *contextual social psychology,* distinguishes the work presented in this volume from much of the work that characterizes mainstream American social psychology today.

Features of Social Psychology

My perspective on social psychology has certain distinct features that are well represented in the various chapters in this volume.

(1) A direct implication of my definition of social psychology is a view of the field as an *interdisciplinary* enterprise. I am not merely referring to the fact that social psychological work often requires forays into other disciplines—which in my case have included, over the years, anthropology, clinical psychology/psychiatry, ethics, political science, international relations, and Middle East studies. I view social psychology itself as an interdisciplinary field, anchored in both psychology and sociology and bridging the levels of analysis peculiar to each of these fields. A symbolic indicator of the coparentage of social psychology is the fact that the first two texts in social psychology, published in the same year, were written by a psychologist and a sociologist, respectively (McDougall, 1908; Ross, 1908). Personally, the fact that I served as both president of the APA's Division of Personality and Social Psychology (1970–1971) and chair of the ASA's Section on Social Psychology (1977–1978) attests to my commitment to social psychology as an interdiscipline. My students have gone in a variety of directions. Of the chapter authors in this book, four have made their careers in psychology departments, three in sociology departments, three in political science or international relations, and one each in a medical school, a business school, and a social service organization.

(2) Social psychology, in my view, must of necessity rely on a *multiplicity of methods.* I was trained as an experimental social psychologist and conducted an active experimental program in the 1950s and 1960s. The work included a number of experimental tests of my three-process model of social influence (see Kelman, 1974, 1980). In the 1960s, during my period at the University of Michigan, I collaborated with Reuben Baron and our associates in a series of experiments designed to test a functional analysis of the effects of attitude-discrepant behavior on attitude change (Kelman, 1980; Kelman & Baron, 1974; Kelman et al., 1969). I have never abandoned my commitment to experimental research as an important and uniquely valuable component of the social psychologist's

methodological repertoire—even though I have not personally pursued an experimental program for many years. Experiments make a unique contribution by constructing a working model of a phenomenon, which allows us to vary its dimensions systematically and to establish causal relations. But I do not believe that social psychology can be a *purely* experimental science, with the goal of establishing general laws of social behavior. The relations observed in the laboratory are limited by their historical and cultural context, as well as by the structure of the experimental situation itself (Kelman, 1967b). Experimental research becomes useful when it is put together with findings yielded by a variety of other methods, which identify the phenomena to be explored in a laboratory setting and which help establish the generality and external validity of laboratory findings—methods that include opinion surveys, intensive interviewing, systematic observation, participant observation, participatory action research, discourse analysis, and content analysis of documents. The research of my students—as exemplified by the chapters in this volume—has been carried out both in the laboratory and in the field, has used experimental as well as the entire range of nonexperimental methods, and has applied systematic approaches to both quantitative and qualitative data analysis. Some of the research programs described in the preceding chapters—as well as some of the doctoral theses I have supervised, including those of Tamra Pearson d'Estrée (Pearson, 1990) and Rebecca Wolfe (2002)—use a triangulation approach, exploring the same phenomenon in different contexts and with different methods, which significantly enhances the generalizability of the findings.

(3) Another aspect of my view of social psychology is its character as a *cross-cultural, international* enterprise. Cross-cultural research does not refer only to research in which cultures, or types of cultures, serve as the independent variable. Clearly, such research is instructive, in correcting for cultural biases in our conceptions of human nature and in sensitizing us to cultural differences in normative expectations and in modes of satisfying basic human needs. A challenge to this genre of research is to avoid the temptation of essentializing cultural differences, by recognizing that such differences arise from particular historical, structural, and situational circumstances and can change as these circumstances change, and that intracultural variations on psychosocial dimensions are often as great as or greater than intercultural variations. But cross-cultural research also refers to studies in which general propositions are tested with cross-cultural data, as in the Whiting and Child (1953) study, on which I held my first assistantship; in which related phenomena are explored in a variety of cultural settings, as exemplified in Lee Hamilton's multifaceted research program (chap. 4, this volume); or in which new research programs are shaped within a different cultural context than the one in which social psychology has so far evolved, well exemplified by the work of Ignacio Martín-Baró and Maritza Montero as discussed by José R. Torregrosa (chap. 2, this volume). Such cross-cultural work is essential to the scientific development of the field, in producing a body of propositions and findings with increasingly general validity and universal applicability. To this end, it is necessary not only to test hypotheses with cross-cultural data, but to assure wide participation of investigators throughout the world (including, of course, the Third World) in the definition of research problems, the formulation of hypotheses, and the interpretation of findings. Sci-

entific requirements thus coincide with the ethical requirements of avoiding exploitation of developing societies and assuring that research carried out in these societies addresses their own problems and serves their own interests (Kelman, 1967a, 1982a). More broadly, my view of social psychology calls for the development of a transnational community committed to enhancing the capacities and opportunities of scholars around the world to participate in building the field. This concept was the underlying purpose of the International Conference on Social-Psychological Research in Developing Countries at the University of Ibadan that I organized and chaired (Kelman, 1968a).

(4) *Applied research and practice* based on social psychological principles are as central to my view of the agenda of our discipline as basic and theory-driven research. Paraphrasing Lewin's (1951) famous dictum, I believe that there is nothing so conducive to theoretical insight as reflective application and practice, and nothing so practical as a good theory.[6] I do not maintain that all social psychologists must engage in applied work or that all social psychological research must have obvious relevance to applied problems. But I do maintain that applied research and practice are not only legitimate foci for social-psychological work, but important avenues for enriching the discipline. The relationship between theory and application can take a variety of forms, ranging from Carl Hovland's research on attitude change—which generally started out with applied questions that he sought to answer with sophisticated theoretical analyses and experimental designs—to action research (of which my work on conflict resolution is one variant), in which theory and practice are fully integrated. Lewin's belief that the "attempt to bring about change in a process is the most fruitful way to investigate it" (Deutsch, 1968, p. 478) suggests that application and practice are particularly capable of contributing to theoretical understanding insofar as they are geared to producing change. The relationship between theory, application, and practice as a central feature of social psychology is clearly proclaimed in the subtitle and the tripartite division of the present book and is reflected in every one of its chapters.

(5) The applications of social psychology that are of particular interest to me are those directed to addressing urgent *social issues* and to the betterment of the human condition. The issues with which I have been especially concerned over the years, from a social psychological perspective, are war and peace, social justice, conflict resolution, civil rights and civil liberties, intergroup relations, social protest, and responsible citizenship. I identify with a social psychology

[6]The exact wording of Lewin's statement is as follows: "Many psychologists working today in an applied field are keenly aware of the need for close cooperation between theoretical and applied psychology. This can be accomplished in psychology, as it has been accomplished in physics, if the theorist does not look toward applied problems with highbrow aversion or with a fear of social problems, and if the applied psychologist realizes that there is nothing so practical as a good theory" (Lewin, 1951, p. 169). Generally, only the last phrase of this statement is cited. In part, no doubt, this reflects the particular interest of those who cite Lewin on this point. In part, however, I believe it is simply due to the fact that the first half of the aphorism is not stated as succinctly and forcefully as the second half. The second half clearly asserts the value of theory to application, whereas the first half merely admonishes theorists not to scorn applied work, without asserting that applied work is actually of value to theory building. I believe—perhaps presumptuously—that my paraphrasing is a more sharply drawn and balanced statement of the point Lewin wanted to make.

that is engaged with the problems of our society at the domestic and global levels, that encourages the systematic analysis of social problems and the integration of research with social action, and that recognizes and takes into account the inevitable involvement of our social and political values in social research (Kelman, 1968b). In line with this orientation, I have been an active member of the Society for the Psychological Study of Social Issues, which—as already mentioned—I joined in 1946, when I was still an undergraduate. In later years, my social-issues orientation to the field has also been expressed through groups like Psychologists for Social Responsibility, the Society for the Study of Peace, Conflict, and Violence (the APA's Peace Psychology Division), and the ASA's Section on Peace, War, and Conflict.

(6) Finally, the *ethical dimension* occupies an important place in my view of social psychology. Many of the traditional topics for social psychological research can be seen as a continuation of moral philosophy in a different guise. Good examples are studies that point to the shortcomings in moral behavior resulting from social pressures and cognitive biases, such as social conformity, groupthink, unquestioning obedience to authority, bystander apathy, prejudice, stereotyping, resistance to new information, and legitimization of oppressive practices. Social psychological research has also focused on conditions that strengthen the moral foundations of social life, including studies of social justice, helping behavior, cooperation, empathy, personal responsibility, forgiveness, moral reasoning, integrity in living up to one's values, and legitimacy in the exercise of power. Social psychology can thus contribute to our understanding of the empirical conditions for moral decision making and behavior, as well as our formulation of the assumptions about human nature and social order that underlie our approach to moral justification. Apart from the ethical dimension in the content of social psychology, I also consider it imperative for social psychologists (and other social scientists) to give systematic attention—as an integral part of their professional role—to the ethical implications of the processes and products of their research (Kelman, 1968b, 1972b).

Social Psychology in Practice

To round out this discussion of my perspective on social psychology, let me offer a few comments on how this perspective has shaped my thinking on the two topics that have been central foci of my work over many years: social influence and international conflict—and the relationship between them.

(1) As Lee Hamilton (chap. 4, this volume) points out, my three-process model is a model of *social influence*, as is clear from the title of my original essay (Kelman, 1956) and from most of my writings—although I may have muddied the waters by referring to "processes of attitude change" (Kelman, 1958) and "opinion change" (Kelman, 1961) in the titles of two early articles. As a social psychological model, it starts out with the structure of the influence situation and looks at influence within the context of the relationship between the influencing agent (O) and the person being influenced (P). The three processes distinguish between three types of relationship, best captured by the source of O's relative power over P (i.e., O's ability to affect the achievement of

P's goals relative to P's own power and the power of competing influencing agents): O's means control in the case of compliance, attractiveness in the case of identification, and credibility in the case of internalization (Kelman, 1958).[7] In view of the nature of the relationship that characterizes each process, compliance-based behavior tends to be manifested and sustained only under conditions of surveillance by O, and identification-based behavior only as long as P's relationship to O remains salient and satisfying, whereas internalized behavior—though rooted in P's relationship to O—becomes part of P's own value system and independent of the original source.

From the beginning, I viewed the three-process model as relevant to the entire range of influence situations, well beyond the persuasive communication setting in which I originally tested it. Thus, I applied it to analysis of changes in psychotherapy (Kelman, 1963), effects of international exchange experiences (Bailyn & Kelman, 1962), and the development of individuals' ethnic identity (Kelman, 1998b). In the 1960s, with my work (in collaboration with Daniel Katz) on nationalism and personal involvement in the national system, and with my increasing fascination with the concept of legitimacy, I began to extend the model to the analysis of the relationship of individuals to the state or other social systems, and to the nation or other collective entities (e.g., Kelman, 1969, 1997b). These efforts eventually led me to reconceptualize social influence, generically, in terms of linkage between the individual and the social system, and the three processes as three ways in which individuals may be linked to the system—three ways in which they meet demands from the state, nation, society, organization, or group and in which they maintain their personal integration in it (Kelman, 1974).

Each process, in this view, refers to a distinct component of the social system that generates standards for the behavior of individual members and provides a vehicle for their integration in the system: system *rules* in the case of compliance, system *roles* in the case of identification, and system *values* in the case of internalization. Rules, roles, and values are social psychological concepts par excellence, in that they bridge the individual and the societal/organizational levels of analysis. Rules, roles, and values are properties of the social system (the society or organization) that define the relationship of its members to the system and that are adopted—to different degrees and in different ways—by individual members. (Individuals, of course, each have their own constellation of rules, roles, and values, corresponding to the array of groups with which they are affiliated.) Conceptualizing social influence in terms of linkages between the individual and the social system places the three-process model squarely within my definition of social psychology as the field concerned with the intersection between individual behavior and societal-institutional processes. Social interaction, it will be recalled, is the point at which individual and organizational processes intersect.

[7] The three-process model—in which the source of O's power is one of three distinct antecedent conditions postulated for each process, and the one that was manipulated in the first experimental test of the model—shows many points of contact with French and Raven's (1959) model, distinguishing five bases of social power, which they developed independently at the same time. The overlaps are not surprising in view of the fact that both models draw heavily on Lewin's discussion of own versus induced forces (see, e.g., Deutsch, 1968, pp. 457–460).

Accordingly, the microprocess of social influence—the relationship between P and O postulated for each of the three processes—can be seen as an episode within the larger social system that provides the context for their interaction and for which that interaction has consequences.

The rule–role–value distinction served as a basis for identifying different emotional reactions experienced by individuals when they find themselves deviating from societal standards of responsibility or propriety (Kelman, 1974, 1980). These distinctions generated a model that predicts the kinds of concerns that are likely to be aroused and the way individuals are likely to deal with them, depending on whether the standards they have violated are compliance based (rules), identification based (role expectations), or internalized (social values). When the violated standards are in the domain of responsibility, the concerns take the form of social fear, guilt, and regret, respectively; when they are in the domain of propriety, the concerns take the form of embarrassment, shame, and self-disappointment, respectively. Nancy Adler (1974) tested this model in her doctoral dissertation with women who had undergone abortion. As she reminds us (especially me) in her contribution to this *Festschrift* (chap. 5, this volume), the edited volume on varieties of discrepant action, to which I invited her to contribute a chapter, never saw the light of day. I am very grateful to her for using her chapter in this volume to present a summary of the model and of her findings. I have never undertaken any empirical tests of this model myself, but I have used it extensively in my undergraduate teaching; my lecture on embarrassment, in particular, was always the highlight of my course.

I have used the concepts of rules, roles, and values most extensively in the distinction between three types of political orientation that characterize the way in which individuals relate themselves to political authority and define the citizen role. Lee Hamilton and I, in collaboration with Frederick Miller and later also John Winkler, developed scales of rule orientation, role orientation, and value orientation (as well as scales of sentimental and instrumental attachment) to the political system. Discussion of the three political orientations and findings based on the use of the three orientation scales are central components of our analysis in *Crimes of Obedience* (Kelman & Hamilton, 1989). Rule, role, and value orientations also formed the core of an analysis of civic responsibility that I presented at the inauguration of Alfred Bloom (another one of my doctoral students) as president of Swarthmore College (Kelman, 1993b). Finally, in my analysis of movements of social protest (e.g., Kelman, 1970, 1984), I eventually distinguished between rule-oriented, role-oriented, and value-oriented protest movements, based on the extent to which a movement focuses primarily on struggle over resources, status, or policy, respectively.

(2) As my work came to focus increasingly on international conflict, I did not abandon my interest in social influence, as I have already pointed out in the earlier comments on the continuities in my work. The microprocess of interactive problem solving, to which I shall return later, is in essence a process of mutual influence. At the macrolevel, as well, influence is a central component of my analysis of international conflict (Kelman, 1997c) and negotiation (Kelman, 1996). As Reuben Baron (chap. 1, this volume) notes, I have even applied the distinction between my three processes of influence to international and

intercommunal conflict resolution—a natural extension, since in both lines of work I have been concerned with the *quality* of change: its depth, durability, sustainability, and integration in the belief systems of individuals and societies. What has eluded me for some time, however, has been a precise match of influence processes at the international/intergroup level to the three processes of social influence that I distinguished in my earlier work. I am indebted to Nadim Rouhana for providing that match with his treatment of conflict settlement, conflict resolution, and reconciliation as three distinct processes (chap. 10, this volume). Although my view of reconciliation—both in general and, specifically, in the Israeli–Palestinian case—differs from Nadim's in a number of important respects, I am persuaded of the value of the qualitative three-way distinction and I feel that it offers the link to the three processes of influence that I have been looking for.

Establishing this link is, of course, esthetically pleasing to me, but the ultimate question is whether it is analytically useful. Does the link of conflict settlement to compliance, conflict resolution to identification, and reconciliation to internalization provide conceptual handles for distinguishing qualitatively different types of peacemaking with distinct antecedent and consequent conditions? I argue that it does in a recent paper (Kelman, in press), which focuses in particular on the correspondence of reconciliation at the intergroup level to internalization at the level of the individual. I conceptualize reconciliation as a change in each side's group identity—at least to the extent of removing negation of the other as part of one's own identity—in a way that strengthens the *core* of the identity, just as internalization represents a change in specific attitudes and beliefs as a way of maintaining the integrity of the person's value system as a whole. In short, conflict settlement in this scheme involves a mutual accommodation of the parties' *interests*, conflict resolution an accommodation in their *relationship*, and reconciliation an accommodation of their *identities*. This distinction points to three broad tasks that all social entities—individuals, groups, organizations, societies—must address as they negotiate their social environment and seek to balance the requirements of self-maintenance and social order: protecting and promoting their interests, establishing and maintaining their relationships, and affirming and expressing their identities.[8]

Interests, relationship, and identity are social psychological concepts, in the sense that they refer to the relationship between individuals and the social system, and also in the sense that they refer to properties of both individuals and social systems. Individuals have interests, relationships, and identities, which they pursue and express through the various groups and organizations with which they are affiliated. The groups and organizations—formed, essentially, to serve their members—in turn develop their own interests, relationships, and identities, which become personally important to the members and

[8]This distinction was foreshadowed in an earlier paper on ethical issues in social science research (Kelman, 1982b), in which I distinguished three types of ethically germane impacts of research, conceptually linked to the three processes of influence and the three types of system orientation: impact on the concrete interests of research participants, on the quality of interpersonal relationships, and on wider social values.

which the members are expected to support. These three concepts broaden the three-process model to capture the interaction of individuals or groups with each other and with larger social systems in a variety of social contexts and their integration in these social systems. The microprocesses of social influence can be subsumed under this broader framework by distinguishing three foci for the interaction between P and O: The interaction may center on participants' *interests,* whose coordination is governed by a system of enforceable *rules,* with which individuals are expected to *comply;* on the participants' *relationship*, which is managed through a system of shared *roles,* with which individuals *identify;* or on participants' *identities,* expressing a *value* system that individuals *internalize*.

(3) In enumerating my mentors, I did not include John Burton, because I did not meet him until 1966, when I was 39 years old—well beyond what I described as my "formative years" earlier in this chapter. But Burton's work (e.g., 1969, 1979) on the analysis and resolution of international conflict and his model of unofficial diplomacy have had a profound impact on my subsequent work (see, e.g., Kelman, 1972a, 1999). What particularly excited me about his approach—when I first heard about it in 1966 and then had the opportunity later that year to participate in an exercise on the Cyprus conflict that he organized at the University of London—was that I saw it as a distinctly social psychological form of practice. Burton's method, in my parochial view, was a way of putting into practice the theoretical ideas about social psychological dimensions of international conflict that I had been thinking and writing about.

My particular variant of conflict resolution—which I have come to call *interactive problem solving*—has evolved out of the problem-solving workshops that my colleagues and I have conducted over the years, particularly on the Israeli–Palestinian conflict (Cohen, Kelman, Miller, & Smith, 1977; Kelman, 1986; Rouhana & Kelman, 1994). The basic principles and procedures of our approach are derived from Burton's work, although the precise form it has taken has been influenced by our particular disciplinary background and intervention style and by the nature and history of the particular conflict on which our efforts have focused. The work has remained exciting to me over the decades because it continues to evolve as historical circumstances change and we are faced with new challenges. What has made it personally rewarding as well is the extent to which it draws on virtually everything I have done as a social scientist and social activist over the years, including my work on international conflict, social influence, individual and social change, group process, nationalism and national identity, and international contact and exchange, and my experiences in nonviolent direct action and my personal involvement in the Middle East.

In my earlier discussion of the definition of social psychology, I repeated my frequent observation that interactive problem solving and its operationalization in problem-solving workshops are quintessentially social psychological in that they seek to induce changes in individuals, through interaction in small groups, as vehicles for change in the larger social system—in the policies and the political cultures of the conflicting societies. I like to tell people that I "think small," which is true in the sense that I organize small-scale events, on a modest budget, with individuals who are generally not political decision makers, and I make no claims to resolving the conflict and bringing peace by these means. My only claim is that we make a small *contribution* to the larger peace process by using our academic base to work with individuals and small groups from the conflicting societies. But, however small the contribution may be, our microprocess is designed systematically to promote change at the macrolevel. The problem of transfer of changes from the workshop to the political process is a central theoretical issue that I have addressed in my writings from the beginning (e.g., Kelman, 1972a, 1993a); more recently, Cynthia Chataway (2002; see also chap. 12, this volume) has written about the issue. Many of the features of the workshop are specifically designed to balance the requirements for maximizing change within the workshop against the requirements for maximizing transfer to the larger process. Most notably, we prefer to work with participants who are not officials, but who are politically influential in their own communities. They are thus less constrained in their workshop interactions, but they occupy positions that enable them to transfer what they have learned to decision makers, political elites, and the wider public.

My conception of the problem-solving workshop reflects my earlier experience with two other social constructions: the social psychological experiment and the nonviolent direct-action project, as illustrated by the lunch-counter sit-ins organized by Baltimore CORE in the early 1950s. As a form of action research, the workshop combines elements traceable to both of these models.

Like an experiment, the workshop creates a microcosm in a relatively isolated, self-contained, and controlled laboratory setting, in which some of the forces that operate in the larger system (or the real world) can be activated, observed, and analyzed.[9] Good conflict resolution practitioners, like good experimenters, know that the microcosm they have constructed is not the real world, and that the contribution of their work to understanding and changing the real world ultimately depends on systematic attention to how the products of the laboratory interaction are generalized and transferred to the larger system.

Like nonviolent direct action, interactive problem solving is based on a model of social change that envisages complementary efforts at many system levels. Microlevel activities, such as bringing together individual members of conflicting parties in a workshop or organizing a sit-in at a neighborhood department store, can contribute to the larger process by challenging assumptions, raising consciousness, and introducing new ideas, which gradually change the political culture and increase the likelihood of change at the level of political leadership, institutional bodies, and official policy. Microlevel projects are more likely to make such contributions insofar as they have built-in multiplier effects, achieved, for example, by strategic selection of the participants in a workshop or of the target of a direct-action campaign.

All three of these models rely on the cumulative effect of small efforts. Each workshop, each experiment, each direct-action project makes its contribution as

[9]It should be noted that workshops differ from experiments in that they are not simulations of the real world. They involve real members of the conflicting parties engaged in a very real and often consequential interaction around the issues that divide their societies.

one element in a larger program, which in turn is one program among many related undertakings that build on each other and together provide some of the insights and tools for gradually improving the world. To produce a cumulative effect, however, requires more than accumulating workshops, experiments, or campaigns. It requires integrating work at this level with work at other levels that it is meant to complement and reinforce. Thus, interactive problem solving needs to be integrated with official negotiations, grassroots efforts, and public education to promote conflict resolution at the macrolevel, just as experimental research needs to be integrated with survey, observational, and historical research to produce valid knowledge of the social world, and nonviolent direct action needs to be integrated with negotiation, political action, and economic pressure to promote change in social policies and practices.

Conclusion

The observation about the cumulative effect of small efforts seems like an appropriate point on which to conclude this chapter, whose underlying theme has been the cumulative effect of our enterprise across generations.

In the spirit of a *Festschrift*, the contributors to this volume have all commented on the influence that I have had on their work. This influence is not necessarily reflected in the content of the work, but may manifest itself in the kinds of problems they have chosen to work on, the way in which they have approached them, and the professional roles they have carved out for themselves. I like to believe that—apart from exposing them to a few useful ideas—I have contributed to the professional development of my students by encouraging, modeling, and legitimizing ways of doing social psychology that are congruent with their own interests and orientations, even if they do not always correspond to traditional patterns.

Contemplating the influence that I may have had on my students led me quite naturally to focus, in this chapter, on those who significantly influenced my own thinking and shaped the kind of social psychology that I practice—ranging from Kurt Lewin, who almost became my mentor; through Daniel Katz and Carl Hovland and my other mentors and teachers during my formative years; to John Burton and Gordon Allport, in whose footsteps I have had the privilege of following. I believe that influences from these diverse sources can be found, not only in my own work, but also in the work of my students. It is probably difficult, if not impossible, to trace specific influences, but the cumulative effect of the flow of influence across generations seems evident in the contributions to this volume.

I am not able to summarize or integrate this diverse set of contributions, but I can, in conclusion, sketch three elements of the perspective on social psychology that the contributions (and the contributors) seem to share:

- The contributions to this volume are all examples of contextual social psychology (Pettigrew, 1991), which systematically looks at the behavior and interaction of individuals in their societal and organizational context. Daniel Katz, incidentally, was a leading exponent of this view of psychology (see, e.g., Katz & Kahn, 1966). Of necessity, such an approach tends to be interdisciplinary, as illustrated by many of the contributions. In fact, perhaps a third of the contributors are not card-carrying social psychologists; they practice social psychology from a different disciplinary base.

- The work discussed in this volume is problem-driven, rather than method-driven or even theory-driven. Though many of the chapters feature theoretical analysis, they tend to direct this analysis to problems of application or practice, in the spirit of Carl Hovland and Kurt Lewin.
- All of the contributors focus on the study of social issues and the solution of social problems, in the spirit of SPSSI (in which Gordon Allport, Kurt Lewin, and Daniel Katz were all leading figures) and of John Burton and the scholar-practitioner model. In keeping with this orientation, they display sensitivity to the ethical dimension of the work of the social psychologist and other social scientists. They embrace a social science that seeks to find ways of enhancing one group's identity without denying the identity of other groups, of resolving social conflicts by peaceful and constructive means, and of otherwise contributing to the betterment of the human condition.

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