

in 2000, and her diploma in mediation from the Swiss Agency of Development in 2004. She has worked as a research assistant in conflict analysis at the Center for Security Studies and Conflict Research at the Eidgenössische Technische Hochschule (ETH) in Zurich. She is currently working on a PhD on interactive problem solving at the University of Zurich, for which she conducted research under the auspices of Herbert C. Kelman's program on international conflict analysis and resolution at Harvard's Weatherhead Center for International Affairs.

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Peace and Conflict, 16: 361–387, 2010
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ISSN: 1078-1919 print/1532-7949 online
DOI: 10.1080/10781919.2010.518083

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Taylor & Francis Group



Looking Back at My Work on Conflict Resolution in the Middle East¹

Herbert C. Kelman

*Department of Psychology and Weatherhead Center
for International Affairs
Harvard University*

This autobiographical article traces the author's work on the resolution of the Israeli–Palestinian conflict back to his childhood experiences in Nazi-ruled Austria. It proceeds to describe his early participation in the American civil rights and antiwar movements; his choice of social psychology as his field of

¹This article is based on an address presented at a conference organized by the Peace Research Institute Frankfurt on the Establishment of a Missile Free Zone in the Middle East, on February, 26, 2010, in Seville, Spain.

Correspondence should be addressed to Herbert C. Kelman, William James Hall, #1304, Harvard University, Cambridge, MA 02138. E-mail: hck@wjh.harvard.edu

study because of its potential relevance to issues of peace, justice, and social change; and his role—as a social psychologist—in the beginnings of the peace research movement. It then describes his introduction to John Burton's work on unofficial diplomacy in the mid-1960s; his development—in collaboration with colleagues and students over the years—of interactive problem solving, an approach derived from Burton's work and anchored in social-psychological principles; and his application of the approach primarily in the Middle East. The article briefly describes the methods of interactive problem solving and reviews his activities during the various phases of the Israeli-Palestinian conflict and efforts to resolve it. The author also offers some reflections on key elements of the third-party role and on personal qualities that have shaped his performance in that role.

During the second half of my life so far—for better than 40 years—an increasingly large portion of my time and effort have been devoted to an action research program, centering on the development, practice, and teaching of a form of unofficial diplomacy that I have come to call *interactive problem solving* (e.g., see Kelman, 1986). The approach derives from the pioneering work of John Burton (1969, 1979, 1984) and is anchored in social-psychological principles. In the mediation literature, the approach has been described as a form of “informal mediation by the scholar-practitioner” (Kelman, 2002, p. 167). My students and associates over the years have applied the approach to a number of different international and intercommunal conflicts. My own primary—although not exclusive—focus has been on the Arab-Israeli case, with special emphasis on the Israeli-Palestinian conflict. My writings have included theoretical papers on the dynamics of that conflict, as well as policy analyses written from a social-psychological, conflict-resolution perspective.

When I am asked to reflect on this work, I find it very tempting to go back to the very beginnings. I have decided to yield to that temptation, particularly because my focus on conflict resolution and reconciliation, with special reference to the Middle East, can be traced back in a nearly continuous line to my earliest years.

EARLY YEARS

I was born into a Jewish family in Vienna. I was 11 years old at the time of Austria's Anschluss to Nazi Germany. After a year under Nazi rule, my immediate family managed to escape to Belgium, where we were given asylum and lived for a year in Antwerp as refugees with the financial support of the Jewish community. In March of 1940, we received the visas to the United States for which my parents had applied two years earlier,

and we sailed to New York just a few weeks before the German invasion of Belgium.

Shortly after the Anschluss, while we were still in Vienna, my sister (who is two years older than I am) and I decided to join a Zionist youth group. At earlier times, as I recall, my parents had misgivings about such a move, but I think they realized that membership in a Zionist group would help us maintain our self-esteem and sense of identity at a time when these were brutally assaulted. Indeed, I am fairly certain that my Zionist affiliation significantly contributed to the fact that there was never a moment during those difficult years when I entertained the notion that I—or my people—somehow deserved the treatment that we were subjected to.

Recently, my sister found a diary that she kept during this period in 1938 in which—among other things—she reported on our search for a suitable Zionist youth group. She mentioned discussions that she was having with some older boys who belonged to a right-wing Zionist organization. One day, according to the diary, she came home to report that organization's solution to the “Arab problem”: Because the Jews have only Palestine, whereas the Arabs have several countries, the Arab population could be relocated elsewhere in the Arab world, leaving Palestine for the Jews. She reported in the diary that this idea was not well-received at home; specifically, she wrote about the reaction of her 11-year-old brother: “*Dem Herbert wollte die Lösung der Araberfrage nicht gefallen, denn sagte er, wir können doch die Araber nicht zwingen, das Land zu verlassen, in dem sie nun sesshaft sind*” (“Herbert did not find the solution to the Arab question to his liking, ‘because,’ he said, ‘surely, we cannot force the Arabs to leave the land in which they are now settled’”). Note that I was not questioning the right of the Jewish people to establish its homeland in Palestine, but merely pointing out the obvious fact that there were other people living in that land who also had rights. Needless to say, we joined a group with more moderate views.

The Zionist youth movement that I joined in Vienna continued to be a central focus of my life during the year in Antwerp, and for several years after my arrival in New York. During those years, I was also intensively engaged in Jewish studies, including the study of Hebrew language and literature. My first two published articles, at age 18, appearing in student magazines in 1945, were in Hebrew. One of them (Kelman, 1945a) was entitled “In Defense of Nationalism,” and distinguished between some of the positive potentialities of nationalism, such as its contribution to the liberation of oppressed peoples and to the self-esteem and self-confidence of individuals, and its negative manifestations, such as exaggerated national pride, hatred of other peoples, and the extremes of selfishness. That article foreshadowed my later research and writings on nationalism and national

identity, starting in the 1960s, which distinguished between different varieties of nationalism and emphasized its dialectical character. My second Hebrew publication that year (Kelman, 1945b) was entitled “On the Question of Jewish–Arab Cooperation.” It discussed the common interests of Jews and Arabs in Palestine and argued that establishment of a Jewish state in Palestine requires cooperation between the two peoples.

In the years prior to the establishment of Israel, I supported the concept of a bi-national state in Palestine, which was advocated by a minority within the Zionist movement, including Martin Buber and some of his colleagues at the Hebrew University of Jerusalem, as well as the left wing of the Labor Movement. I should add here that today I do not advocate a bi-national state. I still think it was a good concept, and the history of the land might have been different if it had been supported by the majority of the Zionist movement and by the Palestinian–Arab community. However, today, a one-state formula for the land between the Mediterranean Sea and the Jordan River—whether in the form of a bi-national state or a unitary (one person–one vote) state—is a prescription for continuing the conflict. I am a staunch supporter of a two-state solution as the only formula for ending the occupation and ending the conflict.

My rather visionary version of a two-state solution is what I have come to call a “one-country/two-state solution” (Kelman, 2009). It is based on the mutual acknowledgment that the land belongs to both peoples—that both have authentic historic roots in it, and both are deeply attached to it. Recognizing that each people’s pursuit of its national aspirations on the basis of its exclusive claims has led to decades of violent conflict that may well lead to mutual destruction, the two sides agree to end the conflict with a historic compromise: They agree to share the land to which both peoples are so deeply attached in a way that allows each to exercise its right to self-determination, fulfill its national aspirations, and express its national identity in a state of its own within the shared land, in peaceful coexistence with the neighboring state of the other. The concept of a one-country/two-state solution allows both Israelis and Palestinians to maintain their attachment to the land as a whole while claiming “ownership”—in the form of independent statehood—over only their part of the land. It builds on the two peoples’ attachment to the land as a unifying, rather than divisive, force. This vision calls for free movement across the borders, as well as a range of cooperative activities that treat the shared land as a unit, and are designed to benefit each state and its population, as well as the country as a whole. Over time, a one-country/two-state solution might enable the two communities to build a new, transcendent identity alongside their separate identities. Institutionally, this solution may culminate in an economic union or even a confederation—conceivably including Jordan as a third partner—but such

options should be left to future developments and depend on how the relationship evolves over time.

FROM SOCIAL ACTIVISM TO SOCIAL PSYCHOLOGY AND BACK

Let me return to my chronological account, which I left in the mid-1940s. In the immediate postwar years, I became actively involved in the American civil rights and antiwar movements. In 1946, I participated in what I believe was the world’s first antinuclear protest as part of a group that picketed the Pentagon in opposition to the atomic bomb test on Bikini Island. On the way back from the Pentagon to the Washington railroad station, we decided to continue holding our placards. Some of us, including myself, were arrested and fined on the charge of “parading without a permit.” In 1947, I participated in a nonviolent direct action campaign in protest against racial discrimination at a popular swimming pool in New Jersey. Our technique involved continuing to stand in the ticket line when Black members of the group were refused admission. In keeping with Gandhian discipline, I refused to move when ordered to do so by the police, and was arrested, along with several colleagues, on a charge of “disorderly person.” I might add here that the organization that sponsored this project appealed our fines, and we won the appeal.

My activism continued beyond my student days. In the 1950s, I was very active in the Congress of Racial Equality (CORE), which pioneered the use of Gandhian methods of nonviolent direct action in the struggle against racial segregation. I was co-founder of a CORE chapter in Baltimore, and actively participated—along with my wife, Rose, whom I met in Baltimore—in a long, but ultimately successful, campaign to end segregation of the luncheon counters in so-called “five and ten cents stores,” all of which belonged to national chains. Our methods combined sit-ins, picketing, public education, negotiations with store managers, and bringing the issue to shareholder meetings of the parent companies of these stores. My experience with CORE has had a major impact on my thinking about the nature of social change and helped to shape my approach to conflict resolution and to the relation of the microlevel at which we work to the macrolevel that we hope to influence. My deepest involvement in CORE was in 1951 to 1954, the years I spent in Baltimore, but I continued to be involved throughout the 1950s as an elected field representative of national CORE. As another example of my activism in those years, I might mention that, during the Korean War, I almost went to prison for resisting the military draft; but, in the end, my draft board recognized me as a conscientious objector. During the Vietnam War, I was actively involved

in the antiwar movement and was a co-organizer of the first teach-in at the University of Michigan and of a conference on *Alternative Perspectives on Vietnam* in 1965 (Converse, Kelman, & Vandenberg, 1966).

However, what I particularly want to focus on here is the fusion of my scholarship with my activist concerns. When I began my college studies in 1943, I chose English language and literature as my field of concentration—not because of any specific career plans, but because I believed that whatever I ended up doing in my life would entail writing. In my third year of college, I switched to psychology as my major field because I became convinced that social psychology was a discipline with great relevance to the issues of peace, justice, and social change, with which I was concerned. By the same token, I decided to pursue graduate studies in social psychology, which I began at Yale University in 1947. The fusion of scholarship with social activism was particularly appealing to me and, indeed, has remained so to this day.

Throughout my training and subsequent career, my concerns with peace, justice, and social change affected the topics that I chose to work on, as well as my assessment and critique of the social research enterprise itself. In the course of my graduate training, I became thoroughly socialized as an academic social psychologist. My excursions into new areas of research, and even my critiques of some aspects of the field, have always been from the perspective of a social psychologist identified with the norms and purposes of the enterprise. My earliest research focused on processes of social influence and attitude change—which remains a continuing interest to this day (Kelman, 1953, 1958, 1961). My theoretical and experimental work in these areas during the 1950s and 1960s was well within the mainstream of my discipline, but it clearly reflected my activist interest in individual and social change. My early ideas about social influence inform my current thinking about mutual influence as conflicting parties move toward conflict resolution, transformation of their relationship, and reconciliation (see Kelman, 2004, 2006).

Another area of interest in the early years of my career was group processes, including some research on group psychotherapy and exploration of training or encounter groups. This focus foreshadows my work with problem-solving workshops, which are the primary tool of interactive problem solving. I hasten to add, however, that I am vehement in pointing out that problem-solving workshops must not be confused with therapy or encounter groups. Although my experience with the latter has influenced my practice in a number of ways, problem-solving workshops have a very different purpose and operate on a different level (Kelman, 1991).

Yet another area in which I have worked from the beginning of my career and which reflects my activist orientation is the ethics and politics of social

research. I have written and lectured on the responsibilities of investigators toward the individuals and groups that are the subjects of their research, on the social consequences of social research, and on the relevance of social research to social action and social change. My early essays in this domain were published in a book entitled, *A Time to Speak: On Human Values and Social Research* (Kelman, 1968).

BEGINNINGS OF PEACE RESEARCH

The one broad area of my early work that most explicitly reflects the concerns that originally brought me into the field and that serves as the most direct bridge between the first and second halves of my life is peace research and the analysis of international conflict. I was deeply involved in the beginnings of the peace research movement in the 1950s. In 1951, my last year in graduate school, Arthur Gladstone—a colleague in the department and, like myself, a conscientious objector to the Korean War—and I published a letter to the *American Psychologist* (Gladstone & Kelman, 1951), pointing out that pacifist theory rests on a number of psychological assumptions that could be put to empirical test, and proposed that psychologists and other social scientists might fruitfully place such efforts on their research agenda. The responses to this letter identified a community of scholars interested in pursuing a peace research agenda, and together we organized the Research Exchange on the Prevention of War, which—to the best of my knowledge—was the first organization committed to promoting peace research. Over the next few years, the Research Exchange published the *Bulletin of the Research Exchange on the Prevention of War*, organized discussion groups at academic conventions, as well as symposia (two of which were published: see Kelman, 1954; Kelman, Barth, & Hefner, 1955), and convened two summer workshops to explore theoretical approaches and research ideas in the emerging field of peace research.

In 1954 to 1955, I had the good fortune of being among the first group of Fellows invited to the newly established Center for Advanced Study in the Behavioral Sciences at Stanford, California. Although I was one of the youngest Fellows at the Center, I convened a group of colleagues to inform them about the Research Exchange and solicit their advice on how to broaden its base and move the enterprise forward. The discussions of this group led to the decision to start a new interdisciplinary journal, which would replace and significantly expand 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.

The Journal of Conflict Resolution began publication in 1957 as the first journal in the newly emerging field of peace research. With the inauguration of the journal, we ceased publication of the *Bulletin of the Research Exchange*, and we decided that the other activities of the Research Exchange could be pursued most effectively by merging our small organization with a newly formed Committee on International Relations of the Society for the Psychological Study of Social Issues. Meanwhile, at the University of Michigan, the work on planning and editing the *Journal of Conflict Resolution* created an interdisciplinary community of scholars interested in issues of war and peace. This group became the nucleus for the university's new Center for Research on Conflict Resolution, which I joined a few years later, when I came to the University of Michigan in 1962, on a joint appointment between the Psychology Department and the Center.

My own research and writing gradually moved to a focus on international relations. In the 1950s and 1960s, some of my thinking and writing was addressed to the question of where social-psychological concepts and methods can contribute to the development of a comprehensive theory of international relations—to identifying the relevant points of entry for social-psychological analysis. A major product of this work was an interdisciplinary volume that I edited, and for which I wrote the opening and closing chapters: *International Behavior: A Social-Psychological Analysis*, which was published in 1965 (Kelman, 1965). That volume, incidentally, was much better known among international relations scholars than among my fellow social psychologists. Over the years, I was repeatedly told by colleagues around the world that they had to read this book for their doctoral examination.

Publication of that book, I believe, significantly contributed to my credibility among international relations scholars who were prepared to accept me as a legitimate member of their guild. Thus, for example (to get ahead of my story), I was elected President of the International Studies Association in 1977; and, in 1976, I was invited to join the Center for International Affairs (now the Weatherhead Center) at Harvard University and its Executive Committee. (For those of you trying to keep track of my movements, let me add that I taught at Harvard between 1957 and 1962; then moved, as I mentioned, to the University of Michigan; and returned to Harvard in 1969 to take up the Cabot Chair of Social Ethics.) The Weatherhead Center has been the all-important base of my conflict resolution work for decades: I have chaired (or co-chaired) its Middle East Seminar since 1976, I founded—together with my students—and directed (between 1993 and 2003) the Program on International Conflict Analysis and Resolution at the Weatherhead Center, and my action research program on conflict resolution in the Middle East has been—and continues to be—based at the Weatherhead Center.

To return to the 1950s and 1960s, my research during those years also increasingly focused on issues in international relations, although it did not directly deal with international conflict. Colleagues and I carried out two extensive studies on the impact of international educational and cultural exchanges (see Bailyn & Kelman, 1962; Kelman & Bailyn, 1962; Kelman, Ezekiel, & Kelman, 1970), and—together with colleagues at the University of Michigan—I pursued a research program on nationalism and the involvement of individuals in the national political system (see DeLamater, Katz, & Kelman, 1969; Katz, Kelman, & Flacks, 1964; Kelman 1969)—which, as I mentioned earlier, was foreshadowed by my 1945 article on nationalism.

Let me also mention here another major line of my research, although it is chronologically out of order in that it was carried out in the second half of my life. In the early 1970s, my then student V. Lee Hamilton and I conducted a U.S. national survey on public reactions to the My Lai massacre in Vietnam and the trial and conviction of Lt. William Calley for that crime. Our focus was on people's attribution of responsibility for crimes committed under orders from authority. This and subsequent research were reported in our book, *Crimes of Obedience: Toward a Social Psychology of Authority and Responsibility*, published in 1989 (Kelman & Hamilton, 1989). Theoretically, this work draws on my earlier work on social influence, legitimate authority, and personal involvement in the national political system. It also reflects my earlier encounters with genocide, destructive obedience, and resistance to unjust authority.

INITIAL EXPLORATIONS IN UNOFFICIAL DIPLOMACY

These then are some of the experiences and activities that prepared me for the work that has dominated the second half of my life. The defining moment that opened the possibility of this kind of work for me was my first meeting with John Burton in the summer of 1966. Burton, a former senior Australian diplomat, had recently established the Centre for Analysis of Conflict at the University College of London, and begun to experiment with a form of unofficial diplomacy for which he initially used the term *controlled communication* (Burton, 1969; see also Burton, 1979, 1984). The method involved bringing together high-level representatives of parties in conflict in an academic setting for confidential, unofficial, analytic communication under the guidance of a third-party panel of political and social scientists. I was immediately intrigued by Burton's model, seeing it as a way of putting into practice the theoretical ideas about social-psychological dimensions of international conflict that I had been thinking and writing about since the early 1950s. I had the sense that this was the kind of direct involvement in the resolution of

international conflicts that I had been searching for. When Burton invited me to join him in London in November 1966 as a member of the third party in an exercise—or a problem-solving workshop, as we would now call it—on the Cyprus conflict, I accepted with enthusiasm.

The Cyprus exercise was only Burton's second venture in controlled communication. He spent a day, therefore, with members of the third party and other colleagues planning the event and discussing agenda, procedures, and third-party interventions. I came away from the entire experience with some questions, many ideas, and a clear interest in further pursuing this approach. Several months later, in reaction to the Middle East war of June 1967, I began to think about applying the Burton model to the Arab-Israeli conflict. Together with John Burton, I explored the possibilities, but—largely because our contacts were limited—nothing came of that effort.

I had a full agenda of other projects, but I continued to think about the method, to discuss it with different colleagues, to stay in touch with the Cyprus conflict, and to explore the idea of Arab-Israeli workshops with colleagues from the region. I also wrote my first article on "The Problem-Solving Workshop in Conflict Resolution" (Kelman, 1972). After reading a draft of that article, Stephen Cohen, a young colleague at Harvard with whom I co-taught a graduate seminar on social-psychological approaches to international relations in 1971, suggested that we organize a pilot workshop in connection with the seminar in which the students would participate as apprentice members of the third party. This workshop in 1971 turned out to be the first in a long series of Israeli-Palestinian workshops that I have conducted over the ensuing years.

Originally, Steve Cohen and I agreed that this pilot workshop should not deal with the Middle East because we considered it inappropriate for two Jews to facilitate an Arab-Israeli workshop. We allowed our class to persuade us, however, to focus on the Middle East, and we decided to organize an Egyptian-Israeli-Palestinian workshop with young scholars from the three communities. To correct for the imbalance in the leadership, we consulted with two scholars with Arab backgrounds, one of whom—the renowned Oxford historian, Albert Hourani, who happened to be a visiting professor at Harvard in 1971—actually participated in the workshop, although he made it clear that he was there as a consultant and not as a facilitator. After a pre-workshop session with Egyptian invitees, they dropped out of the project, giving scheduling problems as their reason; in retrospect, I believe that they had come to the conclusion that this workshop would be a no-win situation for them: They had an agenda with the Israelis because, at the time, President Nasser was searching for ways of reaching an accommodation with Israel, but—in light of Egypt's position as leader of the pan-Arab movement—they could not pursue that agenda in the presence

of the Palestinians. On the Palestinian side, a senior graduate student in Middle East history at Harvard expressed great interest in the project, and told us that he would be able to help recruit other Palestinians if we could persuade a Palestinian intellectual whom he knew—who was working at the Palestine Liberation Organization (PLO) office in New York—to join us. I contacted the man, visited him at his home outside New York, and he accepted my invitation. We had no problem completing the Palestinian team and recruiting the Israeli participants.

This first workshop (Cohen, Kelman, Miller, & Smith, 1977) laid the groundwork for the development of interactive problem solving—our particular approach to conflict resolution. Although interactive problem solving is firmly anchored in John Burton's model, we did develop—starting with that first experiment—our own style of running workshops, which is reflected in the ground rules, the agenda, and the third-party interventions that have characterized our work over the years.² Both the process and the content of our workshops have been more explicitly informed by social-psychological principles. The 1971 pilot workshop also served as a model for a series of workshops—mostly with Israelis and Palestinians—that I have organized as part of my graduate seminar on *International Conflict: Social-Psychological Approaches* in which we were able to provide a unique and valuable learning experience for our students without compromising the conditions required for an effective workshop. Three specific lessons I learned from this first experience that have been reinforced by subsequent work are (a) that the third party need not be neutral in the sense of disinterested, but in situations like mine—of a Jew dealing with the Arab-Israeli conflict—it is important to work with an ethnically balanced team; (b) that—although recognizing that conflicts are almost always multilateral in a variety of ways—interactive problem solving is most effective in achieving its purpose when there are only two parties around the table; and (c) that one cannot meaningfully deal with the Israeli-Palestinian conflict without bringing Palestinians who are identified with the PLO into the process (just as one cannot do so without Israelis who identify with the Zionist enterprise).

During the 1971 to 1972 academic year, I was on leave in Seattle, where I was busily engaged in a variety of projects unrelated to the Middle East conflict. At the end of that year, I suffered a heart attack (while delivering a Kurt Lewin Memorial Address on the topic of "Violence Without Moral Restraint: Reflections on the Dehumanization of Victims and

²These and other features of problem-solving workshops are described in greater detail in my second article in this issue (Kelman, this issue).

Victimizers"; see Kelman, 1973). I was back home recuperating when the 1973 Arab–Israeli war broke out. As I was watching the television accounts of the war while contemplating the possibility that I might not live forever, I committed myself to placing work on conflict resolution in the Middle East at the top of my agenda, and it has stayed there ever since.

MIDDLE EAST INVOLVEMENT

Soon after my recovery, Steve Cohen and I put together a facilitating team that included three Arab-American scholars. I increasingly participated in Middle East-related meetings and conferences. I traveled extensively in the Middle East, sometimes together with other members of the team—but always with my wife, Rose, who became a full partner in this work. My near total immersion in this work, starting in the mid-1970s, would not have been possible if my wife had not been fully committed to it and participated in it at all levels—from making practical arrangements and taking notes at workshops, to making our Middle East work the center of our social life. In the summer of 1975 we traveled for the first time in Arab countries, including Egypt, Lebanon, and Jordan, as well as the West Bank, talking to PLO officials, political figures, journalists, and scholars. We had similar encounters in Israel. Also that year, we organized a workshop (which met at our house) with senior, politically engaged Israeli and Palestinian academics and our five-member third-party team.

On our first trip to Egypt, we established contact with the Al Ahram Center for Political and Strategic Studies. In 1976, Boutros Boutros-Ghali—who was then a professor at Cairo University and President of the Al Ahram Center—and I organized a roundtable on "Mutual Perceptions in Arab–Israeli Relations," which took place over a three-day period. The participants included four of the members of our team and many leading Egyptian political and social scientists. What particularly impressed me was the avid interest of the Egyptian participants in learning about Israeli society and their frequent references to the "post-settlement" period—which suggested to me that they were contemplating a settlement with Israel. The proceedings of this meeting were taped, translated into Arabic, and—as I found out about a year later—published in the Egyptian journal of international relations, of which Boutros-Ghali was the editor. After the meeting, our team went on to Israel, where we shared our impressions with Israeli colleagues and officials.

During the meeting in Egypt, I was asked whether I would be interested in an invitation to come to the American University at Cairo for a 5-week appointment as a Distinguished Visiting Professor. I responded with great

enthusiasm, but also pointed out that I was Jewish and wondered whether that might be an impediment to such an appointment. I was assured that this was not an issue at all; in retrospect, I think it may actually have been an advantage in that my Egyptian colleagues may have seen me as a bridge to Israel at a time when they were rethinking their relations with Israel. We set the date for my visit in the late Fall of 1977.

In November of 1977, on my way to Cairo to start my visiting appointment, I stopped in Israel to attend a symposium on Arab–Israeli peace in Tel Aviv. A day or two into that symposium, Egyptian President Anwar Sadat made the dramatic announcement of his visit to Jerusalem on the coming weekend. It was decided to move the later sessions of the symposium to Jerusalem in anticipation of his arrival there. So, I was in Jerusalem during the truly electrifying days of Sadat's visit there, and a few days later my wife and I went on to Cairo.

As part of my visiting professorship, I was scheduled to give a public lecture. When my hosts learned that I had just come from Jerusalem, they asked me to devote my lecture to that experience. Thus, I ended up speaking on a topic that I could not have anticipated when I packed for my trip: "The Psychological Impact of the Sadat Visit on Israeli Society" (see Kelman 2005b). Even at this early stage of the Egyptian–Israeli peace process and amidst the euphoria of the Sadat initiative, my analysis emphasized the need to resolve the Palestinian issue if the Egyptian–Israeli peace process was to fulfill itself. My lecture was immediately translated into Arabic and published in its entirety (along with a photo of me) in the weekend edition of *Al-Ahram* (and later reprinted in a commemorative brochure on Sadat's visit to Jerusalem; see Kelman, 1978b). On the strength of my appearance in the pages of *Al-Ahram*, I received press credentials that allowed me to join the working press at the short-lived Cairo peace conference and the Begin–Sadat Summit in Ismailiya in December 1977.

Shortly after I arrived in Cairo in November, I met with Boutros-Ghali, who had in the meantime been appointed minister of state for foreign affairs (the number-two position in the foreign ministry) and was, in fact, acting foreign minister at the time because the foreign minister had resigned in protest against Sadat's initiative. What became clear to me from this conversation was that our Al-Ahram roundtable the year before was part of the process of rethinking their relations with Israel by Egyptian political and intellectual leaders, which paved the way for Sadat's historic initiative—and, indeed, it was part of Boutros-Ghali's important contribution to that process.

In light of these historic developments, our team planned a conference on the future of the Egyptian–Israeli relationship and the process of reconciliation following a peace agreement. We recruited a group of leading Egyptian and Israeli intellectuals, as well as some scholars who had been

involved in reconciliation programs outside of the region, such as the Franco-German case. We obtained the Rockefeller Foundation's Villa Serbelloni in Bellagio—a perfect setting for such a conference—and were completing final arrangements for the event, scheduled for January 1979. A few weeks before that date, following the Begin-Sadat agreement at Camp David, I was informed that most of the members of the Egyptian team had decided not to take part in the conference unless it included Palestinian participants. I knew that this was impossible to arrange because politically engaged Palestinians saw the Camp David agreement as a separate peace and a betrayal of the Palestinian cause. I could not persuade my Egyptian colleagues that they could make a greater contribution to the Palestinian cause by meeting with the Israelis and raising the Palestinian issue than by staying away. As a result, to our great disappointment, the conference had to be cancelled. We did organize a workshop at Harvard with a few of the participants to discuss where to go next. Some of the members of my team decided to pursue the Egyptian-Israeli process, primarily by way of back-channel diplomacy. My own conclusion was that a full peace and reconciliation between Egypt and Israel ultimately depended on a solution to the Israeli-Palestinian conflict and I, therefore, decided to concentrate my efforts on that issue—which I have done ever since.

PROBLEM-SOLVING WORKSHOPS

It is clear from everything I have said so far that there are many dimensions to the conflict resolution program that I have been involved in, and that it has entailed many kinds of activities. Starting in the 1970s, it has included a great deal of traveling in the region, meetings with political and community leaders and scholars, participation in conferences, as well as teaching, mentoring young scholar-practitioners, and different kinds of writing (to which I shall return shortly). However, the primary and unique tool of interactive problem solving throughout has been the problem-solving workshop. Over the years, I have been involved in organizing and facilitating some 80 problem-solving workshops or related events—mostly, but not entirely, with Israeli and Palestinian participants. Workshops have differed in a variety of ways, depending on the nature and number of participants, the occasion for convening them, the setting, and the specific purposes. A major distinction is between one-time workshops and continuing workshops, the first of which was convened by Nadim Rouhana and myself in 1990 and met until 1993 (Rouhana & Kelman, 1994). In all of their variations, workshops are governed by a set of key principles that are reflected in their ground rules, procedures, and agenda.

A central issue in the theory and practice of interactive problem solving is the dual purpose of the enterprise: to produce *change*—in the form of new insights into the conflict and new ideas for resolving it—in the particular individuals who are sitting around the workshop table, and to *transfer* these changes to the political debate and decision-making process in their respective societies. My thinking about this issue goes back to some of my early writings about group processes (e.g., Kelman, 1952), and I addressed it in some detail in my very first article on problem-solving workshops (Kelman, 1972). The dual purpose of interactive problem solving presents its theory and practice with the major challenge of structuring workshops in a way that would maximize both the generation of new insights and ideas and their transfer to the policy process.

What is particularly challenging is that the requirements for maximizing change may be not only different from the requirements for maximizing transfer, but, in fact, contradictory to them. I have described this dilemma as the *dialectics of interactive problem solving* (Kelman, 1979). In designing workshops, it is necessary to navigate these dialectics—to create the proper balance between an array of contradictory requirements. The best example is provided by the selection of participants. Officials close to the decision-making process are in a good position to apply what they have learned in a workshop, but they are likely to be more constrained in their interactions and therefore less likely to change. We, therefore, look for participants who are not officials, but politically influential—individuals who are more free to engage in the workshop process but, at the same time, occupy positions within their societies that enable them to influence the thinking of decision makers and the general public.

The third party plays a strictly facilitative role in our model. We do not propose solutions, give advice, evaluate the ideas presented, or take an active role in the substantive discussions. Our task is to create the conditions that allow ideas for resolving the conflict to emerge out of the interaction between the parties themselves. Nevertheless, the third party's role is important. We select and brief participants; set and enforce the ground rules; propose the main lines of the agenda; moderate the discussion; make a variety of interventions in the form of observations about the content and the process of the proceedings and occasional theoretical inputs; and serve as a repository of trust for the parties who, by definition, do not trust each other.

There are four aspects of the third-party role that I have found particularly important in my work:

1. *Networking* is a critical part of the work. The ability to identify and recruit workshop participants and maintain credibility depends heavily on the third party's connections with relevant elements of the elites in the two communities. In my own case, my close association with the

Center for International Affairs at Harvard University was very helpful in the process of building and maintaining our networks. Apart from providing a respectable base, the Center gave me the opportunity, over the years, to get to know and form friendships with Center Fellows and Visiting Scholars from the Middle East. Through them, I was able to make contacts with an ever-widening circle of potential participants in and supporters of our work.

2. Another essential aspect of the work is *teambuilding*. I first learned about the importance of working with a third-party panel from John Burton. Effective facilitation requires attention and sensitivity to all facets of the interaction—what is being said, how it is said, what reaction it elicits, what is not being said, the mood of the group, and the flow of the communication over the course of a session or a series of sessions. A team of two or more facilitators—particularly if they have experience in working with each other—is better equipped than a single facilitator to capture the dynamics of the interaction and move the process forward. In a team it is also possible to represent the different kinds of expertise—in group process, international relations, and the particular conflict region—that the third party should ideally possess. Furthermore, as I have already mentioned, as a Jew dealing with the Arab-Israeli conflict, I have always worked with an ethnically balanced team, partnering with Palestinian and other Arab colleagues. Such balance enhances not only the credibility of our team, but also its sensitivity to the experiences and perspectives of both sides in the conflict.
3. In the course of a workshop, much of the third party's time is spent in *respectful listening*. I have stressed in my teaching that the contribution of the third party is not necessarily measured by how much it says. By listening, we give the parties the opportunity to express their needs and communicate their perspectives, we demonstrate the importance we attach to the parties' listening to each other and penetrating each other's perspective, and we gain a fuller understanding of what is going on in the group and in the larger conflict environment before offering interpretations or making other interventions.
4. The third party can also contribute to the *transfer* of ideas developed in the course of workshops to the policy debate and the political process. Transfer, of course, primarily is the task of the two parties; the expectation that workshop participants will develop new ideas and be in a position to transfer them to the policy process is central to the underlying logic of interactive problem solving. However, third-party members are also in a unique position to observe and facilitate the intensive interactions between the parties out of which new ideas emerge, and may have access to channels for disseminating these ideas (Chataway,

2002). Transfer is not a necessary function of the third party and, indeed, some practitioners have wondered whether writing and speaking about the substantive issues in a conflict is consistent with the third-party role. In my own case, my writings about the Israeli-Palestinian conflict over the years have drawn extensively on what I have learned from my observations of workshop interactions and my conversations with workshop participants. To my knowledge, participants have not had any problem with this aspect of my role—perhaps in part because my analysis tends to take the same evenhanded approach as my practice and, like my practice, is dedicated to the search for a negotiated agreement that addresses both sides' needs and fears, that is durable because both sides consider it fair and are committed to it, and that provides the institutional and psychological basis for a new—peaceful, cooperative, and mutually enhancing—relationship. I believe that my writings about the conflict and its resolution may actually contribute to my credibility, insofar as they demonstrate my familiarity with the environment and the issues of the conflict.

Problem-solving workshops, it must be stressed, are not negotiating sessions: They are entirely unofficial and non-binding. However, it is precisely their non-binding character that constitutes their special contribution. It allows the participants to interact in an open, exploratory way; to acquire new information and share their differing perspectives; and to gain insight into the other's—and indeed their own—needs, fears, concerns, priorities, and constraints and into the dynamics of the conflict relationship that leads to exacerbation, escalation, and perpetuation of the conflict (see Kelman, this issue). Although they are not negotiations, workshops can contribute to the negotiation process at all of its stages. At the *pre-negotiation stage*, they can help create an environment conducive to moving toward the negotiating table. At the *para-negotiation stage*, they may help create momentum and foster a sense of possibility, as well as identify new options and reframe issues. In periods marked by a *breakdown of negotiations*, they can help rebuild trust in the availability of a negotiating partner and a sense of possibility and hope. Finally, at the *post-negotiation stage*, they can contribute to implementation of the negotiated agreements, peace-building, and reconciliation.

ACTIVITIES IN THE PRE-NEGOTIATING PERIOD

Our earliest work, in the 1970s and 1980s, clearly corresponds to the pre-negotiation phase of the conflict. During that phase, the primary purpose of our efforts was to help create a political atmosphere that would encourage the parties to move to the negotiating table. Our workshops and

related activities contributed to the development of a sense of possibility, of new ideas for resolving the conflict, and of relationships among members of the political elites across the conflict lines. Our workshops during those years took a variety of forms, briefly described in the concluding article in this issue (Kelman, this issue).

In 1978, I published my first analysis of the Israeli–Palestinian conflict in an article entitled, “Israelis and Palestinians: Psychological Prerequisites for Mutual Acceptance” (Kelman, 1978a). This was the first of many articles and book chapters over the years that analyzed the conflict and the requirements for its resolution. These writings conceptualized the conflict as one that is perceived by the parties as a zero-sum conflict around national identity and national existence, and identified the conditions for mutual reassurance, acknowledgment, and recognition. A good part of my writing on this topic—not only in professional publications, but also in magazine articles and newspaper opinion pieces—can be described as a form of policy analysis from a social–psychological/conflict resolution perspective: analysis of recent developments in the conflict, the reasons behind them, and their policy implications.

One article that clearly had an impact on my work during the 1980s was my first report on my conversations with PLO Chairman Yasser Arafat (Kelman, 1982b). I first met the Chairman in 1980 in Beirut, through the help of the networking that I mentioned earlier. The meeting started around midnight and lasted about two hours. Only one other person was in the room—a senior PLO official whom I had met on my very first visit to Beirut and whom I later invited to speak at my Middle East Seminar at Harvard in the face of considerable criticism. He introduced me to Arafat and occasionally helped Arafat find the appropriate English word, but the session was mostly a conversation between Arafat and myself. I made it clear that I was not interviewing him; and, in fact, I took no notes (until I returned to my hotel room in the middle of the night)—although I did not promise that his remarks were “off the record,” nor did he ask me to do so. I actually had no intention of writing about the meeting; my purpose in meeting with him was (a) to gain a direct impression of his thinking and (b) to acquaint him with my work in the hope that he would give his approval to Palestinians who asked him or his office about participating in one of our workshops or other activities. (Most of the Palestinians who worked with us wanted to make sure that they would not be seen as presenting themselves as alternatives to the PLO in negotiating with the Israelis.)

I met with Arafat again in Beirut in late 1981, under similar circumstances. I might mention here that I periodically met with him in subsequent years—several times in Tunis, as well as in Algiers, in Amman, in Cambridge, Massachusetts (after the Oslo Accord), and in Ramallah

(shortly before his death in 2004). However, our conversations at the first two meetings, in Beirut, were the richest and most substantive. I had the strong sense from these conversations—along with what I knew about Arafat’s actions within the Palestinian movement—that he was open to negotiating a peace agreement with Israel. I based this conclusion not so much on *what* he said about his positions on the issues, but on *how* he said it: on his cognitive style and his image of the enemy, as these emerged in the course of our conversations. Having reached this conclusion, I was very concerned during the Lebanon war of the summer of 1982 that the chance for a negotiated peace would be seriously undermined if Arafat were to be killed or if the PLO under his leadership were to be marginalized—both of which seemed highly probable. I published an opinion piece in *The New York Times* on that theme in July of 1982 (Kelman, 1982a)—without mentioning my conversations with Arafat. In the fall, I published the article in *Foreign Policy* magazine (Kelman, 1982b) in which I described, in some detail, what I had learned from my conversations with Arafat and why I had come up with the strong hypothesis that he is open to negotiating a peace agreement with Israel. The editor chose the title, “Talk With Arafat,” for this article—a title that conveyed both the fact that the article reports on my conversations with Arafat and my main policy recommendation: test the hypothesis that I am presenting by talking with the man.

The article was heavily criticized in certain quarters in the United States, as well as in Israel—largely, as the editor of *Foreign Policy* put it to me, because it “gentrified Arafat,” treating him as a statesman who must be taken seriously, rather than as an eccentric who needs a shave and wears a rag on his head. As far as my conflict resolution work was concerned, however, I believe that—if anything—it had a positive impact, enhancing my credibility on both sides. Among Palestinians, my credibility rose because I showed a willingness to go public with an unpopular position in support of the Palestinian cause. Interestingly, I found that the article enhanced my credibility even among some of the anti-Arafat elements of the PLO, based in Damascus. The article also enhanced my credibility among those Israelis who were interested in exploring the possibilities for negotiations—in other words, candidates for problem-solving workshops—because it demonstrated that I was not just another American Jew who was trying to intervene, but that I had significant connections on the Palestinian side.

ACTIVITIES IN THE 1990S

The second period of our work, spanning the years 1990 to 1993, can be described as transition to a para-negotiation effort. The most distinctive