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*Reflections on the History and Status of Peace Research**

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Depending on our tastes and definitions, we can take quite divergent views on what constitutes the beginnings of peace research. The range of possible dates becomes narrower when we think of the beginnings of the peace research *movement*. I refer here to deliberate, organized efforts to mobilize the resources of different disciplines—especially, but not exclusively, the mathematical models and quantitative methods developed in the social sciences—for studying the conditions of peace and war. Even the beginnings of the peace research movement, however, cannot be dated precisely, since it is in the very nature of a movement that similar ideas and activities spring up almost simultaneously in different places and under different guises.

I like to think of the organization of the Research Exchange on the Prevention of War in 1952 as representing the beginnings of the peace research movement. I recognize, however, that I am not entirely disinterested in suggesting this date, since I was one of the prime movers in the founding of the Research Exchange. At the very least, there are some important forerunners to the Research Exchange that must be recalled. For one thing, the intellectual activities that anticipated the peace research movement are ultimately more significant than the organizational activities. From this point of view, the beginnings of the movement ought to be traced at least to the work of such pioneers as Quincy Wright, Lewis Richardson, and Pitirim Sorokin in the period between the two World Wars (cf. Singer, 1976). Furthermore, the idea of mobilizing the resources of the social sciences for the study of peace and war was promoted vigorously by Ted Lentz before the advent of the Research Exchange (cf. Lentz, 1955, for a later programmatic statement). There were even some organizational efforts that preceded the establishment of the Research Exchange. One of which I am aware is

the Committee on the Psychology of War and Peace, in which Ross Stagner and Ralph White, among others, were active. This committee was set up by the Society for the Psychological Study of Social Issues shortly before World War II, but became inactive after American entry into the war.

However we date the beginnings of the peace research movement, my own involvement in it goes back more than thirty years to the origins of the Research Exchange. I would like to comment briefly on that early period, both because I enjoy reminiscing about it (and the norms that govern Presidential Addresses allow such self-indulgence up to a point) and because it may help to highlight some of the features and issues of the peace research movement that have characterized it up to the present time. Admittedly, I am presenting the history of peace research from an idiosyncratic perspective.

The Research Exchange and the *Journal of Conflict Resolution*

In April 1951, Arthur Gladstone and I published a letter in the *American Psychologist,* in which we argued that some of the assumptions about human behavior that underlie foreign policy need to be evaluated in the light of psychological knowledge. We proposed that pacifist challenges to these assumptions seemed consistent with various psychological theories and findings and therefore merited the systematic attention of psychologists. This letter elicited a variety of responses, including suggestions for meetings that would pursue the idea further. Such meetings were held and led to the formation of a group to explore the possibilities of research on alternatives to war in resolving international conflicts. This group eventually became the Research Exchange on the Prevention of War. For several years, the *Bulletin of the Research Exchange on the Prevention of War* was published regularly. The group also organized workshops and ran symposia at various professional meetings, which in turn produced some publications. The participants in the activities of the Research Exchange were mostly (but not exclusively) young social scientists. The group was interdisciplinary, but its strongest representation came from the field of psychology, in which the whole enterprise had its origin.

Two historical notes are of special interest, because they are relevant to the subsequent development of the peace research movement:

(1) The choice of "prevention of war" rather than "peace" for the name of the organization can be attributed in large part to the political atmosphere that prevailed at the time. This was the height of the McCarthy era and the term "peace" was highly suspect and controversial. Even though those of us who started the Research Exchange were committed to peace, many of us having a background in pacifism or peace activism, and even though we were prepared to innovate in our professional work

and to take certain risks, we were nevertheless subject to the pervasive pressures and habits of those times. We used the less stigmatized term "prevention of war" in order not to alienate our colleagues any more than necessary. The atmosphere, of course, has changed drastically since 1952, but the term "peace" is still controversial in some circles. What is interesting today, however, is that the concept of peace reasearch is viewed with some suspicion, not only from the right, but also from the left.

(2) The impetus for this organization came almost entirely from individuals who were non-specialists in the field of international relations. Their motivation derived from their commitment to peace. It is not surprising that the group had a large concentration of pacifists, Quakers, and the like. Thus, the enterpise started with the handicap of a weak professional base and a lack of professional expertise and legitimacy. Most of us had not been trained in the field of international relations and were, in fact, not working directly in that field. Starting with such a weak base, the Research Exchange had difficulty in attracting participants anchored in the field of international relations itself. I became more and more convinced that we would have to become professionals if we were to make continuing progress. Harold Guetzkow served as an excellent model in this regard: Coming from a religious-pacifist background and trained in social psychology, he had started—a short time before the founding of the Research Exchange—a systematic process of retraining himself to the point where he eventually became recognized as a political scientist and international relations specialist.

After more than two years of activities, I became increasingly frustrated with the failure of the Research Exchange to attract IR professionals—and with my own failure to become one. I felt that there was a limit to how long one can go on writing programmatic articles and organizing meetings with the message that there are things one can and should do—without actually going out and doing them. It seemed to me that the Research Exchange had reached that limit and I was now concerned with where it should go from here.

Just at that time, fortuitously, I came to the Center for Advanced Study in the Behavioral Sciences at Stanford University, as a member of the initial group of Fellows (1954-55). While there, I convened a group of other interested Fellows to discuss the Research Exchange on the Prevention of War and to solicit their advice on the future course of this enterprise. The group happened to include, among others, Kenneth Boulding and Anatol Rapoport, who found the possibility of systematic contributions from social science to problems of war and peace highly congenial with their own peace commitments and evolving intellectual interests. Another major input into their thinking came from Stephen Richardson, who was also a Fellow at the Center at that time and who introduced them to the unpublished writings of his late father, Lewis Richardson.

Richardson's applications of mathematical models to the analysis of international conflict, which had hitherto appeared only in brief, scattered publications (see, for example, Richardson, 1950a and 1950b), struck a responsive chord in Boulding and Rapoport. They took an active part in arranging the posthumous publication of Richardson's books and in bringing his work to the attention of a wider audience. The work also reinforced their own growing conviction in the possibilities of "peace research."

The meetings we held at the Center led to a decision to replace the Research Exchange with two separate sets of activities. We felt that the organizational functions of the Research Exchange (i.e., arranging symposia, workshops, joint research efforts, and the like) could be carried out most effectively by a committee of the Society for the Psychological Study of Social Issues. As I have already mentioned (in describing some of the forerunners of the Research Exchange), shortly before the outbreak of World War II the Society for the Psychological Study of Social Issues had established a committee in this area, which soon became one of the casualties of the war. The decision to re-establish such a committee, to take the place of the Research Exchange, thus closed the historical circle. At the same time, we decided to transform the *Bulletin of the Research Exchange on the Prevention of War* into a more formal journal, which would combine some of the standard functions of professional journals (including full-scale theoretical and empirical articles) with some of the exchange functions of the *Bulletin*. The hope was that this new journal would serve as the focal point for the development of a new interdisciplinary enterprise. We decided to locate the new journal at the University of Michigan, since Kenneth Boulding was on the faculty there, Anatol Rapoport was about to join that faculty, and William Barth and Robert Hefner had been publishing the *Bulletin* there. Other interested members of the Michigan faculty, including Robert Angell and Daniel Katz, soon joined the planning group and the editorial board. In 1957, the journal began publication under the title *Journal of Conflict Resolution: A Quarterly for Research Related to War and Peace.* Although the term "peace" was used in the subtitle, it still did not appear in the main title itself. The term "conflict resolution" was chosen primarily because it best reflected the interests of the editorial board in the analysis of conflcit across different system levels, but in part also because it was less controversial (less blatantly normative) than the term "peace."

Publication of the *Journal of Conflict Resolution* represented an important turning point, in that it helped to move the field of peace research in a professional direction. The *Journal* attracted a number of IR specialists, including—among the earliest contributors—Richard Snyder, Karl Deutsch, and David Singer. The *Journal* also became an outlet for scholars outside of the IR field who were doing professional

work in the analysis of conflict, including some—best exemplified by Thomas Schelling—who approached the problem primarily from the perspective of strategic studies rather than peace research.

Specialists and Non-Specialists

I have been tracing one line of development during the 1950's which, along with a number of other lines, came together in the peace research movement. In describing the development of peace research during that period, I find it useful to distinguish two major strands (although these are by no means totally separable): the work of scholars who were trained outside of the field of IR and the work of IR specialists themselves.

In the first category, the major pioneer is probably Lewis Richardson and the major prophet Ted Lentz. I have already described the early organizational efforts among the "non-specialists." The *Journal of Conflict Resolution* served as the focal point for organizing the Center for Research on Conflict Resolution at the University of Michigan, which became a major American center of activity in peace research during the early 1960's. Other early centers—similarly started and led by non-specialists—were the Canadian Peace Research Institute and the Oslo Peace Research Institute, which inaugurated the *Journal of Peace Research* in 1964. A number of social scientists from different disciplines, located outside of these centers, also played important roles in the development of this research area. Among the names that come to mind in this connection are Lewis Coser, Morton Deutsch, Thomas Milburn, Raoul Naroll, and Charles Osgood.

In the second category, the major pioneer is Quincy Wright. The major centers of activity, started during the 1950's, were the program at Northwestern University (which, incidentally, brought the two strands together in the joint leadership of Richard Snyder and Harold Guetzkow), the Program on International Conflict and Integration at Stanford University, led by Robert North (in which I would also include Charles McClelland), and the national indicators group, led by Karl Deutsch at Yale and elsewhere. Again, various IR specialists outside of these centers—such as James Rosenau—played major roles in the development of this strand of peace research.

I have identified these two strands in the development of peace research because I want to argue that the emergence of peace research as a substantial discipline in the late 1950's and early 1960's can be credited, to a very large extent, to the coming together of two lines of interest: (a) an interest among scholars outside of the IR field (many of whom were Quakers or pacifists—including Lewis Richardson, Kenneth Boulding, Harold Guetzkow, Walter Isard, Arthur Gladstone, Dean Pruitt, and myself—or else world federalists, peace activists, or what Ted Lentz used to call "peacifists") in using their professional skills for the promotion

of peace; and (b) an interest among the new generation of IR specialists in developing quantitative, mathematical, and behaviorial approaches to the study of their discipline and linking it to general social science. These two groups developed an almost symbiotic relationship to one another. They formed a coalition, which provided reciprocal stimulation and reciprocal legitimization. The non-specialists needed the specialists in order to legitimize their forays into areas in which they had not been trained (a problem, as I mentioned, that we had from the beginning in the Research Exchange), in order to fill in the substantive knowledge that they themselves lacked, and in order to provide reality testing for their conceptual models. The IR specialists, on the other hand, needed their non-specialist colleagues (who were, of course, specialists in the various disciplines on which this new breed of IR scholars was drawing) as sources not only of concepts and methods, but also of the validation and encouragement that they were unlikely to receive from their more traditional IR colleagues.

It would of course be a mistake to draw the lines too sharply. The "non-specialists" were not only starry-eyed searchers for peace. They had strong scholarly interests, were intellectually intrigued by the problems to which they had recently turned their attention, and were able to draw on an extensive reservoir of knowledge and skills in their own disciplines. Had they lacked these characteristics, they would have been of no use to the IR specialists in this evolving coalition. The IR specialists who became part of this movement, in turn, were often people who also had strong commitments to peace (which may, in fact, have led some of them to enter the field in the first place). Had they lacked such commitments, they would not have been comfortable in this evolving coalition. My main point, then, is not so much that the peace research movement represents the coming together of two separate groups of scholars (although, roughly speaking, this may be true), but that it represents the coming together of two sets of interests—an interest in applying scientific knowledge to the promotion of peace, and an interest in developing a scientific base for the field of international relations. It is this unique confluence of interests, I propose, that accounts for the take-off of the peace research movement and for its continuing vitality.

Tensions in the Peace Research Movement

The confluence of interests that is represented in the peace research movement helps to explain some of the tensions that have characterized the movement, both internally and in its relations with the outside world. In its external relations, we find two interesting anomalies in the reactions that peace research arouses:

(1) Among certain traditional academics, peace research is somewhat suspect and subject to criticism simultaneously for two

reasons: both for being too normative and for being too "scientific," i.e., quantitative and behavioral. Both of these reactions are based on certain correct observations, but their joint occurrence appears anomalous because the behavioral/quantitative approach represents, at least in part, an effort to overcome some of the limitations of traditional normative political science. The situation testifies to the speed with which the field has moved into a "post-behavioral" phase, in which "scientific" is not equated with "positivistic," and in which normative commitment is not seen as incompatible with scientific objectivity. Many traditionalists in political science and international relations have not yet come to terms with the behavioral revolution at a time when the post-behavioral revolution is already well under way. Peace research, with its combination of peace commitment and a behavioral/quantitative approach must be both confusing and threatening to such traditionalists.

(2) Peace research is also suspect and subject to criticism simultaneously for being too radical and for being too supportive of the status quo. Again, there is some objective basis for both of these observations. Peace research does imply a preference for a different kind of world order and is guided by an image of such a world order, rather than by considerations of how best to maintain or enhance the power position of a particular government. This is hardly radical, but it is based on a different set of assumptions than those underlying much of traditional IR research. The suspicion from the right, aroused by the adoption of a different set of assumptions, is a familiar phenomenon. It was even stronger in the 1950's, though it is still alive today to a surprising degree. The suspicion from the left is a newer phenomenon. Though the criticism may often be wrong-headed, I believe that there is some real substance to it, as I shall elaborate below. At least in the case of asymmetrical conflicts, and *exclusive* emphasis on peace—on conflict reduction or resolution—does serve the interests of the status quo at the expense of social change and increased justice. While I do not regard such an orientation as inherent in peace research, I take seriously the possibility that the research—depending on its precise nature, on the perspective it takes, and on the way it defines the problem—may at times help to perpetuate an unjust status quo.

This leads me to some of the internal tensions within the peace research movement, which again can be understood in the context of the unique coalition that makes up the movement:

(1) The radical critique of peace research represents one of the issues over which there is disagreement within the peace research community (cf. Singer, 1976). On the one hand, there are those who see peace research as a commitment to social change and the achievement of social justice. This position may be—but is not necessarily—linked to a Marxist or neo-Marxist analysis. For some (e.g., Schmid, 1968), such an analysis has led to a rejection of peace research in its entirety as an inherently

counterrevolutionary enterprise. Others have, instead, called for an expansion of peace research to a concern with the reduction of structural violence as well as personal violence (Galtung, 1969). On the other hand, there are those who—although they can hardly be called defenders of the status quo—prefer to limit the focus of peace research to issues of survival. Karl Deutsch, for example, defines peace research as the effort "to identify the conditions which make large scale war less probable" (1972, p. 8). These two approaches—and the variations within each category—differ in how they define peace, what they see as the task of peace research, and what they designate as the targets of action and application.

(2) A second line of division within the peace research community, more or less orthogonal to the first, is along the dimension of basic versus applied research. On the one hand, there are those who are primarily oriented to long-range research and to building a scientific base for the field. They are, of course, committed to application, which is built into the very definition of the field. They would argue, however, that theoretically oriented research represents the best contribution to peace in the long run, in line with Kurt Lewin's often quoted statement that "there is nothing so practical as a good theory" (1951, p. 169).[1] On the other hand, there are those who are primarily oriented to action, calling for research that has immediate applicability to current policy or action programs. These two views are not necessarily incompatible and the same investigator can be interested in both types of research (as I like to believe I am). Nevertheless, these two views reflect differences in emphasis, in priorities, and in level of patience or impatience, which constitute one of the continuing sources of tension that characterize the field.

(3) A third type of division is based on different views of the research effort itself. On the one hand, there are those who take a relatively narrow view of the field, preferring to restrict the term "peace research" (and, certainly, the term "peace science") to work based on mathematical models and quantitative methods. On the other hand, there are those who favor a broader conception, including under the purview of peace research historical studies, humanistic approaches, and various forms of action research.

In my view, the tensions that I have described—as reflected both in our relationship to various colleagues and critics, and in internal divisions within the peace research community itself—represent a healthy state of affairs. The mix of different disciplines, approaches, and goals and the debates between rigorous model builders and political activistis, between Marxists and pacifists, between minimalists and maximalists, between scientists and humanists, are all elements of strength, vitality, and creativity for the peace research community. At the same time, we want to build an enterprise that has some continuity and intellectual

coherence, to develop approaches and methods that are sound and effective, and to create a body of knowledge that is theoretically grounded and potentially applicable. To this end, we must periodically—and certainly at this relatively early stage in our development—review the assumptions on which we operate and define the boundaries of the field. The remainder of these reflections will be devoted to a modest attempt to contribute to this task.

The Normative Commitments of Peace Research

Anatol Rapoport (1972) stated two fundamental assumptions that he believed to be "accepted by everyone concerned with peace research... (1) peace research must include more than the conventional conceptions of international relations; (2) peace research should be conceived as an applied science with the goal of preventing wars (that is, the preservation of 'negative peace') as a minimum and, more comprehensively, with the goal of promoting 'positive peace,' that is, the unification of mankind into a cooperative enterprise on a world scale" (p. 92). I find this statement of the goals of peace research entirely congenial with my own view. I might just amplify his definition of "negative peace" by equating it with the absence of systematic, large-scale, collective violence, accompanied by a sense of security that such violence is improbable. This sense of security is greatly enhanced by Rapoport's vision of "positive peace," which I would equate with the existence of some kind of world order concerned with meeting the needs and interests of the world population.

As an applied science, peace research explicitly starts out with certain normative commitments. Research and analysis are geared to establishing the conditions for avoiding war and promoting peace. Moreover, the frame of reference within which peace research is carried out is worldwide: It is concerned with peace as a global condition rather than merely as a national policy. This normative commitment may appear trivial since, allegedly, everyone is for peace. In fact, however, much of the work in international relations—especially in foreign policy analysis or strategic studies—does not address itself to the conditions for worldwide peace as an end in itself. To be sure, the investigators would generally prefer peace to war, but these are not the terms in which they formulate the research problem. The emphasis in the area of foreign policy analysis has typically been on establishing the conditions that would advance national interests (or bloc interests) and maximize national power within the international sphere. In this approach, peace is an important good only insofar as it serves the national interest. Even when the concern with peace is global, it is based on the national interest in stability rather than on a universal interest in preserving human life and dignity.

It is important to point out that this normative commitment underlying peace research is not to be confused with a particular

ideological line. Beyond the shared commitment to peace as a goal, peace researchers do not subscribe to a particular position about the causes of war or to a particular program for achieving peace. Indeed, they would want to subject such positions and programs to critical analysis. Nor do they agree on the conditions under which they would consider violence justifiable or on the priority they would assign to peace when it conflicts with other values. Basically, then, peace research is normatively oriented in the same limited sense that medical research is normatively oriented, in that it starts with a commitment to preserving life and health. I actually believe that the medical model has some serious weaknesses—for health research as well as for peace research—but let me just adhere to it temporarily as a minimal model for our purposes.

It should be clear from the comparison with medical research that this kind of normative commitment is entirely compatible with scientific rigor and systematic scholarship. In fact, I would argue that peace research differs from other kinds of research in IR, not in the fact that it is based on normative commitments, but in the fact that it is based on a *different* set of normative commitments and that it makes these *explicit*. Work in IR—and in the social sciences generally—almost always reflects a particular normative bias. The typical work in foreign policy analysis, to which I have already alluded, clearly has such a bias—which may or may not be explicitly recognized by the researchers themselves. Making the value assumptions explicit makes it possible to subject the assumptions themselves to critical analysis and to explore the implications of different value preferences and priorities for policy and action.

To say that we are committed to the value of peace does not mean that we posit peace as the *only* value or even necessarily as the *highest* value under all circumstances—just as medical scientists do not necessarily view life or health as the overarching value. It could be argued that peace—at least in our times—is indeed the highest value, because all other values depend on human survival (cf. Deutsch, 1972). Yet, people continue to be prepared to sacrifice peace for the sake of values they regard as more compelling—such as maintaining national security, independence, and integrity, or achieving freedom and social justice. I am still enough of a pacifist to look with skepticism at claims that war or violence will promote national security or social justice. But I recognize that such claims—in a less than perfect world—are often tenable on moral as well as empirical grounds. I would not be prepared to advise peoples who feel that their national security or their liberation is at stake to disarm unilaterally, much as I am convinced that there is a better way than war. Certainly, it would be inappropriate for the peace research discipline to take a position in favor of peace as the supreme value. This would represent too narrow an ideological commitment on which to base the discipline and would beg some of the important questions which we should be addressing in our research.

Negative and Positive Peace

Having circumscribed the normative commitment shared by peace researchers, I would argue that the commitment to peace—even in the narrower sense of avoidance or minimization of personal violence—is a worthy goal, about which peace researchers need not be defensive or apologetic. My argument is based on the following considerations:

(1) Granting that peace is not necessarily the *highest* value at all times, we are still right in insisting that the preservation of human life and the avoidance of violence and destruction are *extremely high* values, which deserve central consideration in the calculations of national policy or of revolutionary strategy. Even if one is prepared to sacrifice these values at times, such actions should be viewed as unfortunate last resorts. Thus, identifying the conditions that will make it less necessary to resort to violence in the pursuit of higher values is a significant contribution that can be made by peace research.

(2) Even when it is decided to pursue policies and strategies that make violence or war an acceptable option (in the pursuit of higher values), it is important to find ways of reducing the probability, the intensity, and the destructiveness (in terms of human values) of that violence.

(3) Even if we grant that there may be justification at times in sacrificing peace for the sake of national survival or social justice, it is important to ask whether violence is in fact likely to contribute to the achievement of these values. What is often ignored in the calculus is the extent to which war and violence actually *reduce* security or social justice. We must keep in mind, for example, that it is usually the children and the aged, the poor and the socially marginal, who pay the heaviest price of warfare and revolutionary violence.

My point, then, is that we should not allow the slogans of Realpolitik or revolutionary struggle to make us defensive about the search for the conditions of peace—even negative peace, in the sense of avoiding and reducing violence. This remains a high-priority goal even when we concede that it may be outweighed by other values. Of course, a major challenge to peace research is to find ways of achieving some of these competing values without resort to violence—such ways as civilian defense, nonviolent action for social change, and nonmilitary approaches to the conduct and resolution of conflicts.

In stressing that negative peace is not a negligible value, I am disagreeing with the definitions of peace to which some of the radical critics of peace research have subscribed. I consider it unhelpful to include various other values, such as social justice, in the definition of peace. Though my own definition (as I have already shown) is broad, and though I believe (as I will show below) that the study of peace is closely linked to economic justice and human rights, I consider it confus-

ing to limit the term "peace" only to a state of affairs that simultaneously maximizes justice (or, for that matter, national honor). Some kinds of peace may be deemed undesirable because of the cost in other values that they entail, but they may still be peace. To gloss over the difference has the unfortunate effect of ignoring the avoidance of violence as a significant value in its own right.

Far more objectionable than the overinclusive definition of peace is a definition stating, in essence, that some kinds of war—such as wars of liberation—are really manifestations of peace. Such definitions are uncomfortably reminiscent of the use of words like "pacification" to describe the destruction of Vietnamese villages and the uprooting of their populations. This kind of abuse of language is no more acceptable when perpetrated by the left than it is when perpetrated by the right. It is one thing to argue that violence is justified under certain circumstances, but quite another to describe such violence as peace. To do so is to sweep the costs of the violence under the rug and, in the process, to deprive the concept of peace of its meaning.

Now that I have affirmed the importance of negative peace and taken issue with the critics who minimize it, I want to turn around and argue that there is great merit in the criticism of a view of peace research that *restricts* its focus to negative peace. I am not referring here to specific research projects, which may legitimately restrict their focus. Rather, I am referring to definitions of the task for the field as a whole, which take the view that positive peace and social justice are unwarranted extensions of the agenda for peace research. Adam Curle (1976) provides an excellent, non-doctrinaire statement of his reasons for concluding that an approach that focuses exclusively on the conditions for ending hostilities is too narrow for peace research:

> ...even if wars are brought to an end, many of the conditions associated with war continue throughout large areas of the world: people are driven from their homes, unjustly imprisoned, separated from their families, flung into detention camps, virtually enslaved, exploited by landlords, victimised by the police, oppressed by the government, starved and malnourished because of official neglect or official policies; they are humiliated and have their perceptions distorted by propaganda; many, in fact, die because of these conditions. Circumstances such as these inflict such damage on human life, health, capacity for creative and happy existence and work, and for the development of potential, that I find it impossible to refer to them as peaceful: they inflict upon human beings, though in a less direct and concentrated form, many of the same destructive horrors as does war (p. 10).

Curle then goes on to draw the following conclusions:

> First, the study of peace should not be confined to the analysis of means of preventing or terminating wars. Secondly, because many of

these circumstances were intranational rather than international, the study of peace should not be considered as exclusively on the international level. Thirdly, and for the same reason, I came to believe that support for a *status quo* which permitted or encouraged such unpeaceful conditions could in no sense be considered as the promotion of peace: on the contrary, it was the tacit condoning of violence (p. 10).

I would propose that definitions of peace research as restricted to a study of the avoidance of war can be criticized on several grounds. Theoretically, such definitions pay insufficient attention to the systemic processes of international relations. Ethically, they bypass an analysis that considers interconnected and competing values. Ideologically, they lend themselves too readily to a status quo bias.

A Public Health Model for Peace Research

I am now ready to return to the medical model, which I adopted temporarily in order to clarify the nature of the normative commitments underlying peace research, but which I regard as flawed. The flaws of the medical model are shared by a model of peace research that is restricted to studying the avoidance of war.

Without wishing to downgrade the value of medical research on specific disease entities, I feel that the standard, disease-oriented model is inadequate as a basis for policy-related research. It does not have an image of health against which social policy can be evaluated. It does not consider conflicting values or conflicting interests within the society. It does not recognize systemic processes and interdependencies between different sectors and groups within the social system. It does not provide a basis for assessing long-range consequences of different policies, or for creating the conditions conducive to health on a long-term, society-wide basis. In short, it ignores the socioeconomic and sociopolitical context of health. What is needed, to correct for these flaws, is something more akin to a public health model for policy-relevant research in the medical field.

It is precisely such a public health model, rather than the standard model of medical research, that I would propose for our field. Such a model focuses not only on the absence of war, but also on the nature of a peaceful world. Let me propose a set of assumptions on which such a model would be based:

(1) In view of current realities and necessities, the world must be conceived as a global society, analyzed as a total system with multiple actors. Though nation states are primary actors within that system, the actions and interactions of a wide variety of actors—individuals, groups, and organizations (both intergovernmental and nongovernmental)—at the subnational and transnational level must be considered in the analysis. Such a global perspective would place emphasis on the increas-

ing interdependence that marks the international system and on the role of transnational activities, organizations, and institutions (cf. Kelman, 1968 and 1977).

(2) One of the consequences of the interpendence, the cross-cutting interests, and the transnational communication patterns characterizing the global system is the increasing continuity between domestic and foreign policy. Domestic conflicts often have international ramifications in many parts of the world and international conflicts often become domestic issues in countries that are not directly involved. Peace research thus must take into account the contributions of a variety of actors and issues that have traditionally been outside of the domain of the foreign-policy analyst.

(3) Establishment and maintenance of peace presupposes attention to the long-term, systemic conditions conducive to war. These include the nature of modern weapons and the accelerated competition in arms; the problems of scarce resources, economic exploitation, and the gap between the rich and the poor; and the continuing and often growing practice, in many parts of the world, of oppression and exclusion of certain segments of the population and systematic violation of their human rights. Changes in these conditions may remove some of the major sources of instability in the global system and thus reduce the opportunities, capacities, and motivations for waging war. Concern with these systemic causes of war makes the conditions and processes of social change an integral component of peace research.

(4) Peace is not merely the absence of war, but the maintenance of a state of affairs that can be defined in positive terms. Thus, one cannot very well describe as peaceful a world that is constantly on the brink of war and in which war is avoided only by the threat of nuclear annihilation or by the violent repression of discontented elements. Positive peace does not imply an ideal, utopian situation, but merely a livable one—a world in which peace is probable, so that individuals and groups everywhere can have a sense of security about their survival and trust that their basic needs will be met and their basic rights protected (cf. Deutsch, 1972). A major indicator of postive peace is the existence of a pattern of cooperative relationships in the global system and within its subsystems.

(5) Peace does not imply the absence of conflict. Some degree of conflict is an inevitable and often desirable process in any social system. Conflict may be a vehicle for enhancing social justice and introducing needed social change. The problem is not to avoid conflict, but to prevent it from turning into mass destruction. Thus, a major concern for peace research is the development of two kinds of institutionalized mechanisms: (a) mechanisms for conducting conflict by nonviolent means, and (b) mechanisms for managing and resolving conflict (cf. Kelman, 1979).

(6) The establishment of a peaceful and just world order requires a readiness to question current assumptions about the international system and to consider them as only one set of assumptions among various possible ones. Thus, one characteristic of peace research is its future orientation and its concern with planning for the future. Such planning involves not only projecting current trends into the future and exploring their implications, but also actively creating alternative images of the future—including alternative institutional arrangements, belief systems, and patterns of intersocietal interaction—and evaluating their desirability and feasibility.

The view of peace research that derives from these assumptions assigns an important place to the issues of economic justice and human rights on the agenda of peace research. The relevance of these issues to peace research is particularly apparent when we consider asymmetrical conflicts and national liberation struggles, when we conceptualize the world as an interdependent global system, and when we expose policy alternatives to an ethical analysis based on assessment of their consequences for competing values.

I prefer not to include justice (which I shall use as an abbreviation for economic justice and human rights) in the *definition* of peace, because I do consider avoidance of war as a high-order value in its own right. But in my view of peace research, as expressed in the six assumptions I described, peace is intimately linked to considerations of justice. Justice has a strong bearing on the feasibility of peace, on the stability of peace, on the universality of peace, and on the quality of peace.

Justice has a bearing on the feasibility of peace because the peaceful resolution of many conflicts depends on the removal of the injustices that lie at the root of the conflict or at least exacerbate it. The only alternative to a just solution is often the violent suppression of one or both of the parties.

Justice has a bearing on the stability of peace because it is one of the conditions for maintaining a peaceful relationship over the long run. Systematic injustice breeds war; it creates a climate in which outbreaks of war become more probable.

Justice has a bearing on the universality of peace in that failure to address questions of justice may be tantamount to continuing a state of war at an "acceptable" level. Major powers may maintain peace among themselves by ignoring and condoning unsettled conflicts in peripheral parts of the world, which lead to continuation of violence and of the consequences of earlier violence (for example, in the form of political refugees or detention camps).

Finally, justice has a bearing on the quality of peace, because it defines the moral character of the peace that has been achieved with the cessation of overt violence and the human costs of maintaining that peace. It determines whether it is a peace maintained through repression

by the powerful of the powerless, forcing them to accept continuing deprivation, inequity, exploitation, and violation of their human rights, or whether it is a peace maintained by the existence of a world order that provides security, dignity, and satisfaction of their basic needs to the world's population.

• This paper is based on my Presidential Address to the Peace Science Society, International, presented on November 16, 1976, in Ann Arbor, Michigan.

Footnotes

[1] The full quotation from Lewin (1951, p. 169) 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."

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