On Cursing the Darkness versus Lighting a Candle

Morton Deutsch

Teachers College, Columbia University

Let me state my view of the paper, "Resolution vs. Resolution?" directly and bluntly: despite my sympathy with some of the aims of its authors, I think it represents a failure typical of some who like to think of themselves as "radical social psychologists." The failure is in offering no new ideas, no new insights, no new programs, and in resorting to exhortation for others to go out and find the promised land. In the guise of being a radical critique, there is an invocation of the holy term "revolution" and the sacred groups of "Blacks," "Youth," and "Women" in order to sanctify pronouncements about the social psychology of conflict. Despite its appeal to current ideological virtues, I believe there is very little that is truly radical about a paper which is content to present a caricature rather than an accurate portrait of the field it is assessing and which proposes
nothing specific about how social psychologists could foster, through
their scientific work, the values it favors.\footnote{The paper is particularly irritating to me because of its distorted pre-
sentation of my views. Without exception, all of the references to my work
on conflict attribute views to me which do not accurately represent what is
stated in the work to which they refer. Sometimes the distortion represents
a selective presentation out of its context, other times it reflects a
straw man of their own creation. For example, on page 4 of their paper,
they state that I (along with Rapoport and Schelling) assume that the original
paradigm (of the Cold War) provides a model general enough to deal with
all levels of conflict. There is nothing in the reference or in any of my
writing which indicates that I believe that the Cold War provides a general
model of conflict. In fact, the opposite is true. Even though Rapoport,
Schelling, and I have rather different approaches to the conceptualization of
conflict, the one thing that we would all agree upon (now or in the past) is
that the Cold War did not provide a general model of conflict. In their
discussion of "The Cure Approach," they refer to my paper on "Socially
Relevant Science: Reflections on some studies of interpersonal conflict."
They manage to present it as though it had a message opposite to what was
explicitly communicated by the paper. The theme of the paper was that
theoretical research, which had no immediate social relevance, was likely to
be of greater social utility than research which was narrowly focussed on
immediate social concerns. In the paper, I contrasted two early studies of mine,
an applied study of interracial housing and a theoretical study of the effects
of cooperation and competition upon group process. In my discussion of the
theoretical study, I made the point that although my interest had started
with a concern about nuclear war, of necessity, in coming to grips with the
theoretical issues upon which I focussed, the inquiry became transformed and
was no longer directly relevant to my initial interest even as, indirectly,
it became relevant to a wider spectrum of scientific as well as social
concerns.}}
may be little or much of value in past work but without some detailed, systematic theoretical thinking on their part, how will they really know? In their paper, Lubek and Apfelbaum are concerned with "invisible groups." How would they know, for example, whether research on "trust and suspicion" (using the PD game) is relevant to "invisible groups" unless they did some thinking about the conditions affecting group formation? I have given considerable thought to the conditions of group formation (Deutsch, 1973) and believe that I can make the case for "trust" being one of the centrally relevant factors. As a result of not doing the necessary detailed thinking and possibly, also, as a result of too much concern with being ideologically fashionable, the radical social psychologist may be blind to the insights he or she might obtain from work that it is stylish for them to denigrate.

As I turn now to a more specific response to some of the points in their paper, I do not want to be in the position of defending all of the research that has been placed under the rubric of "the social psychology of conflict." Much of the work, as is true in many areas of the social and natural sciences, is mindless. We agree that too much work has involved grinding out studies employing a modish research paradigm (such as the PD game) without any significant ideas underlying these studies. However, I take exception to many of the other points they make. Let me go down the list one by one:

(1.) Their first point appears to be that social psychologists have accepted the definition of conflict that society handed to them and the
problems that society has given them to study, and in so doing they are functioning more like technologists than like theoretical scientists. Presumably, as a result of doing this, the social psychologist has an unduly narrow conception of the nature of conflict defining it as a "combination of Cold War and Cure."

By a somewhat confused use of words, Apfelbaum and Lubek create the image of social psychologists being hired by society to work on its problem of conflict and that their approach was defined and limited by those hiring them. Of course, they know better than this and back away from this formulation and recast it as one point in terms of "pragmatic questions posed for the most part by the American social milieu" which defined the direction for conflict research. No one will deny that the American social milieu and international events played an important part in influencing American social psychologists as they thought about conflict. One would hope that their ideas would have some connection with the realities of their experience. But Apfelbaum and Lubek seem to have the misconception that the American experience of conflict is rather limited--confined to the experience of the Cold War. I can assure them that the American experience of conflict is very rich and diverse; perhaps too rich and full to digest easily. Most Americans have an almost infinite variety of images of conflict to draw upon--the Revolutionary War, slavery, the Civil War, cowboys and Indians, ethnic conflicts, religious conflicts, racial conflicts, industrial strife, sports, political conflict, and all
the diverse conflicts that one experiences in one's daily life as a member of a family, as a job holder, as a tenant, etc.

Yes, American social psychologists drew upon their experiences in their social milieu as raw material to stimulate their thinking about conflict. But it is a rich, diverse experience not limited or even dominated by the Cold War. And even when social psychologists such as myself joined in the debate about issues relating to the Cold War, it was largely to show that the justifications being offered for national policy were based on conflict theories that had little empirical support. Some policy-makers in the United States were casting the Cold War into badly conceived images of interpersonal conflict. Some of us felt that these faulty conceptions of interpersonal conflict underlying hawkish conceptions of international conflict should not go unchallenged.

Apfelbaum and Lubek suggest that social psychologists acted more in the role of consultants and engineers rather than as pure scientists in relation to problems of conflict. I wish it had been so. My own role as a consultant, for example, has been largely confined to working with groups outside the establishment—with groups trying to improve the situation of blacks and with peace groups trying to change national policy with respect to arms and also the Vietnam War. My most sustained consulting work with problems of conflict has been as a practising psychoanalyst dealing with intrapsychic conflict, marital conflict, and family conflict. Research-oriented social psychologists such as myself have had, at most, a very limited consulting role in relation to the
important conflicts of our time. Limited as my experience has been, it is evident that their notion of what happens in such a relationship is vastly oversimplified. Rarely does the "client system's" conception of its problem remain unchanged during a consulting relationship and rarely does the consultant find that his own conceptual framework is adequate enough to deal with the details of actual, on-going conflicts. Even if we had been "consulted" by "society" in relation to its problems of conflict, it is absurd to think that there would not have been a rather thorough consideration of the way the problem had been conventionally defined.

(2.) Apfelbaum and Lubek express the view that "game theory" has played an important role in the social psychology of conflict. I take it that they do not mean "mathematical game theory" (which has had very little influence)\(^2\) but rather the game-situations which have been suggested by game-theorists (the PD game, the Battle of the Sexes, the game of chicken, etc.) that have been widely used in psychological experiments. No one could deny that the game-matrices have limitations as tools for studying conflict as do all particular research settings. Nor could one deny that it would be absurd for social psychologists to limit their study of conflict to the

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2. Let me digress about "game theory" as it influenced my work. The prestige of "game theory" was being employed by some political scientists—e.g., Herman Kahn, Albert Wohlstetter, T. Schelling—for questionable policy recommendations. My work, in fact, was directed at demonstrating the irrelevance of game theory and at showing that incorrect interpretations would be made if the assumptions underlying the applications of game theory were taken to be valid characterizations of social realities.
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game-matrix format or to continue to give it the undue emphasis it has received. Yet, having said this, none of their specific statements about the limitations of game theory nor game-formats are otherwise correct. I do not have the desire to detail all their misstatements here but there is nothing inherent in game-theory or the game-format that implies the protagonists are similar, that their values are identical, that the conflict is momentary and transient, that there is always an "optimal" solution. Nor is there anything inherent in game-theory or games which limits them to two-party interaction. I would have supposed that they had heard of coalition theory and the research on coalitions and multi-person games. Moreover, they appear to misunderstand completely that the maximizing gain principle is purely tautological; it is a definitional rather than empirical statement. Of course, many experiments using games conform to their description, but many do not.

If Apfelbaum and Lubek are making the rather obvious point that it is not feasible to study directly large scale social conflict, with many participants, extending over long periods of time, in the laboratory who could disagree? If they are making the point that games do not simulate such conflicts, I would certainly agree. Despite the misconceptions of some influential textbook writers (e.g., Jones & Gerard), "games" or laboratory experiments of any kind in social psychology are not miniaturized models or simulations of international or other naturally-occurring conflicts. The only relevance that laboratory conflicts have to naturally-occurring
conflicts (or, for that matter, to other laboratory induced conflicts)—whether such conflicts be at the interpersonal, intergroup, or international levels—is through the possibility of applying a similar conceptual analysis to the laboratory and non-laboratory conflicts. The understanding of any specific conflict obviously requires more than knowledge of relevant concepts and propositions; it also entails knowledge of how these concepts and propositions are embodied in the specific historical, economic, and political events occurring in the given conflict. Thus, the relevance of laboratory studies of conflict to large scale social conflicts is not because they are "models" of these conflicts but rather because they permit an investigation of concepts and propositions which bear upon the naturally-occurring large scale conflicts as well as the artificially-induced conflicts of the laboratory. Clearly, we would all agree that if a laboratory study is lacking in ideas or is methodologically unsound it will have no relevance to anything.

(3.) Just as Apfelbaum and Lubek have not adequately portrayed the bride (who is falsely alleged to be a relative of game theory), they caricature the groom in their imaginary marriage. In the initial version of their paper, their caricature focussed on the term "conflict resolution": they interpreted this term to mean that social psychologists were primarily interested in preventing or eliminating conflict and suggested that this orientation reflected a bias toward preserving the status-quo. They now suggest that the bias toward the status-quo arises from a fixation on
the 1950's concept of conflict originating from the Cold War which precludes an awareness of "domination-recognition" conflicts. The earlier version was based upon a gross misinterpretation of the way the term "conflict resolution" had been employed in social psychology; the present one is based upon a somewhat simplistic and grandiose view of social psychological approaches to conflict. The simplism inheres in the assumption that there was a single conception or approach to the Cold War among social psychologists (and among social scientists generally). Anyone who is reasonably well acquainted with the writings of social scientists related to the Cold War should be able to recognize profound differences among their conceptions of its origin, nature, effects, and "cure." Not only was there no single, overriding conception of the Cold War among social scientists, I am confident that those social scientists who had, in fact, made a conceptual analysis of the Cold War would consider the conception presented in the paper by Apfelbaum and Lubek to be a caricature merely serving polemical purposes and not an accurate portrait of any intellectual view of the Cold War which was seriously advanced.

The grandiosity lies in their implicit view of what the social psychology of conflict is or should be. They seem to assume that the experimental studies of conflict were models of the Cold War and its cure; they go on to suggest that since the Cold War is not the only important type of conflict and since it is almost over, we should now model "domination-recognition" conflicts in our research studies. It is grandiose to suggest that the research on the social psychology of conflict ever modeled the
Cold War or that it could now model "domination-recognition" conflicts. As I have indicated in a preceding section, laboratory experiments in social psychology (of whatever kind) are not properly conceived of as simulations or models of naturally-occurring conflicts; they are tools for the investigation and the development of concepts and propositions which bear upon naturally-occurring conflicts. The tools and techniques of social psychological investigation, the concepts and propositions of social psychological theory are by themselves not sufficient to characterize adequately large scale social conflicts such as the Cold War or the "domination-recognition" conflicts which Apfelbaum and Lubek mention. An adequate characterization of such conflicts require the diverse methodologies and conceptual apparatuses of the many different social science disciplines. It is grandiose to assume that social psychology, per se, can provide the theoretical analysis or "cure" to war, poverty, racism, sexism, exploitation, crime, or any social problem. We have our role to play in helping to bring about an understanding of complex social problems, but it is only a small role in a complex drama with many actors. Recognition of the inherent limits of our part would not reduce the gap between what is required to solve urgent social problems and what we can do even at our best, but it might alleviate some of the ill-humored frustration associated with unrealistic aspirations.

Although the role of social psychology is small, its part will be inadequately played unless it uses its research tools more thoughtfully. I agree with Apfelbaum and Lubek that too much research in social psychology
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uses pseudo-variables and that insufficient theoretical work underlies many research investigations. Although I agree with their accusation, I object to their paper being so blatantly guilty of the charge which they make. They make no theoretical analysis of their own nor do they attempt to give a serious critique of the theoretical ideas of others, such as myself, whose work has always been concerned with ideas that go beyond particular experimental formats—ineffective and inadequate as these ideas may be. What they have done instead is, in effect, to attack the ideas without dealing with them directly. They have done this by proclaiming, rather than demonstrating, that the ideas are guilty of association with "games," with the term "conflict resolution," and with an interest in preventing nuclear war. This is not theoretical analysis, it is criticism by innuendo. I plead guilty to the association but not to the implications which they draw from these associations.

(4.) Apfelbaum and Lubek make the charge that social psychologists have concealed from investigation certain types of conflicts which are inconvenient for the maintenance of a certain status-quo. The charge is utter nonsense. If they examine the Journal of Conflict Resolution and the Journal of Social Issues, as well as other social science journals directly concerned with social issues, they will find many research studies and theoretical essays dealing with the issues and problems that they mention. They seem to have the peculiar notion that the work dealing with these issues and problems should always be labelled "conflict;" otherwise,
it will be difficult to grasp the links among work labelled as "conflict," "violence," "collective movements," etc. It is precisely because they refuse to see the links and seem intent upon staying at the level of superficial labels, rather than undertaking a theoretical analysis, that they think nothing of relevance has been done by social psychologists (and presumably other social scientists) in relation to the questions they have raised about "invisible" groups.

The questions they raise are clearly important ones but they are hardly new. A substantial portion of my Lewin Memorial Address in 1968 (Deutsch, 1969) was addressed to these questions and many others have raised similar ones. Some of us have even begun the attempt to answer such questions and many others have raised similar ones. Some of us have even begun the attempt to answer such questions with theoretical analysis and empirical work. In so doing, it is clear that work on such topics as "level of aspiration," "trust," "group formation," "group cohesion," "communication structure," "equity," "bargaining tactics," "power," and "justice" are clearly relevant to these issues. I wonder why Apfelbaum and Lubek can see no linkages among these topics? In a paper of mine, "Awakening the sense of injustice," (Deutsch, 1974) directly dealing with questions similar to those they pose, I found much prior empirical and theoretical work by social psychologists and other social scientists of clear relevance.

None of the foregoing is meant to deny that much more thought has to be given to the questions which are posed by current conflicts; nor is it to deny that current conflicts can suggest new theoretical foci and research
emphases. However, the same statements could be as appropriately made about "old" or even past conflicts. The social psychological study of conflict is relatively young; our state of knowledge is rather primitive. Although we are still mainly in the dark, some of the major dimensions and features of this area are beginning to emerge. Let us not be blind to what can be seen even as we seek to extend our vision.

I conclude by paraphrasing Adlai Stevenson: it is time for those who profess to be interested in radical social change to go beyond cursing the darkness and to help light a few candles.

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3. In a memorial tribute to Eleanor Roosevelt, Adlai Stevenson characterized her as a person who would rather light a candle than curse the darkness.
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References


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