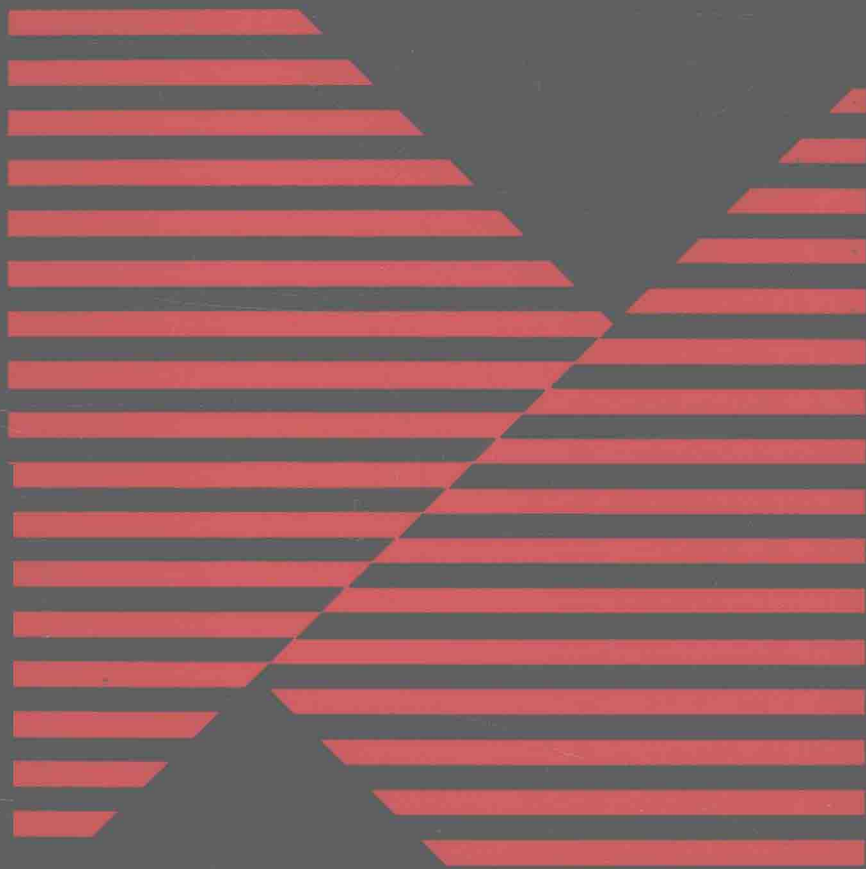


JAMES A. SCHELLENBERG

The Science of Conflict



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JAMES A. SCHELLENBERG

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THE SCIENCE OF CONFLICT

Preface

A student in the Programme of Peace and Conflict Research at the University of Lancaster, England, gathers all the material he can find on simple societies without war or violence. At Bethel College, in Kansas, a student takes an undergraduate major in “peace studies” and does an internship at the Martin Luther King Center in Georgia. Elsewhere in America, graduate students write dissertations on such subjects as counterinsurgency warfare, the economics of disarmament, and the perceptions of international crises by makers of foreign policy. And at Oslo, Norway, peace researchers from around the world gather for a seminar at the International Peace Research Institute, where the scholarly *Journal of Peace Research* is also published.

These examples are illustrations of a growing worldwide interest in conflict and peace studies. Each year more colleges and universities offer courses of study in this general area, although the particular titles and emphases of programs vary greatly. Advanced research goes on too, especially in Western Europe, the United States, and Japan. With such work by scholars of varied disciplines at different locations, it is difficult to capture a sense of this field as a whole—its leading ideas, key research findings, and central continuing questions. Nevertheless, that is exactly what this book attempts to do. I have tried to provide, in terms understandable to undergraduate social science students or the intelligent layman, a reasonably up-to-date summary of conflict and peace studies.

This book is deliberately eclectic and interdisciplinary. A

great variety of scholars study conflict, and I have included as wide a representation as possible. And with the best work interdisciplinary in character, I have not focused upon a particular discipline, such as political science or sociology. The result is a broad overview of conflict studies which includes contributions from all kinds of social scientists—anthropologists, social psychologists, historians, economists, political scientists, and sociologists, and those of a few other fields as well. The attempt to be cosmopolitan extends to two other aspects: a review of important developments in both theory and research, without too much formality or technical emphasis in either direction, and an appreciation both for the classics of social thought and for some of the latest findings. I therefore will treat the ideas of Machiavelli or Marx with as much serious concern as those of contemporary game theorists or the researchers of recent collective violence.

In keeping with this eclectic approach, I have also tried to avoid presenting a clear ideological position. The dogmas of fixed positions tend to dissolve as we approach the complexities of social conflict with a broadly empirical spirit. Nevertheless, I believe that it is impossible to bring together such a broad array of studies as we find here without giving something of my own general framework of thought. Let me then forewarn readers of what they may expect from my (often implicit) presuppositions. One central theme in my approach might be labeled “social realism.” I think we generally fail to inform ourselves fully of the objective basis for conflicts; we too easily assume that they are based on the bad intentions of wrong-headed leaders, who alone prevent some ideal world of peace and justice from flowering. Part of the problem is inadequate attention to the very real conflicts of interest that occur in all aspects of social life. Another part of the problem is a tendency to confuse our subjective experience (which leads us to view things as matters of individual psychology) with the actual underlying causal order of social forces (which is more collective in character).

However, a realistic understanding of conflicts does not require any particular methodological approach. In fact, a broad range of methodologies is precisely what is needed for an adequate understanding of social conflict. If we try to limit our-

selves to the most rigorous forms of research, we usually find that our most important questions are scarcely addressed. What we may call a "soft-nosed empiricism" therefore joins "social realism" as an underlying theme characterizing the present work.

Partly to identify possible biases in the way this book is put together, but more to acknowledge important contributions to my thinking, let me mention a number of personal influences upon my work in social conflict. A religious and family heritage of pacifism is one important influence. So is my training as a sociologist, beginning with the important influences of my undergraduate teachers (L. Roy Just at Tabor College and Paul C. Kochan at Baker University) and continuing with graduate work at the University of Kansas; especially important here was the tempering of my youthful idealism with a respect for empirical research. Several authors were especially important in sharpening my interest in the area of conflict; among the most significant writings were *Conflict and Defense* by Kenneth Boulding, *The Functions of Social Conflict* by Lewis Coser, *Fights, Games and Debates* by Anatol Rapoport, and *The Strategy of Conflict* by Thomas Schelling. My classes in social conflict at Western Michigan University and Indiana State University have also been important in shaping my decisions as to what is suitable for a book such as this. Among other acknowledgments of help in preparing the present work, special mention should be made of the library facilities of Indiana State University; the skill at deciphering a very rough manuscript on the part of my typist, Lois Alberti; and the patient advice of my editor at Oxford University Press, Spencer Carr.

Terre Haute
January 1981

J.A.S.

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Part One

Introduction

1

A New Science?

Lewis Richardson, a Quaker, was personally opposed to taking up arms in World War I. However, as a loyal subject of the British crown, he also felt a call to national duty. This, with his humanitarian impulses, led him to service as an ambulance driver, bringing wounded soldiers in from the front lines in France. During the lulls between battles, he amused himself by playing with numbers. He began to formulate mathematical analyses of war. Could we not quantify the patterns of action and reaction in war? Richardson thought that we could. And could we not derive mathematical equations to summarize such patterns—helping us to predict future events? He began thinking systematically along these lines.

After the war, Richardson spent many years gathering statistics of, as he called them, “deadly quarrels” and formulating mathematical models of conflict processes. Although he was respected for his work in physics and elected a Fellow of the prestigious Royal Society, very few people took seriously his conflict studies. Richardson himself considered them more important than his work in physical science (he retired early from teaching to be able to devote his full time to conflict research), but this appeared to matter little. Except for a handful of scholars, this painstaking work was simply ignored. He was unable to find a publisher for either of the two long manuscripts he produced in this area, and it was not until seven years after his death that his two main works (under the titles of *Arms and Insecurity* and *Statistics of Deadly Quarrels*) were finally published.¹

During the last thirty years Richardson's work has finally been widely recognized, and he is now considered among the pioneers of a new field of scientific study. But why in his own lifetime did so few take seriously this work? Is the scientific study of human conflict so preposterous? It does not seem so today—at least no more preposterous than dozens of other areas in the social sciences. But during Richardson's lifetime the social sciences were apparently not quite ready for what he had to offer. His work was more mathematical than the social sciences were generally prepared to deal with; this is part of our explanation for the limited acceptance of Richardson's work. But another point is that in his day there was no established field into which his work might be placed. This has significantly changed since his death in 1953.

There has been an active growth in systematic studies of conflict since the early 1950s. Scholars have energetically pursued comparative studies of conflict behavior, laboratory investigations of negotiations, and theoretical models of conflict processes. Although worldwide, this burgeoning interest in conflict studies has been especially notable in American and European universities. Side-by-side with specialized pursuits of scholars has been the development of educational programs in what is often called "peace studies." Thus today at a few American colleges it is possible to complete an undergraduate major in peace studies, and at several universities one can now earn a Ph.D. degree in this area.

There are various names to refer to this new field. For purposes of public relations "peace" seems to be preferred over "conflict," though analytically conflict processes are often seen as more fundamental than those of peace.* So the work in conflict and peace studies goes on as people variously talk about "conflict studies," "conflict theory," "peace studies," "peace science," or the "peace research movement."

In taking seriously the question mark in the title of this

*This is not meant to suggest that the divisive forces of human society are more fundamental than those of cohesion. We only suggest that an understanding of peace usually requires an analysis of the underlying forces of conflict, while the study of conflict does not always imply a corresponding attention to peace.²

chapter, we are led to ask whether the new area of conflict studies is really a new discipline. Has it firmly established its own identity among the social sciences, or does it remain simply a point of convergence for scholars of various disciplines? A good argument could be made for either side in answering this question. Let us consider briefly both sides, presenting first—after the pattern of a good debate—the affirmative case for a new discipline.

Kenneth Boulding has solid enough credentials as an economist that he would not need to identify himself with another discipline. Still, as a leading participant in the peace research movement, he believes that a new discipline has been established. He has succinctly summarized this case:

There are perhaps three tests of a discipline: does it have a bibliography? can you give courses in it? and, can you give examinations in it? A fourth criterion should perhaps be added: does it have any specialized journals? On all these four counts, conflict and peace studies can certainly claim to be a discipline.³

But does a common focus of interest and study necessarily mean a discipline? True, there are journals and organizations to represent peace research. But does membership in the International Peace Research Association (a worldwide organization sponsored by UNESCO)—or the Consortium on Peace Research, Education and Development (in North America) or the Peace Science Society (International)—require identification with a new discipline? As one approach to an answer, we can examine how authors of the leading journals identify themselves. As I write this, I have at my desk the latest issues of the *Journal of Conflict Resolution* and the *Journal of Peace Science*, as well as the latest volume of published *Papers* of the Peace Science Society (International). A quick count indicates that 33 authors are represented in these three issues, all but 3 having a university affiliation. Of the 30 university-based contributors, 21 are identified with standard social science disciplines (9 political scientists, 7 economists, 3 sociologists, 1 anthropologist, and 1 psychologist). The other 9 represent a miscellaneous assortment (including mathematics, law, industrial engineering,

and “social systems science”). Only the authors of two articles are identified in a manner which suggests any formal institutional claims of peace research; one economist is also identified with Stanford’s Hoover Institution on War, Revolution, and Peace, and the joint authors of another article are affiliated with the University of Pennsylvania’s Regional Science Department and Peace Science Unit.

From this brief examination, we may conclude that most participants in the peace research movement have a primary scholarly identity in one of the standard social science disciplines. But we also recognize that the combination is quite varied; certainly no single discipline can claim peace research as simply an adjunct to itself.

So, do we have a new discipline here or do we not? Perhaps we need not decide clearly either for or against the disciplinary claim. We can recognize that scholars with diverse backgrounds have developed a common interest in conflict and peace studies. We can further recognize that in doing so they need not give up their identities as economists, political scientists, sociologists, or whatever. There is a high degree of interdisciplinary work and cross-disciplinary interaction. But the new focus in conflict studies has not yet been thoroughly institutionalized. If it is an emerging discipline, it is still not widely recognized as such.

The title of this first chapter ends with a question mark for more than one reason. So far, we have been concerned with whether or not we have *a* new science. The second word in the title might also be seen as problematic: do we have a *new* science?

The focus of scholarly interest in conflict studies and peace research has certainly grown dramatically since World War II. The research productivity of the last twenty years overshadows that of all previous periods combined. But are the fundamental ideas involved really new? Here, again, we shall hedge. As will be apparent in the remaining chapters of this book, the contributions of the last few decades are especially important. The most systematic empirical work has, with only a few exceptions, been carried out since the mid-1950s. But most of the funda-

mental ideas have been around for much longer. As we will see in the next four chapters, the fundamental perspectives we use today were already widespread during the nineteenth century. And we can go even further back in time (which we will do on occasion in this book) to get important insights for our studies. The analysis of the Peloponnesian War by Thucydides and Aristotle's reflections on revolutions provide two examples of writings which, though over two thousand years old, still provide vital insights in this area.

Finally, let us also question the last word in the title of this chapter. Do we really have a new science? If by science we mean merely systematic empirical research, there can be no question about it; much of this has accumulated, especially in the last decade or so. But is this research organized by a clear set of general propositions about human behavior? And are these propositions set forth as *positive* rather than *normative* principles? Here our answers are not quite so clear.

In the early development of most sciences, we find a strong concern for practical problems. Principles are seen as guides for dealing intelligently with these problems as well as guides for the understanding of fundamental truth. The propositions that sum up key ideas therefore have a *normative* (indicating what should be done) as well as a *positive* (simply describing how things are) character. As a science matures, however, it tends to lose this normative basis. The science seeks basic truth; its applications (though admittedly with normative components) are another matter, not to be confused with pure science.

The science of conflict is not at present generally regarded as a pure science in this positive and nonnormative sense. It may someday become such, for most of its research is cast in this mold. However, the normative approach to posing fundamental questions is still very much with us. We ask not only how nations in conflict actually carry on their contest but also how the conflict may or should be resolved. We ask not only how a revolutionary movement arises but what should be its actions if it is to succeed, or how the government should act if the rebellion is to be suppressed. Such questions are almost

inevitably normative; and to the extent that the peace scientists deal with them, their science is not formed directly according to the pattern of the natural sciences.

But the field of conflict studies is not unique in having this problem of a normative-positive mixture. In this regard, it is simply mirroring the general state which is characteristic of most of the social sciences. And, as is the case with other fields, we learn here that normative issues may be as intellectually challenging as are those of a purely positive framework. Normative questions do not necessarily demand that we be wishy-washy.

The predominant spirit with which we embark on our survey of the science of conflict will be that associated with positive science. We are pursuing knowledge primarily for its own sake—which means primarily for our own understanding rather than for any particular applications. But there are limits to how far we can go with this framework in the study of conflict before we are brought eventually to normative issues. Perhaps this is a sign that the science of conflict is not yet a “true” or pure science. Perhaps. But the author prefers to think of it as a sign that we are struggling with issues that really matter, critically and directly, for the world in which we live. Rather than a matter for apology, we may consider the presence of fundamentally normative questions as a cause for excitement. They pose a challenge which is both intellectual and humane, for in the study of social conflict we deal with a most elusive intertwining of empirical facts and human values.