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Volume 67, Number 1 January 2015

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World Politics / Volume 18 / Issue 03 / April 1966, pp 361 - 377

DOI: 10.2307/2009761, Published online: 18 July 2011

Link to this article: http://journals.cambridge.org/abstract_S0043887100008479

How to cite this article:

Hedley Bull (1966). International Theory: The Case for a Classical Approach. World Politics, 18, pp 361-377 doi:10.2307/2009761

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INTERNATIONAL THEORY

The Case for a Classical Approach

By HEDLEY BULL*

I

TWO approaches to the theory of international relations at present compete for our attention. The first of these I shall call the classical approach. By this I do not mean the study and criticism of the "classics" of international relations, the writings of Hobbes, Grotius, Kant, and other great thinkers of the past who have turned their attention to international affairs. Such study does indeed exemplify the classical approach, and it provides a method that is particularly fruitful and important. What I have in mind, however, is something much wider than this: the approach to theorizing that derives from philosophy, history, and law, and that is characterized above all by explicit reliance upon the exercise of judgment and by the assumptions that if we confine ourselves to strict standards of verification and proof there is very little of significance that can be said about international relations, that general propositions about this subject must therefore derive from a scientifically imperfect process of perception or intuition, and that these general propositions cannot be accorded anything more than the tentative and inconclusive status appropriate to their doubtful origin.

Until very recently virtually all attempts at theorizing about international relations have been founded upon the approach I have just described. We can certainly recognize it in the various twentieth-century systematizations of international theory—in works like those of Alfred Zimmern, E. H. Carr, Hans Morgenthau, Georg Schwarzenberger, Raymond Aron, and Martin Wight. And it is clearly also the method of their various precursors, whose scattered thoughts and partial treatments they have sought to draw together: political philosophers like Machiavelli and Burke, international lawyers like Vattel and Oppenheim, pamphleteers like Gentz and Cobden, historians like Heeren and Ranke. It is because this approach has so long been the standard one that we may call it classical.

The second approach I shall call the scientific one. I have chosen to call it scientific rather than scientistic so as not to prejudge the issue I wish to discuss by resort to a term of opprobrium. In using this name

* This paper was read to the tenth Bailey Conference on the university teaching of international relations, which met at the London School of Economics in January 1966.

for the second approach, however, it is the aspirations of those who adopt it that I have in mind rather than their performance. They aspire to a theory of international relations whose propositions are based either upon logical or mathematical proof, or upon strict, empirical procedures of verification. Some of them dismiss the classical theories of international relations as worthless, and clearly conceive themselves to be the founders of a wholly new science. Others concede that the products of the classical approach were better than nothing, and perhaps even regard them with a certain affection, as the owner of a 1965 model might look at a vintage motor car. But in either case they hope and believe that their own sort of theory will come wholly to supersede the older type; like the logical positivists when they sought to appropriate English philosophy in the 1930's, or like Mr. McNamara's Whiz Kids when they moved into the Pentagon, they see themselves as tough-minded and expert new men, taking over an effete and woolly discipline, or pseudo-discipline, which has so far managed by some strange quirk to evade the scientific method but has always been bound to succumb to it in the end.

The scientific approach to the theory of international relations, so defined, is present in the theory of international systems, as developed by Morton A. Kaplan and others, in the various international extrapolations of John Von Neumann and Oskar Morgenstern's theory of games, in Thomas C. Schelling's theory of bargaining, in Karl W. Deutsch's work on social communication, in William H. Riker's study of political coalitions, in the models of foreign policy-making produced by George A. Modelski and others, in Lewis F. Richardson's mathematical studies of arms races and deadly quarrels, and in the theories of conflict developed by Kenneth Boulding and Anatol Rapoport. It also appears to be an important part of the content of what is called "peace research."¹

~ The studies I have named vary enormously in the methods they employ and in the questions to which they are addressed. Their authors, indeed, far from facing the outside world with a united front, com-

¹ See, for example, Kaplan, *System and Process in International Politics* (New York 1957); Morgenstern, *The Question of National Defense* (New York 1959); Schelling, *The Strategy of Conflict* (Cambridge, Mass., 1960); Deutsch and others, *Political Community and the North Atlantic Area: International Organization in the Light of Historical Experience* (Princeton 1957); Riker, *The Theory of Political Coalitions* (New Haven 1962); Modelski, *A Theory of Foreign Policy* (New York 1962); Richardson, *Arms and Insecurity: A Mathematical Study of the Causes and Origin of War*, ed. Nicolas Rashevsky and Ernesto Trucco (Pittsburgh 1960), and *Statistics of Deadly Quarrels*, ed. Quincy Wright and C. C. Lienau (Pittsburgh 1960); Boulding, *Conflict and Defense: A General Theory* (New York 1962); Rapoport, *Fights, Games, and Debates* (Ann Arbor 1960).

monly regard one another with the hostility of leaders of Marxist sects. There are also, it may be argued, great discrepancies among them in the extent to which they have illuminated our subject. What I have called the scientific approach, moreover, is not present in all of them to the same degree. There are dangers in lumping them all together, and it may be inevitable that criticisms directed at the whole of the genre will be unfair to some parts of it. Nevertheless, all of these studies and fashions embody the scientific approach in some measure, and to discuss this it is necessary to confine our attention to what they have in common.

In the United States in the last ten years the scientific approach has progressed from being a fringe activity in the academic study of international relations to such a position that it is at least possible to argue that it has become the orthodox methodology of the subject. The award in 1963 of the American Political Science Association's prize for the best study of the year to a practitioner of the classical approach (to Inis Claude for his *Power and International Relations*) already had the appearance of a perverse action of the rear guard.

In the British academic community, by contrast, the scientific approach to the theory of international relations has had virtually no impact at all. The only Englishman to have made a major contribution in the new genre—Lewis F. Richardson—worked alone and unrecognized in his lifetime, and when a few years ago his work was exhumed and hailed as that of a great pioneer, it was by American editors addressing themselves to a predominantly American audience. Not only have British students of international relations not sought to contribute to theory in this vein, but, with one or two exceptions, the work of the American and other writers who have ploughed this field has failed to command their respect or even their attention.

If it were clear that this disdain has been founded upon an understanding of the scientific approach and a considered rejection of it there might be no cause for us to revise our attitude. We might even see in our imperviousness to this fashion the proof of the fundamental soundness and solidity of our own approach. The actual position, however, is that we are largely ignorant of what the new literature contains and that our rejection of it stems much less from any reasoned critique than it does from feelings of aesthetic revulsion against its language and methods, irritation at its sometimes arrogant and preposterous claims, frustration at our inability to grasp its meaning or employ its tools, *a priori* confidence that as an intellectual enterprise

it is bound to fail, and professional insecurity induced by the awful gnawing thought that it might perhaps succeed.

There is no doubt that the writing that has emerged from the scientific approach should be taken seriously. Judged by its own standards of logical precision and scientific rigor its quality is sometimes high. Moreover, however adverse a view we take of this literature, it is impossible to examine it with any degree of care and sympathy and yet to conclude that its contribution to the understanding of international relations is nil. Indeed, given the great concentration of energy and talent that has gone into producing it in recent years, it would be extraordinary if this were otherwise.

It is therefore desirable that if we are to reject the scientific approach we should at the same time pay attention to it and formulate such objections to it as we may have. It has now developed so much momentum that silence toward it, or worse, the facile abuse with which it is sometimes greeted by British reviewers, will no longer suffice to keep it at bay. If, as I believe, the scientific approach should be kept firmly in the background, this can only be accomplished by rational criticism.

II

In setting out to provide such a rational criticism one may begin by dismissing a number of complaints commonly directed at the scientific approach which are beside the point.

One such complaint made of these theorists, especially, perhaps, of Morton Kaplan, is that their writing is tortuous and inelegant. But the fact that Morton Kaplan's book is not a pleasure to read is no more a criticism of the theory of international politics it contains than is the difficulty of reading Einstein a deficiency of the theory of relativity. If Kaplan could be charged with deliberately constructing an unnecessarily obscure terminology, or with employing it clumsily and inefficiently, this would be another matter; but such a charge would be quite misplaced. Kaplan's terminology is a vital part of his whole attempt to construct a rigorous system, and his use of it is precise and economical.

Indeed, while one need not go so far as to regard literary mediocrity as a positive merit in a book about politics, Kaplan's work derives much of its originality and force from precisely this disdain of the tradition that regards historical and political writing as a branch of belles lettres. The power of this tradition reflects the fact that historical and political writing, in addition to serving the purpose of com-

munication between specialists seeking understanding of the subject, serves such other purposes as education, persuasion, public entertainment, and the exhibition of gentlemanly accomplishments. Kaplan is surely correct in dismissing the literary embellishment that is a proper element in writing for these latter purposes as an irrelevance and an encumbrance in writing for the former.

Another unsatisfactory line of criticism is that which focuses not upon the doctrine of the scientific theorists but upon the motives that have driven them to propound it. Thus it has been observed that those who follow the scientific approach are new scholastics, who have sought refuge in a world of intellectual constructs essentially in order to escape from political reality; that they are natural scientists, mathematicians, and economists *manqués* who, unable to make careers for themselves in their own fields, have moved into another where the going is easier, bringing their techniques with them; that they are interested in elaborating a mathematical or scientific methodology for its own sake—or for the sake of demonstrating their mastery of it to the uninitiated—rather than in illuminating our subject by the use of it; or even that they represent a new form of the cargo cult.

These observations, or some of them, are true or half true, and they help us to understand the character of the new theorizing as an intellectual movement. It is true of any intellectual style or scholarly fashion that it is pursued for a variety of motives of which the disinterested desire for knowledge is only one, and that some of these motives are much removed from any such desire and are even discreditable. But precisely for this reason a discussion of the motivations of theorists does not provide any basis for the defense of one intellectual style against another. It is too easy for the scientifically-minded theorist to turn the tables. Do not those who adhere to the classical approach do so out of a vested interest in their own techniques, a slothful reluctance to learn new ones? Are they not also wedded to a methodology for its own sake, to the art of judgment over and against measurement, and to literary forms as against symbolic ones, clinging to these instruments of their trade like horse cavalrymen in the age of mechanization? Do they not represent an outgoing generation, trained in one set of techniques, expressing its resentment against an incoming generation trained in another? I should need to be surer than I am that my own motives in preparing this paper are wholly disinterested before inviting criticism of them by attacking those of others. We shall be well advised, therefore, to confine our attention to the doctrines themselves.

Finally, it is a mistake to see in the scientific approach, or in any one of the methods that go to make it up, the instrument of any particular political purpose in foreign or defense policy. In the ranks of the systems theorists, game theorists, communications theorists, and conflict theorists, it is possible to find attitudes ranging from the most conservative to the most radical; nor is there any logical connection between any of the techniques and any particular political attitude. Writers like Herman Kahn, Thomas Schelling, and Morton Kaplan, who may be broadly described as proestablishment in their attitudes to foreign and defense policy, have been the object of political attacks that hinge upon their use of these techniques. But such attacks take no account of other writers such as Kenneth Boulding, Anatol Rapoport, or J. David Singer, who are dissenters from United States foreign and defense policies but stand intellectually in the same camp. Similarly the current fashion for "peace research" or "conflict resolution" often seems to embody the misconception that the application of these new techniques to the study of international relations is bound to vindicate radical policies or to facilitate their implementation.

However, the scientific approach has contributed and is likely to contribute very little to the theory of international relations, and in so far as it is intended to encroach upon and ultimately displace the classical approach, it is positively harmful. In support of this conclusion I wish to put forward seven propositions.

The first proposition is that by confining themselves to what can be logically or mathematically proved or verified according to strict procedures, the practitioners of the scientific approach are denying themselves the only instruments that are at present available for coming to grips with the substance of the subject. In abstaining from what Morton Kaplan calls "intuitive guesses" or what William Riker calls "wisdom literature" they are committing themselves to a course of intellectual puritanism that keeps them (or would keep them if they really adhered to it) as remote from the substance of international politics as the inmates of a Victorian nunnery were from the study of sex.

To appreciate our reliance upon the capacity for judgment in the theory of international relations we have only to rehearse some of the central questions to which that theory is addressed. Some of these are at least in part moral questions, which cannot by their very nature be given any sort of objective answer, and which can only be probed, clarified, reformulated, and tentatively answered from some arbitrary standpoint, according to the method of philosophy. Others of them

are empirical questions, but of so elusive a nature that any answer we provide to them will leave some things unsaid, will be no more than an item in a conversation that has yet to be concluded. It is not merely that in *framing* hypotheses in answer to these empirical questions we are dependent upon intuition or judgment (as has often been pointed out, this is as true in the natural as in the social sciences); it is that in the *testing* of them we are utterly dependent upon judgment also, upon a rough and ready observation, of a sort for which there is no room in logic or strict science, that things are this way and not that.

For example, does the collectivity of sovereign states constitute a political society or system, or does it not? If we can speak of a society of sovereign states, does it presuppose a common culture or civilization? And if it does, does such a common culture underlie the worldwide diplomatic framework in which we are attempting to operate now? What is the place of war in international society? Is all private use of force anathema to society's working, or are there just wars which it may tolerate and even require? Does a member state of international society enjoy a right of intervention in the internal affairs of another, and if so in what circumstances? Are sovereign states the sole members of international society, or does it ultimately consist of individual human beings, whose rights and duties override those of the entities who act in their name? To what extent is the course of diplomatic events at any one time determined or circumscribed by the general shape or structure of the international system; by the number, relative weight, and conservative or radical disposition of its constituent states, and by the instruments for getting their way that military technology or the distribution of wealth has put into their hands; by the particular set of rules of the game underlying diplomatic practice at that time? And so on.

These are typical of the questions of which the theory of international relations essentially consists. But the scientific theorists have forsworn the means of coming directly to grips with them. When confronted with them they do one of two things. Either they shy away and devote themselves to peripheral subjects—methodologies for dealing with the subject, logical extrapolations of conceptual frameworks for thinking about it, marginalia of the subject that are susceptible of measurement or direct observation—or they break free of their own code and resort suddenly and without acknowledging that this is what they are doing to the methods of the classical approach—methods that in some cases they employ very badly, their preoccupations and training having left them still strangers to the substance of the subject.

This congenital inability of the scientific approach to deal with the crux of the subject while yet remaining true to its own terms leads me to an observation about the teaching of the subject in universities. Whatever virtues one might discern in the scientific approach, it is a wholly retrograde development that it should now form the basis of undergraduate courses of instruction in international politics, as in some universities in the United States it now does. The student whose study of international politics consists solely of an introduction to the techniques of systems theory, game theory, simulation, or content analysis is simply shut off from contact with the subject, and is unable to develop any feeling either for the play of international politics or for the moral dilemmas to which it gives rise.

The second proposition I wish to put forward arises out of the first: It is that where practitioners of the scientific approach have succeeded in casting light upon the substance of the subject it has been by stepping beyond the bounds of that approach and employing the classical method. What there is of value in their work consists essentially of judgments that are not established by the mathematical or scientific methods they employ, and which may be arrived at quite independently of them.

Let me take as an example the work of Thomas Schelling, who has contributed as much as and perhaps more than any other thinker of the scientific genre to the theory of international relations. His elaboration of the notion of arms control, the elements of deterrence, the nature of bargaining, the place in international relations of threats of force are of a rare originality and importance and will probably prove to have made a lasting impression on the theory and, indeed, the practice of these matters. At the same time he is an economist by training; he has written studies of a technical nature about game and bargaining theory; and he has sometimes seemed to lend his support to the call for more theory of a scientific sort.

It appears to me that Schelling's illuminating observations about violence and international politics in every case have the status of unprovable and untestable judgments, and that they have not been and could not be demonstrated by his work in formal game and bargaining theory. Schelling happens to combine with his interest in the latter techniques a shrewd political judgment and a philosophical skill in thinking out problems in terms of their basic elements. It is possible that his ideas about international relations have been suggested to him by his technical studies, and he has evidently thought it useful to provide illustrations of his ideas in formal, theoretical exercises.

Those of his readers who share his interest in these techniques will find it amusing and perhaps profitable to pursue these illustrations. But they are at best a helpful analogy; they do not represent the foundation of his contribution to international politics or the road that must be travelled in order to arrive at it.

My third proposition is that the practitioners of the scientific approach are unlikely to make progress of the sort to which they aspire. Some of the writers I have been discussing would be ready enough to admit that so far only peripheral topics have been dealt with in a rigidly scientific way. But their claim would be that it is not by its performance so far that their approach should be judged, but by the promise it contains of ultimate advance. They may even say that the modesty of their beginnings shows how faithful they are to the example of natural science: Modern physics too, Morton Kaplan tells us, "has reared its present lofty edifice by setting itself problems that it has the tools or techniques to solve."²

The hope is essentially that our knowledge of international relations will reach the point at which it becomes genuinely cumulative: that from the present welter of competing terminologies and conceptual frameworks there will eventually emerge a common language, that the various insignificant subjects that have now been scientifically charted will eventually join together and become significant, and that there will then exist a foundation of firm theory on which newcomers to the enterprise will build.

No one can say with certainty that this will not happen, but the prospects are very bleak indeed. The difficulties that the scientific theory has encountered do not appear to arise from the quality that international relations is supposed to have of a "backward" or neglected science, but from characteristics inherent in the subject matter which have been catalogued often enough: the unmanageable number of variables of which any generalization about state behavior must take account; the resistance of the material to controlled experiment; the quality it has of changing before our eyes and slipping between our fingers even as we try to categorize it; the fact that the theories we produce and the affairs that are theorized about are related not only as subject and object but also as cause and effect, thus ensuring that even our most innocent ideas contribute to their own verification or falsification.

A more likely future for the theory of international politics is that

² "Problems of Theory Building and Theory Confirmation in International Politics," *World Politics*, XIV (October 1961), 7.

it will remain indefinitely in the philosophical stage of constant debate about fundamentals; that the works of the new scientific theorists will not prove to be solid substructure on which the next generation will build, but rather that those of them that survive at all will take their place alongside earlier works as partial and uncertain guides to an essentially intractable subject; and that successive thinkers, while learning what they can from what has gone before, will continue to feel impelled to build their own houses of theory from the foundations up.

A fourth proposition that may be advanced against many who belong to the scientific school is that they have done a great disservice to theory in this field by conceiving of it as the construction and manipulation of so-called "models." Theoretical inquiry into an empirical subject normally proceeds by way of the assertion of general connections and distinctions between events in the real world. But it is the practice of many of these writers to cast their theories in the form of a deliberately simplified abstraction from reality, which they then turn over and examine this way and that before considering what modifications must be effected if it is to be applied to the real world. A model in the strict sense is a deductive system of axioms and theorems; so fashionable has the term become, however, that it is commonly used also to refer to what is simply a metaphor or an analogy. It is only the technique of constructing models in the strict sense that is at issue here. However valuable this technique may have proved in economics and other subjects, its use in international politics is to be deplored.

The virtue that is supposed to lie in models is that by liberating us from the restraint of constant reference to reality, they leave us free to set up simple axioms based on a few variables and thenceforward to confine ourselves to rigorous deductive logic, thereby generating wide theoretical insights that will provide broad signposts to guide us in the real world even if they do not fill in the details.

I know of no model that has assisted our understanding of international relations that could not just as well have been expressed as an empirical generalization. This, however, is not the reason why we should abstain from them. The freedom of the model-builder from the discipline of looking at the world is what makes him dangerous; he slips easily into a dogmatism that empirical generalization does not allow, attributing to the model a connection with reality it does not have, and as often as not distorting the model itself by importing additional assumptions about the world in the guise of logical axioms. The very intellectual completeness and logical tidiness of the model-

building operation lends it an air of authority which is often quite misleading as to its standing as a statement about the real world.

I shall take as an example the most ambitious of all the model-builders, Morton Kaplan. He provides us with models of two historical and four possible international systems, each with its "essential rules" or characteristic behavior. He claims that the models enable him to make predictions—only, it is true, of a high level of generality—about characteristic or modal behavior within the present international system, about whether or not transformations of this system into some other are likely and what form they might take.

The six systems that Kaplan identifies, and the "essential rules" or characteristic behavior of each, are in fact quite commonplace ideas, drawn from the everyday discussion of international affairs, about the general political structure that the world has had or might have. They are the international political system of the eighteenth and nineteenth centuries, the present so-called bipolar system, the structure that might exist if the present polarization of power were not moderated by the United Nations and by powerful third parties, the system we might have if the United Nations were to become the predominant political force in a world of still sovereign states, a world state, and a world of many nuclear powers.

In discussing the conditions under which equilibrium is maintained in each of these systems, and in predicting the likelihood and direction of their transformation into different systems, Kaplan appears to resort to a kind of guesswork a good deal more arbitrary than any involved in the style of international theory he wishes to displace. In discussing the two historical systems he uses some pertinent examples from recent history, but there is no reason to assume that behavior in future international systems of this sort is bound to be the same. In discussing the nonhistorical systems, his remarks are either tautological extensions of the definitions he employs, or are quite arbitrarily formulated empirical judgments that do not properly belong to the model at all.

Kaplan's six systems are of course not the only ones possible. He admits, for example, that they do not cover the cases of Greek antiquity or of the Middle Ages, and they do not embrace the infinite variety the future might unveil. What reason, therefore, is there to suppose that transformation of any one of the systems must be into one of the others? The whole enterprise of attempting to predict transformations on the basis of these models requires at every stage that

we go outside the models themselves and introduce further considerations.

One objection to Kaplan's models, therefore, is that they are not models; they are lacking in internal rigor and consistency. But even if they possessed such qualities, they would not provide the illumination of reality that Kaplan claims for them. We have no means of knowing that the variables excluded from the models will not prove to be crucial. He has provided an intellectual exercise and no more. I should not wish to contend that someone exploring the question of what changes might take place in the present international system, or the question of what might be the shape and structure of a world of many nuclear powers, is unable to quarry some nuggets of value from Kaplan's work. But how much more fruitfully can these questions be explored, how much better indeed might so gifted a person as Kaplan himself have explored them, by paying attention to the actual variety of events in the real world, by taking note of the many elements that are pushing the present international system this way and that, and the large number of political and technical factors that might contrive to mold a world of many nuclear powers in any one of a dozen shapes different from those that can be confined within the bounds of Kaplan's model.

The fashion for constructing models exemplifies a much wider and more long-standing trend in the study of social affairs: the substitution of methodological tools and the question "Are they useful or not?" for the assertion of propositions about the world and the question "Are they true or not?" Endemic though it has become in recent thinking, I believe this change to have been for the worse. The "usefulness" of a tool has in the end to be translated as the truth of a proposition, or a series of propositions, advanced about the world, and the effect of the substitution is simply to obscure the issue of an empirical test and to pave the way for shoddy thinking and the subordination of inquiry to practical utility. However, this is a theme that requires more amplification than it can be given here, and in introducing it I am perhaps taking on more antagonists than I need do for my present purpose.

A fifth proposition is that the work of the scientific school is in some cases distorted and impoverished by a fetish for measurement. For anyone dedicated to scientific precision, quantification of the subject must appear as the supreme ideal, whether it takes the form of the expression of theories themselves in the form of mathematical equations or simply that of the presentation of evidence amassed in quantitative form. Like the Anglican bishop a year or so ago who began

his sermon on morals by saying that he did not think all sexual intercourse is necessarily wrong, I wish to take a liberal view of this matter. There is nothing inherently objectionable, just as there is nothing logically peculiar, in a theoretical statement about international politics cast in mathematical form. Nor is there any objection to the counting of phenomena that do not differ from one another in any relevant respect, and presenting this as evidence in support of a theory. The difficulty arises where the pursuit of the measurable leads us to ignore relevant differences between the phenomena that are being counted, to impute to what has been counted a significance it does not have, or to be so distracted by the possibilities that do abound in our subject for counting as to be diverted from the qualitative inquiries that are in most cases more fruitful.

I should like to take as an example the work of Karl Deutsch and his pupil Bruce Russett. These writers have sought to investigate the bonds of community that link different nations, and in explaining the cohesiveness or mutual responsiveness that exists between different peoples or different groups within a single people they have especially focused their attention upon social communication, that is to say, upon the flow of persons, goods, and ideas, or of the "messages" they carry. Karl Deutsch, together with a number of collaborators, has provided a study of the extent to which the various peoples of the North Atlantic area are linked by such bonds of community, and he is concerned particularly with the question of the measure in which these peoples form what he calls a "security-community"—that is to say, a group of people who agree that their common problems must be resolved by "peaceful change," and who for a long time have had dependable expectations that their problems will in fact be resolved in this way.³ Bruce Russett has tackled the more manageable subject of community simply in the relationship between Britain and America, and has sought in particular to determine whether these two peoples have become more or less "responsive" to one another as the twentieth century has progressed.⁴

A feature of the work of both these writers is their presentation of quantitative material as an index of the degree of community that exists between one people and another. They produce figures, for ex-

³ Deutsch has, of course, been author or part-author of a number of other works besides *Political Community and the North Atlantic Area*, but apart from his *Political Community at the International Level* (Princeton 1953), this is the one that most comes to grips with the theory of international relations.

⁴ *Community and Contention: Britain and America in the Twentieth Century* (Cambridge, Mass., 1963).

ample, on resources devoted to trade as a proportion of total resources; mail sent abroad, or to a particular destination, as a proportion of total mail; number of diplomatic agreements arrived at with another country as a proportion of total agreements arrived at; student exchanges; "content analysis" of newspapers and learned journals; and so on.

The work of Karl Deutsch and Bruce Russett in this field is certainly original and suggestive. Moreover, these two writers are not uncritical in their use of quantitative analysis. But the prominence they give to it is a source of weakness rather than strength in their arguments. Their counting often ignores (or, if it does not ignore, skates over) the most relevant differences between the units counted: differences between the content of one item of mail and another, the diplomatic importance of one treaty and another, the significance of one inch of newspaper column and another. Differences in these other relevant respects may cancel themselves out, but they also may not; and in practice we are likely to respect these statistics only in cases where they confirm some intuitive impression we already have, as, e.g., where Russett's figures confirm, as many of them do, the very confident judgment we may make that as this century has progressed America has become relatively more important to Britain than Britain is to America. Even so, such a judgment is quite external to the statistics that are provided, and does not establish that they measure anything relevant.

Deutsch and Russett, furthermore, are inclined to attribute to their statistics a place in the total chain of the argument that they do not have. They often seem to assume that there is something so irrefutable and final about a piece of evidence that can be put into figures that they are absolved of the necessity of showing in detail how it supports the general thesis they are seeking to demonstrate. Foreign trade is foreign trade, and a precise measurement of foreign trade is not a precise measurement of anything else unless an explanation is advanced as to why this is so. A number of the crucial but missing links in Deutsch's chain of argument seem to have been lost to sight because of this tendency of those who have succeeded in producing figures to be blinded by the illumination they cast. Are the figures of "communication flow" an index of political community at the international level, or a cause of it? Does the "communication flow" contribute to producing the vital element, in Deutsch's scheme, of "mutual identification," or does the latter arise in some quite different way?

Finally, even if one may concede that statistics have some place in an inquiry into political community and social communication, it ap-

pears to me that Deutsch and Russett have been distracted by them from the more fruitful parts of the subject. By far the most interesting things that these two writers have to say lie in their attempts to think out the distinguishing features of a community, the different sorts of communities that obtain, the elements that make up the cohesion of a community, the determinants of mutual responsiveness between one people and another. And by far the most pertinent evidence they bring forward lies in the qualitative judgments they are able to bring to bear on history and contemporary affairs.

My sixth proposition is that there is a need for rigor and precision in the theory of international politics, but that the sort of rigor and precision of which the subject admits can be accommodated readily enough within the classical approach. Some of the targets at which the scientific theorists aim their barbs are quite legitimate ones. The classical theory of international relations has often been marked by failure to define terms, to observe logical canons of procedure, or to make assumptions explicit. It has sometimes also, especially when associated with the philosophy of history, sought to pursue into international politics implications of a fundamentally unscientific view of the world. The theory of international relations should undoubtedly attempt to be scientific in the sense of being a coherent, precise, and orderly body of knowledge, and in the sense of being consistent with the philosophical foundations of modern science. Insofar as the scientific approach is a protest against slipshod thinking and dogmatism, or against a residual providentialism, there is everything to be said for it. But much theorizing in the classical mold is not open to this sort of objection. The writings of the great international lawyers from Victoria to Oppenheim (which, it may be argued, form the basis of the traditional literature of the subject) are rigorous and critical. There are plenty of contemporary writers who are logical and rigorous in their approach and yet do not belong to the school I have called the scientific one: Raymond Aron, Stanley Hoffmann, and Kenneth Waltz are examples. Moreover, it is not difficult to find cases where writers in the scientific vein have failed to be rigorous and critical in this sense.

My seventh and final proposition is that the practitioners of the scientific approach, by cutting themselves off from history and philosophy, have deprived themselves of the means of self-criticism, and in consequence have a view of their subject and its possibilities that is callow and brash. I hasten to add that this is not true, or not equally true, of them all. But their thinking is certainly characterized by a lack of any sense of inquiry into international politics as a continuing

tradition to which they are the latest recruits; by an insensitivity to the conditions of recent history that have produced them, provided them with the preoccupations and perspectives they have, and colored these in ways of which they might not be aware; by an absence of any disposition to wonder why, if the fruits their researches promise are so great and the prospects of translating them into action so favorable, this has not been accomplished by anyone before; by an uncritical attitude toward their own assumptions, and especially toward the moral and political attitudes that have a central but unacknowledged position in much of what they say.

The scientific approach to international relations would provide a very suitable subject for the sort of criticism that Bernard Crick has applied to a wider target in his admirable book *The American Science of Politics*—criticism that would, by describing its history and social conditions, isolate the slender and parochial substructure of moral and political assumption that underlies the enterprise.⁵ There is little doubt that the conception of a science of international politics, like that of a science of politics generally, has taken root and flourished in the United States because of attitudes towards the practice of international affairs that are especially American—assumptions, in particular about the moral simplicity of problems of foreign policy, the existence of “solutions” to these problems, the receptivity of policy-makers to the fruits of research, and the degree of control and manipulation that can be exerted over the whole diplomatic field by any one country.

III

Having stated the case against the scientific approach I must return to the qualifications I introduced at the outset. I am conscious of having made a shotgun attack upon a whole flock of assorted approaches, where single rifle shots might have brought down the main targets more efficiently and at the same time spared others that may have been damaged unnecessarily. Certainly, there are many more approaches to the theory of international relations than two, and the dichotomy that has served my present purpose obscures many other distinctions that it is important to bear in mind.

Students of international relations are divided by what are in some cases simply barriers of misunderstanding or academic prejudice that cut across the whole field of social studies at the present time. No

⁵ *The American Science of Politics: Its Origins and Conditions* (Berkeley and London 1959).

doubt it is desirable that such barriers be lowered. But in the present controversy, eclecticism, masquerading as tolerance, is the greatest danger of all; if we are to be hospitable to every approach (because "something may come of it some day") and extend equal rights to every cliché (because "there is, after all, a grain of truth in what he says"), there will be no end to the absurdities thrust upon us. There are grains of truth to be had from a speaker at Hyde Park Corner or a man on a Clapham omnibus, but the question is "What place do they have in the hierarchy of academic priorities?"

I hope I have made it clear that I see a good deal of merit in a number of the contributions that have been made by theorists who adopt a scientific approach. The argument is not that these contributions are worthless, but that what is of value in them can be accommodated readily enough within the classical approach. Moreover, the distinctive methods and aspirations these theorists have brought to the subject are leading them down a false path, and to all appeals to follow them down it we should remain resolutely deaf.