

Theorizing about theory in international politics

J. DAVID SINGER

Department of Political Science, University of Michigan

One of the most promising developments in the intellectual growth of a discipline is the appearance of a concern for theory on the part of its students and practitioners. It might even be argued that, in the absence of such a concern, we have no discipline at all but merely a crudely delimited area of inquiry. To qualify as a discipline, there must be (at the very minimum) not only general agreement on the subject matter but some discernible consensus on terminology, taxonomy, and methodology. Furthermore, there must be a body of generalizations which are susceptible to verification; whether such verification has or has not taken place is less crucial and whether it be done empirically or deductively is of minor consequence. Finally, until such generalizations and propositions have begun to take on some sort of compatible coherence, arranged in meaningful juxtaposition to one another, we have something less than theory. And without theory we can have only the barest shadow of a discipline.

The recent preoccupation with theory on the part of students of international politics suggests that there may be a strong urge toward independent disciplinary status among

the political scientists and historians who have (up to now) been the dominant students of the subject. The rationale for such an urge seems quite compelling, inasmuch as it is probably the only way that the student and the subject can be freed of the limitations and liabilities imposed by the incomplete knowledge, inadequate methodology, and often incorrect folklore of political science and history. But breaking away from the established disciplines is not nearly enough; the declaration of independence must be accompanied by earnest efforts to achieve self-government and become a viable scholarly enterprise. A self-conscious evaluation of the "state of the art" is an essential prerequisite. Illustrative of this self-examination are two recent collections of readings which focus on the problems of theory-building in the study of political relations among nations, edited by William T. R. Fox (3) and Stanley Hoffmann (4), respectively. While both are clearly addressed to the development of theory, they differ in several ways. What I should like to do is describe and evaluate them in terms of their genesis, organization,

content, and over-all contribution to the state of the discipline.

Origins—Intellectual and Otherwise

Professor Fox's *Theoretical Aspects of International Relations* seems to have a fairly respectable pedigree. The idea originated with Kenneth Thompson and Dean Rusk, of the Rockefeller Foundation, who invited ten others to join them in Washington for a series of discussions on the general subject of theory in international relations. Of the twelve, five were professional social scientists (Thompson, Fox, Morgenthau, Arnold Wolfers, and Don Price); two were eminent journalists (James Reston and Walter Lippmann); four were practitioners of sorts (Rusk, Paul Nitze, Robert Bowie, and Dorothy Fosdick); and one was a theologian (Reinhold Niebuhr).

Following that conference, which Professor Thompson has summarized so coherently elsewhere (18), Professor Fox arranged a series of three two-day meetings in New York. At these meetings five of the original twelve (Morgenthau, Fox, Wolfers, Nitze, and Niebuhr) presented papers based on the Washington conference, along with two newcomers (Kenneth Waltz and Charles Kindleberger); in Fox's words, the earlier sessions revealed that the "participants were talking about several different kinds of theories" and suggested the need "both for clarification of norms and for ordering of events." Thus the new series was scheduled in order that those concerned could "state their theoretical positions in terms so that the issues . . . would be clearly defined" and so that "lines of inquiry for clarifying problems and resolving differences could be sketched out" (pp. ix-x). The papers in the Fox book, therefore, represent an explicit attempt to enhance the development of theory

and are the culmination of a sustained and conscious effort in that direction.

Professor Hoffmann's *Contemporary Theory in International Relations*, however, seems to have originated in a somewhat different fashion. In the editor's own words, "the idea of this publication originated with Mr. Donald Hammonds," then a salesman for Prentice-Hall.¹ What Hoffmann has done, quite simply, is to take sixteen articles or book excerpts, all of which are easily available in the original, and link them together with partially edited sections of an article he had published earlier in *World Politics* (5). Though the discipline may turn out to have profited from this exercise, such profit must clearly be regarded as a subordinate and fortuitous by-product. So much for the genesis of these two volumes; let us now consider their organization.

Rationale of Structure

In a sophisticated critique of several contemporary textbooks a few years ago, the reviewer expressed sharp dissatisfaction with what he called the "freight-train pattern" of organizing such works (15). What he was criticizing is the all-too-frequent tendency to string together a number of topics ("boxcars") in rather indiscriminate fashion, providing no theoretically coherent rationale by which the book might have been structured.² Have the two volumes under consideration here escaped that weakness?

¹ That this is a publisher actively promoting the growth of theory in the field is attested to by another recent title: *The Theory and Practice of International Relations*; this, too, is a collection of articles drawn primarily from the conventional literature (10).

² This was the same reviewer who is a coeditor of the readings book (10) mentioned in n. 1. The table of contents is strikingly similar to that of the texts which had earlier been criticized.

In his *Theoretical Aspects of International Relations*, Fox admits that the papers are "not an orderly survey of the main problems of international relations theory," because each author was invited to "develop an essay on a topic of his own choosing" (p. x). He does, however, try to impose some order on the results by dividing them into three groups. The first deals with "the characteristics and uses of speculative analyses"; the second "examines the prospects for developing theories of international relations 'from the outside'"; and the third group address themselves to "substantive theoretical problems." Without engaging in a semantic quibble, one may nevertheless ask whether these are identifiable and mutually exclusive categories. Nor do the titles of the papers clarify the situation any. Only the two papers in the second group seem to have any attribute which makes them relatively comparable and at the same time distinguishes them from the others; here Kenneth Waltz discusses the value of the traditional political philosophers, and Charles Kindleberger draws some analogies from economics.

This absence of structure and scheme is, however, partially justified, as Fox himself chose not to impose any design on the other contributors, and thus found himself at the editorial stage confronted with something of a *fait accompli*. The *post facto* rationale is difficult to conjure up, and the results are often either Procrustean or (in this case) wispy.

Nor is Hoffmann's rationale any more rigorous. Even though all his papers were written beforehand, this editor, at least, had the choice of which articles and excerpts to use. The inevitable trinity appears this time in these guises: "International Relations as a Discipline"; "Contemporary Theories of International Relations"; and "Suggestions for the Study of International Relations."

While the contents of these parts will be discussed below, it is again pertinent to ask whether these are distinguishing and separable classifications.

Perhaps it should be emphasized that the absence of coherent structure in these two collections is not so much a function of thoughtlessness on the part of the editors as it is the fact that little of the writing on theory to the present is susceptible to any systematic breakdown. For example, how often do we find a given article or chapter which focuses on the role of hypotheses in theory-making, or on assumptions regarding discernible dimensions of the environment, or on empirical methodology, and so on? The answer is "very infrequently." Because few of us really understand the requirements of theory in general and international political theory in particular, our theory-oriented writings are often wide-ranging, impressionistic, and haphazard ruminations of a non-empirical nature. Having given our editors a suspended sentence on the charges of structural disorder, let us move on to our central concern—the contents themselves.

Contents and Substance: Fox

In reviewing a collection of readings, the inclination is to describe them in a collective fashion rather than individually. This may be the easier path, but it is often unfair to some of the contributors and unduly generous to others. And since there are only seven papers in Professor Fox's collection, it would be doubly remiss to treat them in so indiscriminate a manner; I will therefore attempt to note and evaluate the particularly outstanding characteristics of each in the order presented by the editor.

The first of the two articles which allegedly deal with the "characteristics and uses of speculative analyses" is Paul Nitze's "Necessary and Sufficient Elements of a

General Theory of International Relations." His major theses are three: a general theory must deal with the concepts of structure, purpose, and situation; it must permit a multiplicity of viewpoints; and it must deal with the realm of fact and the realm of value. There are two points raised by Mr. Nitze which seem to illustrate the low probability of our ever getting much in the way of a contribution to theory from those deeply involved in the formulation and execution of policy.³ The first is his demand that a theory be able to encompass the point of view of everyone from a responsible decision-maker to the "hypothetical observer from Mars" (p. 9). Were this not so common a view, it would be absurd. The fact is that any theoretical statement must proceed from one viewpoint, and from one viewpoint only—that of the observer. The theory may employ a phenomenological approach and include as its empirical data the perceptions, evaluations, and responses of the actors, but it cannot satisfy the requirements of objectivity and replicability while shifting the position and perspective of the observer; such observation must be made from a fixed and constant station.

Second, Nitze reiterates approvingly (p. 14) the "accepted maxim that politics is an art and not a science," and with this I am not prepared to quarrel here. As a matter of fact, the author himself calls for putting as much science and reason into politics as is possible. But there seems to be the inevitable mental spill-over and the implication that the *study* of politics is likewise an "art and not a science." Let it be stated flatly and categorically: there is, and must be, a world of difference between the *prac-*

³ As Hans Morgenthau points out in his paper, it is this "practical concern which has prevented the practitioners of international politics from developing an explicit theory of what they are doing" (3, p. 25).

tice of a particular activity and the *study* of that activity. Just as the biologist is in a far better position than the microbes he studies to understand and predict their behavior, the social scientist is more likely to understand and predict the behavior of states than their policy-makers. For too long have political scientists accepted the easy logic that, since the decisions and acts of political groups are not (or perhaps cannot be) scientific, neither are the observation and study of such groups.

The other paper in this part of *Theoretical Aspects of International Relations* is Hans J. Morgenthau's "The Nature and Limits of a Theory of International Relations." Professor Morgenthau begins refreshingly with a re-articulation of his well-known assumptions: "first, that for theoretical purposes international relations are identical with international politics; second, that a theory of international politics is but a specific instance of a general theory of politics; and that the latter is identical with political science" (p. 15). These assumptions have been too widely debated already to require re-examination here; with minor qualifications they are basically acceptable.⁴

From these assumptions, Morgenthau goes on to defend several theses. First, he insists that any body of theory requires a central concept, and for international relations that concept must be power; without this central concept, he argues, we might have the equivalent of a photograph "which shows everything that can be seen by the

⁴ My main reservation concerns the author's tendency to equate "the Western tradition of political thought" with "political theory." Political *philosophy* would seem a more accurate description of the works of Plato, Augustine, Montesquieu, Locke, etc. And, as Morgenthau himself points out, "philosophic knowledge may be, but is not of necessity, empirically verifiable" (10, p. 17).

naked eye." But the photograph is far less useful than a painted portrait which "shows one thing that the naked eye cannot see . . . the rational essence of its subject matter" (p. 17).

The main focus of this paper, however, is on the obstacles which stand in the way of theory development. The first of these is what he calls the "relativist conception of man and society," a conception which denies the "existence and intelligibility of objective, general truths in matters political." Once committed to the relativist view, one's empirical analysis is reduced to the mere "description of an ephemeral historic situation," and one's normative theory "becomes undistinguishable from political ideology" (pp. 18-19). On the other hand, after making the well-accepted comment that the task of theory is to "detect in the welter of the unique facts of experience, that which is uniform, similar, and typical," he sounds a rather relativistic note. Noting the difficulty of identifying all relevant elements in a situation, Morgenthau concludes that here we "can only play by ear and must be satisfied with a series of hunches which may or may not turn out to be correct" (p. 20). It is unfortunate that he fails to call in, at this point, the notion of probabilism. Accepting the fact that very little can be literally "known" or "proven" in the social sciences, the probabilist may nevertheless avoid the nihilism of agnosticism by falling back on a more-or-less actuarial approach to his phenomena. Perhaps Professor Morgenthau is, in an implicit way, aware of this solution when he notes elsewhere in the paper that "the truth of political science is of necessity a partial truth" (p. 21).

A second obstacle which he discovers in the path of theoretical growth is the moral position of the political scientist. Pointing out that the scholar is a member and product of his own society as well as an ob-

server, Morgenthau observes that he is confronted with "a choice between social advantage and the truth. . . . A political science which is true to its moral commitment ought at the very least to be an unpopular undertaking. At its very best, it cannot help being a subversive and revolutionary force with regard to certain vested interests—intellectual, political, economic, social in general" (p. 22). Amen!

The balance of his article is, as always, full of meaty and provocative observations, most of which have been made before. Suffice it to say, therefore, that here is a paper worth reading; for the political scientist it might stimulate an awareness of the possibilities (and pitfalls) which lie before us, and for other social scientists it reveals the concerns and *modus operandi* of one of the most fertile and stimulating minds in twentieth-century political science.

In chapter iii the editor himself takes the stand to discuss the relevance of theory for the making of policy; in one sense the Fox article is a justification for the entire collection. Entitled "The Uses of International Relations Theory," it argues that the development of theory would be a distinct but indirect asset to policy-making (presumably in *all* states). This would take the form of either broadening or constricting the range of choices which officialdom might be willing to consider; he cites the works of E. H. Carr (1) and Nicholas Spykman (16) as illustrative of theoretical contributions which have in the past served to "clarify choice by limiting it" (p. 30). The theorist can also serve policy by challenging "otherwise serious students of the social sciences whose views of world politics are uninformed by any tenable underlying theory" and preventing them from "crashing directly into the councils of high policy" (p. 32).

Professor Fox is not, however, carried away with his enthusiasm for the interna-

tional relations specialist's policy-advising role. First, he notes that the scholarly theorist may lack up-to-date information and the means to acquire it, access to final decision-makers, and something called "skill in effective writing addressed to persons of lesser influence." Furthermore, Fox warns that "if a first-class international relations scholar tries too hard to be *immediately* useful, he may only succeed in becoming a fourth-class journalist." He abandons his social science role if he "becomes a peddler of 'current events' or an apologist for the reigning priests of high policy" (p. 32).

Another of the more interesting points raised by this paper is that concerning the possible usefulness of the "non-political sciences" to theory in this area. Fox suggests that the anthropologists' demolition of racial supremacy notions and the evidence that man is a multiple-valued animal have first affected the scholars' image of world politics and then filtered up (or down?) to policy-makers. As to the relationship between disciplines, Fox observes:

This type of contribution to the continuing reconstruction of the scholar's image of world politics can be made on the initiative of the scholar himself, provided he has the curiosity to keep abreast of advances in the behavioral sciences and to ask questions of his non-international relations colleagues which he alone can ask and they alone can answer. It can also be made on the initiative of the non-international relations behavioral scientist, provided his knowledge of the matrix of thinking about world politics is sufficient to enable him to know what kinds of questions the international relations scholar would ask if he knew the data existed, or could be made to exist, to answer those questions.

In conclusion, the editor warns that "the social scientist must be aware both of his potentialities and his limitations as a *social scientist* if he is to perform his distinctive function, but he need be no moral eunuch to perform that function" (p. 49).

The first of the two articles designed to

suggest the possibility of outside help in the development of theory is Kenneth Waltz's "Political Philosophy and the Study of International Relations." Given Professor Waltz's predilection for the early Greek philosophers and their intellectual descendants, it might be appropriate to couch my remarks in the form of the familiar Socratic dialogue as follows:

CRITIC: Yes, the development of theory would certainly be desirable. To whom shall we turn for intellectual sustenance?

ORACLE: One can profit greatly from a close familiarity with the works of those commonly regarded as major figures in the history of political philosophy.

CRITIC: What makes these ancient sages particularly useful to us? Why not turn to biologists and mathematicians?

ORACLE: The problem of identifying and achieving the conditions of peace, a problem that plagues man and bedevils the student of international relations has, especially in periods of crisis, bedeviled political philosophers as well.

CRITIC: I see—they are more familiar with the subject. Well, how about the social scientists?

ORACLE: As Hume so well argued, it is a logical error to assume that certainty can be produced by piling up experimental data.

CRITIC: Is contemporary social science devoid of theoretical content? Professor Merton . . .

ORACLE: Proceeding in this manner one may, no doubt, produce systematic theory. . . . And how does one formulate the middle-range theories?

CRITIC: Theories must be preceded by hypotheses, and I find the hypotheses of social scientists more relevant, unambiguous, parsimonious, and operational . . .

ORACLE: Say, aren't you the one who berated me (19) on this question before (13)?

The other paper in this section is Charles P. Kindleberger's "International Political Theory from Outside." This eminent economist, who is no stranger to international politics (8), offers a number of intriguing

interdisciplinary analogies. The most attractive is that concerning power, profits, and the long- and short-run interests of commercial firms and nation-states, both pairs of which are in relationships of competition as well as cooperation. What he seems to be saying is that for the firm and the nation, the short-run interests are profit and power, respectively, but that the long-run interest is survival. "Profit is clearly the object of short-run maximization in economics. But in the long-run, this will not do. . . . The Aluminum Company of America can rejoice in the success of Reynolds and Kaiser, although anti-trust laws may prevent it from extending development loans and granting technical assistance" (pp. 80, 82). Just as the "long-run maximizing oligopolist" gives subsidies to schools and hospitals, pays its employees well, often holds prices down, and fears for the bankruptcy of its competitor, in order to enhance its own survival, Kindleberger suggests that states might shift from their short-run pursuit of power, prestige, colonies, satellites, etc., to the "long-run interest in survival as members of the world community." He concludes that, if both the Soviet and the United States were to take this longer view, "another opportunity would be afforded for a demonstration that when economic and political man maximizes in the long run, rather than the short, it is impossible to distinguish him from a Christian" (p. 82).

The paper may be unsystematic and chaotic, but it is full of suggestive analogies; if it was included for its heuristic value alone, the choice was a happy one.⁵

We now turn to the last part of the collection, which deals "more or less directly

with substantial theoretical problems." The first of these is Arnold Wolfers' "The Actors in International Politics," probably the most valuable contribution to theory in the entire book. Professor Wolfers is concerned with one specific and well-identified question: Which is the most accurate and fruitful level of analysis—the individual, the state, or extra-national institutions? In seeking to identify the most useful focus, he makes a highly persuasive case for the traditional state-as-actor focus. This focus, he points out, rests on the assumptions that (a) "all men acting for states share the same universal traits," and (b) "the anarchical multistate system creates a condition of constant danger . . . and . . . provides frequent opportunity for new acquisitions," leading statesmen to act "under external compulsion rather than in accordance with their preferences" (p. 93). Given the basic similarity in human propensities when faced with dangers (to escape from a house which is on fire) or opportunity (moving up to the race track rail when an opening appears in the crowd), Wolfers concludes that most decision-makers—and hence most states—will respond in essentially similar fashion to these two ubiquitous situations in the global arena.

However, the author is not completely satisfied with this state-oriented focus and suggests that concern with a smaller unit of analysis (the individual) or a larger unit (extra-national agencies) might be quite justified. The former tendency he attributes to the belief—embodied in the UNESCO approach—that the state-as-actor model emphasizes and extols the ahuman interests of the state at the expense of "genuine human needs." But he then points out that individuals play a multitude of roles and seek to maximize a number of needs; "often enough he (the individual citizen) is ready to compromise his own well-being for the

⁵ For another economist whose suggestions are equally provocative, more systematic, but non-empirical see Thomas C. Schelling (11, 12).

benefit of the groups and organizations with which he identifies himself" (p. 86). "Whether a state has a 'vital interest' . . . depends on the relative values attached by its citizens to these national objectives, on the one hand, and to private interests which would be sacrificed in the pursuit of the national objectives, on the other." Wolfers also observes that the decision-making approach (which is a variation on the individual-as-actor theme) results from dissatisfaction with the gross and sweeping generalizations on which the state-oriented approach has rested. And, while admitting the distinction between propensities, on the one hand, and actual behavior, on the other, he concludes that in non-crisis situations states "find it expedient to act according to established rules," and as they are "drawn to the pole of complete compulsion, the more they can be expected to conform in their behavior and to act in a way that corresponds to the deductions that can be made from the states-as-actors model" (pp. 96-97).

As to the utility of the corporate, extra-national focus, Wolfers argues that, because corporate bodies other than states "play a role on the international stage as co-actors," they deserve some degree of attention. On the other hand, an empirical investigation would indicate that the degree to which the United Nations, NATO, ECSC, the Arab League, or Aramco "affect the course of international events" is still quite limited; only when they become able to "operate as international or transnational actors" can they be treated as such in a theoretical context. It is not enough that they become merely new instruments of national policy (p. 104).

Professor Wolfers concludes, then, that "while it would be dangerous for theorists to direct their primary attention from the nation-state and multi-state systems which

continue to occupy most of the stage of contemporary world politics, theory remains inadequate if it is unable to include such phenomena as overlapping authorities, split loyalties, and divided sovereignty . . ." (p. 106). If the phenomena which deviate from the billiard-ball model can be dealt with in an empirical and theoretical way and then combined with a state-as-actor theory, we will have legitimate claim to a realistic theory of international politics. The author has dealt with a crucial question in a cogent and informed fashion and provides the scholar with what is perhaps the best discussion of the actor-focus problem in the literature to date.⁶

In the final paper, Reinhold Niebuhr offers the sort of essay which we have come to expect from his gifted pen. Gracefully written and (to this reviewer) wise in its conclusions, "Power and Ideology in National and International Affairs" is a somewhat ambitiously titled discussion of the rise and rationale of democratic and autocratic political systems, their reliance on force, and their accompanying belief patterns. While Wolfers' article does not claim to offer any theory, this second paper of the last section clearly does; furthermore, the author finds some important policy recommendations in his model. Basically, Niebuhr's thesis seems to be that, since power is as much a function of ideological conviction as of physical force and since the Communist appeal is heavily reliant on ideological dogma, the West must present an image which refutes the Marxian claim of European colonialism. He therefore urges that

⁶ This is not to detract from the extremely valuable study by Waltz (19), as the latter goes beyond the question of identifying the most useful actor-focus and deals with that of whether a system-oriented focus is not perhaps more useful than *any* actor orientation, be it individual, state, or corporate agency.

the United States eschew its tendency to claim innocence of colonialism by "fastening the charge" on its allies, even inferentially. "We are inextricably bound up with the fate of Europe [and its past?] and we cannot avert this fate by calling attention to the ideological differences between us on the matter of colonialism." Rather, he suggests distinguishing between Britain, which has "creatively extricated" itself from previous colonialism, and France, which is "hopelessly bogged down" in it (p. 117). And by a more intimate alliance with Britain, the "free nations" would be provided with a core whose ultimate prestige might be sufficient to "dissuade France from her present course which has such catastrophic consequences in the ideological struggle." Combined with an expected increase in Hungary-type acts on the Soviet side, such an approach might be expected to turn the ideological tide in favor of democracy.

It is difficult to find fault with the paper, yet one wonders whether it is appropriate in a collection such as this. While it may be a well-conceived and interesting essay, it is hardly a contribution to the growth of theory in the field. The model lacks precision, is almost devoid of empirical referents, and is more concerned with a problem in national rather than international politics. Nor is the reviewer quite sure that the "theorizing" and the prescribing enjoy a particularly intimate relationship. Perhaps this paper got confused with one of the many Dr. Niebuhr is always writing for certain monthly magazines or the quasi-intellectual supplement which accompanies a highly regarded newspaper every Sunday!

Contents and Substance: Hoffmann

If the reader faces with apprehension a rundown of the Hoffmann articles as detailed as those in the Fox collection, let him relax. Whereas the papers in *Theoretical*

Aspects of International Relations were written exclusively for that publication, those in *Contemporary Theory in International Relations* should all be familiar to the serious student. Thus it should suffice merely to comment on the rationale of their selection.

The articles by Frederick S. Dunn and Kenneth W. Thompson have already been reprinted frequently, perhaps too frequently. Appearing in Part I ("International Relations as a Discipline"), the Dunn article defines the area of inquiry, while that by Thompson—as was noted earlier—summarizes the papers presented at the seminar out of which the Fox book later emerged. In Part II ("Contemporary Theories") we are presented with an uneven array of eight articles or excerpts. Of these, only the ones from Kaplan and Liska approximate "theories," and the excerpts are too incomplete to do justice to the books (7, 9), even if the original sources had been coherent and comprehensible treatises. (Without deprecating their scholarly contributions, neither could be termed an adequate piece of communication.)⁷ Jessie Bernard's paper, excerpted from her important article in *The Nature of Conflict* (6), is misleadingly entitled "The Sociological Study of Conflict"; it turns out to be a critique of a number of mathematical approaches. The excerpt from the Snyder, Bruck, and Sapin monograph is the rationale of their decision-making approach (14) and is too familiar to require further exegesis here.

The most useful part of the collection, pedagogically and theoretically, is the final one, entitled "Suggestions for the Study of International Relations." Again, however, three of the six are from well-known books,

⁷ For a highly intelligent review of these two attempts at theory see Kindleberger's article in *World Politics* (8).

two are from widely read journals, and only one (Herbert Kelman's "Societal, Attitudinal and Structural Factors in International Relations") is likely to have escaped the attention of the conscientious scholar or graduate student; this excellent piece was in the *Journal of Social Issues*.

As to the lengthy commentary which Professor Hoffmann has included in the book, the reviewer cannot agree with those who criticize so great an intrusion by the editor. However, it is essentially the same material which he presented in an earlier article; equally verbose and circuitous and just as full of meaty, provocative, and valuable insights. This reviewer would quarrel sharply with some of Hoffmann's arguments, but this is not the appropriate place.

Conclusions

Of the collections under review, only that prepared by Professor Fox represents a quantitative addition to the literature; regardless of its other attributes, it provides us with some materials which might otherwise not have been available. Such cannot be said of Hoffmann's book. Despite the admitted pedagogical usefulness of having well-known papers in a single volume (the reviewer used both these collections in a graduate seminar on research and theory), one still wonders whether this alone justifies its publication; perhaps this is purely a commercial decision and should be left to the publishers.

The more important question, however, is whether either collection really makes a visible and direct contribution to the development of a theory of international political relations. With only the barest hesitation (recalling Wolfers' paper) this reviewer would have to answer in the negative. This is partly due to the non-scientific characteristics of the collective papers and commentary, but only partly. The major reason

would seem to lie in the triviality of much of our contemporary theorizing about theory. Most such theorizing is sterile and repetitive of the old clichés and bromides; only rarely does such an attempt produce results of value.

Thus, despite the healthy self-examination which such collections reflect and the admitted convenience of having them in single bindings, the dividends are small. We have had too many books, articles, lectures, and conferences devoted to theorizing about theory. Now is the time to declare a moratorium on preaching the virtues of theory and discussing what we could do with good theory and how we might perhaps develop it. It is high time that we replace preaching with practice. Let me close, then, by exploiting the critic's privilege (or license) and doing a bit of "last word" preaching myself.

There are three general propositions which may be worth some brief consideration. First, we must divest ourselves of the notion that certain forms of research are inherently more useful than others. Here I refer to the level of generalization at which one prefers to work. It seems to me that the scholar who works with meticulous care in original documents dealing with a rather limited series of events can be just as useful as one who is consciously concerned with theorizing about a large and sweeping class of events or phenomena. Moreover, those of the latter, who merely engage in arm-chair speculation, spinning grand schemes without so much as a glance at the mountains of empirical data at hand are no more constructive than those who gather every minute detail in a haphazard or purely chronological fashion, letting "the facts speak for themselves." The one is disconnected from reality, and the other is without meaning.

Second, when engaged in the gathering of specific empirical data, let us do so in such a fashion that we *do* contribute to theory. This requires that we have specific hypotheses in mind when we design the study, and it requires that the design be such that the hypotheses may be put to the test. This, in turn, demands that we not only make our hypotheses explicit but that they be specific and operational; i.e., the terms must be unambiguous, the independent and dependent variables must be well-defined and grounded in the data, and the causal or correlative relationships logically interconnected. Furthermore, we might do well to regard our empirical data not as unique and discrete but rather as belonging to a particular case in a general class of cases, all of which have something in common. This is not to suggest that we should assume that all declarations of war are homologous or that bilateral negotiations are merely labor-management negotiation writ large; it is to indicate that the search for analogy and comparison is the most fruitful way of converting isolated research at a low level of generalization into work of theoretical significance.

Third, when we work at higher levels of generalization, we might do well to reflect upon the efforts of our predecessors; our current precociousness seems to have led to the cavalier dismissal of such productive scholars as Quincy Wright, Hans Morgenthau, Frederick Schuman, or Georg Schwarzenberger. And when we do not ignore such writers, we often dissipate our energies in destructive and carping criticism. The work of these and other students is there for us to exploit; let us take that which is valuable and build from there. Though the field has suffered from some theoretical poverty, it has not been entirely destitute. Again, regarding research at the middle and upper

levels of generalization, there is available to us a body of empirical data which should take a generation to utilize. Though it be scattered, disorganized, and incoherent, it could be codified and converted into an impressive array of propositions and hence made susceptible to theoretical synthesis.

So much for theorizing about theory. The best minds in our field have more urgent tasks than the belaboring of familiar generalities; all agree that the growth of theory is both desirable and feasible. Let us get on with the job!

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