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An American Social Science: International Relations

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STANLEY HOFFMANN

## An American Social Science: International Relations

IN THE PAST THIRTY YEARS, international relations has developed as a largely autonomous part of political science. Even though it has shared many of political science's vicissitudes—battles among various orientations, theories, and methods—it also has a story of its own. What follows is an attempt at neither a complete balance sheet nor a capsule history—merely a set of reflections on the specific accomplishments and frustrations of a particular field of scholarship.<sup>1</sup>

### *Only in America*

Political science has a much longer history than international relations. The attempt at studying systematically the patterns of conflict and cooperation among mutually alien actors—a shorthand definition of the subject matter—is recent. To be sure, we can all trace our ancestry back to Thucydides, just as political scientists can trace theirs to Aristotle. But Thucydides was a historian. He was, to be sure, a historian of genius, rightly convinced that he was writing for all times because he was using one particular incident to describe a permanent logic of behavior. Yet he was careful to avoid explicit generalizations, “if . . . then” propositions, and analytic categories or classificatory terms. Modern sociology and political science emancipated themselves from political and social history, political philosophy, and public law in the nineteenth century. International relations did not, even though the kind of social (or asocial) action described by Thucydides never disappeared from a fragmented world, and flourished particularly in the period of the European balance of power. One can wonder why this was so. After all, here was a realm in which political philosophy had much less to offer than it did to those who wondered about the common good in the domestic order. Except for the vast body of Roman Catholic literature preoccupied with just war, and not very relevant to a world of sovereign states, there were only the recipes of Machiavelli; the marginal comments on the international state of nature in Hobbes', Locke's, and Rousseau's writings; some pages of Hume; two short and tantalizing essays of Kant; compressed considerations by Hegel; and oversimplified fragments by Marx. Even so, the little political philosophy that was available should have been sufficiently provocative to make students want to look into the realities. For the philosophers disagreed about the nature of the international milieu and the ways of making it

more bearable; and they wrote about the difference between a domestic order stable enough to afford a search for the ideal state, and an international contest in which order has to be established first, and which often clashes with any aspiration to justice. Similarly, the contrast between the precepts of law and the realities of politics was sufficiently greater in the international realm than in the domestic realm, to make one want to shift from the normative to the empirical, if only in order to understand better the plight of the normative. Without a study of political relations, how could one understand the fumbblings and failures of international law, or the tormented debates on the foundation of obligation among sovereigns unconstrained by common values or superior power? And the chaos of data provided by diplomatic history did not require any less ordering than the masses of facts turned up by the history of states and societies.

Why did a social science of international relations nevertheless fail to appear? The answer to the discrepancy may well be found in that sweeping phenomenon which Tocqueville identified as the distinctive feature of the modern age: democratization. As domestic societies moved from their Old Regimes to their modern conditions—parties and interests competing for the allegiance of large classes of citizens; the social mobilization of previously dispersed subjects; the politics of large agglomerations and unified markets; an increasingly universal suffrage; the rise of parliamentary institutions or plebiscitarian techniques; the fall of fixed barriers, whether geographic or social, within nations—the study of flux began in earnest, if only in order to provide concerned observers and insecure officials with some clues about regularities and predictions of somewhat less mythical, if also less sweeping nature than those grandiosely strewn around by philosophers of history. With democratization, as Comte had predicted, came the age of positivism (his only mistake was to confuse his own brand of metaphysics, or his grand speculations, with positive science). But international politics remained the sport of kings, or the preserve of cabinets—the last refuge of secrecy, the last domain of largely hereditary castes of diplomats.

Raymond Aron has characterized international relations as the specialized activity of diplomats and soldiers. However, soldiers, to paraphrase Clausewitz, have their own grammar but not their own logic. It is not an accident if armies, having been democratized by the ordeals of the French Revolution and Napoleonic era, found their empirical grammarian in Clausewitz, whereas the still restricted club of statesmen and ambassadors playing with the fate of nations found no logician to account for its activities. Indeed, the historians who dealt with these succeeded only in keeping them beyond the pale of the kind of modern science that was beginning to look at societies, by perpetuating the myth of foreign policy's "primacy," isolated from domestic politics. There was, to be sure, one country in which foreign policy was put under domestic checks and balances, knew no career caste, and paid little respect to the rules and rituals of the initiated European happy few: the United States of America. But this country happened to be remarkably uninvolved in the kinds of contests that were the daily fare of other actors. Either it remained aloof, eager merely for continental consolidation and economic growth; or else it expanded, not by conflicts and deals with equals, but by short spurts of solipsistic exuberance at the expense of much weaker neighbors. International relations is the science of the tests and

trials of several intertwined actors. Where they were intertwined, no science grew. In the United States before the 1930s, there was no reason for it to grow.

It was only the twentieth century that brought democratization to foreign policy. Diplomatic issues moved from the calculations of the few to the passions of the many, both because more states joined in the game that had been the preserve of a small number of (mainly European) actors and (mainly extra-European) stakes, and above all because within many states parties and interests established links or pushed claims across national borders. And yet, a World War that saw the mobilization and slaughter of millions, marked the demise of the old diplomatic order, and ended as a kind of debate between Wilson and Lenin for the allegiance of mankind, brought forth little "scientific analysis" of international relations. Indeed, the rude intrusion of grand ideology into this realm gave a new lease of life to utopian thinking, and delayed the advent of social science. Not "how it is, and why," but "how things should be improved, reformed, overhauled," was the order of the day. Old Liberal normative dreams were being licensed by the League of Nations covenant, while at the same time the young Soviet Union was calling for the abolition of diplomacy itself.

It is against this reassertion of utopia, and particularly against the kind of "as if" thinking that mistook the savage world of the 1930s for a community, the League for a modern Church, and collective security for a common duty, that E. H. Carr wrote the book which can be treated as the first "scientific" treatment of modern world politics: *Twenty Years Crisis*<sup>2</sup>—the work of a historian intent on deflating the pretenses of Liberalism, and driven thereby to laying the foundations both of a discipline and of a normative approach, "realism," that was to have quite a future. Two paradoxes are worth noting. This historian who was founding a social science, did it in reaction against another historian, whose normative approach Carr deemed illusory—Toynbee, not the philosopher of the *Study of History*, but the idealistic commentator of the *Royal Yearbook of International Affairs*. And Carr, in his eagerness to knock out the illusions of the idealists, not only swallowed some of the "tough" arguments which the revisionist powers such as Mussolini's Italy, Hitler's Germany, and the militaristic Japan had been using against the order of Versailles—arguments aimed at showing that idealism served the interests of the status quo powers—but also "objectively," as Pravda would say, served the cause of appeasement. There was a triple lesson here: about the springs of empirical analysis (less a desire to understand for its own sweet sake, than an itch to refute); about the impossibility, even for opponents of a normative orientation, to separate the empirical and the normative in their own work; and about the pitfalls of any normative dogmatism in a realm which is both a field for objective investigation and a battlefield between predatory beasts and their prey.

But it was not in England that Carr's pioneering effort bore fruit. It was in the United States that international relations became a discipline. Both the circumstances and the causes deserve some scrutiny. The circumstances were, obviously, the rise of the United States to world power, a rise accompanied by two contradictory impulses: renewed utopianism, as exemplified by the plans for postwar international organization; and a mix of revulsion against, and guilt about, the peculiar prewar brew of impotent American idealism (as symbolized by the "nonrecognition" doctrine), escapist isolationism (the neutrality laws),

and participation in appeasement. Two books brought to America the kind of realism Carr had developed in England. One was Nicholas Spykman's *America's Strategy in World Politics*,<sup>3</sup> which was more a treatise in the geopolitical tradition of Admiral Mahan or Mackinder than a book about the principal characteristics of interstate politics; but it told Americans that foreign policy is about power, not merely or even primarily about ideals, and it taught that the struggle for power was the real name for world politics. The other book was Hans Morgenthau's *Politics Among Nations*.<sup>4</sup> If our discipline has any founding father, it is Morgenthau. Unlike Carr, he was not a historian by training; he had been a teacher of international law. Like Carr, he was revolting against utopian thinking, past and present. But where Carr had been an ironic and polemical Englishman sparring with other Englishmen about the nature of diplomacy in the thirties—a discussion which assumed that readers knew enough diplomatic history to make pedantic allusions unnecessary—Morgenthau was a refugee from suicidal Europe, with a missionary impulse to teach the new world power all the lessons it had been able to ignore until then but could no longer afford to reject. He was but one participant in the “sea change,” one of the many social scientists whom Hitler had driven to the New World, and who brought to a country whose social science suffered from “hyperfactualism” and conformity the leaven of critical perspectives and philosophical concerns.<sup>5</sup> But he was, among his colleagues, the only one whose interests made him the founder of a discipline.

Eager to educate the heathen, not merely to joust with fellow literati, Morgenthau quite deliberately couched his work in the terms of general propositions and grounded them in history. Steeped in a scholarly tradition that stressed the difference between social sciences and natural sciences, he was determined both to erect an empirical science opposed to the utopias of the international lawyers and the political ideologues, and to affirm the unity of empirical research and of philosophical inquiry into the right kind of social order. He wanted to be normative, but to root his norms in the realities of politics, not in the aspirations of politicians or in the constructs of lawyers. The model of interstate relations which Morgenthau proposed, and the precepts of “realism” which he presented as the only valid recipes for foreign policy success as well as for international moderation, were derived from the views of nineteenth-century and early twentieth-century historians of statecraft (such as Treitschke, and also Weber). Hence the paradox of introducing to the America of the cold war, and of making analytically and dogmatically explicit, notions and a “wisdom” about statecraft that had remained largely implicit in the age to which they best applied, and whose validity for the age of nuclear weapons, ideological confrontations, mass politics, and economic interdependence was at least open to question.

Be that as it may, Morgenthau's work played a doubly useful role—one that it may be hard to appreciate fully if one looks at the scene either from the outside (as does Aron), or thirty years later, as does the new generation of American scholars. On the one hand, his very determination to lay down the law made Morgenthau search for the laws, or regularities, of state behavior, the types of policies, the chief configurations of power; by tying his sweeping analyses to two masts, the concept of power and the notion of the national interest, he was boldly positing the existence of a field of scientific endeavor, separate from history or law. On the other hand, the very breadth of his brushstrokes, the

ambiguities hidden by his peremptory pronouncements about power, the subjective uncertainties denied by his assertion of an objective national interest, and even more the sleights of hand entailed by his pretense that the best analytic scheme necessarily yields the only sound normative advice—all of this incited readers to react and, by reacting, criticizing, correcting, refuting, to build on Morgenthau's foundations. Those who rejected his blueprint were led to try other designs. He was both a goad and a foil. (Indeed, the more one agreed with his approach, the more one was irritated by his flaws, and eager to differentiate one's own product). A less arrogantly dogmatic scholar, a writer more modest both in his empirical scope and in his normative assertions, would never have had such an impact on scholarship. Less sweeping, he would not have imposed the idea that here was a realm with properties of its own. Less trenchant, he would not have made scholars burn with the itch to bring him down a peg or two. One of the many reasons why Raymond Aron's monumental *Peace and War*<sup>6</sup>—a book far more ambitious in its scope and far more sophisticated in its analyses than *Politics Among Nations*—incited no comparable reaction from scholarly readers may well have been the greater judiciousness and modesty of Aron's normative conclusions. Humane skeptics invite nods and sighs, not sound and fury; and sound and fury are good for creative scholarship. Moreover, Aron's own scholarship was overwhelming enough to be discouraging; Morgenthau's was just shaky enough to inspire improvements.

Still, *Politics Among Nations* would not have played such a seminal role, if the ground in which the seeds were planted had not been so receptive. The development of international relations as a discipline in the United States results from the convergence of three factors: intellectual predispositions, political circumstances, and institutional opportunities. The intellectual predispositions are those which account for the formidable explosion of the social sciences in general in this country, since the end of the Second World War. There is, first, the profound conviction, in a nation which Ralf Dahrendorf has called the Applied Enlightenment,<sup>7</sup> that all problems can be resolved, that the way to resolve them is to apply the scientific method—assumed to be value free, and to combine empirical investigation, hypothesis formation, and testing—and that the resort to science will yield practical applications that will bring progress. What is specifically American is the scope of these beliefs, or the depth of this faith: they encompass the social world as well as the natural world, and they go beyond the concern for problem-solving (after all, there are trial-and-error, piecemeal ways of solving problems): they entail a conviction that there is, in each area, a kind of masterkey—not merely an intellectual, but an operational paradigm. Without this paradigm, there can be muddling through, but no continuous progress; once one has it, the practical recipes will follow. We are in the presence of a fascinating sort of national ideology: it magnifies and expands eighteenth-century postulates. What has ensured their triumph and their growth is the absence of any counterideology, on the Right or the Left, that challenges this faith either radically (as conservative thought did, in Europe) or by subordinating its validity to a change in the social system. Moreover, on the whole, the national experience of economic development, social integration, and external success has kept reinforcing this set of beliefs.

Second, and as a kind of practical consequence, the very prestige and sophis-

tication of the "exact sciences" were bound to benefit the social ones as well. The voices of gloom or skepticism that lament the differences between the natural world and the social world have never been very potent in America. Precisely because the social world is one of conflict, precisely because national history had entailed civil and foreign wars, the quest for certainty, the desire to find a sure way of avoiding fiascoes and traumas, was even more burning in the realm of the social sciences. The very contrast between an ideology of progress through the deliberate application of reason to human concerns—an ideology which fuses faith in instrumental reason and faith in moral reason—and a social reality in which the irrational often prevails both in the realm of values and in the choice of means, breeds a kind of inflation of social science establishments and pretensions. At the end of the war, a new dogma appeared. One of the social sciences, economics, was deemed to have met the expectations of the national ideology, and to have become a science on the model of the exact ones; it was celebrated for its contribution to the solution of the age-old problems of scarcity and inequality. This triumph goaded the other social sciences. Political science, the mother or stepmother of international relations, was particularly spurred. It was here that the temptation to emulate economics was greatest. Like economics, political science deals with a universal yet specialized realm of human activity. Its emphasis is not on the origins and effects of culture, nor on the structures of community or of voluntary association, but on the creative and coercive role of a certain kind of power, and on its interplay with social conflict. This also drew it closer to that other science of scarcity, competition, and power, economics, than to disciplines like anthropology or sociology, which deal with more diffuse phenomena and which are less obsessed by the solution of pressing problems by means of enlightened central action.

Nations in which this grandiose and activist ideology of science is less overwhelming have also known, after the Second World War, a considerable expansion of the social sciences. But the United States often served as model and as lever.<sup>8</sup> And political science abroad has usually been more reflective than reformist, more descriptive than therapeutic; although, here and in sociology, foreign social scientists reacted against the traditional intelligentsia of moralists, philosophers, and aesthetes by stressing that knowledge (not old-fashioned wisdom) *was* power (or at least influence), they were not driven by the dream of knowledge *for* power. Moreover, when (inevitably) disillusionment set in, it took often far more drastic forms—identity crises within the professions, violent indictments outside—than in the United States. An ideology on probation cannot afford a fall. An ideology serenely hegemonial reacts to failure in the manner of the work horse in Orwell's *Animal Farm*, or of Avis: "I will try harder."

A third predisposition was provided by a transplanted element: the scholars who had immigrated from abroad. They played a huge role in the development of American science in general. This role was particularly important in the social sciences. Here, they provided not merely an additional injection of talent, but talent of a different sort. No social science is more interesting than the questions it asks, and these were scholars whose philosophical training and personal experience moved them to ask far bigger questions than those much of American social science had asked so far, questions about ends, not just about means; about choices, not just about techniques; about social wholes, not just about

small towns or units of government. So they often served as conceptualizers, and blended their analytic skills with the research talents of the “natives.” Moreover, they brought with them a sense of history, an awareness of the diversity of social experiences, that could only stir comparative research and make something more universal of the frequently parochial American social science. In the field of international relations, in addition to Morgenthau, there was a galaxy of foreign-born scholars, all concerned with transcending empiricism: the wise and learned Arnold Wolfers, Klaus Knorr, Karl Deutsch, Ernst Haas, George Liska, and the young Kissinger and Brzezinski, to name only a few. They (and quite especially those among them who had crossed the Atlantic in their childhood or adolescence) wanted to find out the meaning and the causes of the catastrophe that had uprooted them, and perhaps the keys to a better world.

The last two names bring us to politics. And politics mattered. Hans Morgenthau has often written as if truth and power were bound to be enemies (Hannah Arendt has been even more categorical). And yet he shaped his truths so as to guide those in power. The growth of the discipline cannot be separated from the American role in world affairs after 1945. First, by definition (or tautology), political scientists are fascinated with power—either because they want it, at least vicariously, or because they fear it and want to understand the monster, as Judith Shklar has suggested with her usual devastating lucidity.<sup>9</sup> And in the postwar years, what part of power was more interesting than the imperial bit? America the sudden leader of a coalition, the sole economic superpower, the nuclear monopolist, later the nuclear superior, was far more interesting to many students than local politics, or the politics of Congress, or the politics of group pluralism. Almost inevitably, a concern for America’s conduct in the world blended with a study of international relations, for the whole world seemed to be the stake of the American-Soviet confrontation. Here was a domain which was both a virgin field for study and the arena of a titanic contest. To study United States foreign policy was to study the international system. To study the international system could not fail to bring one back to the role of the United States. Moreover, the temptation to give advice, to offer courses of action, or to criticize the official ones was made even more irresistible by the spotty character and the *gaffes* of past American behavior in world affairs, by the thinness of the veneer of professionalism in American diplomacy, by the eagerness of officialdom for guidance—America was the one-eyed leading the cripples. Thus, two drives merged, for the benefit of the discipline and to its detriment also, in some ways: the desire to concentrate on what is the most relevant, and the tendency (implicit or explicit) to want to be useful, not only as a scientist, but as an expert citizen whose science can help promote intelligently the embattled values of his country (a motive that was not negligible, among newcomers to America especially). For it was all too easy to assume that the values that underlie scientific research—the respect for truth, freedom of investigation, of discussion, and of publication—were also those for which Washington stood in world affairs.

Second, as I have just said, what the scholars offered, the policy-makers wanted. Indeed, there is a remarkable chronological convergence between their needs and the scholars’ performances. Let us oversimplify greatly. What the leaders looked for, once the cold war started, was some intellectual compass



which would serve multiple functions: exorcise isolationism, and justify a permanent and global involvement in world affairs; rationalize the accumulation of power, the techniques of intervention, and the methods of containment apparently required by the cold war; explain to a public of idealists why international politics does not leave much leeway for pure good will, and indeed besmirches purity; appease the frustrations of the bellicose by showing why unlimited force or extremism on behalf of liberty was no virtue; and reassure a nation eager for ultimate accommodation, about the possibility of both avoiding war and achieving its ideals. "Realism," however critical of specific policies, however (and thus self-contradictorily) diverse in its recommendations, precisely provided what was necessary. Indeed, there was always a sufficient margin of disagreement between its suggestions and actual policies, and also between its many champions, to prevent it from being nothing but a rationalization of cold war policies. And yet the first wave of writings—those of Morgenthau, Wolfers, Kissinger, Kennan, Osgood, Walt Rostow, or McGeorge Bundy—gave both the new intellectual enterprise and the new diplomacy the general foundations they needed. The second wave—roughly, from 1957 to the mid-1960s—turned strategy in the nuclear age into a dominant field within the discipline. This coincided with the preoccupation of officialdom to replace the reassuring but implausible simplicities of massive retaliation with a doctrine that would be more sophisticated; but it also reflected the conviction that force, in a mixture of nuclear deterrence and conventional (or subconventional) limited uses, remained both the most important aspect of power and a major American asset. Here again, in the literature, the attempt at finding principles for any "strategy of conflict" in a nuclear world is inseparable from the tendency to devise a strategy for America, at a time when both sides had weapons of mass destruction, and when there were serious problems of alliance management, guerrilla wars, or "wars of national liberation." A third wave is quite recent: I refer to the growing literature on the politics of international economic relations. It coincides with what could be called the post-Viet Nam aversion for force, and with the surge of economic issues to the top of the diplomatic agenda, caused by a combination of factors: the degradation of the Bretton Woods system, the increasing importance of economic growth and social welfare in the domestic politics of advanced societies, the resurgence of aggressive or protectionist impulses in order to limit the bad effects or to maximize the gains from interdependence, the revolt of the Third World. Once more, the priorities of research and those of policy-making blend.

The political preeminence of the United States is the factor I would stress most in explaining why the discipline has fared so badly, by comparison, in the rest of the world (I leave aside countries like the Soviet Union and China, in which it would be hard to speak of free social science scholarship!). Insofar as it deals primarily with the contemporary world, it seems to require the convergence of a scholarly community capable of looking, so to speak, at global phenomena (i.e., of going beyond the study of the nation's foreign policy, or of the interstate politics of an area) and of a political establishment concerned with world affairs; each one then strengthens the other. When the political elites are obsessed only with what is happening to their country, because it lacks the power to shape what is happening elsewhere, or because this lack of power has

bred habits of dependence on another state (such as the United States), or because (as in the case of Japan and West Germany) there are severe constraints on the global use of the nation's power, the chances are that the scholars will not have the motivation or receive the impulse necessary to turn individual efforts into a genuine scientific enterprise, and will either turn to other fields with more solid traditions and outlets (such as, say, electoral behavior in France and Britain) or merely reflect, more or less slavishly, and with some delays, American fashions; or else there will be often brilliant individual contributions, but unconnected and unsupported: a Hedley Bull in Australia (and England), a Pierre Hassner in France, to name just these two, do not make a discipline. Even in England and France, which have become nuclear powers, strategic studies have been to a very large extent the preserve of a few intellectual military men, concerned either with reconciling national policy with the predominant doctrines of deterrence, or with challenging these. But the predominant doctrines have remained American, as if even in the more abstract efforts at theorizing about a weapon that has transformed world politics, it mattered if one was the citizen or host of a country with a worldwide writ. Scholars do not like to think about their intellectual dependence on the status of their country, and on the ambitions of its political elite; it disturbs their sense of belonging to a cosmopolitan, free-floating community of science. Even the sociology of knowledge, which has often looked at the debts of scholars to their countries, has been singularly coy about this particular kind of bond. And yet, the link exists. And it is sometimes reinforced by institutional arrangements.

In the case of the United States, there have been three institutional factors that have acted as multipliers of political connection—factors which have not existed, and certainly not simultaneously, elsewhere. One is the most direct and visible tie between the scholarly world and the world of power: the “in-and-outer” system of government, which puts academics and researchers not merely in the corridors but also in the kitchens of power. Actually, it may be wise to distinguish two phases. In the late forties and fifties, those kitchens remained the preserves of the old establishment: a mix of career civil servants, businessmen, and lawyers. They had to cope with the whole world, with a persistent enemy, with the travails of economic reconstruction and the turmoil of nuclear deterrence. They needed both data and ideas, and they turned to the universities. This was the age of the academic as consultant (officially or not), and this was the period in which much research got funded by those departments that had the biggest resources (Defense more than State). The year 1960 was a turning point. Academics became proconsuls and joined the old boys; often they tried to prove that they could cook spicier dishes and stir pots more vigorously than their colleagues. If one had some doubts about “policy scientists,” these could only be doubled by the spectacle of scientific policy-makers. Be that as it may, the Washington connection turned an intellectual interchange into a professional one. In countries with a tight separation between the career of bureaucracy or politics and the academic *métier*, such exchanges are limited to occasional formal occasions—seminars or colloquia—and frequent *diners en ville*; but the former tend to be sterile, and the latter hover between witty debates on current affairs, and small talk.

A second institutional factor of great importance is the role of what I have

elsewhere called the relays between the kitchens of power and the academic salons. The most important of these dumbwaiters is the network of foundations that fed international relations research after the war, and whose role is essential if one wants to understand exactly why the three waves of scholarship coincided so aptly with the consecutive concerns of the statesmen. A combination of intellectual encouragement to "frontiers of knowledge" and civic desire to be of service, the sociological peculiarities of boards of directors composed, to a large extent, of former academics and former officials, the happy accident of vast financial resources that kept growing until the end of the sixties, all this made of the foundations a golden half-way house between Washington and academia. Wasps served in the CIA—pardon, the institution—as well as State; ex-State officials served in the foundations; and even those professors who had some reservations about serving in the government, had no objection to applying to the foundations. It was a seamless pluralism. These precious relays exist virtually nowhere else.

The third institutional opportunity was provided by the universities themselves. They had two immense virtues. They were flexible; because of their own variety, which ensured both competition and specialization, and also because of the almost complete absence of the straitjackets of public regulations, quasi-feudal traditions, financial dependence, and intellectual routine which have so often paralyzed the universities of postwar Europe. The latter got caught in the contradiction between their own past—a combination of vocational training and general education for the elites—and the sudden demands of mass higher education; they could vacillate from confusion to collapse, but the one thing they could rarely do was to innovate. The other virtue of American universities resulted in part from the fact that mass higher education was already a *fait accompli*: they had large departments of political science, which could serve as the matrices of the discipline of International Relations. In France until the late sixties, in Britain until the spread of the new universities, international relations remained the handmaid of law, or the laughingstock of historians; and when political science departments began to mushroom, the other reasons for the development of the discipline in America were still missing. Only in America could a creative sociologist write about the university as the most characteristic institution of the postindustrial age, the laboratory of its discoveries.<sup>10</sup> In other countries, universities are rarely the arenas of research; and when they are, the research funded by public institutions concentrates on issues of public policy which are rarely international—partly for the political reason I have mentioned above, partly because the existence of a career foreign service with its own training programs perpetuates the tendency to look at international relations as if it were still traditional diplomacy. Civil servants obliged to deal with radically new tasks such as urbanization, the management of banks and industries, or housing sometimes think they can learn from the social sciences. Civil servants who deal with so "traditional" a task as national security and diplomacy do not always realize that the same old labels are stuck on bottles whose shapes as well as their content are new. And when diplomats discover that they too have to cope with the new, technical issues of technology, science, and economics, it is to "domestic" specialists of these subjects that they turn—if they turn at all.

*Even in America*

If one looks at the field thirty years after the beginning of the “realist” revolution, can one point to any great breakthroughs? The remarks which follow are, of course, thoroughly subjective, and undoubtedly jaundiced. I am more struck by the dead ends than by the breakthroughs; by the particular, often brilliant, occasionally elegant, but generally nonadditive contributions to specific parts of the field, than by its overall development; by the contradictions that have rent its community of scholars, than by its harmony. The specific contributions have been well analyzed in a recent volume of the *Handbook of Political Science*,<sup>11</sup> and I shall not repeat what is said there. If I had to single out three significant “advances,” I would list the concept of the international system, an attempt to do for international relations what the concept of a political regime does for “domestic” political science: it is a way of ordering data, a construct for describing both the way in which the parts relate, and the way in which patterns of interaction change. It emerged from the first period I have described above, and continues to be of importance. Next, I would mention the way in which the literature on deterrence has analyzed and codified “rules of the game” which have been accepted as such by American statesmen, and which have served as the intellectual foundation of the search for tacit as well as explicit interstate restraints: MAD (“Mutual Assured Destruction”) and arms control are the two controversial but influential offsprings of the doomsday science. Third, there is the current attempt to study the political roots, the originality, and the effects of economic interdependence, particularly in order to establish whether it shatters the “realist” paradigm, which sees international relations as marked by the predominance of conflict among state actors. And yet, if I were asked to assign three books from the discipline to a recluse on a desert island, I would have to confess a double embarrassment: for I would select one that is more than two thousand years old—Thucydides’ *Peloponnesian War*, and as for the two contemporary ones, Kenneth Waltz’ *Man, the State and War*<sup>12</sup> is a work in the tradition of political philosophy, and Aron’s *Peace and War* is a work in the grand tradition of historical sociology, which dismisses many of the scientific pretenses of the postwar American scholars, and emanates from the genius of a French disciple of Montesquieu, Clausewitz, and Weber. All three works avoid jargon; the two contemporary ones carry their erudition lightly: the sweat of toil is missing. How more unscientific can you get?

Let us return to the ideology I alluded to earlier. There was the hope of turning a field of inquiry into a science, and the hope that this science would be useful. Both quests have turned out to be frustrating. The desire to proceed scientifically, which has been manifest in all the social sciences, has run into three particular snags here. First, there was (and there remains) the problem of theory. I have discussed elsewhere at some length the difficulties scholars have encountered when they tried to formulate laws accounting for the behavior of states, and theories that would explain those laws and allow for prediction. A more recent analysis, by Kenneth Waltz, comes to an interesting conclusion: if theory is to mean here what it does in physics, then the only “theory” of international relations is that of the balance of power, and it is unfortunately insufficient to help us understand the field! The other so-called general theories

are not more than grand conceptualizations, using "confused, vague and fluctuating definitions of variables."<sup>13</sup> This may well be the case; Waltz seems to blame the theorists, rather than asking whether the fiasco does not result from the very nature of the field. Can there *be* a theory of undetermined behavior, which is what "diplomatic-strategic action," to use Aron's terms, amounts to?

Aron has, in my opinion, demonstrated why a theory of undetermined behavior cannot consist of a set of propositions explaining general laws that make prediction possible, and can do little more than define basic concepts, analyze basic configurations, sketch out the permanent features of a constant logic of behavior, in other words make the field intelligible.<sup>14</sup> It is therefore not surprising if many of the theories dissected, or vivisected, by Waltz, are, as he puts it, reductionist, such as the theories of imperialism, which are what he had called in his earlier book "second image" theories (they find the causes of interstate relations in what happens *within* the units); or else, the theories he dismisses were all produced during the first phase—the neophytish (or fetish) stage—of postwar research: the search for the scientific equivalent of the philosopher's stone has been far less ardent in the past twenty years. Waltz' own attempt at laying the groundwork for theory is conceptually so rigorous as to leave out much of the reality he wants to account for. I agree with him that a theory explaining reality must be removed from it and cannot be arrived at by mere induction; but if it is so removed that what it "explains" has little relation to what occurs, what is the use? One finds some of the same problems in all political science; but Waltz is right in stating that international relations suffers from a peculiar "absence of common sense clues": the key variables are far clearer in domestic political systems, whereas here "the subject is created, and recreated, by those who work on it."<sup>15</sup> Still, here as in the rest of political science, it is the fascination with economics that has led scholars to pursue the chimera of the masterkey. They have believed that the study of a purposive activity aimed at a bewildering variety of ends, political action, could be treated like the study of instrumental action, economic behavior. They have tried in vain to make the concept of power play the same role as money in economics. And they have acted as if the mere production of partial theories unrelated to a grand theory was tantamount to failure.

A "science" without a theory may still be a science with a paradigm; and, until recently, the paradigm has been that of permanent conflict among state actors—the realist paradigm. However, in the absence of a theory, a second question has been hard to answer: what is it that should be explained? The field has both suffered and benefited from a triple fragmentation—benefited, insofar as much ingenious research has been brought to each fragment, yet suffered because the pieces of the puzzle do not fit. First, there has been (and still is) the so-called level of analysis problem. Should we be primarily concerned with the international system, that is, the interactions among the units? Or should we concentrate our efforts on the units themselves? There are two conflicting hypotheses behind these strategies. One postulates that the system has, so to speak, some sort of life of its own, even if some of the actors obviously have a greater role than others in shaping and changing the rules of interaction. The other approach postulates that the actors themselves are the strategic level for understanding what goes on among them. One says, in effect: Grasp the pat-

terns of interaction, and you will understand why the actors behave as they do; the other one says: Look at the actors' moves, and you will comprehend the outcomes. Students of the international system and students of foreign-policy making have never really blended their research. My own conclusion is that of a writer who has worked both sides of the street: I am dissatisfied with each, but I admit that it is hard to be on both at once. The study of the international system provides one with a fine framework, but no more—precisely because the system may well put constraints on and provide opportunities for the actors, but does not “dictate” their behavior; and the study of the actors tells you, inevitably, more about the actors than about the interactions. But what used to be called linkage theory (before linkage became a Kissinger-inspired technique), that is, propositions about the bonds between foreign policy and international politics, has remained in the frozen stage of static taxonomies.

Second, there has also been fragmentation at each level of analysis. One could say, not so flippantly, that each student of international systems has hugged his own version of what that abstract scheme “is.” Aron’s is not Richard Rosecrance’s, which is not Morton Kaplan’s. Moreover, each one has tended to look at the postwar international system in a different way (once again, in the absence of a single theory, it is not easy to determine authoritatively the dynamics of a particular system that still unfolds under one’s eyes). A dozen years ago, scholars acted as if they were competing for a prize to the best discourse on the subject: are we in a bipolar system? Waltz, Liska, Kissinger, and many others (including me) took part, but since there was no Academy, there was no prize. In recent years, the new contest is about “Persistence or Demise of the Realist Paradigm?”: Is the state-centered concept of international politics, with its focus on the diplomatic-strategic chessboard and its obsession with the use of force, still relevant to the age of interdependence? Aron, Joseph Nye and Robert Keohane, Edward Morse, Bull, and many others (including myself) are busy evaluating. As before, I suspect that the verdict will be history’s, and that like the long-awaited Orator in Ionesco’s *Chairs*, it will speak in incomprehensible gibberish. At the other level of analysis, we have accumulated masses of studies of concrete foreign policies, and moved from the period of Chinese boxes—the decision-making theories of the 1950s—to the age of the “bureaucratic politics” model. The former provided endless items for laundry lists; the other one draws attention to the kitchen where the meal is being cooked, but forgets to tell us that what matters is whether the chefs cook what they want or what they are ordered to prepare, and assumes all too readily that what they do is determined by their particular assignment in the kitchen, rather than by what they have learned outside, or their personal quirks.

Third, there has been functional fragmentation as well. If there is, or can be, no satisfactory general theory, if the “overarching concepts” are excessively loose-fitting clothes, why not try greater rigor on a smaller scale? At the systemic level, we have thus witnessed such clusters of research as work on regional integration (where, for once, the theoretical ingenuity of scholars has far out-reached the practical, “real-life” accomplishments of statesmen), modern theories of imperialism, arms race models and measurements of wars, recent studies of transnational relations and international economics. At the foreign policy level (although it tries to straddle both) the main cluster has been that of strate-

gic literature; and there is now a growing literature on decision-making in the United States. Unfortunately, each cluster has tended to foster its own jargon; and this kind of fragmentation has had other effects, which will be discussed below.

Finally, the quest for science has led to a heated and largely futile battle of methodologies, in answer to a third question: Whatever it is we want to study, how should we do it? Actually, it is a double battle. On the one hand, there is the debate between those "traditionalists" who, precisely because of the resistance the field itself opposes to rigorous theoretical formulations, extol the virtues of an approach that would remain as close to historical scholarship and to the concerns of political philosophy as possible (this is the position taken by Hedley Bull), and all those who, whatever their own brand of theorizing, believe that there can be a political science of international relations—if not in the form of a single theory, at least in that of systematic conceptualizations, classifications, hypotheses, etc.—a science which can be guided in its questions by the interrogations of past philosophers, yet finds reliance on philosophical discourse and diplomatic intuition both insufficient and somewhat alien to the enterprise of empirical analysis. There is little likelihood that this debate will ever come to a conclusion—especially because neither side is totally consistent, and each one tends to oversimplify what it actually does. On the other hand, here as in other branches of political science, there is the battle of the literates versus the numerates; or, if you prefer, the debate about the proper place and contributions of quantitative methods and mathematical models. The fact that the practitioners of the latter tend to hug the word science, and to put beyond the pale of science all those who, while equally concerned with moving "from the unique to the general" and with considering "classes of events and types of entities," believe that these cannot be reduced to numbers or that science does not consist in "accumulating coefficients of correlation" . . . "without asking which theories lead one to expect what kind of a connection among which variables"<sup>16</sup>—this fact has made for rather strained relations among scholars of different methodological persuasions. In domestic political science, behaviorists and old-fashioned scholars have found coexistence easier, because their respective approaches fit separate parts of the field—electoral behavior or the behavior of legislative bodies lends itself to mathematical treatment. In international affairs, such a functional division of labor is much harder to apply. As a result, the prophets of quantitative methodologies dismiss as mere hunches based on "insight" (a word they often use as if it were an insult) the elaborate ruminations of their opponents, and these in turn ridicule the costly calculations that tell one nothing about causes or lump together different types of the same phenomenon (say, wars), and the endless correlations among variables lifted from their context, that all too often conclude that . . . no conclusive evidence can be derived from them: endless nonanswers to trivial questions.

If there is little agreement as to what constitutes a science, and little enthusiasm for the state of the science of international relations, what about the other great expectation, that of usefulness? I am struck by one apparent contradiction. The champions of a science of international affairs have, on the whole, declared their independence from philosophy and their allegiance to objective empiricism. And yet, most of them have wanted to draw consequences for the real

world from their research: the greater the drive to predict (or the tendency to equate science, not just with intelligibility but with control and prediction), the greater the inclination to play the role of the wise adviser—or of the engineer. It is in the nature of human affairs, and of the social sciences.

But in this specific realm, there are some very peculiar problems. The first could be called: advice to whom? Many scholars, especially those whose level of analysis is systemic, implicitly write as if they were addressing themselves to a world government, or as if they aimed at reaching those who wish to transcend the traditional logic of national self-righteousness and state calculations (the same can be said, even more strongly, of theorists of regional or functional integration; they tend to distribute recipes for going beyond the nation-state). Unfortunately, the chair of World Statecraft is empty, and change comes (if at all) through the operations of state agents. And so, scholars of this kind oscillate from condemnation of state practices that make for conflict, or retard integration, or promote injustice, to advice to state agents on how to transcend the limits of the game which it is however these agents' role and duty to perpetuate, or advice to international secretariats and subnational bureaux on the best strategy for undercutting and turning the resistance of national statecraft. These are all perfect guarantees of unhappy consciousness for the scholars.

Other scholars, especially among those whose level of analysis is national decision-making, see themselves as efficient Machiavellians—they are advising the Prince on how best to manage his power and on how best to promote the national interest. This is particularly the case of the strategists, the group which contains the highest proportion of researchers turned consultants and policy-makers. "Systemic" writers who are fully aware of the differences between an international system and a community of mankind, that is, the "realists," do their best to make advice to the only Prince who still matters—the national statesman, bound to enhance the interests of his state—coincide with their views of the interests of the whole. They advocate "enlightened" concepts of the national interest, or "world order" policies that would somewhat reconcile the needs of the part and of the whole. But this is a difficult exercise. The logical thrust of "realism" is the promotion of the national interest, that is, not unhappy global consciousness but happy national celebration. "Realists" who become aware of the perils of realism in a world of nuclear interconnection and economic interdependence—writers like Morgenthau, or myself—suffer from the addition of two causes of unhappiness: that which afflicts all "systemic" writers in search of a radically new order, and that which comes from knowing only too well that utopianism does not work.

Thus, basically, in their relations with the real world, the scholars are torn between irrelevance and absorption. Many do not like irrelevance, and want even the most esoteric or abstract research to be of use. The oscillation I have described above is what they want to escape from, and yet they do not want to be absorbed by that machine for self-righteousness, the service of the Prince. But their only excuse is the populist dream—the romantic hope that "the people" can be aroused and led to force the elites that control the levers of action, either out of power altogether or to change their ways. Much of peace research, once it got tired of advocating for the solution of world conflicts the discrete techniques used for accommodation in domestic affairs, has been traveling down that route.



It is one on which scholarship risks finding both irrelevance and absorption, for the policies advocated here do inspire both those intelligentsias that want to displace certain elites in developing countries, and those established elites that are eager to boost national power against foreign dominance. Yet if the former come to power, and if the latter follow the advice of “dependencia” theorists, the result is not likely to be a world of peace and justice, but a world of revolutions, and new conflicts, and new inequities.

As for the scholars who want to avoid esoterica or romanticism and who set their sights on Washington, they, in turn, run into problems. There are two reasons why the Washingtonian temptation is so strong. There is the simple fact that international politics remains the politics of states: whether or not, in the abstract, the actor is the shaper of or is shaped by the system, in reality there is no doubt that the United States remains the most potent player. And there is the fact that a science of contemporary politics needs data, and that in this realm, whereas much is public—in the records of international organizations, speeches, published state documents—a great deal remains either classified or accessible only to insiders: the specific reasons for a decision, the way in which it was reached, the bargains that led to a common stand, the meanderings of a negotiation, the circumstances of a breakdown. Far more than domestic political science, international relations is an insider’s game, even for scholars concerned with the systemic level.

But a first problem lies in the fact that gathering information from and about the most potent actor, creates an irresistible urge to nudge the player: the closer the Washingtonian connection, the greater the temptation of letting oneself be absorbed. Second, outsider advice always suffers from oversimplification. When it comes to tactical suggestions, the insiders, who control not only all the facts but also the links connecting separate realms of policy, have the advantage. This increases the scholar’s urge to get in closer. Third, once one starts rolling down the slope from research-with-practical-effects, to practical-advocacy-derived-from-research, the tendency to slight the research and to slant the advocacy for reasons either of personal career or of political or bureaucratic opportunity, will become insidious. Which means that the author may still be highly useful as an intelligent and skilled decision-maker—but not as a scholar. Either his science will be of little use, or else, in his attempt to apply a particular pet theory or dogma, he may well become a public danger. This does not mean that the experience of policy-making is fateful to the scholar, that the greatest hope for the science would lie in blowing up the bridge that leads across the moat into the citadel of power. A scholar-turned-statesman can, if his science is wise and his tactics flexible, find ways of applying it soundly; and he can later draw on his experience for improving his scholarly analytical work. But it is a delicate exercise which few have performed well.

### *Because of America*

The problems I have examined have arisen mainly in America, because the profession of international relations specialists happens to be so preponderantly American. Insofar as it flourishes elsewhere, the same difficulties appear: they result from the nature of the field. But because of the American predominance,

the discipline has also taken some additional traits which are essentially American, and less in evidence in those other countries where the field is now becoming an object of serious study.

The most striking is the quest for certainty.<sup>17</sup> It explains the rage for premature theoretical formulation, the desire to calculate the incalculable (not merely power but status), the crusade to replace discussions of motives with such more objective data as word counts and vote counts, the crowding of strategic research (here, the ends are given, and it becomes a quest for the means). International relations should be the science of uncertainty, of the limits of action, of the ways in which states try to manage but never quite succeed in eliminating their own insecurity. There has, instead, been a drive to eliminate from the discipline all that exists in the field itself—hence a quest for precision that turns out false or misleading. Hence also two important and related gaps. One is the study of statecraft as an art. With very few exceptions (such as *A World Restored*) it has been left to historians. (One could say much of the same about domestic political science). The other is the study of perceptions and misperceptions, the subjective yet essential side of international politics. Robert Jervis' work is beginning to fill that gap, but it is not certain that his example will be widely followed.<sup>18</sup> Almost by essence, the study of diplomatic statecraft and of perceptions refuses to lend itself to mathematical formulations, or to a small number of significant generalizations (one may generalize, but the result is likely to be trivial). Taxonomies and case studies do not quench the thirst to predict and to advocate.

A second feature, intimately tied to the discipline's principal residence rather than to its nature, is the preponderance of studies dealing with the present. Historians continue to examine past diplomatic history in their way. Political scientists concerned with international affairs have concentrated on the politics of the postwar era; and when they have turned to the past, it has all too often been either in highly summary, I would say almost "college outline" fashion, or in the way long ago denounced by Barrington Moore, Jr., which consists in feeding data detached from their context into computers. This is a very serious weakness. It leads not only to the neglect of a wealth of past experiences—those of earlier imperial systems, of systems of interstate relations outside Europe, of foreign policy-making in domestic policies far different from the contemporary ones—but also to a real deficiency in our understanding of the international system of the present. Because we have an inadequate basis for comparison, we are tempted to exaggerate either continuity with a past that we know badly, or the radical originality of the present, depending on whether we are more struck by the features we deem permanent, or with those we do not believe existed before. And yet a more rigorous examination of the past might reveal that what we sense as new really is not, and that some of the "traditional" features are far more complex than we think.

There are many reasons for this flaw. One is the fear of "falling back into history"—the fear that if we study the past in depth, we may indeed find generalizations difficult and categorization either endless or pointless; and we may lose the thread of "science." A related reason is the fact that American political scientists do not receive sufficient training either in history or in foreign languages, indispensable for work on past relations among states. A third reason is

to be found in the very circumstances of the discipline's birth and development. In a way, the key question has not been, "What should we know?" It has been, "What should we do?"—about the Russians, the Chinese, the bomb, the oil producers. We have tried to know as much as we needed in order to know how to act—and rarely more: a motivation that we find in other parts of political science (the study of political development, for instance), where some disillusionment has set in. But we can say to ourselves that there are no shortcuts to political development, that the United States cannot build nations for others, and that we should go back to the foundations, that is, to an understanding of the others' past. We are unable to say to ourselves that we must stop having a diplomacy, and impose a moratorium on our advising drive until we have found out more about the past of diplomatic-strategic behavior. And the interest which, quite naturally, the government and, less wisely but understandably, the foundations have shown in supporting research that deals with the present (or extrapolates it into the future, or scrutinizes the near future so as to discern what would be sound action in the present) has kept the scholars' attention riveted on the contemporary scene.

The stress on the present and the heavily American orientation have combined to leave in the dark, at least relatively, several important issues—issues whose study is essential to a determination of the dynamics of international politics. One is the relation of domestic politics (and not merely bureaucratic politics) to international affairs—we need to examine in far greater detail the way in which the goals of states have originated, not (or not only) from the geopolitical position of the actors, but from the play of domestic political forces and economic interests; or the way in which statesmen, even when they seemed to act primarily for the world stage, nevertheless also wanted their moves abroad to reach certain objectives within; or the way in which external issues have shaped domestic alignments and affected internal battles. The desire to distinguish the discipline of international relations from the rest of political science is partly responsible for this gap; scholars who study a given political system do not usually pay all that much attention to foreign policy, and the specialists of international politics simply do not know enough about foreign political systems. The only country for which the bond between domestic and external behavior has been examined in some depth is, not so surprisingly, the United States. Here again, an assessment of the originality of the present—with its visible merging of domestic and foreign policy concerns, especially in the realm of international economic affairs—requires a much deeper understanding of the past relations between domestic politics and foreign policy. We may discover that the realist paradigm, which stresses the primacy of foreign policy, has to be seriously amended, not only for the present but for the past.

Another zone of relative darkness is the functioning of the international hierarchy, or, if you prefer, the nature of the relations between the weak and the strong. There has been (especially in the strategic literature) a glaring focus on bipolarity, accompanied by the presumption that moves to undermine it (such as nuclear proliferation) would be calamitous (it may not be a coincidence if the French have, on the whole, taken a very different line). Much of the study of power in international affairs has been remarkably Athenian, if one may refer to the famous Melian dialogue in Thucydides (the strong do what they can, the

weak what they must). How the strong have often dealt with the weak in ways far more oblique, or less successful than the simple notion of a high correlation between might and achievements would suggest; how and under what conditions the weak have been able to offset their inferiority—these are issues which, until OPEC came along, had not been at the center of research and for which, again, far more historical work ought to be undertaken.

What was supposed to be a celebration of creativity seems to have degenerated into a series of complaints. We have found here an acute form of a general problem that afflicts social science—the tension between the need for so-called basic research, which asks the more general and penetrating questions that derive from the nature of the activity under study, and the desire of those who, in the real world, support, demand, or orient the research, for quick answers to pressing issues. And if the desire often seems more compelling than the need, it is because of the scholars' own tendency to succumb to the Comtian temptation of social engineering. This temptation is enhanced by the opportunities the United States provides to scholar-kings (or advisers to the Prince), or else by the anxiety which scholars, however "objective" they try to be, cannot help but feel about a world threatened with destruction and chaos by the very logic of traditional interstate behavior.

Born and raised in America, the discipline of international relations is, so to speak, too close to the fire. It needs triple distance: it should move away from the contemporary, toward the past; from the perspective of a superpower (and a highly conservative one), toward that of the weak and the revolutionary—away from the impossible quest for stability; from the glide into policy science, back to the steep ascent toward the peaks which the questions raised by traditional political philosophy represent. This would also be a way of putting the fragments into which the discipline explodes, if not together, at least in perspective. But where, in the social sciences, are the scientific priorities the decisive ones? Without the possibilities that exist in this country, the discipline might well have avoided being stunted, only by avoiding being born. The French say that if one does not have what one would like, one must be content with what one has got. Resigned, perhaps. But content? A state of dissatisfaction is a goad to research. Scholars in international relations have two good reasons to be dissatisfied: the state of the world, the state of their discipline. If only those two reasons always converged!

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