Theories of International Relations:
Parochial or International?

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More than a third-century has passed since the first rounds of the
great debate between "behaviouralism" and "traditionalism" in the study
of international politics took place. The arguments continue in many
guises, but it may be accurate to claim that a somewhat mellowed form of
scientific inquiry in international politics has become the norm, at least in
North America and a few other regions of the world. As is often the case in
initial debates about the merits and shortcomings of different methodolo-
gies and general approaches to a field, claims were exaggerated on both
sides. The most zealous among the behavioralists demanded more rigor,
empirical verification, and cumulation than they and their successors
could possibly deliver, while the traditionalists exalted too vigorously the
quality of knowledge gained primarily from insight, intuition, and the les-
sions that could be learned from a close reading of diplomatic history.
Today, most would agree that the firm dichotomization of paths to knowl-
edge overlooks areas of commonality: traditionalists do measure, while
much "hard" science is launched on the basis of insights that do not derive
from adhering to strict canons of scientific method.

Many of the early proponents of scientific methods—especially those
who developed formal deductive models, operationalized concepts,
"tested" hypotheses against data, and measured precisely—made extensive
claims and predictions about the superior results of their methodologies. Among them were greater reliability and cumulation, results which by and large have been achieved. The most ambitious claimed, furthermore, that a science of international politics could lead to a general theory, accurate “laws”, and predictions. If we were to judge the field today in terms of such claims, the mark assigned would be low. Most investigators have recognized that while formal scientific modes of inquiry hold many virtues, the subject of study inherently limits most results to more or less significant associations, correlations, probabilities and trends. Those statements which have had the qualities of “laws” have on the whole been too trivial to stimulate research agendas or knowledge.

My purpose, however, is not to evaluate all the debates about the philosophy of science, the claims made by leaders of various methodological schools, or even the position which holds that “scientism” buttresses the status quo. These issues have received extensive attention elsewhere. I will confine my comments to a problem that has received much less exposure. This is the problem of parochialism. It is in part a problem created by national perspectives on international relations. But it is not only an issue arising out of differences in geography, history, language, and culture. There is also a habit among some theorists of international relations that might be called conceptual or methodological parochialism. This is the stance, prominent among more than a few scholars in our field, that there is only a single legitimate approach, model, methodology or theory of international politics. The quarrels about behaviouralism versus traditionalism, among many other debates in the field, clearly revealed this position. This form of parochialism is really an argument for intellectual orthodoxy and, as such, contradicts the scientific spirit which emphasizes exploration, novelty, and innovation. I do not suggest that this form of parochialism is always a conscious decision on the part of analysts to accept or reject certain approaches to a field. Rather, it reflects a trait of many scholars in international relations, namely an unwillingness to confront directly the relationship between our personal values, including political perspectives, and the subject we study. I would like to examine briefly some of the roles values play in the development of international theories, because understanding of this problem may help us locate some sources of parochialism.
Values and Research

We have tended to be prudes about the relationship between personal values, theoretical activity, and research agendas, either pretending it does not exist, or claiming that if it does, we can handle it quietly and effectively by a methodological quick fix. In most discussions the issue is posed as essentially a technical one: how to develop research tools that will sensitize investigators to their value biases; the effect of interviews and polls on subsequent behaviour; developing "unobtrusive measures;" "squeezing" unwarranted inferences from data; and the like. But from its origins, the study of international politics has been critically influenced by normative and philosophical concerns. We study a subject not because "it is there," but because we are morally concerned about it. We should acknowledge and, like some peace researchers, even celebrate this. For it is these concerns that basically determine what we study and deeply influence how we study it.

Let me illustrate this assertion by examining briefly three main theoretical approaches to international relations: "realism" and its modern versions; dependency theory; and world society models. I have discussed the main thrusts and intellectual roots of these schools elsewhere. Here I want to emphasize the extent to which normative concerns underly these approaches to the field.

The classical theory of international politics, of which realism constitutes just one version, developed coincidentally with the formation of the European states system. One of the fundamental characteristics of that system was war, a phenomenon, according to Rousseau, which was an inevitable consequence of a system of sovereign, independent states with no central authority to regulate their mutual relations. The early theorists of the states system condemned war from a variety of perspectives. For some, it was wasteful and destructive. For others, war was an instrument of ruling dynasties to perpetuate their monopoly of power and to supress the rising tide of republicanism. And, for those who saw virtues in the system of independent states, war was the evil instrument which imperially-minded rulers could use to unite Europe into a single imperium, that is, an instrument to destroy the states system. Various writers of the era, from Grotius and the Abbe Saint-Pierre, to Rousseau, Kant, and Bentham
thus had a common moral concern: to identify the etiology of war and, having done that, to outline the necessary and sufficient requisites for peace.

Since the eighteenth century, these questions have remained the theoretical cores of our discipline, and no matter what our preferred solutions to the problem, we all call ourselves scholars of international relations because ultimately we have a moral and ethical concern with the problems. Studies that are not rooted in this concern have been, perhaps appropriately, relegated to the peripheries of the field.

For example, the sudden flourish of studies on transnational relations in the 1970s has generated neither major theoretical statements nor a body of cumulative knowledge about international relations. Why? Certainly not because there is an absence of empirical data, or because transnational relations are declining. It is, I believe, because descriptive studies of transnational relationships are for the most part far removed from the war-peace problematic. The main streams of contemporary theoretical work converge on issues such as crisis decision-making, the requisites for effective deterrence, patterns of bargaining in international conflicts, the sources of international cooperation, the problem of misperception and miscalculation in foreign policy-making, and the like. All of these are intimately related to the core ethical concerns of our field. But a large proportion of transnational relations have not yet been empirically linked to the causes of war and the conditions of peace—although someday they may be. To study systematically all phenomena that involve communications across national borders would lead to the amassing of great quantities of data; but as yet, these have not been directed toward the solution of any important moral puzzles. Transnational relations, focussed on the connections, for example, between a Japanese prefecture government and state governments in the United States; or the politics of the International Olympic Committee; or between the cities of Strasbourg and Munich, might be of some interest just to demonstrate the interconnectedness of societies. But we are unlikely to build a field of international politics around such studies, because they are not derived from the war-peace problem. In brief, we earn our identity as scholars of international politics because, ultimately, we are all concerned with the causes of war and the conditions of peace, order, and security.
Dependency theories, a second major perspective on international relations, have different normative and ethical concerns. These are the causes of inequality and exploitation, and the conditions of international justice, as measured essentially by economic equality and opportunity. Questions of war and peace are not central concerns; indeed, most dependency writers treat them as mere epiphenomena of basic economic structures, as instruments used by the centre, or industrial countries, to control the developing countries. There can be no solution to the problem of war until there is a restructuring of the world capitalist system. In the meantime, major intellectual concerns have to focus on the condition of poverty and the steps necessary to end exploitation and domination. Justice, conceived in economic terms, is the goal; and concern or outrage about inequality propels theoretical and empirical analysis. Dependency theorists disagree on many critical methodological, conceptual, and empirical problems, but they are united in their ethical and moral concerns. The ultimate test of dependency theories will be an empirical one: to what extent do the various hypotheses and generalizations coincide with the real world. We cannot reject them on the grounds that their central normative concerns are unimportant.

World society theories also reflect strong normative concerns. War and peace are important problems, but to them they have added questions of equality and justice, pollution, the depletion of resources and a whole host of other global problems. The basic argument, perhaps rooted more in wishful thinking than in demonstrated fact, is that the genesis of all of these problems resides in the continuation of the states system. Decisions and management that optimize the global good cannot ensue from bargaining between 160 or more nation-states, each pursuing its short-run self-interest. The solution to global problems, all of which are integrally linked, can only come from centers of authority or leadership that have the interests of the human family in the forefront of their concerns. This would require some sort of restructuring or transcendence of the states system. Some writers see evidence that this is already occurring, as the interconnectedness of societies continues to grow; others are more skeptical, and suggest that structural systems of domination and dependency will have to be overcome—that is, all 160 states will have to become genuinely independent and autonomous—before there can be any sort of world au-
thority. Like the proponents of dependency theories, world society modellers have intense internecine debates, but their essential normative concerns are similar. There is an ethical core to scholarly research and analysis.

Unlike many of the physical sciences, all of these major surviving approaches to international relations are driven by more than just curiosity. Ethical concerns are at the centre of our activities, and yet we often hear the argument that values are nothing more than a slight intrusion into “objective” work. All we need to do, it is claimed, is to be aware of our biases and to make certain that our methodologies are as neutral and “scientific” as possible. And many of the critiques of these major theoretical approaches are based more on our political distaste for them than on grounds of logic or empirical adequacy. We do not often admit this, but if pressed, one could make at least a moderately persuasive argument in support of the assertion.

Perhaps the comments above are fairly obvious. It is not the first time that someone has pointed to some essential differences between the physical and social sciences. I understand, too, that one implication of these differences may be great, namely that we can never develop a science of international politics, if by that term we mean a body of cumulative and reasonably reliable knowledge that transcends location, time, and personality. I do not believe such a pessimistic conclusion is warranted yet, because, looking back over the past thirty years or so, it is obvious that we have much greater knowledge about some important aspects of the field.

A less obvious implication is that we will never have a theory of international politics, for the simple reason that our ethical and moral concerns are different. We can only have different approaches to the field, none of which is inherently superior to others because no one can argue that the problem of war (excluding of course nuclear war, because it is truly the ultimate problem) scientifically takes precedence over the problem of justice or inequality. I will return to this question when we examine briefly the second form of parochialism.

The values animate much of our research and analysis I hope is by now reasonably clear and established. But values play a much more subtle role as well. They determine or influence not only what we study, but also how we approach a particular subject. Here, we may be essentially unaware of
how exactly our personal views of the world, our ideologies, political preferences, and assumptions about the human interactions intrude upon our otherwise scientifically correct concepts and methodologies. It is a problem of what kinds of questions we ask, and how we ask them. Unfortunately, I cannot state a generalization, but perhaps one example will illuminate the point.

The body of literature known as "integration theory" was among the first to employ non-ideographic research approaches. Much of the literature was formally "scientific," and many of the generalizations were assumed to be of universal validity. But the whole enterprise was based on some very traditional liberal views on the diplomatic consequences of interaction between societies. Indeed, we could argue that integration theory was less a scientific exercise about international politics than a research agenda exploring the consequences to inter-state relations of a liberal economic order. While the research often employed formal measurement devices, and thus satisfied some of the criteria of science, the whole enterprise was fundamentally concerned with liberalism. This political preference determined not only the problem to be explored, but how certain research questions could be asked.

Much of the research stressed the positive effects of increasing transaction flows between societies. The experience of the EEC in the 1950s and 1960s seemed to bear out optimistic predictions about such trends, predictions that had been voiced in one form or another since the time of Jeremy Bentham. The ultimate value of integration was not enhanced economic welfare—as most policy-makers saw it—but peace. While some of the integration theorists acknowledged their normative concern with peace, they seldom subjected the precondition of increased transactions to critical scrutiny. They never entertained the opposite hypothesis, so ably enunciated by Rousseau, that increased transactions increase the likelihood of conflict.

Researchers often fail to contemplate what I would call counter-intuitive hypotheses. Suppose that they had started off from the assumption that integration is a "bad" thing. Had they held such a view, their research designs would have been fundamentally different, and the questions they might have asked—for example, why has there not been more opposition to integration—would lead us to view the problem of integration
from an entirely different angle. I am not proposing that arms races, imperialism, international crises, misperception, and many other phenomena are in fact good, but the integration of normative preferences and scientific methodology will continue until we are willing to confront directly a variety of counter-intuitive hypotheses before we outline research designs. Sensitivity to the role of values and political preferences in theoretical and empirical work can help produce more reliable knowledge. It can also help reduce the two forms of parochialism—national perspectives and the demand for orthodoxy—that are prominent characteristics of our field today.

Parochialism I: International Theory as a National Enterprise

One keystone of a scientific enterprise is the search for generalizations, associations, and correlations that transcend temporal or spatial location. Although there is no a priori reason to believe that patterns of behaviour are changeless or that highly idiosyncratic behaviour never exists, a bias of the study of international politics has been to go beyond the reporting of current events, as if they were unique, to look for patterns and to explain variations in classes of events. The problems of alliances, misperception, bargaining, integration, and the like, though usually explored in specific historical-geographic contexts, are not assumed to be germane only to these contexts.

If we grant the assumption of regularity, or patterned behaviour, then science cannot be “national,” in the sense that the methods of inquiry in one place are unique to, and different from, those of scholarly communities located in other countries. There cannot be American, Japanese, or Latin American schools of international politics, although obviously there are different substantive studies of these areas. We would expect those who examine international politics in general—those concerned with the traditional questions—to have a pronounced global outlook as far as questions asked, data sources, and research procedures are concerned. The notion of national “schools” of thought, whether theoretical or methodological, is inconsistent with the requirements of reliable knowledge.

If the claims about the superiority of knowledge gained through universally-valid scientific procedures are correct, we would expect that a significant proportion of scholars working in the field today adhere to
similar canons of enquiry. And, we would also expect that by now there
would be at least the nucleus of a global community of scholars, each
member "producing" knowledge, each aware of the work of others in far
corners of the world, and all acknowledging important breakthroughs and
findings, no matter where their location. A global community of scholars
implies some degree of symmetry in production and consumption of knowl-
edge, as distinct from a situation of intellectual dependency, where schol-
ars of just a few nations produce, while a large proportion on the peripher-
ies "consume" the new ideas, models, findings, and data sets but otherwise
contribute nothing original into the communications network. Moreover,
theories of international politics should be uncontaminated by national or
parochial distortions. For example, case studies should come from a vari-
ety of geographic and historical locations; aggregate data should come
from reasonable samples of countries, over time. And innovative models
supposedly of universal validity should not be mere distillations of a single
country's diplomatic or organizational experience. What has been the re-
cord of achievement over the past thirty years in terms of developing a
genuine global community of scholars, and a field of inquiry that is largely
liberated from parochial habits rooted in geography?

Most of the evidence available to answer this question is unsystematic
and impressionistic. I acknowledge this fundamental weakness in the com-
ments that follow. To lend some credence to the observations, however,
one can call upon an initial exploration of this question published by Fred-
erick H. Gareau\(^4\) five years ago; in addition, I have some data from my own
recent work that provides at least a few clues.\(^5\) Both these sources sug-
gest that the early expectations are not being achieved, or achieved only
very slowly. There are significant residues of parochial habits, and con-
siderable evidence that American scholars, in particular, are significantly
uninformed about work going on in other areas of the world. This overall
conclusion is surprising given the recent growth of international scholarly
infrastructural organizations.

Opportunities for scholarly exchanges have grown rapidly over the
past few decades. Organizations such as the International Studies Associ-
ation, the International Peace Research Association, and the Interna-
tional Political Science Association have organized varied and numerous
facilities to bring academics together: conferences, workshops, news-
letters, research groups, and the like. I am certain the number of conferences and meetings of specialists, coming from various areas of the world, have also grown significantly in number. Most journals in the field are published in English or other major languages, and numerous works in minor languages are eventually translated into English. There is, then, no overbearing physical or language impediment to scholarly communication on a global basis. But there is no community of scholars defined as reasonably symmetrical patterns of production, awareness, and acknowledgement of knowledge and research.

Evidence about the awareness and acceptance of scholarship in various areas of the world can be obtained from reviewing citations in scholarly articles and reference or bibliography sections in textbooks on international politics. Gareau has already drawn our attention to the fact that North American academics rarely cite in journal articles the research and publications of foreign authors. In part of another study, I have identified the nationalities of authors cited in the reference and bibliography sections of American (and other) textbooks since 1948. While acknowledging some difficulties in using such data as indicators of scholarly globalism or parochialism (an American textbook author is not likely to recommend a work in German or Russian to an undergraduate student), on the whole they are useful because they reflect the authors' explicit judgements about works which are important contributions to the field. The figures emerging from the research are startling. In a sample of six textbooks published in the United States between 1948 and 1968, almost 69 percent of the bibliography entries or recommended readings were to American authors. For the 1970-1982 sample (also six volumes) this figure had increased to more than 79 percent. In the latter sample, bibliography entries to American and British authors combined reached almost 87 percent, which leaves only slightly more than 13 percent for all other nationalities in the world. The increase over time toward greater concentration of references to authors of one's own nationality is hardly consistent with a model of an international community of scholars whose members are aware of any significant work, regardless of the authors' nationality. The American pattern, though duplicated in less stark numbers in some other academic communities, is distinct from countries such as Canada, Australia, and India, where there is increasing recognition of scholarship being
There may be several types of explanation for the data summarized here. Questions of quality, the comparative numbers of academics in the field in various regions of the world, the general tendency of many scholars outside the English-speaking countries not to be interested in theoretical questions, language, the nature of the publishing industry and the like come to mind. But the fact remains that as the scholarly communications infrastructures proliferate, and as international politics becomes established as an organized teaching field in more countries (and hence the entire pool of IR scholars increases), the trend for bibliography entries and suggested readings becomes increasingly concentrated towards national colleagues. Patterns of intellectual dependence—identified as one-way flows of awareness and acknowledgement—are also visible in England and Korea, so the American experience is not unique. Hence, we have a puzzle in the sociology of knowledge. The predictions of the increased "globalization" of the field of international politics have in some important respects not been borne out by subsequent practices. Characteristics of parochialism remain significant and in some countries are increasing.

Some rough impressions about research developments support the findings reported above. The selection of case studies, development of models, and use of data often reflect limited historical experiences. For example, although there has been significant new work comparing crisis behaviour across many different countries and eras, until recently much of the literature derived from only two experiences: the Cuban missile crisis and the origins of World War I. Second, the bureaucratic politics model, also developed initially on the basis of the Cuban experience, has numerous references to American practices that may not be particularly germane to the analysis of foreign policy in small countries, parliamentary systems, or in many authoritarian regimes. Third, interdependence is often described as a relatively new characteristic of the world, whereas in fact it may be new only to the United States, which until recently was not a major trading nation. Interdependence is also proclaimed to be a global characteristic, whereas it is a rather poor descriptive or explanatory concept when applied to a majority of states in the world. Other examples could be cited, but the three mentioned here may be sufficient to sensitize researchers against the habit of projecting on to others the characteristic
practices and problems of one's own government, or of globalizing generalizations derived from a single historical experience.

A final example of parochialism I is less impressionistic, but more personal. While it may not be entirely typical, it certainly illustrates the nature of the problem. An anonymous American reader of a book-length manuscript I completed several years ago offered a severe criticism for having chosen Bhutan as one of several countries in a comparative examination of foreign policy change. The charge was that Bhutan is no more important in international politics than Monaco or San Marino. Whatever the merits of the other criticisms of the manuscript, I could not accept this one. To an American, Bhutan is indeed remote and virtually unknown; it is not therefore surprising that there are few specialists who have an interest in the country. But to an Indian, just to take one example, Bhutan is extremely important in terms of that country's security interests. Indeed, in many ways, it is more significant and theoretically interesting than El Salvador, a small remote country to most American political scientists until a few years ago. The anonymous reader's reaction was typically parochial in that he or she defined the importance of the case study only in terms of American foreign policy interests. The implication was that unless the United States is directly engaged in a country, it is not a subject worthy of inquiry.

Parochialism II: The Demand for Theoretical and Methodological Orthodoxy

As I indicated at the beginning of this talk, the debates between "behaviouralism" and "traditionalism" of the 1950s and 1960s had much of the flavour of an intellectual crusade, the purpose of which was to win and to convert. Behaviouralists dismissed the traditionalists as purveyors of superstition and anecdotes, incapable of producing genuine knowledge. Those pigeonholed into the category of the quantifiers were characterized by their protagonists as illiterate and obsessed with measurements of things that inherently cannot be measured. Happily, the debate has largely subsided and the extreme claims made by both sides have been muted. Perhaps more worrisome about the debate than the particular issues that it raised was the demand for orthodoxy, the idea that there can be only a single method of analysis in our field.

This trait has reappeared in many of the theoretical debates that have
surrounded our field during the past decade. The attack on realism, for example, has not been confined to pointing out some of the discontinuities between the realist version of the world, and the world as it actually is and operates. It has been, rather, to destroy realism (and its successor, neo-realism) totally, replacing it with something else. Some prominent critics of realism have not sought theoretical amendment, reconciliation, synthesis, or development. They have sought instead an intellectual knockout, the victory of a “new paradigm” or world view.6 This is a stance that also pervades international relations scholarship in many of the socialist countries. While for many years after World War II, scholars in those countries were effectively sealed off from the rest of the world, in the past fifteen or twenty years intellectual contacts with western scholars have proliferated. But instead of using those contacts to develop a field of inquiry, to cross-fertilize, and to point towards new paths of development, many analysts from these countries have regarded western scholarship as simply and totally wrong philosophically, methodologically and empirically.7 The promotion of orthodoxy rather than pluralism seems to be the goal of scientific contacts between east and west.

The demand for orthodoxy which is, I suggest, a form of parochialism, may have the deleterious consequence of compartmentalizing and fragmenting the field into separate schools or approaches, the members of which then develop their separate journals, institutes, newsletters, and scientific organizations. These institutionalized aspects of the field then help perpetuate narrowness and exclusiveness, the exact opposite of what a true community of scholars requires, namely contacts and mutual learning. In other words, intellectual parochialism becomes institutionalized.

A more charitable interpretation of the demand for orthodoxy—the view that only a single approach or methodology is the appropriate path towards reliable knowledge—is that it is a necessary step for ultimate synthesis, and for refashioning our field to fit with political, economic, and technical developments in the world. Just as the hard-line debates between behaviouralists and traditionalists ultimately led to the dismissal of the most absurd positions, and to an eventual mutual respect and even mutual learning, so it could be argued that initial statements in favour of a particular “paradigm” or methodology have to be laced with exaggeration and an aura of exclusivity if they are to make a point vigorously. At a minimum,
polemics such as Ashley's critique of realism and neorealism, may be useful because they force adherents of those schools to re-examine their assumptions, to uncover hidden values, and to find new ways of reconciling their views of the world with the main trends and changes in the real world. Perhaps conventional wisdom needs a shock, a fundamental challenge, to become renewed. Synthesis, restructuring, and addition, it could be argued, can come only if those comfortable with a body of knowledge are forced to undergo an exercise of self-examination. In other words, intellectual knockouts, even if intended to replace one orthodoxy with another, may have the consequence of developing knowledge.

I am not certain which interpretation is correct. Certainly the strident tones of many of the debates, and the extent to which we have developed theoretical ghettos, whose members are barely aware of each others' work, is a cause for concern. I do not think, for example, that a student of international relations of the more conventional schools can consider him or herself well-grounded in the field until he or she has made a reasonably strenuous effort to understand at least the major points of dependency theory. Similarly, dependency theorists who simply dismiss strategic theory and the areas of great power politics that resemble the main propositions of realism as a mere facade for western imperialism in the developing countries, cannot be a complete theorist in the field. Reliable knowledge would seem to depend upon a healthy pluralism rather than the all-too-prevalent view that a particular school of approach is either right or wrong.

I believe we should be at least sensitive to the possibility that theoretical parochialism renders a disservice to knowledge and to the progress of our field. Pluralism and intellectual development should not be inconsistent. We should not require exclusiveness and theoretical ghettos as the price for progress. Reliable knowledge should come from intellectual persuasion and superior empirical work, rather than from polemics, denunciations, and political preferences masquerading as science.

Conclusions

Nineteen fifty six, the year the Japan Association of International Relations was founded, constituted an important date also for the development of the field of international relations. Publications by Quincy Wright
and Karl Deutsch provided fundamental stimulus to the movement to make of international politics a more disciplined and theoretical enterprise.\textsuperscript{8} Their works demonstrated that a healthy blend of systematic empirical methodology with theoretical questions posed in the classical literature of our field could produce knowledge of a very high order. Since that year, we have seen much progress in developing the discipline in both scope and depth. If we do not have a "grand theory" of international politics—which is probably a chimera—at least we have bodies of fairly reliable knowledge, knowledge that transcends time, location, and personality. Many of the hypotheses about international diplomatic life proposed by Thucydides, Machiavelli, Rousseau, Bentham and others have been either confirmed, disproved, or at least highly qualified. Our habit of making assertions of opinion and passing them off as facts has been largely overcome. Those who write textbooks in our field are more careful to specify when they are proposing a hypothesis, a hunch, or an established fact or trend. We have learned, too, to distinguish between political preferences masquerading as "analysis," and genuine scholarly research. I am certain all of us could extend this list of accomplishments. Taken together, they suggest that our discipline has matured in many ways. It is fitting that the JAIR would choose to celebrate its 30th anniversary with a special conference, because those thirty years coincide with the greatest period of intellectual development of our field.

Yet, as my comments suggest, we have some way to go before we can claim that ours is a discipline characterized by an international community of scholars, by a healthy respect for innovation and older approaches that still tell us important things about the essential characteristics of the world of diplomacy and war. My own situation is symptomatic of the first type of parochialism. In order to stay abreast of my field, I try to read several dozen books and many more journal articles annually. I am aware that Japan has a vibrant community of international relations scholars, and yet I probably read annually not more than one or two articles authored by Japanese scholars. For a variety of reasons, most of which ultimately have to do with language and the social structure in which academics operate—that is, the various needs to write to a national audience—I do not have adequate access to the work of most Japanese scholars. Until I can say with certainty that I am as familiar with the research and theoretical
efforts of my colleagues in Japan, India, the Soviet Union, Germany, and many other countries, as I am with my Canadian or American colleagues, we will continue to face the problem of national parochialism.

The development of a genuine international community of scholars in our field is a very long-range endeavour. International scholarly organizations help, but they are by no means sufficient as an instrument for overcoming the problem. Indeed, as I have shown elsewhere, there is evidence that as our international scholarly organizations have developed, academics in the United States and Great Britain have become even more parochial than they were thirty years ago. A re-emphasis on language training as part of our graduate programmes is just one of many essential steps toward eventual progress.

The second form of parochialism bedevils all of us. Its roots are in the human character rather than in social institutions or graduate training. The demand for theoretical orthodoxy, the claim that every insight is a "new paradigm," and the wholesale rejection of traditional modes of analysis simply because they do not conform to the fads of the moment betrays forms of intellectual intolerance. Sometimes a vigorous challenge to old ways of looking at the world is healthy; indeed, it may be a necessary step towards intellectual development and renewal. But many of the debates of the last thirty years have not been conducted in the spirit of mutual learning or the desire to amend, synthesize, or to do whatever is necessary, within a healthy pluralism, to develop more reliable and extensive knowledge. We seem more prone to seek theoretical or methodological victories, to prove that our colleagues are wrong while we are right, that a single (and therefore partial) view of the world is correct while all others are incorrect. I am not certain what can or should be done about this problem. A starting point, however, might be to point out that it exists, and that it is not consistent with the scholarly enterprise. Beyond that, we might acknowledge that we will never have a single theory of international politics, that different perspectives on the world are probably a reasonable expression of the complexities of real life and of different value preferences. We may not feel comfortable with those complexities, but surely ambiguity, grey areas, and uncertainty are preferable to intellectual orthodoxy.
REFERENCES


2. The developing field of international political economy must be excluded from this statement. Its value concerns are primarily distributive, with the core problems of wealth maximization—usually within societies—and equity or reciprocity between societies. Concerns of efficiency are also prominent. The links between wealth maximization and security considerations have not been explored systematically. For a case study of the relationship, however, see K.J. Holsti, “Politics in Command: Foreign Trade as National Security Policy,” *International Organization*, 40 (Summer 1986), 643-71.

3. Waltz is critical of studies of transnational relations on other grounds, principally that they have not demonstrated that such relationships fundamentally alter the essential properties of international systems, which are their structures, units, and capabilities. Hence, transnational relations cannot be a variable in a parsimonious theory of international politics. Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979), ch. 5. Neither my comments nor those of Waltz implies that it is impossible to make theoretical statements about transnational relations. But they would be theories of transnational relations, not of war and peace.


7. Soviet scholars, while having become familiar with most of the theoretical international relations scholarship in the West over the past two decades, have condemned it for being class-based, “subjective,” and, of course, unMarxist. In their view, the only “objective” analyses of the field are those represented by policy-makers in Moscow. For recent English translations of Soviet scholarship in international relations, see the Soviet Political Sciences Association, *Global Problems of Mankind and the State* (papers delivered at the 13th World Congress, International Political Science Association, Paris, July 1985) Moscow: USSR Academy of Sciences, 1985; O.B. Borisov, Y.V. Dubinin, I.N. Zenskov et al., *Modern Diplomacy of Capitalist Powers* (New York: Pergamon Press, 1983).