

THE QUESTIONABLE STATUS OF BOUNDARIES

The Need for Integration

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Extreme differentiation without unification mistakes the trees
for the forest.

—Ernst B. Haas¹

This book is concerned with the role and limits of boundaries separating scholarly endeavors in the study of international life. It is specifically concerned with two different kinds of boundaries: (1) the enduring and seemingly entrenched boundaries defining and separating disciplines and subfields in the social sciences (e.g., the boundaries between political science, psychology and economics, or the boundaries between “political economy” and “historical sociology”); and (2) the boundaries that divide distinct theoretical schools or research traditions regularly vying for supremacy as “paradigms” in the analysis of international life (e.g., the boundaries between neorealism, pluralism, and constructivism).

For the better part of the past century, both sets of boundaries have been thought of as playing an important and necessary role in the advancement of “knowledge” in the social sciences, albeit for quite different reasons. The first set of boundaries—those defining distinct disciplines and subfields—is often thought to reflect a necessary and valuable division of labor in the quest for knowledge. This division supposedly enables investigators to gain a more detailed knowledge of a particular set of phenomena and to share their research products with a community of scholars who can more efficiently grasp the significance of these findings since they share a certain preexisting set of skills, vocabularies, research tools, and common stocks of knowledge bearing on the phenomena around which a discipline originally emerged.

The second set of boundaries—those defining research traditions, theoretical schools, or “paradigms”—has enabled individual investigators to systematically and confidently carry out their research projects on the basis of shared fundamental assumptions concerning the nature of social life, the goals of social science, the appropriate objects of empirical analysis, and standard methods for conducting and evaluating scientific inquiry. The emergence of a research tradition in a particular field is thought to provide a necessary foundation for defining and carrying out specific research projects deemed meaningful by others working in that tradition, while the competition among research traditions espousing different epistemological and methodological assumptions is often regarded as a precursor for the emergence of a new Kuhnian “paradigm”² or, at least, progressively more sophisticated conceptual frameworks.

It is an open question, however, as to whether these two sets of boundaries are presently serving the purposes invoked to justify their existence. Is the division of labor between disciplines, subfields, or programs serving the purpose of more efficiently and thoroughly acquiring empirical knowledge concerning different actors, structures, and processes in international life? Are the present debates among adherents of competing theoretical schools or research traditions indeed contributing to the progressive cumulation of theoretically significant knowledge about international life by spurring revolutionary breakthroughs or by producing incremental advances in concepts and research methods? This book is motivated by the strong suspicion that the separate stocks of “knowledge” being produced by social scientists are not being sufficiently integrated across different disciplines, subfields, and competing research traditions so that we can see the forest *and* the trees.

The essays in this volume are by scholars who are formally trained in the discipline of political science, but who, in pursuing their varied substantive interests in the study of international life, have found it necessary and valuable to cross the existing boundaries that, on the one hand, define and separate the various social science disciplines, subfields, or multidisciplinary programs, and on the other hand, define and separate competing research traditions or “paradigms.” The authors do not share a single epistemology or a single research agenda; nor are they interested in offering an alternative unifying foundation for social science research. What they share is a common concern for concrete problems of cooperation and collective action in international life, along with a conviction that “normal” research in the social sciences has not paid sufficient attention to the *integration* of potentially related stocks of knowledge produced in different disciplines, subfields, or research traditions. The remainder of the chapter (1) discusses the origins of boundaries separating disciplines, subfields and specialized

multidisciplinary research programs; (2) examines the impact of the contentious debates among distinct research traditions on “progress” in the field of international relations; and (3) emphasizes the value of question-driven integrative theoretical frameworks while providing an overview of the chapters to follow.

Disciplinary Structures and Specialization in the Social Sciences

Boundaries clearly serve an important and valuable function in the organization of social life. Indeed, a complex division of labor has been viewed as an integral feature of the “modern” era ever since the nineteenth century when the predecessors of contemporary social science—Spencer, Durkheim and Weber, for example—all pointed to the emergence of clearly defined roles and spheres of competence within increasingly rational bureaucratic structures. Through most of the twentieth century, the growing complexity of this division of labor became widely regarded as an intrinsic characteristic of “progress” in social and international life.³

Until recently, it was not unreasonable to assume that, as with the increasingly complex division of labor in society, an increasingly complex division of labor in the social sciences would contribute to the efficient expansion of the total reservoir of knowledge in society. Scholars focusing on different processes and objects of analysis could specialize in their tasks, developing further the skills necessary to investigate and analyze these particular processes and objects. Even before the nineteenth century, the natural sciences had already come to be separated from the study of law and philosophy; the former was defined as an experimental, empirically based scientific endeavor, and the latter was increasingly regarded as a nonscientific, and hence less privileged, realm of discourse. By the late nineteenth century, the Comtean ideal of a unified science of the natural and social worlds had not only been fractured but had given rise to distinct social science disciplines, with history first separated from the nomothetic social sciences, and the latter then developing into the distinct disciplines of economics, political science, and sociology. Anthropology and geography also emerged as distinct disciplines, but were confined to the margins of the social sciences, while psychology became a component of the natural sciences.⁴ Finally, toward the end of World War I, international relations (IR) emerged as a field in its own right—either as a discipline or subfield—separated from the faculties of law and history within which international phenomena were previously studied.

This increasingly complex and “rational” division of labor in the social sciences was deemed to be intellectually productive because it was believed that “systematic research required skilled concentration on the

multiple separate arenas of reality, which was partitioned rationally into distinct groupings of knowledge.”⁵ More significantly, once emergent, these disciplinary boundaries became so deeply entrenched that their significance and necessity became taken for granted. The fundamentally discipline-focused character of social sciences became a defining feature of the organization of research and education, shaping the roles and identities of every component and every actor within academia. In effect, disciplinary structures acquired “the force of necessity, implying that the academic institution could hardly be structured otherwise and emplotting knowledge in a narrative of increasingly specialized material.”⁶

In the postwar period, the emergence of distinct subfields and distinct clusters of research based on particular areas in the world or particular actors (belonging to a particular gender, race, or ethnicity) came to occupy an important place in the division of labor in the social sciences. These new programs of research, often spanning at least two disciplines, posed new challenges for the existing disciplinary structure of the social sciences, but ultimately came to be incorporated as permanent fixtures within a more specialized division of labor. There was, for example, the emergence of subfields in each of the disciplines defined in relation to contributions from other disciplines (e.g., the emergence of “historical sociology” and “economic sociology” and the reappearance of “political economy”).⁷ While this enabled a few scholars belonging to two particular disciplines to work on similar phenomena or similar problematics, it remained unclear why politics was not relevant for economic sociologists or sociology was not relevant for political economists.⁸ More generally, the fact that the new subfields were defined in terms of preexisting disciplines meant that the social sciences remained grounded in assumptions concerning the separability of social phenomena and the desirability of studying these separate phenomena within distinct realms of specialized research. Similarly, the disciplinary structures within the social sciences were challenged by, but ultimately managed to coopt, the multidisciplinary research programs that emerged for the study of particular areas of the world or particular sets of actors.⁹ Such fields as the various area-studies, ethnic studies, or women’s studies certainly did more to integrate the insights from various disciplines than the new subfields discussed above. Yet, it is important to recognize that these interdisciplinary research programs also became permanent institutional fixtures that took for granted the importance of the bounded knowledge gained by focusing in detail on particular sets of actors employing particular combinations of concepts, methods, and metatheoretical assumptions.

Although “boundary-crossing” has slowly become an integral aspect of the social sciences in practice,¹⁰ the principles undergirding the division

of labor in the social science research have remained firmly entrenched. Thus, most members of multidisciplinary research programs continue to regard their departmental affiliations as primary while the few scholars primarily affiliated with multidisciplinary research programs (ethnic or gender studies, for example) are still regarded within the social sciences as performing marginal roles at best. Graduate training remains linked to the dominant social science departments as does the administration of research grants and training fellowships. While the emergence of dual-discipline subfields and multidisciplinary research programs demonstrate the questionable status of the disciplinary boundaries that developed prior to World War II, neither of these developments has resulted in any fundamental reassessment of disciplinary structures. Efforts to create interdisciplinary research structures around particular areas or actors, rather than prompting a serious reexamination of the purpose, limits, and structure of social science research, simply came to coexist with disciplinary structures as part of an even more complex division of labor in the social sciences. Thus, as one social scientist has recently pointed out, "[t]he intellectual eco-system has with time been carved up into 'separate' institutional and professional niches through the continuing processes of boundary-work designed to achieve an apparent differentiation of goals, methods, capabilities and substantive expertise."¹¹

In order to more seriously reassess the merits of increasing differentiation and the role of disciplinary structures in the social sciences, a brief comparison of the role of boundaries within the social sciences and natural sciences might serve as a valuable point of departure. To the extent that disciplinary boundaries may be defended in the natural sciences (and the status of these boundaries, too, is certainly open to question), it is at least conceivable that the boundaries serve an essential function in specifying distinct domains of scientific analysis and providing a set of distinct concepts, assumptions, and research tools for efficiently acquiring and organizing stocks of knowledge pertaining to those domains. It is possible, for example, that while chemical, biological, and physical processes may affect one another, a large set of phenomena can be reasonably defined as purely chemical, biological, or physical outcomes that merit a distinct mode of observation and analysis. In the social sciences, however, while economic, psychological, political, or cultural processes and structures may be analyzed within separate realms of inquiry, this is primarily driven by which aspect of a complex social reality *is of interest to the investigator*. It is far from clear that most of the questions addressed within each of the disciplines correspond to *objectively separable, empirically distinct outcomes* that must necessarily be analyzed on the basis of distinct analytic frameworks and research methods. The gravitational attraction between two

celestial bodies is a phenomenon that can be analyzed and understood entirely by astrophysicists without any assistance from biologists or chemists. Explanations of why wars occur or how democracy emerges, however, require us to simultaneously consider several phenomena normally studied independently within the domains of economics, sociology, psychology, or political science. It is highly unlikely that the specialists within any one of these disciplines will be able to offer a convincing model or interpretation of international conflict or democratic transitions without transgressing at least partially into the proclaimed intellectual domains of the other disciplines.¹²

This point may have been *implicitly* recognized within the context of the area studies programs that emerged in the 1950s. It is also implicitly acknowledged in the work of some scholars who have come to partially appreciate the interconnectedness of the "political," "economic," "psychological," or "sociological" dimensions of such major events and processes as the two world wars, the rise and fall of Nazism and Leninism, East Asian development, North-South cooperation, and more recently, "globalization." What has yet to be *explicitly* considered, however, are the possibility that knowledge about social and international phenomena is *essentially* interdisciplinary in character, and the implications of this possibility for the organization of social science research. The boundaries that separate disciplines and subfields in the study of social and international phenomena, while they clearly serve an analytic purpose, cannot be taken for granted. And, in the absence of complementary mechanisms designed to integrate research in the social sciences, inflexible disciplinary structures may very well come to constitute a hindrance to whatever "progress" is possible in our collective efforts to understand aspects of international life.

Expert knowledge is certainly an essential component of all inquiry, but "[o]nce expertise is thought of in terms of tools and instruments as opposed to the privilege it may assume in theoretical terms, then its status is open to changes all the time."¹³ In other words, the formation and proliferation of permanent specialized structures supposedly organized around intellectually and empirically distinct objects of research, needs to be accompanied by efforts to study a particular set of actors, processes, or events in a manner that *brings to bear on particular questions the full range of relevant concepts, theoretical frameworks, hypotheses, interpretations, insights, and methods, regardless of their disciplinary origins*, and simultaneously recognizes that *this cluster of concepts and tools are not necessarily applicable to a different set of questions, even where the same actors, processes, or events may be involved*.¹⁴ This does not require that we dismantle or reject all existing disciplinary structures; it simply requires that we make more room in the division of labor for those scholars who con-

sciously and deliberately approach their questions and objects of analysis in an *essentially* interdisciplinary manner, integrating the relevant concepts, facts, ideas, and insights from different disciplines, subfields or narrowly focused multidisciplinary programs.

Interparadigm Debates and “Progress” in the Study of International Life

This volume is also motivated by a desire to explore the intellectual returns to be gained from transcending—or ignoring—a second set of boundaries: the sharp, sometimes bitter, divisions separating adherents of competing research traditions in social science disciplines. I argue below that ever since Thomas Kuhn published his analysis of scientific “paradigms” in 1962, the stakes have been raised in the competition among theoretical schools and research traditions in the social sciences (arguably much more so than in the natural sciences, the primary subject of Kuhn’s analysis). The intensity of the resulting “interparadigm debates”¹⁵ has paradoxically undermined the efforts of those committed to developing a unifying paradigm, and, more importantly, has tended to marginalize the significance of more eclectic scholarly research transcending the divisions among competing research traditions. As a result, “progress” in the field of international relations has primarily taken the form of *intraparadigm* advances in theoretical sophistication and empirical knowledge; but these advances have been at the expense of incremental, *trans-paradigm* advances in our collective ability to understand the complexity of international life. The latter type of progress, I contend, is more significant and will require more room in the social sciences for deliberately eclectic and integrative conceptual frameworks designed to illuminate concretely defined problems.

In Kuhn’s treatment of scientific knowledge, “progress” was marked not by the steady cumulation of objective knowledge through the standardized application of universally valid methods, but by the emergence of new “paradigms,” each breaking with past strands of research and offering a new foundation for research consisting of a fundamentally different *system* of concepts, assumptions, questions, methods, and evaluative standards. A paradigm would first emerge when such a revolutionary foundation enabled a group of scientists to pioneer unprecedented achievements, and when these new achievements attracted scholars previously committed to other research programs and inspired them to pursue a new set of research questions framed within a common conceptual, epistemological, and methodological foundation. The initial appearance of a paradigm marked the achievement of “maturity” for a discipline, ending a period of initial competition among a variety of theoretical schools and research tra-

ditions. Subsequently, each new paradigm represented a fundamental departure from past research, setting new boundaries for research under conditions of "normal science." When these boundaries started to become blurred and the consensus undergirding key concepts and assumptions began to fade, conditions were ripe for a "scientific revolution" that would involve a new era of competition among a variety of research programs until one would emerge as the new dominant paradigm.¹⁶

Kuhn's treatment of scientific knowledge was predictably met with a flurry of criticism almost immediately. Most positivists, in both the natural and social sciences, challenged the notion that the development of knowledge is not cumulative but rather marked by a series of "new beginnings" that "occur in a random and unpredictable way."¹⁷ They were also critical of the idea that most knowledge claims resulted from paradigm-bound practices rather than from the application of uniform scientific methods capable of revealing universally valid, objective laws of nature.¹⁸ Others challenged the sharpness of the distinction Kuhn drew between "normal science" and "scientific revolutions," and faulted him for failing to recognize the value of "trans-paradigm" components of knowledge or the gradual evolution of new "conceptual variants" paradigm shifts.¹⁹ In fact, in his subsequent writings, including the second edition to the book, Kuhn himself chose to downplay the relativistic implications of his work, dropped the language of "paradigms," and had to back away from some of his earlier claims about the sharp difference between "normal" and "revolutionary" aspects of scientific research.²⁰

Nevertheless, ever since *The Structure of Scientific Revolutions* was first published in 1962, the very idea of a unifying "paradigm" for research has come to exert a powerful hold on students of international and social life. The concept has since been used widely by scholars across social science disciplines in referring to clusters of related research agendas organized around particular collections of concepts, assumptions, questions, and methodologies. Intellectual histories of disciplines and subfields have been rewritten using the language of "paradigms" and "paradigm-shifts."²¹ A variety of research traditions, methodological approaches, and epistemologies have repeatedly attempted to claim the mantle of new "dominant" paradigms under a variety of labels often encompassing overlapping assumptions. As a result, most scholarly analyses of international life (and indeed, the analysts themselves) have come to be identified with one research tradition or another, with most new research being framed as a contribution toward turning one of these research traditions into a unifying "paradigm" (neorealism, neoliberalism or constructivism, for example).

This fixation on "paradigms" over the last three decades, if anything, has actually had an adverse effect on whatever "progress" may be possible

in the social sciences by raising the stakes of the competition between research traditions, by subsequently raising the status of those contributing to the advancement of one research tradition in the estimation of others also working in that tradition, and by marginalizing (even penalizing) eclectic research agendas not founded on the system of concepts, assumptions, methods, or evaluative standards provided by one of the established research traditions. As a result, as Hirschman has noted, the "search for paradigms" has constituted more of a "hindrance to understanding," prematurely closing off possibilities for pursuing many socially important problems and slowing down whatever "progress" may be possible in the study of the social world.²²

This view is informed by three related observations on the applicability of the "paradigm" concept to the social sciences and on the prospects for "progress" in international relations. These have to do with the prevalence and endurance of supposedly dominant research traditions, the degree to which the core concepts and assumptions of the competing traditions are fundamentally novel and mutually exclusive, and the extent to which research designed to promote a prospective paradigm has contributed to some sort of "progress" in the field at large.

First, while there have been periods when certain theoretical schools or approaches have been prevalent in the social sciences, nowhere in the history of IR or any other social science discipline do we find research traditions comparable to Kuhnian scientific paradigms in terms of their predominance in the field and their "staying power." In every discipline or subfield in the social sciences, except for brief periods of time, several contending schools have typically been in existence simultaneously, competing with each other for dominance, and intensifying intellectual debates over ontological, epistemological, methodological, and/or normative issues, rather than paving the way for the emergence of a unifying paradigm. To the extent that the status of "paradigm" is achieved by a research tradition's ability to attract away adherents of competing traditions, it is significant that in every decade since the establishment of the first IR department in 1919, there have been at least two clear alternatives schools or approaches, each with sufficient adherents to challenge the claims of others to dominance in the field.²³ Certainly, in the postwar period, behavioralism and structural realism (or "neorealism") have been strong contenders for the status of "paradigm" at different points in time; but even at the height of their popularity within IR, neither achieved the kind of dominance represented by the Newtonian paradigm in physics, and neither survived more than a decade before being subjected to severe criticisms and spurring the growth of competing research traditions.²⁴ Even the more recent attempt to erect a unified "rationalist" tradition²⁵ combining several

of the concepts and assumptions of neorealism and neoliberalism has not had the effect of drawing away adherents from “reflectivist” alternatives as evident in the attention received by many poststructuralists, critical theorists, and a new generation of constructivists and cultural theorists in IR.²⁶

Second, it is important to note that many of the ontological, epistemological, methodological, and/or normative orientations of different research traditions overlap substantially, and that many of the points of contention among these traditions involve recurrent debates over familiar issues. The problem of mutual exclusivity is nowhere more evident perhaps than in the aforementioned attempt of neorealists and neoliberals/pluralists—the two most popular contestants in the 1970s “interparadigm debate”—to combine forces by offering a unified “rationalist” approach to IR, to be distinguished from older behavioralist approaches and newer “reflectivist” alternatives. Such a synthesis would not have been possible had the neorealist and pluralist research traditions consisted of mutually exclusive systems of concepts, assumptions, and research methods as would be the case with Kuhnian paradigms. Moreover, the recurrence of certain familiar ontological, epistemological, and methodological debates since the beginning of the century makes it difficult to define “progress” in IR or any other social science field in terms of either a succession of paradigm shifts or the transition from a “preparadigmatic” stage to an age of “maturity.” It is important to note, for example, that many of the epistemological differences between contemporary rationalists and reflectivists are essentially similar to the epistemological differences between the adherents of the “behavioral” and “classical” approaches in the 1950s–1960s, not to mention the differences evident in the nineteenth-century debate between British empiricists and German idealists.²⁷ Similarly, it is worth noting that the debate over culture and identity in the 1990s is not new in either international relations or in any other discipline or subfield; for over a century now, social scientists have been at odds with regard to the possibility of operationalizing nonobservable factors such as values, beliefs, and attitudes in a truly “scientific” analysis of social and international life.²⁸ Moreover, many of the core assumptions and concepts of self-proclaimed paradigms survive or reemerge within the disciplines or subfields within which they were originally conceived,²⁹ while the alternative research programs developed on the basis of the critique of these paradigms end up announcing the “return” of some concept or another, or “bringing back in” some actor or another.³⁰ It is therefore difficult to classify research traditions in the social sciences as Kuhnian paradigms that are supposed to be based on *fundamentally different, and mutually exclusive* systems of concepts, assumptions, and methods based on revolutionary departures from past strands of research.³¹

Third, while the most recent “interparadigm debates” in IR involve different combinations of ontological, epistemological, methodological, and normative stances than may have been the case in the 1950s or 1970s,³² these shifting debates do not represent a substitute for Kuhnian paradigm-shifts or an alternative path to the cumulation of knowledge, especially when one considers the absence of consensus on what constitutes “progress” in the study of international life. Many of the supposed “break-throughs” and “syntheses” have reflected little more than the efforts of a particular community of scholars to come to grips with new, unexpected, empirical phenomena on the basis of new vocabularies and different permutations of ontological, epistemological, methodological, and normative assumptions. At the same time, critiques of particular research traditions have ended up pointing the way not to a single, unifying alternative paradigm, but to several different kinds of alternative theoretical projects each with its own standards of “progress.” Thus, just as one distinguished scholar sets out to ponder the “growing relevance of pluralism,” another equally distinguished scholar simultaneously insists on the “timeless wisdom of neorealism.”³³ Moreover, as Haggard has noted, where “progress” in IR theory can be said to have occurred, it is not as a result of the emergence of a dominant paradigm, but because research traditions claiming this mantle have *lost* their hegemonic positions, allowing for more flexible approaches that raise new questions and introduce new factors. Structural realism, for example, has contributed to “progress” in IR theory, but not as a paradigm breaking with past research and providing a new system of concepts, assumptions, methods, and questions; the progress has come from the *unintended* consequences seen in theories and research into non-systemic factors designed to *challenge* the claims of structuralists!³⁴ Under these conditions, “interparadigm” debates may indeed contribute to increasing theoretical sophistication and new empirical research *within* a given research tradition, each offering scholars the ability to investigate empirical phenomena in far greater detail than previously possible.³⁵ But, if we are interested at all in some sort of theoretical “progress” that can be shared by a wider community of scholars subscribing to different epistemological, ontological, and methodological assumptions, then a different approach must be taken to appreciating the roles and limitations of different research traditions.

It is possible—and much more reasonable in our view—to opt for an altogether different notion of “progress” than the one assumed by social scientists invoking the language of “paradigms” or “scientific revolutions.” One such reasonable approach may be found in Stephen Toulmin’s evolutionary view of a part-rational, part-instrumental selection of useful conceptual variants from an expanding population of concepts. Toulmin focuses on

concepts as “micro-institutions” rather than full-blown logical systems that constitute Kuhnian paradigms. He examines the gradual expansion of “conceptual populations” as new variants emerge over time. Certain variants then come to be “selected” for their staying power, partly because of their utility in solving certain conceptual problems, partly because of their value in connection with various disciplinary and professional considerations, and partly because of their unanticipated payoffs for certain interests crucial to the maintenance or reproduction of the academe as a whole. The result is not a purely “rational” process of progress through purely objective procedures and evaluative processes, but it is progress nonetheless with the “selected” concepts at least contributing to the collective ability of scientists to define and solve the crucial problems of their time.³⁶

The question remains, however, as to how this kind of progress can emerge and how the utility of new conceptual variants may be recognized if social scientists remain focused exclusively on the advancement of a single research tradition or theoretical school vying for supremacy as paradigms. For example, while the recent efforts to “bridge” neorealism and certain strands of pluralism or neoliberal institutionalism may have yielded some new concepts for those seeking a structured understanding of regularities in international behavior, this bridge is designed to advance a single unifying paradigm while marginalizing several other research traditions and dismissing the core concepts and assumptions of the latter. At the same time, the renewed interest in culture, identity, and norms, albeit helping to illuminate much of the complexity in international life in the eyes of many “reflectivist” theorists, by and large ignores the concepts and assumptions embedded in the “rationalist” formulation offered by neorealists or neoliberal institutionalists. In the end, most scholars remain inclined to take for granted the importance of defending particular research traditions, and as a result, we are left with incommensurable theories, each designed to advance a research tradition—and a particular system of concepts—without regard for whether or not this advancement is appreciated by a community of scholars larger than the adherents of one of the traditions. If research into problems of international life is ever going to bear any resemblance to even the modest view of evolutionary progress offered by Toulmin, it will only be as a result of people hitting on particular concepts for their utility in defining and addressing certain problems regardless of which research tradition these concepts or problems originated within.

Beyond Boundaries?

These observations concerning disciplinary structures and “interparadigm” debates are not meant to suggest that the ideal of cumulative

knowledge must be sacrificed to an ever-increasing “proliferation of incommensurable theories.”¹⁷ Nor can we ignore the fact that specialized research has produced significant—indeed, critical—contributions to our understanding of international phenomena. This volume simply offers a modest plea for a more extensive and more flexible realm of interdisciplinary social analysis, and for a shift away from contentious (and probably unresolvable) “interparadigm” debates toward a focus on substantive problems and issues. Such a shift would be oriented neither toward the permanent establishment of new programs or fields of inquiry, nor toward the construction of dominant paradigms, but toward a more integrative approach to concrete empirical objects, structures, and processes that brings to bear a wider range of relevant concepts, theories, evidence, and interpretations regardless of which discipline or research tradition these originated within.

It is also essential to recognize that it is historically emergent *questions* and concretely defined categories of phenomena that represent the driving force for social research rather than *a priori* commitments to disciplinary traditions or methodological perspectives. If *questions* are what drive social scientists, then the debates in the social sciences ought to be focused on questions, and not on whether a particular study is appropriate for a particular discipline or whether a given approach is consistent with the epistemological and methodological assumptions of an identifiable research tradition. As James Rosenau has recently noted: “Instead of focusing prime attention upon the substance of world politics and/or criticising each other for their conceptions of how the system functions and changes, all too many analysts drift into a preoccupation with what constitutes the proper route to understanding and/or faulting each other for their methodological premises.”¹⁸

Moreover, different questions can be posed at different levels of analysis and different levels of abstraction, and in order to shed light on these different questions, it will be necessary to invoke different theoretical frameworks and different combinations of methods. But the choices made by different scholars interested in a given problem cannot then be turned into monolithic theoretical foundations designed to inform each and every analysis of each and every social phenomenon. Once we abandon *a priori* commitments to disciplinary traditions or particular analytic perspectives, it becomes possible to determine the relationships between the concepts and assumptions driving the various frameworks. It also becomes possible to discern how apparently contradictory assertions about social phenomena stemming from competing research traditions may, in fact, simultaneously represent partial “truths” about broader, more complex international phenomena that defy elegant, mono-causal explanations.

Constructing Integrative Frameworks

The chapters in this volume can be separated into two groups, each addressing the problem of boundaries in the social sciences at different levels of abstraction. The four essays in Part I of the volume—by Eileen Doherty, Anne Clunan, Norrin Ripsman and Jean-Marc Blanchard, and Tadashi Anno—all tackle the problem of boundaries concretely by consciously incorporating important concepts and analytic theories from various disciplines, subfields, and research traditions in order to construct innovative, integrative theoretical approaches for analyzing substantive problems in the study of international life. These authors, albeit interested in different substantive questions, share a common interest in aspects of cooperation and collective action in international life as well as a common intellectual orientation toward a question-driven social science. Each of the essays serves to highlight the *essentially* interdisciplinary nature of certain core concepts and problems in international life, and each of the approaches builds on a variety of methodological approaches linked to integrative theoretical “toolboxes,” designed to shed light on different aspects of cooperation and collective action in international life—ranging from the dynamics of international bargaining to the study of collective identity and the relationship between interdependence and conflict.

The chapter by Eileen Doherty, “Negotiating Across Disciplines: The Implications of Judgment and Decision Making Research for International Bargaining Theory,” explores the ways in which two research traditions in the discipline of psychology—prospect theory and social judgment theory—can be incorporated into a more comprehensive and useful theoretical approach to the study of international bargaining. Doherty begins with a discussion of traditional rational choice models of human behavior and briefly examines well-known variations on these models (bounded rationality, satisficing behavior and incremental decision making). She then turns the possibility of integrating into the study of international relations two distinct bodies of research in the field of judgment and decision making (JDM) in psychology, both of which question the validity of rational choice models. Prospect theory emerged from within (and as a reaction to) the tradition of rational choice, while social judgment theory (SJT) developed as a parallel tradition based on a different set of assumptions, methodological techniques, and assessment criteria for evaluating human decision making. Each of these research traditions offers a separate set of hypotheses about the factors that shape human cognition, but taken together, these hypotheses promise to offer novel and valuable insights in the study of certain problems traditionally thought to be strictly within the domain of international relations specialists. Doherty proceeds to develop

some of her own preliminary hypotheses based on the implications of these two research traditions for the analysis of international bargaining. She notes, for example, that negotiations often break down even when actors' preferences begin to converge. A complete understanding of the dynamics of international bargaining, she concludes, requires taking seriously the cognitive obstacles to successful negotiation, and this, in turn, requires integrating the contributions of theoretical traditions in the field of judgment and decision making with existing rationalist approaches in IR to the study of cooperation.

In their chapter, "Contextual Information and the Study of Trade and Conflict: The Advantage of a Cross-Disciplinary Approach," Norrin Ripsman and Jean-Marc Blanchard argue that to reach definitive judgments about the relationship between international trade, economic interdependence, and international conflict, researchers must reach beyond the boundaries of political science and economics to employ the relevant concepts and tools of geographers and historians. Their question driven analysis leads them to define and measure economic interdependence not merely with reference to the quantity of international or bilateral trade, but also with reference to more contextual concepts introduced by political geographers and geopoliticians, such as the material composition of trade and its importance to each state as a function of its geographical location and access to strategic resources. Furthermore, they employ the historian's method of primary-source, decision-making analysis to assess whether national leaders are actually aware of the constraints of economic interdependence and how such awareness conditions national security decision-making. Employing this interdisciplinary approach in concrete historical contexts, Ripsman and Blanchard demonstrate that interdependence prior to World War I, while not uniformly high as previous studies indicated, was not insignificant in 1930s Europe. They also ascertain that decision makers in Germany, France, and Great Britain were aware of the constraints of interdependence, yet did not consider their dependence relevant to national security decisions in times of crisis, when they were motivated primarily by strategic considerations and domestic political imperatives. Thus, Ripsman and Blanchard cast doubts on both the commercial liberal argument and the conventional neorealist assumptions concerning the origins of national security decisions while developing a more integrative, cross-disciplinary approach to the study of trade and conflict.

In her chapter, "Constructing Concepts of Identity: Prospects and Pitfalls of a Sociological Approach to World Politics," Anne Clunan emphasizes that the interests of actors are best understood by analyzing how their identities shape those interests. She notes that international politics, however defined, is shaped not only by interests, but to a large extent, by how

actors construct sets of ideas, norms, and identities, in order to cope with their interactions with other human beings. Clunan shares with Doherty a common desire to challenge the universality of rationalist assumptions, but her focus on the problem of shared norms and identities leads her to engage the efforts of a new generation of “constructivists” who are seeking to integrate the insights of sociology and political science in their search for approaches that more fully illuminate the growing complexity of international life. While some continue to claim that the fuzzy causal logic and fuzzy variables of constructivism are a hindrance to the pursuit of knowledge, Clunan’s chapter demonstrates that sociological approaches can complement, and even partially reconcile, competing mainstream approaches as long as careful attention is paid to the articulation and framing of the core analytic concepts. Clunan recognizes important differences among scholars labeled “constructivist,” but points out that these theorists generally agree that the reality we see is socially constructed, and that therefore the analysis of social forces (particularly, identity, cultures, and norms) must accompany studies of individual rationality or material forces in the study of international processes. She also notes that constructivists are in a unique position to develop a new, more integrated research program for examining the roles of learning and emergent shared interests in the development of international society. As an example of the utility of a sociological approach, Clunan demonstrates how a constructivist analysis of identity formation allows us to “unpack” some of the most problematic assumptions smuggled into mainstream theories and paves the way for more integrated explanations of specific aspects of state behavior. Finally, Clunan considers the potential returns of integrating the constructivist agenda into the study of socially constructed international life: the investigation of new substantive issues, the cumulation of knowledge already produced by many constructivists, and the progressive refinement of such knowledge through comparison and critique with works in the rationalist tradition; and she concludes that these intellectual gains are worth the sacrifice of universal models of choice and behavior.

In the final chapter of Part I, titled “Collective Identity as an ‘Emotional Investment Portfolio’: An Economic Analogy to a Psychological Process,” Tadashi Anno notes that “identity” has become a “buzzword” not only in international relations, but in various other disciplines in the social sciences and humanities as well. Like Clunan, he notes that discussions of “collective identity” in the field of international relations are characterized by a fuzziness that makes many mainstream social scientists uncomfortable, and he suggests that a more in-depth understanding of identity *formation* and the relationship between interests and identity can help us get beyond the apparently fruitless and endless debate between

rational-choice theorists and students of identity. However, unlike Clunan who focuses on the contributions of sociology to a constructivist research agenda, Anno focuses on the problem of individual motivation and on the partial convergence between psychological and economic understandings of the sources of that motivation. His chapter begins by attempting to clarify the relationship between the concept of “collective identity” and the broader theoretical models for the study of choice and action, focusing especially on the possible linkages between identity-based and interest-based explanations of group behavior. Anno’s own approach to the problem of collective identity-formation is based on the notion of an “emotional investment portfolio,” a concept derived from the strong analogy between, on the one hand, the processes and politics of identity formation, and on the other hand, the investment decisions of investors and their impact on the “fund managers.” While fully aware of the limits of analogies for the purposes of theory building, Anno nonetheless makes a compelling and original case for simultaneously appreciating the importance of both, interest-driven explanations of group behavior and the emotional aspects of individual commitment to a group identity. In order to demonstrate more concretely the utility of the “emotional investment portfolio” approach, Anno examines the process of national identity-formation in Japan, with comparative references to Russian national identity.

Reorienting the Foundations?

The essays in Part II of the volume—by Rudra Sil, Wade Huntley, and Timothy Luke—address the problem of boundaries at the level of ontology and epistemology, offering critical reflections on how to understand and reconceptualize the role of boundaries in defining what constitutes “truth,” “knowledge,” and “method” in international studies. The authors reject simplistic notions of a unified, discipline-bound, positive science applicable to each and every human endeavor, but at the same time, they seek to constructively identify alternative foundations for a social science characterized by a greater tolerance for different modes of inquiry carried out in a variety of settings by a larger and more diverse community of scholars. The authors share a common view that whatever cumulation may be possible in the study of international life can only take place when genuinely interdisciplinary analysis is accompanied by conscious efforts at a more philosophical level to reorient our thinking about the intellectual and practical significance of the various disciplinary, methodological, and epistemological boundaries in social science research. Each of the essays then offers a distinctive perspective on the what role boundaries might play in furthering or hindering the quest for a more pragmatic or inclusive social science.

In the first essay in Part II, "Against Epistemological Absolutism: Toward a 'Pragmatic' Center," Rudra Sil notes how the contemporary debates between positivists and postmodern relativists—over issues that are at least a century old—has resulted in the fragmentation of communities of scholars who share a common interest in a general phenomenon or a particular historical instance thereof. Only the most agnostic of scholars are able to take full advantage of the insights garnered from the range of hypotheses, descriptive inferences, interpretations, or narratives that pertain to the phenomenon or episode in question. Seeking to improve the possibilities for communication among a wider community of scholars embracing a variety of epistemological positions, Sil rejects epistemological "absolutism" and proceeds to identify a wide range of more nuanced, intermediate positions undergirding many of the great works of social science on such foundational issues as the objective/subjective nature of social reality, the inductive/deductive character of theory building, the purpose of social analysis, the significance of the fact/value distinction, and the question of how interpretations and theories might be evaluated in the absence of uniform methods of verification/falsification. In between the positivist and relativist extremes, Sil identifies a "spectrum" of epistemological perspectives on these issues, and he attempts to locate a "pragmatic" center in which the social construction of reality is acknowledged, but historical and empirical observations remain the primary basis for persuading audiences that theories, descriptive inferences, or context-sensitive interpretations are plausible and deserving of further exploration. Such a center is offered not as a superior or definitive alternative to positivist social science or postmodern relativism, but as a more flexible and practical approach to facilitate greater communication among scholars adhering to competing research traditions in the absence of a clear consensus on foundational issues.

In his contribution, "Thresholds and the Evolution of Scientific Knowledge: A Cautionary Note on Boundaries," Wade Huntley takes another approach to the problem of boundaries, or "thresholds" as he refers to them. A "threshold," as Huntley conceptualizes it, is meant to be crossed even as it marks a separation between fields or styles of inquiry. Thus, while Sil seeks to identify a pragmatic epistemological "middle ground" as a first step to overcoming deeply entrenched boundaries, Huntley argues that before we can even begin to consider strategies for transcending boundaries, it is essential to gain a more nuanced understanding of why thresholds emerged in the first place. He employs analogies from the natural sciences in conjunction with a Kantian epistemology to suggest that there may be sound reasons why distinctions between disciplines and paradigms have emerged and why further thresholds are likely to emerge

in the future. As the “universe of knowledge” expanded, boundaries became essential to the management and further expansion of that knowledge. The emergence of international relations as a discipline in its own right, a discipline whose concerns were separated from those of students of law and philosophy, was also a part of this process. Even the appearance of cross-disciplinary research, he notes, is essentially a component of this trend, reflecting rather than rejecting the process of increasing specialization. Finally, Huntley turns to one of the problems that distinguishes the social sciences from the natural sciences, the problem of the relationship of knowledge to action, or specifically, social knowledge to political action. It is this threshold—and not the boundaries between disciplines or paradigms—that Huntley considers to be the most fundamental challenge if the foundations of social science are to be made more meaningful. Considering the “constructivist” turn in international relations as well as “postmodern” and “critical” perspectives, he concludes by suggesting that while Kant may provide some answers for coping with fundamental epistemological problems in the social sciences, it is Marx who may provide some help in the search of logics to justify crossing the threshold between the “good citizen” and the “good scholar.”

Finally, Timothy Luke, in a chapter titled “The Discipline as Disciplinary Normalization: Networks of Research,” offers a critical—and provocative—analysis of the “normalizing effects of disciplinary practices and values in contemporary American political science.” Drawing in part on his own experiences and in part on his observations, he applies the “strong program” of the sociology-of-science perspective to “read” the discipline of political science. He does so by analyzing in Foucaultian fashion how the construction and application of professional “standards,” along with the system of departmental rankings, reputational concerns, doctoral training, and pressures on thousands of individual careers in different university settings, combine to produce “an implicit system of rules, which exerts, in turn, a normalizing effect upon both thought and action.” Luke’s chapter concentrates on three specific aspects of discipline in the discipline of political science. First, most generally, he examines the problem of how the discipline serves to provide an “ontological stability” for the larger social order by bringing to bear its own dominant constructions of power and knowledge to reproduce existing systems of social control. Second, he considers the requirements of “success” in the discipline, and links these requirements to the expectations of “professional correctness” that serve to normalize and provide symbolic order to professional-technical life. Finally, Luke investigates the question of reputation as institutionalized in “nomenclatures” that define the visibility of individual scholars and their departments, and carry with them imputations of prominence or insignifi-

cance without reference to the teaching or research practices of the thousands of scholars who make up the discipline.

The concluding chapter by Eileen Doherty provides a tentative appraisal of the intellectual returns to be had from the various chapters in Parts I and II of this volume. Doherty aims to show how the integrative frameworks contained in these may serve to improve the lines of communication between different disciplines, subfields and research traditions in order to bring together related stocks of knowledge. She also compares the payoffs of the different essays in light of the others, and considers the different ways in which each of the authors may be able to further extend his or her analytic frameworks and theoretical perspective by incorporating or accounting for insights and concepts from the work of the other authors in this volume.

Conclusion

If the sources of change in social and international life are complex, so must be the division of labor within the social sciences. However, as mechanisms of integration are essential to the preservation of order in society, so must we make room within this complex division of labor for scholars who, rather than deriving their projects within existing disciplinary structures or research traditions, consciously and deliberately pursue interdisciplinary, eclectic and integrative approaches organized around empirically grounded problematics. Original, question-driven integrative frameworks that transcend boundaries between disciplines or subfields and eschew *a priori* methodological commitments may very well interfere with attempts to construct a new universal system of concepts or a new analytic paradigm leading the social sciences to an age of scientific "maturity." But, rather than social proceeding blindly along the path of further specialization into progressively narrower "turfs," it may be worthwhile to seek a better understanding of how different concepts and units of analysis from across disciplines, subfields, and methodological traditions relate to each other in the study of particular aspects of international life. In order to do this, however, we need to place less emphasis on the definition of disciplinary boundaries or the formulation of new "paradigms," and we need to place a much greater premium on methodological pluralism and theoretical integration developed in the context of discrete, empirically-grounded questions. That is, we need a shift in focus away from the competition among research traditions and toward the expansion of "interdiscursive communities" in which there is a division of labor between those who seek to work within the framework of an established research tradition and those who seek to pursue eclectic, "paradigm-less" approaches that might produce