
Chapter 1

THE AMERICAN SCHOOL

We live in an era of interdependence.

—Robert Keohane and Joseph Nye

Politics determines the framework of economic activity.

—Robert Gilpin

AS GILPIN ONCE FAMOUSLY suggested (1975b, 43), may be defined as “the reciprocal and dynamic interaction in international relations of the pursuit of wealth and the pursuit of power.” By the pursuit of wealth, Gilpin had in mind the realm of economics: the role of markets and material incentives, which are among the central concerns of mainstream economists. By the pursuit of power, he had in mind the realm of politics: the role of the state and the management of conflict, which are among the central concerns of political scientists. IPE was to be understood as a marriage of two disciplines, integrating market studies and political analysis into a single field of inquiry. To a remarkable degree, that is precisely what IPE has come to mean today for most specialists—the common denominator even for critics who would prefer some alternative approach to the subject. IPE at its most fundamental, in short, is about the complex interrelationship of economic and political activity at the level of international affairs.

This chapter explores the origins of the dominant version of IPE that I have referred to as the American school. The notion of a “school” here is used loosely, by no means implying any sort of common goal or unified agenda. Scholars in the United States, as elsewhere, differ greatly on matters of substance as well as emphasis; in terms of theory, consensus is often lacking on even the most basic causal relationships. Yet at the level of ontology and epistemology, there does tend to be a shared understanding that distinguishes most inquiry in the United States from the way IPE is typically studied in many other places. The mainstream of U.S. scholarship can be considered a school because of its overarching worldview—a broadly accepted, if typically unacknowledged, sense of how things work and how they should be studied.

What accounts for the birth of IPE in the United States, and why did its early development take the path that it did? The story is vital, since the decisions taken early largely determined how the field was later constructed. Gilpin is a key part of the story, as we shall see. But most central are two familiar

names: Gilpin's fellow political scientists Keohane and Nye, a pair of intellectual entrepreneurs par excellence. More than anyone, Keohane and Nye may take credit for setting the study of IPE in the United States on its present course. They were not the earliest on the scene. Others preceded them in seeking to marry international economics and politics. Nor were they alone in midwifing the new field's birth. Others, too, recognized the potential for fresh insights and understandings. But no one came close to matching them for the speed and ingenuity with which they acted to convert potential into reality. Keohane and Nye were uniquely instrumental in nurturing the infant in its earliest years, shaping its growth in ways that today are simply taken for granted.

BIRTH

Though we date the modern study of IPE only from the 1970s, the field's roots go much further back. What I call the pioneer generation was not the first in history to think of connecting the economics and politics of international relations. Others in earlier generations had also tried. But never before had such efforts, however determined, managed to make it into the scholarly mainstream. The pioneers of the 1970s and beyond were the first to succeed in making IPE a recognized and respected academic specialty. That is their accomplishment, their real claim to fame. On the scale of human achievement, the resulting construction may not rank up there with the taming of fire or the start of organized farming. Yet it was certainly something more than a mere reinvented wheel. A true intellectual breakthrough, the new field's birth was a product of both individual initiative and historical circumstances—agency as well as contingency.

Dialogue of the Deaf

In terms of intellectual antecedents, IPE may be said to have a long and distinguished lineage, going back to the liberal Enlightenment that spread across Europe in the seventeenth and eighteenth centuries. Even before there were the separate disciplines of economics and political science, there was political economy—the label given to the study of the economic aspects of public policy. In a work published in 1671, the English administrative reformer Sir William Petty first spoke of “Political Oeconomies.” The term soon stuck. The classical economists of the eighteenth and nineteenth centuries, from the French physiocrats and Adam Smith onward, all understood their subject to be something called political economy, a unified social science closely linked to the study of moral philosophy. The earliest university departments teaching the subject were all designated departments of political economy. John Stuart Mill's monumental summary of all economic knowledge in the mid-nineteenth

century was titled *Principles of Political Economy*. Oscar Wilde, as we have seen, was still using the term at the dawn of the twentieth century.

Not long after Mill, though, a split occurred, fragmenting the social sciences. Like an amoeba, classical political economy subdivided. In place of the earlier conception of a unified economic and political order, two separate realms were envisioned, representing two distinct spheres of human activity. One was “society,” the private sector, based on contracts and decentralized market activity, and concerned largely with issues of production and distribution. The other was the “state,” the public sector, based on coercive authority, and concerned with power, centralized decision making, and the resolution of conflict. University departments were systematically reorganized to address the divergent agendas of the two realms.

The immediate cause of the split was an increasing formalization of economic study and a growing abstraction of its more advanced theoretical ideas. A “neoclassical” school emerged, inspired by the so-called marginalist revolution of the 1870s, when differential calculus first began to be used to explore the effects of small (“marginal”) changes in price or quantity. The aim was to develop a pure science, well removed from the minutia and distracting complications of everyday life. Gradually the discipline of economics, as it now came to be known, distanced itself from many of the practical policy and normative concerns that had previously motivated practitioners. So scholars with a more direct interest in institutions or issues of governance gravitated elsewhere, mostly toward the new discipline of political science with its central focus on the workings of political systems. In 1890, Alfred Marshall’s *Principles of Economics* supplanted Mill’s *Principles of Political Economy* as the English-speaking world’s leading font of economic wisdom. By the start of the twentieth century, the divorce of political science from economics was largely complete, with few points of intellectual contact or communication remaining between them. As one source puts it, “Both disciplines grew increasingly introspective” (Lake 2006, 758).

The gap was never absolute, of course. Even after academic economics and political science went their separate ways, a few hardy souls continued to stress connections between the pursuit of wealth and the pursuit of power. This was especially true at the radical fringes—in Marxist or neo-Marxist circles on the Left, where the superstructure of politics was unquestioningly assumed to rest on a foundation defined by the prevailing modes of production; or among laissez-faire liberals or libertarians on the Right, determined to preserve capitalism against the oppressive power of the state. There were also some notable exceptions in the mainstream, such as John Maynard Keynes, who cared deeply about the relationship between markets and politics, or Herbert Feis, whose study of the politics of preWorld War I global finance remains an early classic (1930). Many would also include Karl Polanyi, whose classic treatise on the social foundations of markets, *The Great Transformation* (1944), has

proved a lasting source of inspiration for later generations of scholars. For the most part, however, the void only grew deeper with time. References to political economy soon disappeared from polite conversation.

By the mid-twentieth century, the relationship between the two disciplines could best be described as nonexistent, a dialogue of the deaf. Radical perspectives were not entirely extinguished. Especially on the Left, the tradition was preserved in polemics over the allegedly inequitable relations between rich and poor economies. Earlier in the century the issue was framed in terms of “economic imperialism,” stirred by the writings of John Hobson, a radical liberal, and such Marxists as Rosa Luxemburg and Rudolph Hilferding—culminating in Vladimir Ilyich Lenin’s still widely quoted *Imperialism: The Highest Stage of Capitalism* (1917). Later, following the great wave of decolonization after World War II, debate was recast in various versions of dependency theory, stressing the political underpinnings of economic “underdevelopment” in newly independent states. Though divided on details, dependency theory’s many variants were united by the idea that the prospects for development in poor countries (the periphery) were conditioned by a global economy dominated by the rich (the core). Across mainstream academia, though, the frontier dividing economics and political science had become firm and impassable. Scholars working in the separate specialties of international economics and IR simply did not speak to each other.

My own experience as a university student half a century ago was typical. In my one IR course, taken while still an undergraduate, the emphasis was all on the “high politics” of conflict and national security in a dangerous, anarchic world. The policy agenda was preoccupied, not to say obsessed, with the cold war and the threat of nuclear weapons. Foreign economic relations were relegated to the realm of “low politics,” not really deserving of serious attention. Conversely, in the several courses on international economics that I sat through, first as an undergraduate and later in pursuit of a PhD, the spotlight was on issues of efficiency and stabilization, with public policy evaluated solely in terms of its implications for consumer welfare. No account was taken of the influence of differing institutional contexts or the political underpinnings of economic relations. The only dimension of power acknowledged was market power, stripped of any connection to interstate politics or issues of war and peace.

The dichotomy was summarized acutely in a seminal article published in 1970 by Strange, a British citizen and one of the most familiar—and beloved—names in IPE. We will hear more about Strange in the next chapter. Her article, as mentioned earlier, was provocatively titled “International Economics and International Relations: A Case of Mutual Neglect,” and provocation was what she had in mind. The void between international economics and IR had endured for too long, she declared, leading scholars from both traditions to neglect fundamental changes in the world economy. The dialogue of the deaf

should not be allowed to persist. A more modern approach to the study of international economic relations was needed—a new effort at “bridge building” to spotlight the crucial “middle ground” between economic and political analysis. Here, for the first time, was a full and compelling case laid out for a new field of study, a clarion call expressed in the fierce and uncompromising manner that came to be Strange’s trademark. The article was for all intents and purposes a manifesto.

To a degree, Strange was pushing against an open door. The impassable frontier between the disciplines had never been without its critics, even in the mainstream of the academic world. In the 1940s, political scientist Klaus Knorr (1947) had lamented the lack of economics in mainstream IR courses, suggesting that new efforts were needed to acquaint students of IR with the chief concepts and methods of inquiry used by economists. Discontent with the dialogue of the deaf was already brewing by the time that Strange spoke out. Here and there, scholars were groping their way toward reconnecting the two realms of inquiry, “reintegrating what had been somewhat arbitrarily split up” (Underhill 2000, 808).

Interestingly, most of these scholars were economists rather than political scientists. An early example was Jacob Viner, who had once been one of Knorr’s professors (and obviously left a mark on the younger man’s thinking). In a historical study, Viner (1948) sought to explore the relationship between “power” and “plenty” as objectives of foreign policy—a choice of terms foreshadowing Gilpin’s “pursuit of wealth” and “pursuit of power.” Viner’s focus was on the early doctrine of “mercantilism,” which dominated European economic thought for much of the seventeenth and eighteenth centuries. Mercantilism, in principle, called for the subordination of economics to the demands of politics. But as Viner demonstrated, how this might actually translate into practice was no simple matter. The relationship between power and plenty was in fact quite complex, and subject to change over time.

Twenty years later Richard Cooper published *The Economics of Interdependence* (1968), highlighting the political challenges posed by the growing interdependence of national economies. Establishing a theme that has echoed through the IPE literature ever since, Cooper drew attention to the inevitable tensions generated by market liberalization in a system of sovereign states. In 1970, the same year as Strange’s article, there was *Power and Money*, a short book by Kindleberger (1970). Kindleberger’s theme, too, was the growing tension between economic and political activity in an increasingly interdependent world. Basic Books, that same year, also began the Political Economy of International Relations Series, with myself as general editor. And 1971 saw the publication of economist Raymond Vernon’s memorable *Sovereignty at Bay*, which heralded the arrival of the multinational corporation as a key political actor on the world’s stage.

The period also saw the reissue of a long-neglected study by Albert Hirschman, *National Power and the Structure of Foreign Trade* (1945/1969), now rightly regarded as a classic. Written during World War II, the book highlighted the hidden politics of international trade: how relations of dominance and dependence among states may arise naturally from the asymmetries of foreign commerce, and how import and export policies may be used opportunistically by governments to exert political pressure and leverage. Hirschman's themes too have echoed through the IPE literature ever since.

In a sense, therefore, the new integrated approach that Strange was calling for had already begun to coalesce. Can we date the infant's arrival precisely? Unfortunately, scholarly fields of study don't come equipped with an official birth certificate, listing the time and place of origin. Articles or books start to appear, such as those by Viner and Cooper. At first a scattered few, the contributions eventually multiply until—eureka—we all realize that something new has been discovered. Only in retrospect do we become aware that a birth has taken place. Today all we can say is that by 1970, it was already becoming evident that there was something new under the sun (the pessimism of the Old Testament Ecclesiastes notwithstanding).

Nonetheless, Strange's manifesto deserves a special place in the annals of IPE. Her summons to battle was by no means the sole spark to ignite a renewed interest in the political economy of IR; indeed, appearing in a British journal, the article may not even have been seen at the time by many on the U.S. side of the Atlantic. Yet looking back, we can now appreciate how significant it was. Its publication marked something of a turning point. Never before had the brewing discontent among specialists been so effectively distilled and bottled. Nowhere else had the issue been posed in such concise and focused terms. As such, it is as good a candidate as any to mark the moment of the new field's birth.

A Changing World

What triggered the discontent that Strange captured so effectively? The birth of a new field does not take place in a vacuum. Particularly in the social sciences, intellectual developments tend to be tied to a historical context—to new events and trends that make old ways of thinking inadequate. And so it was with IPE. Fundamental changes were occurring in the world—the “real” world, as we social scientists like to call it (mostly without any sense of irony). Both the politics and economics of global affairs were mutating, calling for new understandings of how things work and how they might be studied.

Most striking was the remarkable recovery of the European and Japanese economies after the devastation of World War II. By the 1960s, a decisive shift seemed to be taking place in the balance of economic power among industrialized nations. At midcentury, the United States bestrode the world economy

like a colossus. But with its growth rate slowing and its balance of payments mired in deficits, the country now looked to be on the brink of decline. Continental Europe and Japan, meanwhile, were roaring back, once again forces to be reckoned with. America's moment of economic dominance—of "hegemony"—appeared just about over (mistakenly, as it turned out, but that's another story). Meanwhile, postwar decolonization had brought new attention to the challenges and dilemmas of economic development. Pressures were mounting for a New International Economic Order that would fundamentally transform the rules governing relations between the wealthy "North" and the poverty-stricken "South."

Behind these changes was a growing interdependence of national economies, as noted by Cooper and Kindleberger, among others. At the end of the Second World War, following years of depression and conflict, links between national economies had reached a nadir. Trade and capital movements were tightly controlled. Insularity was the rule. But then came a gradual liberalization of barriers, particularly among the industrialized democracies, promoted by the liberal policies of the United States and shepherded by the newly created GATT and IMF. Slowly, then with more speed, insularity was replaced by competition, as tariffs declined and currency convertibility was restored. Year by year world trade grew more rapidly than output, bringing greater openness and mutual dependence. And soon financial flows began to accelerate as well with the growth of offshore currency markets—the so-called Eurocurrency markets—from the late 1950s onward. By the end of the 1960s, it was evident that the expansion of international economic networks had reached a critical point. Power now seemed to be slipping from states, limiting their ability to attain critical goals. For governments, markets were becoming a distinct threat, whatever their material benefits.

Following the disastrous experience of the 1930s, when economic warfare was rife, the victorious Allied powers had sought to "depoliticize" international economic relations. The aim had been to control the "mercantilist" temptation to use trade or financial controls to promote national interest at the expense of others. To the extent possible, protectionism—interventionist measures designed to protect national economies from the vicissitudes of international competition—were to be firmly suppressed.

Politics could not be eliminated entirely, of course—certainly not so long as the cold war persisted. How else, for example, could we explain Washington's support of the construction of the Common Market in Europe despite its discrimination against U.S. exports? But the hope was to hold political considerations to an absolute minimum. GATT and the IMF were created to ensure that disputes among states would center largely on technical issues, not broader security concerns. Although trade or monetary negotiations might be tough, even bitter, they would remain at the level of detail. Outside the Soviet bloc,

the shared commitment was to a vision of a peaceful and prosperous world economy based on liberal market principles.

In the immediate postwar period, the strategy had worked. Despite challenges, the underlying structure of the system had seemed sound, even immutable. By the 1960s, though, with state power now being threatened, economic peace was becoming increasingly difficult to sustain. Tensions were once again on the rise. By the 1970s they were to multiply exponentially, following the first oil shock in 1973 and the collapse of the Bretton Woods system of pegged exchange rates. A new era of slower growth and higher inflation—“stagflation”—appeared to be on us. Protectionism, once again, seemed to be on the rise. The world economy could be depoliticized no longer.

The threat to state power, as Cooper insightfully pointed out, was actually threefold. First, growing interdependence was increasing the number and magnitude of potential shocks to national economies—what we could call a *disturbance* effect. Second, interdependence was retarding the achievement of diverse policy objectives, political as well as economic—a *hindrance* effect. And third, it was increasing the risk of mutually damaging competition between national authorities—a *competitive* effect. Such challenges could hardly be dismissed as merely technical, especially not when matters of vital state interest were at stake. Clearly, the salience of international economic relations was rising. No longer, it seemed, could the pursuit of wealth be casually relegated to the realm of low politics.

Conversely, the salience of national security concerns now appeared in abeyance. This was because of a growing détente between the United States and the Soviet Union, the two nuclear superpowers. The cold war had for years held center stage, reaching a dramatic peak in the brinkmanship of the 1962 Cuban missile crisis. But by the late 1960s, despite the distractions of the protracted Vietnam conflict, the competing Western and Soviet blocs seemed to be entering a new era of decreased tensions. Détente did not mean that the high politics of war and peace had suddenly lost all relevance; indeed, in the 1980s the cold war was to intensify once again, as Ronald Reagan declared battle on the “Evil Empire.” But for the time being at least, it meant that students of world politics could now safely divert some of their attention elsewhere—for example, to IPE.

INFANCY

Enter Keohane and Nye—two individuals, in the words of their former professor Stanley Hoffmann (1989, 275), “whose character matches their intellectual gifts.” Few scholars moved as quickly or effectively to nurture the newborn field in its infancy. Their impact was felt in three ways: through the force of their ideas, through their encouragement of the work of others, and through

their role in providing a powerful vehicle, the journal *International Organization* (IO), to help drive the field's development. In all three respects, the fruits of their early entrepreneurship have proved remarkably durable. Their legacy lives on.

A Collaboration Begins

Consider first their ideas. Neither Keohane nor Nye went into political science with a special interest in political economy; that came later. Nor did they begin to work together as a team until as late as 1969, just prior to the appearance of Strange's manifesto. But their collaboration over the next decade, culminating in the first edition of their landmark volume, *Power and Interdependence* (Keohane and Nye 1977), can fairly be described as foundational. Later, though they remained close and still joined forces on occasion, their careers took divergent paths. Yet the general worldview they articulated in those early years, while still working partners, has endured and remains basic to the discourse of IPE.

Nye, the older of the two (by four years), entered the PhD program at Harvard University in 1960 still somewhat unsure of a career direction. After graduating with high honors from Princeton University in 1958, followed by two years at Oxford as a Rhodes Scholar, he arrived in Cambridge, Massachusetts, thinking that he might perhaps specialize in comparative political development. His doctoral dissertation, which he completed in 1964, was about regional integration in East Africa. Nye then accepted a junior faculty position at Harvard, which has remained his professional home ever since, apart from the occasional visiting professorship elsewhere along with two extended stints of government service during the presidencies of Jimmy Carter and Bill Clinton. It was only after he started teaching that he began to study IR seriously.

In the 1980s, following two years as a deputy to the undersecretary for security assistance, science, and technology in President Carter's Department of State, he and Keohane went their separate ways. Nye's interests gravitated more toward applied policy, where he has made a mark as a public intellectual with such books as *Bound to Lead* (1990), *The Paradox of American Power* (2002), and *Soft Power* (2004b). During the Clinton presidency he returned to Washington, first as chair of the National Intelligence Council and then as the assistant secretary of defense for international security affairs. For each of his three posts in Washington he received distinguished service awards from the U.S. government. Coolly cerebral, yet comfortable in the corridors of power, Nye was once asked why he kept returning to academia after occupying positions of high authority in the nation's capital. "Because I want to understand how policy is made," he replied. The pace of government life, he has written, does "not permit wide reading or detailed contemplation" (Nye 1989, 206). A

scholar-statesman needs time and the perspective of distance in order to see things with sufficient clarity.

Keohane, by contrast, preferred to remain firmly planted in the groves of academe after receiving his PhD from Harvard in 1966. His first teaching post was at Swarthmore College, a well-known liberal arts school in Pennsylvania, where he remained until 1973. It was there that he had his first systematic exposure to political economy, in a course that he taught with economist Van Doorn Ooms. Since 1973, Keohane has held faculty positions at some of the highest-ranked universities in the United States, including Duke, Harvard, Princeton, and Stanford. An outstanding scholar of remarkably broad erudition, he has the universal respect of his peers (witness the unanimous vote he received in the private survey reported in my introduction). He has been elected president of both the American Political Science Association (APSA) and the International Studies Association (ISA). Over a career spanning more than a third of a century, Keohane has remained a major force in the ongoing construction of IPE. His contributions are multiple, as will be evident in subsequent chapters.

Twice, Keohane gave up prestigious professorships, once at Stanford and once at Harvard, so that his wife, Nannerl Keohane, could accept attractive academic appointments—first as president of Wellesley College, and later as president of Duke University—while keeping their family together. In that selflessness, some might see an homage to his mother, a public school teacher and political activist whose “moral energy,” as Keohane puts it (1989b, 404), made a lasting impression on his personal value system. Others might see a form of tribute to the political theorist Judith Shklar, who Keohane has described as his strongest intellectual mentor during his years of graduate study (Institute of International Studies 2004, 1). Though Shklar was, according to him, the most powerful mind in Harvard’s Government Department at the time, she could win no more than an untenured lecturer’s position because of discrimination against her as a woman. Today, Keohane is well-known for having mentored some of the most prominent women in IPE, including Lisa Martin, Helen Milner, and Beth Simmons, among others. In 1997, he received the first Mentorship Award from the Society for Women in International Political Economy.

Though not insensitive to the feelings of others, Keohane is a person of strong convictions, which he does not mind sharing openly or even bluntly. Once, a colleague at a conference made an offhand remark that while intended to be humorous, could have been interpreted as a bit sexist. Keohane dressed him down in a manner that left the rest of the conference participants, including myself, temporarily speechless. Another time, I witnessed him serve as a discussant for a research paper presented by a young woman just out of graduate school. Not impressed by her scholarship, Keohane tore her work to shreds, questioning her understanding of basic IR theory. I left the room thinking that



Figure 1.1. Robert Keohane. Photograph by Jon Roemer

the young woman's career was over before it had begun. She had the memorable name of Condoleezza Rice.

Keohane does have a sense of humor. But like the man himself, his wit is refined and just a bit remote. Nothing illustrates that more than the cover of a collection of essays he published not long ago titled *Power and Governance in a Partially Globalized World* (Keohane 2002). Pictured on the cover is a sculpture called *Capricorn* by Max Ernst, featuring a seated king and queen rendered in a Dadaist style not unlike that of early Pablo Picasso when he was most influenced by the primitive art of Africa. The king, a version of the sea god Capricorn, sits imperiously on his throne, holding aloft a scepter; his

queen, portrayed as a mermaid, sits demurely at his side. What did this odd regal couple have to do with Keohane's sophisticated ruminations on interdependence and institutions? I wondered when I first picked up the book. But it was not long before I grasped the relevance. Capricorn and his consort were in fact the embodiment of power and governance in a world still far from fully globalized. Keohane had come across the sculpture in a museum in Houston, Texas, while putting the final touches on his essay collection, and had immediately decided to make it the centerpiece of the book's cover. Like a good *New Yorker* cartoon, the joke was calculated, and not obvious. It could hardly have been more clever.

Though Nye and Keohane overlapped as graduate students at Harvard, they did not meet until 1967. Their active collaboration began two years later when they began planning a conference, held in 1970, on new forces in world politics. The resulting collection of papers, which they jointly edited, appeared as a special issue of *IO* in 1971 and was soon published as an edited volume under the title *Transnational Relations and World Politics* (Keohane and Nye 1972). In their introductory and concluding essays for the collection, as well as in subsequent publications, they began to work out a new conception of the dynamics of international economic relations. A few years onward, these efforts culminated in *Power and Interdependence*, widely hailed as a milestone in the construction of modern IPE. In *Power and Interdependence* they laid out a vision of the world that remains influential to the present day. They even gave their vision a name: "complex interdependence" (Keohane and Nye 1977).

Complex Interdependence

Complex interdependence was defined by three main characteristics: multiple channels of communications, an absence of hierarchy among issues, and a diminished role for military force. The vision was posed as a challenge to the classic state-centric paradigm of world politics that had long dominated the study of IR in the United States. For decades, students of IR theory like themselves had been taught to think as "realists." States were seen as the only significant actors in world politics, conceived for analytic purposes as purposive, rational, and unitary actors. Moreover, states were assumed to be motivated largely by issues of power and security, and to be preoccupied above all with the danger of military conflict. In U.S. scholarly circles, realism ruled.

For Keohane and Nye, the realist paradigm had now become dated. World politics was being transformed. Just look around, they suggested. Who could fail to notice the seeming breakdown of order in the global economy? The government-controlled system of pegged exchange rates established at Bretton Woods had collapsed following America's abandonment of the convertibility of the dollar into gold in 1971. The 1973 oil shock had sharply raised energy prices, bringing stagflation and uncertainty in their wake. Disputes and ten-

sions were on the rise; arguments over everything from tariff policy to the management of the oceans were becoming more and more politicized. For Keohane and Nye, writing in the mid-1970s, the radical change of atmosphere was pivotal. The safe certainties of the earlier postwar period had seemingly vanished into thin air.

The key, they contended, lay in the increasing fragmentation and diffusion of power in economic affairs, stemming from the growing interconnectedness of national economies. As Keohane and Nye said in the opening line of *Power and Interdependence* (1977, 3), “We live in an era of interdependence.” States might still be central actors in international affairs, but with the expansion of the global marketplace they could no longer claim sole authority to determine outcomes. The liberalization of trade and finance was widening the range of “transnational” relations, adding new cross-border contacts, coalitions, and interactions beyond those controlled by the foreign policy organs of government. Economic interdependence was spawning a growing swarm of transnational actors—individuals and entities whose control of resources and access to channels of communication enabled them, too, to participate meaningfully in political relationships across state lines. Hence, Keohane and Nye maintained, a new way of thinking was needed: a broader paradigm that would explicitly admit the full panoply of relevant actors. Governments could no longer monopolize analysis. If we really wanted to understand how things work, scholarship would have to catch up with the facts.

The transformation of world politics was not universal, of course. Keohane and Nye did not offer complex interdependence as a fully accurate characterization of the new era of world politics. Rather, it was best understood as an “ideal type” that could be compared with the realist state-centric paradigm in order to highlight what realism had overlooked. The idea was analytic, not descriptive. Much of the real world still exhibited elements emphasized by realist theory. Between the two superpower blocs, for example, security issues continued to dominate the policy agenda despite an evident lessening of tensions after the late 1960s; even in a period of *détente*, direct economic contacts remained “thin.” Nor had the salience of military force declined all that much in a number of other hot spots around the globe, including especially the Middle East as well as parts of Africa and Asia. But at least among the advanced industrial nations of Western Europe, North America, and Japan—encompassing the world’s dominant economies—the facts clearly had changed. Economic relations were growing exponentially; the threat of war seemed increasingly remote; and transnational actors were posing an ever-greater challenge to government. Here it was certainly legitimate to speak more of a “zone of complex interdependence,” as Keohane later described it (1989a, 9).

The pair’s timing was exquisite. According to Harry Johnson (1971), the most important requirement for successful propagation of a new, revolutionary set of ideas is the existence of an established orthodoxy that has clearly grown

inconsistent with reality—a dominant paradigm no longer able to account for increasingly recurrent anomalies. For Johnson, once known as “the economist’s economist,” the orthodoxy he had in mind was Keynesianism, which was then in the process of being dethroned by the monetarism of Milton Friedman and his disciples at the University of Chicago. For Keohane and Nye, the political scientists, the orthodoxy at issue was realism. In a world of accelerating interdependence, the state-centric paradigm was indeed vulnerable to challenge, demonstrably in growing conflict with the facts of everyday experience. At least among the industrial nations, realism had become unrealistic. It also helps, Johnson went on, if the new ideas are genuinely novel yet absorb as much as possible of the valid (or at least not readily disputable) components of existing orthodoxy. This Keohane and Nye accomplished by conceding the still-central role of states even while insisting that governmental authority was being eroded.

Where did their ideas come from? Keohane insists that “in the genealogy of the idea of complex interdependence, Joe should certainly be given top billing. . . . At this time (1972–74), Joe was going to meetings, networking, and picking up impressions. His sense of the contradictions between realism as taught at Harvard then and the complex world of multiple actors and issues that he dealt with in these meetings generated the impetus for our reflections on complex interdependence.” But of course it was their *joint* reflections, mostly the result of long conversations at Nye’s home in nearby Lexington, that ultimately translated one partner’s sense of contradictions into a revolutionary new paradigm. Their collaboration was facilitated by a visiting fellowship that Nye arranged for Keohane at Harvard’s Center for International Affairs (CFIA) in 1972. The well-endowed CFIA, thanks to Nye’s efforts, had also funded the 1970 conference that resulted in *Transnational Relations and World Politics*.

According to a later recollection (Katzenstein, Keohane, and Krasner 1999b, 15), the transnational paradigm was first inspired by contact with Raymond Vernon, whose *Sovereignty at Bay* was then in its final stages of preparation. Vernon was a colleague of Nye’s at the CFIA and was instrumental in encouraging the pair to organize their 1970 conference (Nye 1989, 203). The institutional connection between the two political scientists and the economist was critical. “Without that connection,” Keohane suggests, “I am not sure we would have gone down the IPE route.” More than a third of the publications cited in the introduction to *Transnational Relations and World Politics* have to do with multinational corporations.

Equally influential was the pioneering work on regional integration by political scientists Karl Deutsch (1957) and Ernst Haas (1958), inspired by developments in postwar Europe that highlighted the role of voluntary agreements among states and the possibility of multiple actors in world politics. An extrapolation from integration studies to the notion of interdependence came quite

easily for Nye, whose dissertation on East African integration had been published in 1965. As Nye wrote later (1989, 203), “I felt that many of the insights from regional integration theory were applicable to the broader dimensions of international economic interdependence that were becoming more prominent.” His and Keohane’s debt to Deutsch and Haas was explicitly acknowledged in a paper published in 1975 (Keohane and Nye 1975).

Major debts were also owed to the economists Cooper and Hirschman. Following Cooper’s lead, Keohane and Nye stressed the tightening constraints that interdependence was imposing on the policy autonomy of states—the widening “control gap,” as they put it, between state aspirations and state capabilities. Following Hirschman, they laid emphasis as well on the concept of asymmetrical interdependence as a basic power resource.

Looking back later, Keohane and Nye (1987) expressed a degree of modesty over what they had wrought. Their goal, they claimed, simply had been to provide an alternative “ideal type” for analytic purposes. Their intent was not to discredit realism but rather to supplement it. In their words, “We regarded the two as necessary complements to one another. . . . The key point was not that interdependence made power obsolete—far from it—but that patterns of interdependence and patterns of potential power resources in a given issue-area are closely related—indeed, two sides of a single coin” (728, 730). The concept of complex interdependence itself, they insisted, was “underdeveloped” (733). “We did not pursue complex interdependence as a theory, but as a thought experiment about what politics might look like if the basic assumptions of realism were reversed. We therefore did not draw upon . . . theory as fully as we might have” (737).

Their modesty, however, is misplaced. It is true that realism was not discredited. Complex interdependence was indeed best conceived as a complement to realism, not a substitute. It is also true that the new paradigm failed to make the grade as a formal theory. A theory is best defined as a set of general statements combining the features of logical truth and predictive accuracy. Logical truth means that some of the statements (the assumptions or premises) logically imply the other statements (the theorems). Predictive accuracy means that the statements can be cast in the form of falsifiable predictive statements about the real world. Clearly, complex interdependence did neither. It could not be used directly to explain state behavior or bargaining outcomes.

Yet the accomplishment was undeniable. In ontological terms, the idea broke new ground. Here was a wholly different alternative to IR’s then-prevailing paradigm—a fresh vision of the world that contrasted sharply with the realist model of unitary states single-mindedly preoccupied with the high politics of war and peace. Here was real value added. Keohane and Nye made us look at the world anew. In so doing, they facilitated the birth of a new field of study.

Today we take for granted that interdependence in the world economy can be analyzed in political terms, not just as an economic phenomenon. We also

take for granted that we can examine patterns of interdependence by separate issue areas. We do so because, implicitly or explicitly, we all now share the ontology bequeathed to us by Keohane and Nye—a sense that the three characteristics of complex interdependence define the essential nature of the present-day international system. Without this intersubjective understanding, systematic study of the low politics of trade and finance would have been more difficult, if not impossible. With it, we have a whole new insight into how things work. The term complex interdependence itself may no longer be particularly fashionable in the IPE literature. Many scholars have forgotten it completely. But the *weltanschauung* it represents is now undeniably a part of the collective unconscious of the field.

Three Models of the Future

Keohane and Nye also made a lasting impact through their encouragement of the work of other scholars, largely via collective research projects that they were instrumental in organizing. Their first such undertaking was, of course, *Transnational Relations and World Politics*. A second special issue of IO came four years later, following two conferences of authors in Massachusetts and Washington, DC (in which I was privileged to participate as a discussant). That special issue, too, was subsequently published in book form, under the title *World Politics and International Economics* (Bergsten and Krause 1975). Keohane and Nye helped to conceive the second project and served, together with the issue's coeditors, on an informal steering group charged with bringing it to fruition. Together with one of the coeditors, the pair also wrote the project's introductory chapter laying out a basic framework for analysis. Even after he and Nye subsequently went their separate ways, Keohane for his part continued to play a major role in yet more such joint efforts, some of which (as we shall see) have achieved landmark status of their own in the field's history. One great advantage of collective efforts of this kind is the part they can play in framing a program for research. Certainly that was the case with those two projects in the 1970s, which between them provided a compelling set of signposts for the next generation of scholars. The first project, *Transnational Relations and World Politics*, was subsequently described as "chiefly a pointing exercise that made clear how much interesting activity had escaped the attention of analysts" (Katzenstein, Keohane, and Krasner 1999b, 16). The second, according to Keohane, "put political economy on the agenda" (Institute of International Studies 2004, 3).

Another great advantage is the debate such efforts can stimulate among the participants, which in turn aids in identifying critical problems and clarifying issues. That too was the case with those two projects in the 1970s. Most notable was a running debate through both projects between Keohane and Nye, on the one side, and their good friend Gilpin, on the other—a debate that did much

to refine thinking about interdependence and its consequences. Keohane and Nye promoted their new vision of complex interdependence. Gilpin became the chief defender of the older realist tradition—"the dean of realist international political economy in the United States," as one source puts it (Murphy 2000, 798).

Realism came easily to Gilpin, who served four years as an officer in the U.S. Navy before going on to complete a doctorate at the University of California at Berkeley in 1960. Gilpin's earliest scholarly publications (1962, 1968), on nuclear weapons policy, were focused entirely on the high politics of conflict and national security. At Princeton University's Woodrow Wilson School of Public and International Affairs, where he taught from 1962 until his retirement in 1998, Gilpin says he was considered "the last of the Cold Warriors" (*International Relations* 2005, 368). But his was not a doctrinaire realism, rigidly closed to alternative perspectives. For him, realism represented a philosophical view of society and politics—one way, among many, of looking at the world—not a definitive portrayal of reality. His own preferred label was "soft realist" (361). Though Gilpin took a certain amount of pride in being the sole Republican on the Woodrow Wilson School faculty, he was always quick to add that he was a "Vermont Republican," not the more doctrinaire Goldwater-Reagan type.

At the outset of his scholarly career, Gilpin had no more interest in political economy than did Keohane or Nye when they were getting started. Yet like his two friends, he couldn't help but notice the seeming breakdown of order in economic affairs, which appeared to challenge inherited presumptions. For him, too, the radical change of atmosphere was pivotal. His early work on nuclear weapons policy, which had highlighted the links between technological development and world politics, provided a natural segue into a new set of issues.

Driven by an intense curiosity, Gilpin began to read the work of Cooper, Strange, and Vernon, all of whom, he acknowledges, were major influences on his thinking (*International Relations* 2005, 367). He also began to teach himself the low politics of economics in order to better understand the implications of interdependence. In this pursuit, he showed all the best characteristics of a true intellectual: an open mind, a keen devotion to learning, and an admirable dedication to craft. Throughout his career, Gilpin had little interest in seeking fame and fortune as an academic superstar, though that might well have been within his grasp. A modest person, self-effacing and even a bit shy, he just wanted to *understand*.

Gilpin had no hesitation in seeking help from his colleagues. Until 1971, I taught alongside him at the Woodrow Wilson School and can remember many an occasion when he would wander into my office to ask a question about some fine point of economic analysis. Usually the question had been scribbled down on a small notepad that he carried with him to record thoughts in situ. Even after I moved on to another university, our private seminars continued at

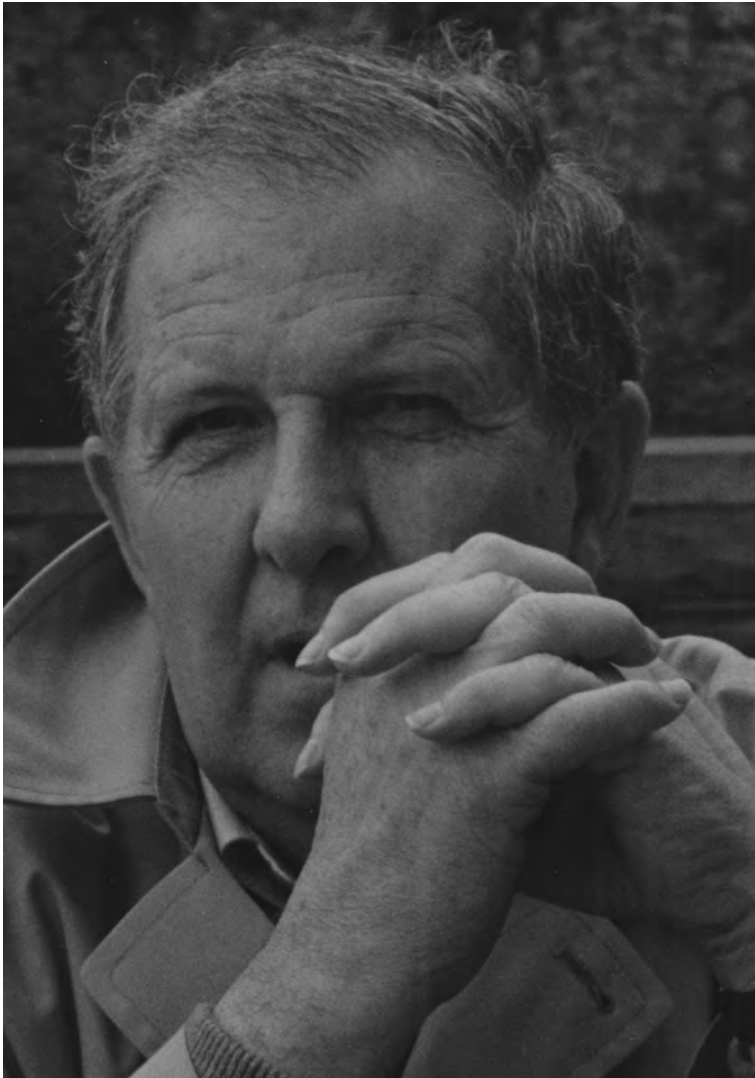


Figure 1.2. Robert Gilpin. Courtesy of Princeton University Press

long distance, as Gilpin he noted (1975b), with characteristic graciousness, in the preface to *U.S. Power and the Multinational Corporation*. It wasn't long before he was ready to take on the issues raised in *Transnational Relations and World Politics*.

The emergence of transnationalism, he acknowledged, could not be denied. But that didn't mean that realist theory had thus become obsolete; quite

the contrary, in fact. In insisting that a transformation was occurring in world politics, Gilpin argued, Keohane and Nye were guilty of hyperbole. Transnationalism could only be understood within the context of the traditional state system, dating back to the Peace of Westphalia of 1648. For the former naval officer, states were still the primary actors on the world stage and security interests remained the key determinants of economic relations. In Gilpin's words (1972, 54), "Politics determines the framework of economic activity and channels it in directions which tend to serve . . . political objectives." Where Keohane and Nye went astray, he felt, was in failing to recognize the extent to which transnational actors and processes ultimately remain fundamentally dependent on the pattern of interstate relations. "Bob was crucial in pointing out that markets rest on political decisions," a colleague has written me about Gilpin. "Others have made this point about domestic markets, but Bob said it best about the international economy."

At issue was the nature of the underlying connection between economic and political activity, an age-old question that had long divided scholars of political economy. Does economics drive politics, or vice versa? Three schools of thought could be identified, Gilpin suggested, all drawn from traditional IR theory—liberalism, Marxism, and realism—each offering students of IPE its own distinct "model of the future." Liberals and Marxists shared a belief that economics was bound to dominate politics, though of course they differed enormously on whether this was a good or bad thing. By contrast, realists retained faith in the power of political relations to shape economic systems. Keohane and Nye, with their paradigm of complex interdependence, could be understood as the latest heirs of liberalism; their approach, widely seen as a new variation on an old theme, was soon given the label "neoliberal institutionalism." Emphasizing the pair's early Harvard connections, one source called it simply the "Harvard school of liberal international theory" (Long 1995). I will have more to say about neoliberal institutionalism in chapter 4. Gilpin himself, of course, was a barely reconstructed realist.

In *World Politics and International Economics* as well as *U.S. Power and the Multinational Corporation*, published the same year (Gilpin 1975a, 1975b), he sought to respond to the new concept of transnationalism by carefully spelling out the strengths and weaknesses of each of the three approaches. Gilpin's aim was to facilitate clearer and more consistent theorizing about the implications of interdependence. But in so doing, he also happened to provide a convenient template for future scholarship—an "intellectual edifice," as one friend describes it in private correspondence—that stands as perhaps his most lasting contribution to the construction of the infant field.

In IPE textbooks today, Gilpin's three "models"—also referred to as paradigms or perspectives—are still regarded as the logical starting point for most serious discussion, even if then amended or combined in various ways. Few sources even bother any more to credit Gilpin for the taxonomy. Like the

notion of complex interdependence, this classification has simply become an unexamined part of every specialist's toolkit. Even fewer sources credit Keohane and Nye for their role in animating the projects that prompted Gilpin's creative thinking.

International Organization

Finally, Keohane and Nye deserve recognition for their leadership in building a solid launching pad for the new field. That was the journal *IO*, which has long been recognized as the premier platform for the publication of IPE scholarship in the United States. The U.S. version of the field, as one source observes, "has been centered in *IO* since 1971" (Katzenstein, Keohane, and Krasner 1999b, 5). According to another, the American school might just as well be called the "*IO* school" (Murphy and Nelson 2001). Keohane and Nye played a critical role in establishing *IO* as a core venue for IPE's pioneer generation.

Disseminating innovative ideas in the academic world is not easy, given the importance of peer review in determining what gets into print and what does not. Editorial boards instinctively tend to favor submissions that run along familiar lines. Anything out of the ordinary—such as an effort to marry traditionally separate disciplines—is bound to encounter skepticism, if not outright hostility. Back in the early 1970s, therefore, there was no guarantee that any of this newfangled hybrid work, no matter how well packaged, would ever see the light of day. A publishing outlet was needed, and that is what *IO* provided. Without the journal, the infant field in the United States might well have ended up stillborn.

Not that nursing an infant field of study was *IO*'s original intent. The journal was founded in 1947 by the World Peace Foundation, a Boston-based philanthropic institution that itself dated back to 1910. The journal's first mandate was implied by its name—to focus on the new international organizations that had been set up at the end of World War II. These included the full range of agencies established under the aegis of the United Nations, not just economic institutions like the IMF and GATT. The idea was to promote a comparative study of international organizations and how they worked in practical terms. For more than two decades, that is just what the journal did. Most articles published in *IO* in the 1950s and 1960s emphasized applied policy analysis or commentary; substantial space was also devoted to detailed summaries of the activities of various UN bodies. Little work was informed by general social science theory. In the words of a later survey, there was no "theoretical hook . . . no conceptual framework that could tie these insights together" (Martin and Simmons 1999, 92–93).

But then the journal was effectively taken over by Keohane and Nye, who along with several other younger scholars were invited to join the board of

editors in 1968. It might have been, as Nye avers (1989, 202), a “serendipitous event.” Yet it was also a turning point. As Keohane has written (1989b, 408), “The established members of that board may well have been dismayed by the consequences of their decision, because their new colleagues proceeded within five or six years totally to reconstitute the board.” By 1972, Nye had become chair of the board; by 1974, Keohane had become the journal’s editor, a position he was to hold for six years.

Almost immediately, the pair engineered a shift of emphasis in editorial policy. Attending a conference in early 1969 on U.S. foreign policy and international organizations, which was intended for a special issue of *IO* (Finkelstein 1969), the two were appalled by the atheoretical content of most of the papers—“old thinking par excellence,” in Keohane’s biting words. The journal’s traditional approach, they argued, needed a new orientation: toward the study of world politics in general and a more systematic analysis of political behavior in particular. When invited to demonstrate what they meant, they responded with the collective project that became *Transnational Relations and World Politics*, soon to be followed by other initiatives. The intent of their first special issue, Nye later explained, was to escape from the journal’s “dull” preoccupation with roll call votes and other institutional trivia. The idea was to “put the horse back in front of the wagon by first describing patterns of interaction in world politics and then asking what role international institutions do or should play” (Nye 1989, 202).

Keohane was especially cognizant of the opportunity that *IO* presented. As he later remarked in an interview (Institute of International Studies 2004, 3):

The advantage of being a journal editor is that you’re at the center of the process. You see it and you can shape it, to some extent. In my first issue I wrote an editor’s essay called “*International Organization and the Crisis of Interdependence*,” where I made a claim (and this was in 1974 or 1975, right after the oil crisis) that there was a crisis going on of interdependence, that growth had slowed down or stopped in the West. There was a general sense that we had to restructure the system after Bretton Woods collapsed, and so it was a moment when it was clear that people needed to think politically about the world economy, because it wasn’t automatically taking care of itself. I was in the right place at the right time.

By the mid-1970s, *IO* was firmly established as the flagship of the growing fleet of IPE scholars in the United States. In the years since, it has been consistently ranked as among the most influential of political science journals (Crewe and Norris 1991; Nisonger 1993; Garand and Giles 2003). A survey of U.S. political scientists in 2003 placed *IO* fifth among all journals in the discipline and, by a wide margin, first among specialists in IR (Garand and Giles 2003). Researchers in the U.S. tradition still look to *IO* first for the latest developments in IPE scholarship.

CUSTODY

Against the odds, then, the newborn field not only survived but flourished, as the healthy offspring of a marriage of two disciplines. But the liaison was a limited one, a narrow marriage de convenance of the two subspecialties of international economics and IR. On a broader scale, the two parent disciplines remained divorced—still firmly separated in terms of orientation and formal academic organization. So who would take custody?

In principle, both parents might have played a role in shaping the infant interdiscipline's development. In practice, however, economists effectively abdicated after the baby's birth, leaving custody to the political scientists. Despite the fact that much of the earliest work in the field was actually done by economists—the likes of Hirschman, Viner, Cooper, and Kindleberger—IPE to this day remains a peripheral interest in most economics departments.

While in the minority, some economists do pursue something that they call political economy, applying the tools of economic analysis to the study of public policy. Many, such as Thomas Willett (1988; Willett and Vaubel 1991), follow the early lead of the Swiss economist Bruno Frey (1984), emphasizing so-called public choice theory. But little of this work makes a serious attempt to incorporate the insights or intuitions of political science. With its methodological individualism, focusing on the individual as the basic unit of analysis, along with its stress on formal modeling, the oeuvre of most economists amounts to little more than an application of traditional economic theory to decision making in the political arena. It might best be labeled the microeconomics of IR. It can hardly be described as a genuine merger of disciplines.

As IPE has grown in the United States, political science, rather than economics, clearly has been the major influence. In no small measure, this was due to the determined entrepreneurship of Keohane and Nye. From the start these two, along with Gilpin and others, moved to claim “ownership” of IPE, raising the newborn as a branch of their own discipline. IPE, even while absorbing elements of economics theory and methodology, seemed a natural extension of IR. As one colleague privately suggested to me, Keohane and Nye “opened the door for scholars with an IR framework to think systematically about international economic relations.”

Soon every self-respecting political science department began to reserve a faculty slot or two for specialists. Every political science curriculum began to feature one, if not several, IPE courses. Textbooks in the field—once a trickle, now a veritable flood—were targeted directly at students of political science. By the turn of the twenty-first century, in the words of Helen Milner (2002b, 207), IPE had become “an established part of international relations.” Echoing this, Lisa Martin notes (2002, 245), IPE's “questions, methods, and theories

were drawn from the study of international relations, not from economics. . . . IPE was seen as fitting into the mold defined by IR.”

Why didn't economists fight harder for custody? Had they done so, the field might have followed a different trajectory—addressing different questions, and offering different answers. The basics might have been defined in another manner altogether. But despite the dramatic changes then occurring in the international environment, the mainstream of the economics profession remained largely indifferent. The reasons were threefold: ideological, ontological, and epistemological.

First, there was the chilling effect of postwar anticommunism. Political economy tended to be equated unthinkingly with Marxism or other unacceptable leftist doctrines. By the late 1960s détente was melting the ice of the cold war, reducing tensions between the nuclear superpowers. Even so, the battle to defend the market system went on—a battle in which economists inevitably found themselves on the front lines. Political scientists might be called on to defend the virtues of democracy, but not capitalism. Economists, on the other hand, could not avoid being drawn into the ongoing contest between Marxism and market liberalism. Few U.S. economists at the time had much taste for ideas or arguments that might smack of an anticapitalist sentiment. In any attempt to reunify the economic and political orders, most of the profession saw ideological bias.

The impression was understandable. Marxists, after all, had been among the most prominent of the hardy souls who continued to promote political economy after the split between economics and political science in the nineteenth century. In the United States, during the first decades after World War II, almost all work on international political economy had come from leftist economists associated with the Marxist journal *Monthly Review* and its book publishing arm, Monthly Review Press. Most focused on dependency theory, emphasizing the uneven development and inequalities of the world capitalist system. A prime example was Harry Magdoff's *The Age of Imperialism* (1969), which claimed to find in America's postwar economic hegemony a new form of empire: an updated version of classic colonial imperialism, driven by the imperatives of capitalism and the interests of multinational corporations. Other examples included Andre Gunder Frank's *Capitalism and Underdevelopment in Latin America* (1967), which popularized the notion of the “development of underdevelopment,” and Arghiri Emmanuel's *Unequal Exchange* (1972; first published in France in 1969), which turned traditional trade theory on its head, claiming to find capitalist exploitation where others saw mutual gain. The very idea of political economy seemed tainted by socialist sympathies.

That didn't stop a vocal group of younger economists, who in 1968 formed the new Union for Radical Political Economics (URPE). URPE's origins lay in the New Left politics of the turbulent 1960s, driven especially by a revulsion with the war in Vietnam, which many felt simply confirmed the imperialist

nature of U.S. foreign policy. The group's aim was to promote a new interdisciplinary approach to political economy, a fresh look at the connections between economics and politics. Like Keohane and Nye, URPE even provided a publishing outlet, the *Review of Radical Political Economics*, to showcase its members' work. But in tying its aspirations to the social movements of the day, URPE merely served to reinforce the impression of ideological bias. According to the group's Web site (www.urpe.org), URPE "presents constructive critical analyses of the capitalist system and supports debate and discussion on alternative left visions of a socialist society." At the time, rhetoric like that was more than enough to persuade mainstream U.S. economists to keep a safe distance.

Second was a kind of intellectual myopia in the prevailing ontology of economics. The new discipline that emerged after the marginalist revolution of the 1870s preferred to concentrate on the private sphere, mainly addressing considerations of technical efficiency and economic welfare. Economists were simply not trained to think in terms of the public sphere—the issues of authority and conflict that are inherent in processes of governmental decision making. Nor were they comfortable when confronted with the very political question of distribution—how the economic pie gets divvied up.

This created two blind spots. For one, the importance of institutions was discounted. In the "timeless" analytic framework favored by the mainstream of the profession, political structures, if considered at all, were introduced only as a constraint on economic activity, with underlying power relationships being taken more or less for granted. Neoclassical economics discouraged any interest in questions concerning how rules or norms are created, or how over time they might support or undermine different patterns of economic activity. Second, attention was directed to the outcomes of policy rather than to its inputs. The aim of theory was to *evaluate* policy, not *explain* its origins in the give-and-take of distributional conflict. An old adage has it that politics is like sausage making: you really don't want to know what goes into it. Neoclassical economics took that advice seriously.

There have always been exceptions, of course. For example, in an earlier era the study of institutions actually enjoyed a place of some prominence in the economics discipline, promoted by the likes of Thorstein Veblen and John Rogers Commons. A high point was reached with the publication in 1934 of Commons's *Institutional Economics*. But by the 1960s, the mainstream of the profession had taken a quite different tack, preferring instead to focus narrowly on the direct links between opportunities and behavior, leaving little room for the influence of alternative institutional settings. It was only in the 1980s—with the work of inter alia Douglass North (1981), Ronald Coase (1984), and Oliver Williamson (1985)—that economists once again began to think seriously about the significance of organizational variety and path-dependent historical processes on actor behavior. Known as the "New Institutional Economics" or the "New Institutionalism," the movement stressed

the role of institutions as devices to lower transactions costs and protect property rights.

By contrast, resistance to looking into the process of sausage making has always been strong. To this day, few mainstream economists show much interest in seriously exploring the connection of policy to the dirty game of politics. John Odell, who edited *IO* from 1992 to 1996, tells a story about a conference paper he once presented analyzing why, at the time of the breakdown of the Bretton Woods currency system in the early 1970s, there had been no negotiated agreement to restore pegged exchange rates. Included in the paper was some counterfactual reasoning about how such an agreement might have been attained and how it might have worked. For this approach he was sharply criticized by his paper's discussant, an economist, who devoted his time to explaining why floating exchange rates were actually to be preferred to a pegged-rate regime. Continues Odell: "In the room was one Bob Keohane, who complained impatiently that the discussant was not reading carefully, that Odell was not engaging in that debate but trying to *explain* a political outcome." When IPE was getting started, mainstream economists naturally resisted modes of thought that might sully pure analysis with the stain of scruffy bargaining or political compromise.

Finally, there was resistance to IPE on epistemological grounds. Mainstream economists also were understandably hesitant to take up issues that could not be addressed comfortably using the standard toolkit of neoclassical economics. For a century, especially in the United States, the discipline had been growing increasingly abstract, relying ever more on deductive logic and parsimonious theoretical models to pare messy reality down to its bare essentials. The style was reductionist. The aim was to uncover core relationships—"to predict something large from something small," as Johnson put it (1971, 9).

In the postwar period, Paul Samuelson, one of the first Nobel laureates in economics, set the standard. With his seminal *Foundations of Economic Analysis* (1947), Samuelson sparked a new emphasis on the use of high-powered mathematics in economic analysis. A few lone voices held out for more inclusive approaches that might more closely approximate the complexities of life as it is actually lived. Perhaps most prominent was Harvard's John Kenneth Galbraith, who deplored what he called the "willful denial of the presence of power and political interests" (quoted in *Economist* 2006). But Galbraith and his like were drowned out by the clamor for ever-greater numeracy and abstraction in the formulation of economic theory.

Theorists of every stripe face a fundamental trade-off between parsimony and detail—between the deductive simplicity required for theoretical generalization and the inductive description required to assure external validity. Mainstream economists favor deductive simplicity, showing little interest in Galbraithian-style complexity. Galbraith himself published more than forty books during his lifetime, making him one of the most widely read authors in the

history of economics. He held a variety of public offices and was consulted frequently by national leaders (Parker 2005). Yet for all his professional achievements, he is barely remembered today by any economist born since World War II. In the words of the *Economist* (2006), noting his death in April 2006 at the age of ninety-seven, Galbraith's "contributions to economics are underappreciated by a profession obsessed with mathematical formulae."

In effect, mainstream economics presumes that social phenomena are amenable to scientific explanation in essentially the same manner as are natural phenomena. Hence, the same principles of positivism and empiricism that are employed to isolate causal mechanisms in the physical sciences can be applied to the study of social relations as well. Universal truths are out there, just waiting to be discovered. Galbraith labeled the approach "imitative scientism"—a replication of the methods of the natural sciences that "is carried further in economics than in any other [social science] discipline" (1970, 8). The highest rank in what Galbraith called the "prestige system of economics" goes to those who best mimic the reductionist epistemology of the physical sciences.

To achieve deductive simplicity, strong assumptions must be tolerated or even encouraged as an integral part of reasoning. Parsimony demands that the largest range of phenomena be explained with the smallest number of premises. The more "heroic" the assumptions, the better. This may make economists the butt of humor, such as in the old joke about a can opener. (A case of canned goods washes up on a desert island, where the survivors of a shipwreck are stranded. They wonder how to open the cans. "No problem," says the economist. "Assume a can opener.") But it has also enabled them to derive theorems and predictions that are scrupulously fine-tuned. Imprecision, to the extent possible, is banished from discourse.

In this context, political economy seemed to fit like a square peg in a round hole. How was formal analysis to account for the uncertainties of the political process? How could theory model the exigencies of war and peace? How could existing empirical methods cope with seemingly vague notions like power or dependency? Questions like these ran against the grain of the discipline's methodological standards. Thus mainstream economists could be excused for demurring. As one economist colleague said to me back when IPE was first getting started, "If I can't quantify it, I'm not interested." His remark was only partially in jest.

AN IRONY

Yet in the end, who is to say which of the parent disciplines has really taken custody of IPE in the United States? There is a deeper irony here. As the epistemology of the American school has become increasingly standardized, it has come to resemble nothing so much as the methodology of neoclassical econom-

ics, featuring the same penchant for positivist analysis, formal modeling, and where possible, the systematic collection and evaluation of empirical data. More and more, what gets published features the same sorts of mathematical and statistical techniques that we have come to expect in economics journals.

Why is this? Puzzling over the trend, which has been evident for years, the economist Vernon once suggested that it might have something to do with the deceptive accessibility of a reductionist style. “The ideas that appear to travel most easily between the social sciences are the simpler, more inclusive ideas; and when gauged by the criteria of simplicity and inclusiveness, neoclassical propositions have had a decisive edge” (Vernon 1989, 443). But there may also be an element of envy involved. Political scientists have an inferiority complex when it comes to economics. Even such notables as Katzenstein, Keohane, and Krasner—three of the Magnificent Seven—bow their heads, describing economics as “the reigning king of the social sciences” (1999b, 23). Whether the title is deserved or not, it is certainly true that the “imitative scientism” of economics now appears to set the standard for what passes for professionalism among social scientists in the United States. If today the most highly rated work in the American school tends to mimic the economist’s demanding hard science model, it may be simply be to demonstrate that the still-young field, for all the uncertainties of the political process, is no less capable of formal rigor. Specialists in IPE want respect too.

The trend is easily observable. For illustrative purposes, we may consider how much change has occurred in the pages of *IO* since the Keohane-Nye takeover in the early 1970s. Among the total of some 170 articles that appeared in 1975–1979, less than 10 percent (9.4 percent) emphasized formal modeling or econometrics. During the next half decade, 1980–1984, the percentage was even lower, at 7.4 percent. By the second half of the 1990s, by contrast, in 1995–1999, the percentage was up to nearly 26 percent, and in the first five years of the new millennium, 2000–2004, it was almost half (47.5 percent). A simple Pearson correlation shows the trend over the entire thirty-year period to be statistically significant at the 1 percent level—a result that can hardly be dismissed as chance.

In fact, the trend may be considered official editorial policy. In 1998, a special issue of *IO* was published to celebrate the journal’s fiftieth anniversary, appearing a year later in book form (Katzenstein, Keohane, and Krasner 1999a). Although all but one of the collection’s seventeen authors were political scientists, the final word was given to a respected economist, Barry Eichengreen, who seized the occasion to celebrate the superiority of economic methodology. “The strength of economics,” Eichengreen argued (1999, 354), “is the complementary and mutually supporting character of theoretical and empirical work.” IPE could only benefit by going the same route. As he concluded enthusiastically in the collection’s last pages, “The field needs to move in the direction of formulating parsimonious models and clearly refutable null

hypotheses, and toward developing empirical techniques that will allow these hypotheses to be more directly confirmed by the data” (372).

An enthusiasm for the methodology of economics is understandable, offering as it does both technical sophistication and intellectual elegance. Who wouldn't like to be able to predict something large from something small? But it is also undeniable that reductionism comes at a price in terms of descriptive reality and practical credibility. On the one hand, the full flavor of life is sacrificed for what one critic calls a “tasteless pottage of mathematical models” (DeLong 2005, 128), often wholly unintelligible to a wider public. On the other hand, the true character of life is often caricatured by the implausible assumptions that parsimony demands. The increasing standardization of IPE methods in the United States is by no means costless.

Once upon a time, it was possible to joke about the epistemological differences between economics and political science. A political scientist, one quip had it, was someone who thought that the plural for anecdote was data. The economist, by contrast, was someone who might not be able to remember your phone number but was willing to estimate it for you. Today, however, it is clear those differences between the disciplines are rapidly disappearing. Political scientists may feel they still own IPE, and so they do in terms of the research agenda. But in terms of methodology—*how* things are studied—in practice the dominant role seems to be circling back to economics, along the lines urged by Eichengreen. The trend might be described as a kind of “creeping economism.” Economists, it would appear, may have the last laugh.

Chapter 2

THE BRITISH SCHOOL

Question authority.
—Susan Strange

THERE WAS NOTHING inevitable about the way that IPE was constructed in the United States. Had economists fought harder for custody, the infant field might have developed a different sense of priorities. Had Nye and Keohane not produced their vision of complex interdependence, others might have seized the moment to promote another paradigm altogether. The ontology and epistemology that the American school takes for granted are the product of a particular confluence of place and personality, historical circumstance and individual action. Given a different cast of characters or a different location, the outcome might have been something else entirely. For a case in point, consider the British school.

In Britain as well as elsewhere in the former British Empire, the field has diverged sharply from the U.S. model, giving rise to what has been dubbed the British school (Murphy and Nelson 2001). Here too as in the previous chapter, the notion of a school is used loosely to denote a shared understanding of broad basics rather than a narrower agreement on specific goals or agenda. In the British style, scholars work from a distinctively different vision of how the world works and how it should be evaluated. The study of IPE generally tends to be more multidisciplinary in scope and more normative in ambition, more critical of established orthodoxies and more engaged with social issues, more impatient with the status quo and more eager to change attitudes or practices. The British school's worldview is anything but dispassionate. Likewise, its methods are anything but reductionist. As compared with the U.S. style, scholarship tends to be more qualitative than quantitative, attaching less importance to the systematic analysis of hard empirical evidence. Work is typically more interpretative in tone, and harking back to the traditions of classical political economy, more institutional and historical in nature. Scientific method is valued less. A broad comprehension of "society"—the social context of IPE—is valued more.

Many factors help to account for these differences. Above all looms the towering figure of Strange, another intellectual entrepreneur par excellence. Among American school scholars, Strange's influence has been limited. But in her own country her impact was immense. What Keohane and Nye managed

to do to get the field started in the United States, Strange did in Britain, shaping an approach that quickly became as distinctive in style as in substance. Her legacy also has proved incredibly durable. In the words of one obituary published after her untimely death in 1998, it was “a remarkable legacy that few can match. . . . Her impact is hard to overestimate” (Sen 1998).

GETTING STARTED

It all began with her memorable manifesto, “International Economics and International Relations: A Case of Mutual Neglect” (Strange 1970). Characteristically, Strange did not just sit back to wait for others to heed her call to arms. If a more modern approach was needed, she might as well get it started. So that is precisely what she proceeded to do. Over the next three decades she went on to produce a series of IPE classics, from *Sterling and British Policy* (1971) and *Casino Capitalism* (1986) to her final two works before her death, *The Retreat of the State* (1996) and *Mad Money* (1998a). Like Keohane and Nye across the Atlantic—on the other side of the pond, as the saying goes—she also made an impact through her encouragement of the work of others and her role in providing a vehicle to help drive the field’s development. The study of IPE in Britain today clearly bears the stamp of her powerful personality.

“*I Never Meant to Be an Academic*”

“I never meant to be an academic,” Strange once confessed (1989, 429). “If I had had the chance . . . I would have dearly liked to be an independent newspaper columnist.” The admission is telling. When we think of a newspaper columnist, we think of someone who is both perceptive and articulate: whose interests are practical rather than abstract; whose style is informal, not formal; and whose ideas are eclectic, unbounded by the limits of established disciplines. We read columnists for their opinions, not for weighty analysis. We value them for their insight, not their methodology. In short, we look for a shrewd observer, not a disciplined theoretician. Even in her most serious work, Strange exhibited all the best traits of a columnist—and those traits, in turn, became a standard for all who followed her.

Her formal university education was limited, terminating with an undergraduate degree in economics from the London School of Economics (LSE) in 1943. With a war on and a newborn baby to care for, graduate school seemed out of the question. So she opted instead for journalism, taking a job with the *Economist* and later with the *Observer* as the youngest White House correspondent of her time. This of course helps to explain her ambition, which was never realized, to don the mantle of a columnist. It also explains the distinctive style



Figure 2.1. Susan Strange. Courtesy of The London School of Economics and Political Science

of her writing, which was always direct, clear, and admirably free of jargon. She knew how to communicate.

Strange's first academic position did not come until 1949, when she began lecturing on IR at University College London. Her intention, Strange later related (1989, 432), was to "stick to it for a couple of years and use it as a learning device to broaden my reading and deepen my understanding." But she ended up staying for a decade and a half, attracted by the opportunities that academia offered to speak her own mind. "I was increasingly impatient

of The Establishment and all it stood for. . . . Academic life at least gave the freedom to teach and write independently” (432). A university position also made it easier for her to raise the two children from her first marriage and then four more from her second.

Political economy began to enter the picture in 1964 when Strange moved to the Royal Institute of International Affairs, commonly called Chatham House (after the building in central London where the institute is housed). Appointed a research fellow, she was asked to write something on the travails of the pound sterling, Britain’s crisis-ridden currency—a project that culminated seven years later with her landmark *Sterling and British Policy*, which firmly established the political sources and consequences of international currency use. “The connections between power, influence, the reserve currency role, and the national economy were an enticing subject,” she recalled, “and there was very little that was political written about it” (433). The politics of international monetary relations soon became her specialty and remained at the heart of her intellectual interests even after her return in 1978 to her alma mater, LSE, as a tenured professor.

Once started on her project at Chatham House, she adopted the cause of IPE with a missionary zeal. I received my personal baptism in 1968, when I arrived in London intending to write a book of my own on sterling (Cohen 1971). If you’re going to research the pound, I was told, you should meet Strange. So I sought her out. When we got together, I dutifully told her about my plan to do a strictly economic cost-benefit analysis of sterling’s role as an international currency. “Oh, Jerry,” she replied, squinting at me through hooded eyes, “you can’t possibly write about the pound without talking about the politics, too”—a remonstrance that she would subsequently repeat on every possible occasion. At the time I resisted, with a stubbornness born of my orthodox economics training. The loss was mine: her book turned out to be far more interesting. But ultimately her message did get through. By the time I got back to the United States, I was determined—thanks in good part to her—to dip my toe into this new current of IPE.

Taking Action

Strange’s first initiative, following her “Mutual Neglect” manifesto, was to create an institutional vehicle to help promote the more modern approach she was calling for. In her case the vehicle was not a journal, though she did later have a hand in the genesis of the *Review of International Political Economy* (RIPE). The first issue of RIPE appeared in 1994. For the British school, RIPE quickly became as indispensable an outlet for IPE scholarship as IO had long been for the American school. Strange’s first contribution, however, was an organized research network, the International Political Economy Group (IPEG), which she set up under the auspices of Chatham House in 1971.

The aim of IPEG was to bring together scholars, journalists, and policymakers for regular discussions of issues concerning the world economy. For adherents of the British school, choosing to ignore parallel developments then occurring on the other side of the Atlantic, this truly was the moment of IPE's birth. Declares one source flatly, "Today's field of international political economy can be traced back to 1971, when Susan Strange . . . founded the International Political Economy Group" (Murphy and Nelson 2001, 393). IPEG still exists as a research group within the British International Studies Association (BISA), which Strange was also instrumental in founding in 1974. It is symptomatic of the differences between the American and British schools that while the preeminent showcase for IPE scholarship in the United States is the annual meeting of the American Political Science Association, in Britain it is BISA, a more multidisciplinary organization.

Getting these vehicles under way was no easy task given the limited financial resources available in Britain at the time. "We did not have the resources available to Nye and Keohane in America," Strange later noted ruefully (1989, 434), but "we too were convinced that this was the coming new wave in international relations." For the conference that led to *Transnational Relations and World Politics*, their first collaborative effort, Keohane and Nye had the support of Harvard's Center for International Affairs. For their second project, *World Politics and International Economics*, they received generous financing from two foundations and the Brookings Institution in Washington, DC. And for *Power and Interdependence*, they got help from inter alia the Rockefeller Foundation and a university consortium for world order studies. By contrast, Strange had to scrounge shillings and pence wherever she could. For IPEG, she was able to obtain a minimal amount of funding from the British Economic and Social Research Council. To get BISA off the ground, she wrote to every university vice chancellor in the country, asking for the derisory amount of two pounds each for start-up money.

She had more success in gaining support for another early initiative—the memorable Cumberland Lodge Conference of July 1972 (Brown 1973). Her "Mutual Neglect" manifesto had thrown down the gauntlet: Could international economics and IR be successfully merged? So she decided to respond to her own challenge by inviting a gaggle of scholars together to try. The funding was obtained from Chatham House and two British foundations. For ten days, some forty economists and political scientists sought ways to overcome the long-standing gap between their respective (if not always mutually respectful) disciplines. It is rare for an academic field of study to be able to boast of what amounts to a constitutional convention, where the foundations are laid for future growth and development. But to a large degree that is precisely what the Cumberland Lodge Conference did for the British school of IPE.

Not that the conference was without its difficulties. On many points, not surprisingly, discord reigned. Political scientists accused the economists of being essentially one-dimensional in their preoccupation with the pursuit of wealth, focusing on just a single aspect of human behavior. Economists, in turn, were critical of the political scientists for their seeming lack of methodological rigor or theoretical sophistication. On the fundamental trade-off between parsimony and detail—between the pursuit of eternal truths and the imperative of external validity—the void remained as deep as ever.

Yet there were also key points of consensus, which encouraged participants to persist in their efforts to nurture the infant field, following Strange's lead. More important, they all agreed on the need to keep trying. In the words of one of those in attendance, "There was general agreement that the gap between, on the one hand, the changing nature of the real world and, on the other, the conventional divisions of academic study has increased, is increasing, and should be decreased . . . general agreement about those trends and tendencies in the world which were making the isolated study of politics or economics untenable" (Brown 1973, 52). They also agreed that an integration of political and economic analysis into a viable field of study would not be easy. But after ten days they returned home energized, determined to find ways to bridge the gap between the two disciplines that would not discount the multiple dimensions involved. As a final report of the meeting concluded (Brown 1973, 60)

The conference revealed serious disagreements over methods and a discouraging degree of ignorance of each other's disciplines. At the same time, it confirmed the participants' belief in the importance of not rigidly confining the study of social problems within the conventional boundaries and strengthened their resolve to overcome the difficulties involved in putting the study of international political economy on a proper footing.

In ensuing years, Strange continued to labor to put the new field "on a proper footing." Like Keohane and Nye (and later Keohane and others), she promoted collective research projects that might push thinking forward in new and innovative directions. One notable example was *Paths to International Political Economy* (Strange 1984a), a collection of essays that she commissioned and edited at the invitation of the British Political Studies Association. Another was *Transcending the State-Global Divide*, edited by two colleagues (Palan and Gills 1994), which developed from a seminar that Strange organized at the European University Institute in Florence, Italy. Volumes like these have been decisive in defining how the British school understands the world.

And then, of course, there was her role as a mentor of students. Strange's aim in returning to the LSE in 1978 was to build Britain's first IPE graduate program, which she ultimately managed to do despite resistance from more traditional faculty colleagues. At first, she wasn't even permitted to use the term IPE. Economics department faculty, arguing that what they taught

was international political economy, did not want to give up the label. So Strange's program instead had to be branded Politics of the World Economy, only later changed to IPE. Similar programs soon began to spring up elsewhere as well, including at the universities of Leeds, Manchester, Newcastle, Sheffield, and Warwick.

Strange's blunt style as a teacher could be demanding, even intimidating. Yet she is best remembered for her warmth and the graciousness of her critical encouragement. Her first PhD student at the LSE recalls that Strange "was a good-humored and fun person, willing to listen and cross swords good-naturedly with colleagues and students over a beer, never resorting to intellectual terrorism by wielding her authority. If she thought she had encountered an interesting idea she would yield with enthusiasm" (Sen 1998). Even after reaching the formal age of retirement, she persisted in offering support to aspiring young scholars, first at the European University Institute and then, lastly, at Warwick, where she aided in putting together that university's new IPE program.

It is perhaps a measure of the widespread affection and respect Strange inspired that her career prompted the publication of no fewer than three substantial volumes of essays written in her honor (Morgan et al. 1993; Lawton, Rosenau, and Verdun 2000; Lorentzen and de Cecco 2002) as well as a posthumous edited collection of many of her own writings (Tooze and May 2002). To all those she encountered, including myself, she was a lovable character, tough but tender. A friend at the LSE, Fred Halliday, perhaps put it best when he wrote (1998), "Her ideas were pioneering, robust and convincing. It was, however, as much as anything her personality—the smiling eyes and the exigent criticism—that her colleagues and former students will treasure." Wouldn't we all like to be recalled so fondly?

Founding a School

Strange may never have meant to be an academic, but she became one. According to a sympathetic source, she never meant to found a school either (Murphy and Nelson 2001, 398–99). Yet she did that too. In the words of Barry Gills, one of the founding editors of RIPE, "She founded IPE as we know it here in Britain and she left a great hole in it when she left." Ronen Palan, an admirer, calls her "an empire builder" (2003, 117). Intentionally or not, the substance and style of the British school came to be constructed very much in her image.

In purely intellectual terms, Strange's contributions were limited. She was, by her own admission, no theoretician. Indeed, she always had a suspicion of grand theory. Gills remembers that "she told me and others in confidence that 'I do not consider myself a theorist' but rather someone who was primarily empirical and analytical." Her aim, according to two colleagues, was not "to

develop a full theory of IPE, but a way of thinking, a framework for thinking” (Tooze and May 2002, 15). Her long-term impact is measured less by her own ideas than by the critical entrepreneurial role she played in stimulating the ideas of others.

Strange did have ideas of her own, of course—lots, in fact—but she rarely took the time to develop them in a systematic fashion. In the United States, Strange’s lack of formal rigor was considered a distinct liability. She was admired for her daring—her willingness, even eagerness, to defy conventional wisdom. As one U.S. scholar wrote to me, she “was never swayed by fashion [and was] always ahead of the curve.” But for most Americans in the field, the erstwhile journalist was more pundit than profound, more agent provocateur than savant. Her work, in general, was not thought to be particularly compelling. Yet in Britain her writings took on the aura of gospel, inspiring devotion and emulation.

Illustrative was her approach to the issue of power, which has been described as her “most significant contribution towards IPE” (Tooze and May 2002, 8). Power, for Strange, was central to any explanation of the character and dynamics of the global economy. Her views were best spelled out in her textbook *States and Markets* (1988b). Traditional studies of world politics, which she criticized as narrow and old-fashioned, had mostly tended to identify power with tangible resources of one kind or another: territory, population, armed forces, and the like. But in economic affairs, Strange perceptively argued, what mattered most was not physical endowments but rather structures and relationships—who depends on whom, and for what. Hence power could be understood to operate on two levels: structural and relational. *Relational power*, echoing more conventional treatments in the IR literature, is the familiar “power of A to get B to do something they would not otherwise do” (24). *Structural power*, by contrast, is “the power to shape and determine the structures of the global political economy . . . the power to decide how things will be done, the power to shape frameworks within which states relate to each other” (24–25). Four key structures were identified: security, production, finance, and knowledge.

Strange’s distinction between relational and structural power was not entirely new. Keohane and Nye in *Power and Interdependence*, echoing Hirschman’s long-neglected *National Power and the Structure of Foreign Trade*, had already stressed the role of asymmetrical interdependence as a source of power in economic affairs, highlighting the twin dimensions of sensitivity and vulnerability at the relational level. And in a study that I published the same year as *Power and Interdependence* (Cohen 1977)—a book that Strange reviewed (1979), not unkindly—I had tried to make the same distinction that Strange did later, labeling the two levels as “process power” and “structure power.” Nor could Strange’s insight be genuinely described as theory. Her approach was essentially descriptive. Though her discussion in *States and Markets* of-

ferred a rich array of illustrations and a historical narrative, it lacked any kind of formal analysis of either the sources of power or the determinants of its use. Strange's "famous four structures," acknowledges Palan (2003, 121), were really no more than "a mere organising framework, a heuristic typology in place of a theory." Nonetheless, her layered conception of power in IPE quickly became a core theme of the British school.

Much the same can also be said of other of Strange's ideas. Keohane (2000), in an appreciative retrospective, lists five major themes in her scholarship: her emphasis on structural power; a focus on agency and the behavior of self-interested actors; a pervasive skepticism about international organizations and regimes; an emphasis on competition among authorities within different sectors of the world economy; and a profound ambivalence about the role of the United States. Though none of these themes was developed by Strange with the kind of attention to causal relationships or predictive accuracy that American school scholars would have preferred, they remain central to the study of IPE in Britain.

Promoting a School

It is easy to understand why one might think Strange never meant to found a school. She was tireless in her crusade for a new field of IPE. But she was also indefatigable in arguing for as much openness of inquiry as possible. Scholarly ecumenism was a persistent refrain in her writing. "International political economy is still unfenced, still open to all comers," she declared (1984b, ix). "It ought . . . to remain so." In her opinion, "the study of international political economy would do well to stay as an open range, like the old Wild West, accessible . . . to literate people of all walks of life" (1991, 33). She firmly abhorred any artificial constraints on inquiry. Leading by example, she freely crossed academic boundaries in pursuit of her scholarly interests.

For Strange, an open range meant eclecticism. It meant resisting the disciplinary compartmentalization of the social sciences that had prevailed since the nineteenth century—what she jokingly referred to, with a nod to earlier British history, as the enclosure movement. And it especially meant transcending the cliquish jargon of narrow specialties that retarded fruitful dialogue. "One of the fatal weaknesses of social science during the enclosure movement," she wrote (1984b, ix), "has been the tendency of each specialism to become a closed shop, a self-perpetuating secret society of the initiated." The fences that had grown up between disciplines misdirected choices and resulted in bad theory, she insisted.

The message was clearly conveyed to her students. Typical was a pair of questions included in the 1988 exam for the core IPE course that she offered in what was then still called the Politics of the World Economy program at the LSE. Students were directed to explain and discuss one of the following: "The

eclecticism of international political economy prevents the drawing of boundaries which would define it as a discrete field of study"; or "the international political economist, by definition, must take note of work done in other social sciences." Intellectual ecumenism was simply a given.

The problem was the dialogue of the deaf. The solution, for Strange (1984b, ix), was to remain "alertly attuned to a variety of special insights and concerns"—in other words, to remain *multidisciplinary*. "It would be fatal," she concluded (ix), if IPE "were ever to become the exclusive preserve of economics, of international relations, or of political science." As matters turned out, a school was indeed founded, based on that very principle of openness.

The contrast with the American school could not have been greater. For Strange, allowing custody of IPE to go to the political scientists, as happened in the United States, would be a serious error. IR should be viewed as a subset of IPE, Strange felt (1994, 218), and not the other way around:

The whole point of studying international political economy rather than international relations is to extend more widely the conventional limits of the study of politics, and the conventional concepts of who engages in politics, and of how and by whom power is exercised to influence outcomes. Far from being a subdiscipline of international relations, IPE should claim that international relations are a subdiscipline of IPE.

In short, a more heterodox approach was needed. As first articulated in her "Mutual Neglect" manifesto, this meant integrating two specialties—a "middle ground" between political and economic analysis (Strange 1970, 307). By the time that Strange edited *Paths to International Political Economy* (1984a), it had come to mean much more—an inquiry open to the perspectives and concerns of an even broader range of disciplines, from sociology and history to psychology, anthropology, and even gender studies. The essays that Strange commissioned for *Paths to International Political Economy* were deliberately designed to highlight what could be learned from tearing down fences. Contributors came from a wide variety of backgrounds, including fields as diverse as population studies, ecology, and the law. Each one, separately, was invited to explain the insights that might be offered by their particular specialty. (Strikingly, there were no women among the authors.) Collectively, the purpose was to emphasize the value that could be added to the study of IPE by following different paths.

Others quickly took up the theme of eclecticism, which soon coalesced to become a hallmark of the British school. Scholars were motivated, wrote one young lecturer, by a "growing realization that many of the world's problems . . . cannot be understood within the conventional framework of knowledge whereby historically defined academic disciplines each have their own exclusive area of inquiry" (Tooze 1984, 2). In effect, the aim was to resurrect the broader, more inclusive perspective of classical political economy as it had

existed before the amoeba subdivided in the nineteenth century. “The classical political economists were polymaths who wrote on a variety of subjects,” an analyst has written (Watson 2005, 18). “They did not study ‘the economy’ as an enclosed and self-contained entity.” IPE was about human experience; nothing should be left out.

By the 1990s it was clear from comparative surveys of British and U.S. university curricula that “inclusiveness” had become the key factor distinguishing IPE in Britain from the American version (O’Brien 1995; Denmark and O’Brien 1997). Inclusiveness meant transcending all traditional disciplinary boundaries, broadening the focus of analysis to open up new, exciting areas of inquiry. Bridges were to be built not just between international economics and IR but between them and other disciplines as well. Some in the British school have even promoted a different name for the field—Global Political Economy (GPE), rather than IPE—to affirm the school’s more holistic approach (Gill and Law 1988; Palan 2000a; Tétreault et al. 2003). Others are content to retain the label IPE, but insist on the need to “globalize” the field’s range of study (Phillips 2005).

When the first issue of RIPE appeared in 1994, the commitment to an open range was, in effect, codified by the editors. “RIPE’s *raison d’être*,” they announced, “is to bring together these exciting new attempts to understand contemporary social change by facilitating dialogue and debate across existing academic divides. . . . The journal will inevitably be ‘multidisciplinary’ in scope and ‘interdisciplinary’ in spirit” (Editors 1994, 2). Inclusiveness has marked the journal from the start. Of the four founding editors, two—Gills and Palan—were trained in IR and two—Ash Amin and Peter Taylor—were geographers. In subsequent years, several Americans played a critical editorial role, including Jeffry Frieden and Helen Milner. And most recently, the editorial board has included *inter alia* both a geographer and a development economist.

Similarly, when RIPE was joined two years later by *New Political Economy*—now widely seen as RIPE’s biggest rival for IPE scholarship in the British style—the new journal called for “a readiness to tear down intellectual barriers and bring together many approaches, methods and disciplines. . . . We want to encourage conversations and exchanges of ideas and experiences across boundaries which in the past have often been unnecessarily fixed” (Gamble et al. 1996, 5, 11). The theme of eclecticism had by now become official.

Eclecticism did not mean a relaxation of scholarly standards. Tearing down intellectual barriers would not require the abandonment of analytic rigor or nuance. In fact, British school scholars on the whole are every bit as committed to the careful, systematic use of theory and theoretical paradigms as are their U.S. counterparts. But a more open range plainly was antithetical to the sort of reductionist epistemology that is characteristic of a hard science model. With greater inclusiveness came a lessened emphasis on formal empirical in-

quiry. A breadth of vision was not to be sacrificed on the altar of standard social science methodology.

Perhaps that is why economics, the only discipline that Strange ever studied formally, became her particular *bête noire*. The “imperialism of economics” was to be fought at all costs. “Always attack the economists,” she would tell her colleagues. Their methods were narrow and in her opinion often downright wrong. It was no accident that economists were excluded from *Paths to International Political Economy*. “If there are would-be ‘enclosers’ lurking about,” Strange declared (1984b, x), “they are more likely to be found in departments of economics than elsewhere.” The economics profession’s commitment to “imitative scientism” (Galbraith 1970, 8) was, for her, “phoney science, not social science” (Strange 1994, 217).

Strange’s antipathy for reductionist epistemology quickly came to be widely shared by others, including the editors of RIPE. By way of comparison with the trend in the pages of IO that I noted in the previous chapter, we can look at the kinds of articles published in RIPE during its first decade. Among the total of 161 articles published in the British journal’s first five years, 1994–1998, only six (3.7 percent) emphasized formal modeling or hypothesis testing. Similarly, in the next half decade, 1999–2003, the numbers were 8 of 144 (5.6 percent), showing little change. It is obvious that the methods of economics hold much less sway in RIPE than they do in IO. No creeping economism here.

Today, the British school is owned by no one academic specialty. Rather, discourse ranges widely, resisting the forms and norms of any single tradition. A mix of ingredients from different perspectives is welcomed, even celebrated. As Craig Murphy and Douglas Nelson, two U.S. social scientists (the first a political scientist, and the second an economist), conclude in a comparative survey (2001, 404), “The British School is clearly heterodox relative to American social science in general and the IO school in particular.” All this, in no small measure, may be attributed to Strange’s Unusual powers of persuasion.

Imprinting a School

Other contrasts with the U.S. version of IPE are also attributable to Strange’s influence. The British school tends to be more normative in ambition, more critical in inclination, and more passionate in tone. All these, too, were traits of Strange’s personality that she succeeded in imprinting on the infant field.

For example, no one could miss Strange’s intense engagement with social issues. Indeed, for the would-be newspaper columnist, the whole point of intellectual inquiry was to find ways to right the wrongs of the world. In this, Palan (2003) sees a commitment to the social philosophy of pragmatism as it emerged in the late nineteenth century in the work of, among others, William James and John Dewey. Theory, for the pragmatists, could only be judged by its usefulness. Asks Palan: “Does philosophical pragmatism hold the key to

Susan Strange's brand of IPE? I think so" (123). Distributional considerations in particular were always on her mind, whether speaking of the pursuit of wealth or the pursuit of power. The key question was always: *Cui bono?* For whose good?

Nor did she shy away from judgments about matters of ethics or equity. For her, scholarship was inseparable from values. She loved to remind everyone of the link that had long existed between classical political economy and the study of moral philosophy prior to the fragmentation of the social sciences in the nineteenth century (Strange 1985, 14). That link, she felt, needed to be revived. IPE should be "about justice, as well as efficiency: about order and national identity and cohesion, even self-respect, as well as about cost and price" (Strange 1984b, x). True to her beliefs, she continually dared colleagues to make moral judgments.

Strange had no illusions that sweetness and light would ultimately prevail; she was too worldly for that. As one longtime acquaintance remarked, "She belonged to a pedigree that echoed the Manchester School liberals, believing in the pragmatic possibilities of human improvement, although she had fewer ideological illusions" (Sen 1998). She was determined to give it her best shot, though, and thought others should try as well. As Strange once reminisced (1989, 436), "I have tried to teach both students and children not to expect justice in life—but to try hard to get it." The broad range of practical issues addressed by the British school, in the pages of RIPE and elsewhere, attest to the fact that many of her students did indeed take her teaching seriously.

Nor could anyone miss her impatience with "The Establishment," as she put it, which led her to adopt a skeptical attitude toward orthodoxy of any kind. She was an iconoclast and radical, recalls one former student. "She was no revolutionary, but she questioned prevailing nostrums with ill-concealed glee" (Sen 1998). Strange herself attributed her irreverence to her late start as a full-time academic. "By that time, I suppose I felt there was no point in being too conformist. To make any kind of mark, I had to develop ideas on my own" (Strange 1995, 295). But there can be no doubt that her disdain for "the barons and the top brass" (295) was also rooted in her personal experience as a woman trying to make her way in a man's world. Another lesson she sought to teach students was "to question authority, whether political or academic" (Strange 1989, 436). And here, too, those who followed her took her teaching seriously. A thread running through much of the British school today is a strong distaste for anything that might be regarded as mainstream thinking.

Finally, there was Strange's passion, which suffused everything she undertook. This was not a woman who could do things halfway. Passion was evident in the initiatives she took to organize IPEG and the Cumberland Lodge Conference. It could be seen in the wide range of issues she chose to take on, from the decline of sterling to the rise of globalization. It could be found in her confrontational, in-your-face approach to academic debate and teaching. To be

a serious scholar, she told her students, you must have “fire in the belly.” And most of all, it was manifest in her prose, which made even the dreariest subjects come vividly alive. What might seem grimmer, for instance, than a book titled *States and Markets*? But who could fail to be enticed by Strange’s witty prologue—beginning with the cliché “It was a dark and stormy night”—that establishes its basic argument through an allegory about a disastrous shipwreck and three groups of survivors on a desert island? Passion is not a word normally associated with the kind of parsimonious, positivist analysis encouraged by the American school. Even after Strange’s passing, however, passion remains a central hallmark of the contrasting British approach to IPE.

THE CONTRAST

Nowhere have I seen the contrast between the two schools drawn more clearly than in an anecdote related to me by a young friend of mine, a U.S.-trained political scientist who, with his freshly minted PhD, not long ago accepted a faculty position at the LSE, Strange’s old home. “Our department requires PhD students,” he wrote me, “to be questioned on their dissertation chapters by a panel of faculty each year. On one panel on which I served, I encouraged the student to clarify the ‘puzzle’ or ‘gap’ that his approach was addressing or filling. On hearing my advice, one of my colleagues suggested that while such advice was appropriate, it tended to reflect an American concern with positivist social science. On the other side of the pond, he suggested, we don’t necessarily do things that way.”

In my own experience, the contrast was best brought home by a 1991 paper coauthored by the U.S. political scientist Craig Murphy and Roger Tooze, a once-prominent member of the British school. The paper commented on a survey article that I had written a year earlier for IO reviewing five new books on the political economy of international trade (Cohen 1990). Describing my piece as “authoritative [and] representative of orthodoxy” (that is, the American school), Murphy and Tooze took me to task on both ontological and epistemological grounds. Together, their objections defined the key differences between the American and British schools.

On ontological grounds, they criticized me for what they called a preoccupation with “interstate material relationships.” At the outset of my survey, I suggested that the research agenda of the then newly emergent field of IPE “focuses largely on two broad sets of questions. One set has to do with *actor behavior*—meaning, in particular, government behavior, since the fundamental unit of authority in the international system still remains the sovereign nation-state. What motivates government behavior in foreign economic relations, and how is it best explained and analyzed? The other has to do with *system management*—coping with the consequences of economic interdependence. How do

state actors manage (or fail to manage) their conflicts, and what determines whether they cooperate or fail to cooperate to achieve common objectives?" (Cohen 1990, 264).

Today, of course, the latter set of questions would be labeled system *governance* rather than system management, in keeping with the standard language of political science. But whatever the language used, for Murphy and Tooze this was all far too state-centric—too reminiscent of the traditional agenda of IR theory. It was also too concerned with strictly material considerations, discounting matters of culture, identity, and the like. The focus, Murphy and Tooze argued (1991, 16), was too narrow, "a fundamental ontological weakness."

To some extent, the objection was misplaced. Murphy and Tooze appeared to equate the agenda I outlined with nothing more than traditional realism, which treats the state as a rational and unitary actor. "The state is important," they conceded (15—16), but "it is essential [also] to consider a large number of other nonstate social and economic actors . . . through analysis that disaggregates the state and encompasses a broad understanding of society." In fact, that is precisely what I had in mind, as I tried to make clear in my discussion of the five books under review. The paradigm I was working from was not realism but complex interdependence, which does indeed encompass a much wider range of relevant actors. Their reproof was telling nonetheless. In raising the issue, Murphy and Tooze highlighted the British school's determined resistance to disciplinary compartmentalization in general and any subordination to IR in particular.

On epistemological grounds, they criticized me for a preoccupation with "scientific" method. They acknowledged that I had not insisted on the superiority of any single methodology. Indeed, I wrote that the methods of IPE vary, "depending both on the disciplinary training of the individual scholar and on the nature of the specific issue-area under consideration" (Cohen 1990, 264). But not even this formulation left sufficient latitude, according to the pair. To consider just these two factors as determining methodology, Murphy and Tooze contended (1991, 16), "is to adopt a relatively narrow and particularist conception of methodology as scientific technique, along with a wholly positivist conception of the possibility and desirability of objective analysis." As a card-carrying member of the American school, I will not deny my allegiance to the principles of positivist social science, which seem to me to be essential for valid cumulative research. Murphy and Tooze, by contrast, clearly preferred to leave more room for normative concerns and value judgments—another hallmark of the British school.

Ultimately, the issue comes down to the basics: what understanding is shared about how things work and how they may be studied. Murphy and Tooze attacked my "authoritative" article as representative of a specific *culture* of orthodoxy—"a particular view of what and who . . . constitute the legitimate

study of IPE” (16—17). The British school manifestly has a different culture—a different intersubjective understanding, as Murphy and Tooze demonstrated. Each school, in the end, is the product of a different construction.

FOLLOW THE LEADER

But why did the constructions turn out so differently? Why did the British school ultimately diverge so sharply from the American model? Strange’s powerful personality was certainly part of the explanation. With her determined entrepreneurship and persuasive style, she opened the door wide to an alternative understanding of the way the world works. Yet to gain acceptance for her distinctively different vision, she also needed a receptive audience—a critical mass of scholars prepared to follow her lead rather than that of Keohane and Nye, Gilpin, or others across the Atlantic. I have said that there was nothing inevitable about the way that IPE was constructed in the United States. There was nothing inevitable about the way IPE was constructed in Britain, either.

In fact, the audience for Strange’s ideas turned out to be remarkably broad. An old slogan for an American bread product proclaimed, “You don’t have to be Jewish to love Levy’s Jewish rye.” In Britain, you didn’t have to be British to be in the British school; indeed, you didn’t even have to reside in Britain. Though rooted in geography, the distinction is as much intellectual as territorial. The school of course has included many Britons, such as R. J. Barry Jones and Tooze. But it has also included the likes of Philip Cerny (American), Gills (dual U.S.-British citizenship), Richard Higgott (dual Britain-Australia citizenship), Palan (Israeli and British passports), and Timothy Sinclair (from New Zealand). It includes Geoffrey Underhill, who decamped to the University of Amsterdam, and John Ravenhill, who has spent much of his career at the Australian National University. And it even includes a fair number of U.S.-based scholars, such as Murphy and Nelson.

The audience also includes many Canadians, still receptive to British intellectual influence despite Canada’s close proximity to the United States. True to the old cliché about Canadian moderation (Question: Why does a Canadian cross the road? Answer: To get to the middle), many of that country’s best-known IPE specialists have tried to keep a foot in each camp, respecting the American school’s emphasis on scientific positivism and empiricism even while hearkening to Strange’s message of multidisciplinary eclecticism and inclusiveness. Scholars like Eric Helleiner and Louis Pauly (an American based in Canada) in effect seek to build bridges between the two versions of the field. But Canada is also the home of some of the best-known members of the British school, such as Stephen Gill and most notably Cox. We will hear more about Cox in the next chapter.

Why was the audience for Strange's ideas so receptive? Standard explanations point to a basic difference in intellectual culture—broadly, the way international studies traditionally had been approached in British universities as compared with the United States. On the U.S. side of the Atlantic, links with political science had always dominated. International studies grew up in an environment framed by the norms of conventional social science, with a particular emphasis on training in quantitative methods. Once IPE was born, it seemed natural for U.S. scholars to channel the infant field's development along similar lines.

In Britain, by contrast, international studies had roots that were spread much more widely into a variety of other disciplines such as sociology, philosophy, religion, and law. Direct links with political science were weaker, with most universities maintaining a strong institutional separation between IR faculty and others. At Cambridge, international studies was for a long time located in the history faculty. At the LSE and some other schools, IR had a department of its own, quite distinct from other disciplines. British academics were already conditioned to think about the international realm in multidisciplinary and normative terms.

Particularly influential was the so-called English school of IR, which stressed the existence of a global society that could be studied only in the broadest social and historical terms—an idea rooted in the classical legal tradition whose origins could be traced as far back as Hugo Grotius. Built on foundations laid by *inter alia* Hedley Bull (1977), the English school was “skeptical of the possibility of a scientific study of International Relations,” as one sympathetic history puts it (Dunne 1998, 7). Indeed, resistance to any kind of hard science model was actually a point of some pride among its adherents. As another survey summarizes it, “The epistemological status and methodological principles of English school arguments are left rather obscure” (Linklater and Suganami 2006, 114). Hence, it was no surprise that in Britain IPE might develop in the same open manner. Formal methodology was de-emphasized. Instead, the milieu encouraged what Tooze labeled a “historical-relativist paradigm . . . drawn from an eclectic mix of factors” (1985, 121).

Historical circumstances also differed sharply on the two sides of the Atlantic. In contrast to the United States, the new top dog in the global economy, Britain seemed a spent force—a nation mired in a long, painful decline. Once the country had been the proud center of an empire on which the sun never set. Now it feared becoming little more than a collection of sad offshore islands, overshadowed by the newly prosperous economies of the Continent and prone to a seemingly endless streak of financial crises. U.S. scholars understandably accepted the new world order as natural or even desirable. British scholars, on the other hand, could be excused for adopting a more jaundiced view of the status quo and a greater openness to alternative perspectives.

But these broad differences were hardly the whole story. There were also other, more specific factors at work to build an audience for Strange's ideas. First was a latent anti-Americanism often found in British universities, which helped encourage scholars to define their efforts more in opposition to, rather than in imitation of, U.S. trends of thought. In the jargon of IR theory, British academics were more inclined to balance than bandwagon. Second was a more relaxed attitude toward Marxism or other leftist doctrines, which reinforced a critical disposition toward markets and their consequences. And third was the less formal approach to economic studies in Britain, as compared with the United States, which reduced pressures to conform to a demanding set of methodological standards.

Anti-Americanism in British universities, where it existed, involved two intertwined strands. One was geopolitical, concerning the emergence of the United States after World War II as leader of the Western world. In Britain, now eclipsed as a global power, this rankled. The resentment of U.S. dominance at some level was natural or perhaps even inevitable. (Remember the old wartime line about the Americans: overfed, overpaid, oversexed, and over here.) Britons, like their counterparts in the antiwar movement in the United States, were particularly revolted by America's seemingly imperialist war in Vietnam. The other strand was intellectual, concerning the rise of U.S. universities after 1945 to the peak of the world's academic hierarchy, eclipsing the likes of Cambridge and Oxford. U.S. scholars were seen as privileged by their access to the resources of a much wealthier economy. They were also thought to be unduly influenced by the foreign policy concerns of a hegemonic power.

These strands came together to encourage resistance to any new scholarly fashion emanating from the other side of the pond. Strange, with her marked ambivalence about the United States, helped to set the pattern. In some ways, Strange truly admired the United States—the only country, she once noted, where you can buy a T-shirt emblazoned with her favorite slogan, “Question Authority” (1995, 295). As Keohane has accurately observed (2000, xiv), “She loved the openness and irreverence of American society. . . . In a sense, she was by instinct and temperament a woman of the American West.” Yet this did not stop her from being offended by what she perceived as the selfishness of U.S. power and the arrogance of American academics, who she attacked with unrestrained vigor. In one famous essay, Strange (1983) compared her U.S. counterparts to those medieval scholastics who, in their superstitious ignorance, imagined dragons lurking beyond Europe's western horizon. In another piece, Strange (1994) publicly challenged a respected U.S. professor to “wake up” and face the facts.

Jibes like these found a ready audience among British academics, many of who were understandably eager to create an alternative to what Murphy and Tooze describe as the “self-identified U.S. ‘supremacy’ in the scholarly fields of international relations and IPE” (1991, 17). As one admiring source puts it,

Strange's "stinging criticism of US intellectual trends provided room for British scholars and students to ask different types of questions and use different methodologies from their US counterparts" (O'Brien and Williams 2004, 28). Typical was a broadside published in the second issue of RIPE, when the journal was just beginning its campaign for a more open range. The U.S. version of IPE, proclaimed Peter Burnham (1994, 221—22), is "a vulgar, fraudulent discipline . . . a crude amalgam of neoclassical economics, pluralist domestic political science, and realist international relations theory. . . . We can only wonder why the tradition of classical political economy is passed over in such haste. . . . [The Americans] fail to grasp the complex organic set of social relations which is the global political economy." A more "scientific" political economy was needed to supplant the American school's "unsatisfactory eclectic mixture of analytical methods and theoretical perspectives," Burnham concluded (222).

It may be an exaggeration to suggest that the British school defined itself simply by its opposition to U.S. thinking. But that would not be entirely inaccurate, either. Even for its sympathizers, the British school's hostile attitude toward scholarship on the other side of the Atlantic has long been one of its chief sources of inspiration. In the words of Murphy and Nelson (2001, 405), "The success of British school IPE is relatively easy to explain. American hegemony and the hegemony of IO school IPE created opportunities for those who opposed either or both projects."

Attitudes toward Marxism or other leftist doctrines, by contrast, were far more relaxed than in the United States, where most academics were wary of anything that might seem tainted by socialist sympathies. Hence, there was less inclination in Britain to resist a new field of study that could possibly smack of anticapitalist sentiment; quite the contrary, in fact. Skepticism regarding markets and their consequences was much more acceptable there than in the United States. After all, wasn't Britain where many fashionable leftist doctrines, such as Fabianism, had first developed? Wasn't one of the country's two biggest political parties at the time avowedly socialist in intent? Criticism of the allegedly oppressive nature of markets came easy—especially markets as they were allowed to operate in the United States. Scholars needed little prodding to look for the politics in economic relations; nor did they find it difficult to heed Strange's call to make fundamentally moral judgments on matters of public concern.

Least of all did British academics require any encouragement to question authority. So-called critical theory, challenging orthodoxies of all kinds, has long found a comfortable home in the country's universities. Given its many variants, critical theory is not easily characterized. A "broad church" is how one practitioner calls it (Mittelman 1998, 64). Encompassed by the term are varieties of Marxian analysis, some forms of feminism, and other radical

schools of thought. One sympathetic source suggests that a more adequate label would be “ideologically oriented inquiry” (Griffiths 1999, 114).

Yet there is a common denominator—what one observer, himself a critical theorist, calls an “oppositional frame of mind” (Brown 2001, 192). Though divided over issues of ontology and epistemology, most versions of critical theory converge on a revisionist critique of modern capitalism, a heretical disposition that many scholars found easy to carry over into the new field of IPE. Cox, as we shall see in the next chapter, was among the first to go down this road; others soon followed, making an oppositional frame of mind a key element of British school discourse. Indeed, many in the school, one source suggests, might actually prefer to see their field called simply Critical IPE (Murphy and Nelson 2001, 394). Another source, using a biological metaphor, amusingly defines the “diverse critical species that comprise the genus” of the British school as *Querimonia*, in contrast to the rationalist species *Ratiosuarus rex* that we know as the American school (Dickins 2006, 480).

Finally, there was the difference in the British approach to economic studies, which was far less abstract than in the United States. British economists may have considered themselves more intellectually uncompromising than their political science brethren, as they argued at the Cumberland Lodge Conference, for example. But as compared with the way the economics discipline was developing in the United States, there was still much less emphasis on reductionism in Britain and certainly less reliance on numeracy. British economists still wrote with words. (Jokes a U.S. economist, “That’s what we call ambiguity.”) Typically they also remained more inclusive in their analysis, sensitive to the role of institutions and history; many British universities, including the LSE, even had a separate economic history department, counting among their faculty some of the best-known economists in the land. Thus as the infant field of IPE developed, there was correspondingly less pressure to conform to a highly demanding positivist or empiricist epistemology. One’s professional status did not require sacrificing detail for parsimony. Scholarship could be every bit as heterodox as Strange was suggesting.

ANOTHER IRONY

None of this is meant to imply that one of the two schools is somehow better or worse than the other. Any such comparison would obviously be invidious. The relationship between the two constructions is more akin to Akira Kurosawa’s 1950 film *Rashomon*, where separate narrators recalled the same basic story in vastly different ways. Here the story is the interaction of economics and politics in international relations. The American school has one way of telling the story, and the British school has another. Despite what many on either side of the Atlantic might contend, neither is inherently superior.

Quite to the contrary, in fact. In practice, the two schools complement each other neatly, the strengths of one largely balancing weaknesses of the other. The American school may take justifiable pride in its allegiance to the demanding principles of positivism and empiricism. But arguably it may also be reproached for its narrow preoccupation with scientific method and disdain for normative work. Scholars in the U.S. style, absorbed with midlevel theory building, are frequently insular in their intellectual interests, and indifferent to matters of equity or justice. Daring new ideas tend to be discouraged by the need to demonstrate careful methodological rigor. History and social context take a backseat to the parsimony of abstract, deductive logic.

Scholars in the British style, by contrast, help to compensate for such shortcomings with their intellectual ecumenism and critical attitude toward orthodoxy. The British school may be fairly criticized for its less rigorous approach to theory building and testing, thereby making generalization difficult and the cumulation of knowledge virtually impossible. But the British school may also legitimately claim to make a useful contribution by opening discourse to a wider range of insights and highlighting the normative element in scholarly inquiry. The more open range for research, as we shall see in subsequent chapters, permits consideration of grander issues of social transformation and historical change. Each school adds value in its own way.

But here too there is a deeper irony. When Strange began her campaign for a more modern approach, her aim was to end a dialogue of the deaf—the mutual neglect of two self-contained academic traditions. That battle is now won. IPE has become a recognized field of study. Yet the new school that was born in Britain has not only evolved in a manner quite different from that of its U.S. counterpart; it has also grown apart. In effect, a new dialogue of the deaf has emerged—a new case of mutual neglect of two academic traditions. In the words of one keen observer, “US-based and British school IPE have . . . evolved largely separately from each other, identified little with one another as parts of the same enterprise, and spoken largely to their own audiences rather than to each other” (Phillips 2005, 12). As the Murphy and Tooze critique of my 1990 essay suggests, the two schools have diverged on even the most basic questions of ontology and epistemology.

Each school has its own showcase for scholarship: respectively, BISA and APSA. Each also has what amounts to its own house journal: RIPE versus IO. Whereas RIPE proudly promotes the work of the British school, IO retains a distinctly American flavor. About nine of every ten articles published in IO are authored by scholars of U.S. nationality or affiliation (Aydinli and Mathews 2000; Breuning, Bredehoft, and Walton 2005). And the divergence is reflected in reading habits as well. Surveys have shown little correlation between the journals read on either side of the pond (Crewe and Norris 1991; Norris and Crewe 1993), nor is there much overlap in the authors favored by British and U.S. scholars (Murphy and Nelson 2001). The point was well illustrated in a

comment to me by a good friend (and leading member of the American school) who consented to read an earlier draft of this book. Referring to RIPE, my friend wrote that “it is hard to think of a journal that was first published in 1994 as having a big impact on the field. I see few cites to this journal in the stuff I read.” I was tempted to respond simply, “Your Honor, I rest my case.”

Divergences like these are not entirely surprising. Across the discipline of political science as a whole, it has long been known that U.S. and British scholars tend to tread separate paths—reading different literatures, taking part in different debates, and following different agendas (Norris 1997). Why should the subset of specialists in IPE be any different? Moreover, given their separate starting points, it was to some degree inevitable that over time their mutual insularity would be reinforced by divergent patterns of socialization. Winston Churchill, echoing an earlier thought of George Bernard Shaw, said that the United States and Britain are two nations divided by a common language. In similar fashion, the American and British schools of IPE have come to be two factions divided by a common subject.

This sort of phenomenon is hardly unfamiliar in academic life. Disciplines and specialties often fragment as researchers seek out the comfort of others who share the same perspective. As Margaret Hermann has observed (1998, 606), “Our identities become intertwined with the perspectives and points of view of the theoretical cohort to which we perceive ourselves belonging. And we tend to distance ourselves from those we do not understand or whose ideas seem discordant with our group’s theoretical outlook.” The process is a natural one and tends to be self-reinforcing. Once begun, its momentum is hard to overcome.

A new dialogue of the deaf was surely not what Strange had in mind. Her own inclination, true to her convictions about U.S. academia, was to blame it all on the Americans. Three years before her death, she suggested that U.S. scholars needed a hearing aid. Americans, in Strange’s words (1995, 290), are “deaf and blind to anything that’s not published in the U.S.A.” Unfortunately, there is some truth to that, as a recent survey of IPE curricula in U.S. universities testifies. The survey concludes that “frankly, American international political economy could benefit from becoming a little less American and a little more international” (Paul 2006, 733). But that is hardly the whole truth. Arguably, Strange too might be said to bear a good part of the responsibility, owing to the vigor of her entrepreneurial efforts and the sheer strength of her personality. Just as she never meant to be an academic, she may not have meant to create a new case of mutual neglect. Nonetheless, that is the result.