# *INTRODUCTION*

*Miss Prism*. Cecily, you will read your Political Economy in my absence. The chapter on the Fall of the Rupee you may omit. It is somewhat too sensational. Even these metallic problems have their melodramatic side.

*Cecily*. (*Picks up books and throws them back on table*.) Horrid Political Economy!

—Oscar Wilde, *The Importance of Being Earnest*, act 2

HORRID? After more than four decades of university teaching, I must concede that I have known students who seemed to agree with Cecily, though they did not often express their feelings in quite so forceful a manner. But I have also known many others who learned to appreciate the value—perhaps even the melodrama—that came from reading their political economy. Not everything in political economy may be as sensational as the fall of a currency, whether the rupee or any other. But much of political economy is indeed dramatic and just about all of it is important.

This book is about the academic field of study known as International Political Economy—for short, IPE. More precisely, it is about the *construction* of IPE as a recognized field of scholarly inquiry. Following standard practice, the term IPE (for the capitalized words International Political Economy) will be used to refer to the field itself, understood as an area of intellectual investigation. The field of IPE teaches us how to think about the connections between economics and politics beyond the confines of a single state. Without capital letters, international political economy refers to the material world—the myriad connections between economics and politics in real life.

As Oscar Wilde's witty dialogue suggests, sharp observers have long understood that such connections do exist. As a practical matter, political economy has always been part of international relations (IR). But as a distinct academic field, surprisingly enough, IPE was born just a few decades ago. Prior to the 1970s, in the English-speaking world, economics and political science were treated as entirely different disciplines, each with its own view of international affairs. Relatively few efforts were made to bridge the gap between the two. Exceptions could be found, of course, often quite creative ones, but mostly among Marxists or others outside the "respectable" mainstream of Western scholarship. A broad-based movement to integrate market studies and political analysis is really of recent origin. The field today has been described as a "true interdiscipline" (Lake 2006). IPE's achievement was to build new bridges between older, established disciplines, providing fresh perspectives for the study of the world economy.

An academic field may be said to exist when a coherent body of knowledge is developed to define a subject of inquiry. Recognized standards come to be employed to train and certify specialists; full-time employment opportunities become available in university teaching and research; learned societies are established to promote study and dialogue; and publishing venues become available to help disseminate new ideas and analysis. In short, an institutionalized network of scholars comes into being—a distinct research community with its own boundaries, rewards, and careers. In that sense, the field of IPE has existed for less than half a century. This book aims to offer an intellectual history of the field—how it came into being, and why it took the shape that it did.

## Who Cares?

But who cares? Why would anyone be interested in an intellectual history? Could anything be more dull? Or as Cecily might put it, could anything be more horrid?

In fact, there are three main reasons for an intellectual history of IPE. The first one is the practical importance of the subject matter. We are all affected, daily and deeply, by the nexus of economics and politics in international affairs. The gasoline that powers the world's cars comes mainly from nations like Saudi Arabia and Iran. Can anyone doubt that politics plays a critical role in determining the cost and availability of energy? The shirts and socks that can be bought at Wal-Mart come mainly from China. The largest part of U.S. grain production is sold in Europe and Asia. More than half of all Federal Reserve banknotes circulate outside the United States. The most popular car in the United States is produced by Japan's Toyota. Is there any question that all these market relations have political ramifications? IPE can be found every day in the pages of our local newspaper.

The second reason is the inherent allure of ideas, on which we all rely, consciously or unconsciously, to interpret the world around us. An academic field rests on ideas. Essentially, it is a mental construct that teaches us how to *think* about our experience—how things work, and how they may be evaluated. An intellectual history adds to our understanding by teaching us where a field's ideas come from—how they originated, and how they developed over time.

An intellectual history also reminds us that the construction of a field like IPE is never complete. History does not mean closed. A field of study in social science reflects the world in which we live, and since the material world is always changing, so too is the way we examine and evaluate it. Ideas and events are forever interacting and evolving. Our understanding, therefore, can always be improved. The construction of IPE has been an investment in intellectual progress. The field is also very much a work in progress.

At issue are profound questions of what scholars call *ontology* and *epistemology*. Ontology, from the Greek for "things that exist," is about investigating reality: the nature, essential properties, and relations of being. In other contexts, ontology is used as a synonym for metaphysics or cosmology. In social science, it is used as a synonym for studying the world in which we actually live. What are the basic units of interest, and what are their key relationships? Epistemology, from the Greek for "knowledge," has to do with the methods and grounds of knowing. What methodologies do we use to study the world? What kinds of analysis will enhance our understanding? The construction of a field of study requires development of a degree of consensus on both ontology and epistemology—a shared ("intersubjective") understanding of the basics. Whatever differences specialists may have on particular matters of substance, they must craft a common language in which to communicate. The process is never easy.

Finally, there is the *human* quality of the IPE story, which involves real people in real time. Ideas do not combat each other in some abstract, ethereal void. Ideas are the product of human imagination, pitting one scholar against another in verbal jousting or printed debate. Intellectual history is also a personal history. As we shall see, the key individuals involved in the construction of IPE have been anything but dull.

## Diversity

The field of IPE is united in its effort to bridge the gap between the separate specialties of international economics and IR; that is its common denominator. But IPE is hardly a monolith. The bridges are many and varied, making for a colorful interplay of ideas. Indeed, as a practical matter, there is no consensus on what precisely IPE is all about. Once born, the field proceeded to develop along divergent paths followed by different clusters of scholars. One source characterizes IPE today as "a notoriously diverse field of study" (Payne 2005, 69). Another describes it simply as "schizoid" (Underhill 2000, 806). Too often, students are exposed to just a single version of the field. One purpose of this book is to remind readers that there are in fact multiple versions, each with its own distinct insights to offer. Another purpose is to help readers understand why, among different groups of scholars, some ideas have come to enjoy greater weight and influence than others.

Globally, the dominant version of IPE (we might even say the hegemonic version) is one that has developed in the United States, where most scholarship tends to hew close to the norms of conventional social science. In the "American school," priority is given to scientific method—what might be called a pure or hard science model. Analysis is based on the twin principles of positivism and empiricism, which hold that knowledge is best accumulated through an appeal to objective observation and systematic testing. In the words of Stephen Krasner (1996, 108–9), one of the American school's leading lights, "International political economy is deeply embedded in the standard methodology of the social sciences which, stripped to its bare bones, simply means stating a proposition and testing it against external evidence."

In U.S.-style scholarship, most of the emphasis is placed on midlevel theory building. In contrast to macrotheory (or metatheory), midlevel theory eschews grand visions of history or society. Rather, work tends to concentrate on key relationships isolated within a broader structure whose characteristics are assumed, normally, to be given and unchanging. (Economists would call this partial-equilibrium analysis, in contrast to general-equilibrium analysis.) The American school's ambition has been self-consciously limited largely to what can be learned from rational, empirical inquiry.

Even its critics concede that the mainstream U.S. version of IPE may be regarded as the prevailing orthodoxy. Perched at the peak of the academic hierarchy, the U.S. style largely sets the standard by which IPE scholarship worldwide is practiced and judged. The American school's history, it is fair to say, is the story of the *core* of the field as we know it. Because of its acknowledged primacy, the U.S. version is the one that will receive the most attention in this book.

The U.S. version, however, hardly represents the only way that the field could have been constructed. The uniqueness—some would say the idiosyncrasy—of the U.S. style must also be stressed. In practice, the American school's self-imposed limitations have been challenged in many parts of the world and in many different languages. In France, more emphasis is placed on regulatory issues; in Germany, on institutions; and elsewhere, on various elements of Marxist theory. The range of alternative approaches, in fact, is remarkably broad—regrettably, too broad to encompass in a single brief history.

For want of space, this book will concentrate on work in the English language—in particular, on an alternative approach that has emerged in Britain and outposts elsewhere in the former empire, such as Canada or Australia. In these locales, scholars have been more receptive than in the United States to links with other academic disciplines, beyond mainstream economics and political science; they also evince a deeper interest in ethical or normative issues. In the British style, IPE is less wedded to scientific method and more ambitious in its agenda. The contrasts with the mainstream U.S. approach are not small; this is not an instance of what Sigmund Freud called the "narcissism of small differences." Indeed, the contrasts are so great that it is not illegitimate to speak of a "British school" of IPE, in contrast to the U.S. version.

The distinction is not strictly geographic, of course. There are Britons or others around the world who have happily adopted the U.S. style, just as there are those in the United States whose intellectual preferences lie more with the British tradition. The distinction, rather, is between two separate branches of a common research community—two factions whose main adherents happen to be located, respectively, on opposite sides of the Atlantic. The two groups may all be part of the same "invisible college," to adopt the term of Susan Strange (1988b, ix), patron saint of the British school. But between them lie deep ontological and epistemological differences.

To underscore the diversity of the field, contrasts between the American and British schools will be explicitly drawn in the chapters that follow. My main point is a simple one. Each style has its strengths—but also its weaknesses. Neither may lay claim to comprehensive insight or exclusive truth. To complete the construction of IPE, it is not enough to build bridges between economics and politics. Bridges must be built between the field's disparate schools, too.

## Intellectual Entrepreneurs

Students today take for granted the elaborate edifice of concepts and theories that has been erected to help sort out the mysteries of international political economy. It's there in the textbooks; therefore, it must always have been there. But it wasn't. Someone had to do the heavy lifting. IPE did not spring forth full-blown, like Athena from Zeus's forehead. It was quite the opposite, in fact. The construction of the field demanded time and a not inconsiderable amount of creative energy. No great tower of ideas stood hidden in the mists, just waiting to be discovered. IPE's architecture had to be put together laboriously, piece by piece, step by step. Indeed, the edifice is still being constructed.

How did it happen? As in all academic constructions, the achievement was ultimately a collective one—the product of many minds, each making its own contribution. Yet as every student of collective action knows, leadership is also vital to getting a complex project on track. Critical to the construction of IPE were some extraordinary individuals: a generation of pioneering researchers inspired to raise their sights and look beyond the horizon—beyond the traditional disciplines in which they had been trained—to see the politics and economics of international relations in a new, more illuminating light. Call them *intellectual entrepreneurs*, eager to undertake a new scholarly enterprise.

To stress the catalytic role of intellectual entrepreneurs is not to subscribe to a Great Man (or Woman) theory of history. I do not mean to caricature how knowledge is constructed. Ralph Waldo Emerson surely exaggerated when he declared, "There is properly no history, only biography." Yet individuals do matter. Every academic endeavor owes much to the determined efforts of a few especially creative master builders. "If I have seen further," the great Isaac Newton once wrote, "it is by standing on the shoulders of giants." We in IPE may say much the same. If today we can see beyond the horizon, it is because we too are able to stand on the shoulders of giants.

The intellectual entrepreneurs of IPE were economists, political scientists, and historians. Some were lifelong academics; others came to university research only after careers in other fields. Some collaborated actively; others cogitated in relative isolation. Some offered broad visions; others strove more to fill in the details. They didn't always concur. Indeed, disagreements among them were rife. Nor were they always right. But through their arguments and disputes—through the give-and-take of their enthusiastic debates—a new academic field gradually, if fitfully, emerged.

Time does take its toll, however, as inevitably it must. The ranks of the pioneer generation are thinning. Regrettably, a couple of old friends have already passed on; others have opted for blissful retirement. Among those remaining I modestly include myself. Originally trained in economics, I began my own foray into IPE in 1970 when, at the invitation of the New York publishing house Basic Books, I agreed to commission and edit a series of original treatises on international political economy—the first such project ever conceived. Ultimately, five books were published in the Political Economy of International Relations Series, including Robert Gilpin's classic *U.S. Power and the Multinational Corporation* (1975) as well as two volumes of my own, *The Question of Imperialism* (1973) and *Organizing the World's Money* (1977). The rest, as they say, is history.

More than a third of a century later I remain actively engaged in the field, which I have long regarded as my natural home. One source describes me as "one of the rare cases of an economist who came in from the cold" (Underhill 2000, 811). Almost all of my work has been in the general area of international money and finance, where by now, for good or ill, I have attained something of the aura of senior scholar status. "Godfather of the monetary mafia" is the way one younger colleague recently characterized me in a private correspondence. I like to think he meant it as a compliment.

Overall, I may not be the most qualified person to write on the construction of IPE. But neither am I entirely without credentials. I have been there from the start; I have been associated, directly or indirectly, with some of the most notable advances in the field; and I have been personally acquainted with almost everyone involved. Above all, I have no ax to grind, and so hopefully can remain reasonably objective in what I have to say here.

## Agency and Contingency

Throughout this book, two leitmotifs predominate. One involves *agency*—the indispensable role of individual action. Ideas may be in the air, but it takes determined initiative to grasp and wrestle them to the ground. Intellectual entrepreneurs were needed. The field's pioneers contributed to the construction of IPE in all kinds of ways—by their own writing and ideas, by mentoring students, by animating the work of colleagues, by editing, or simply by inspiration. Without their diverse efforts, the bridges between international economics and IR might never have been built.

The other leitmotif involves *contingency*—the unavoidable influence of chance. A new edifice was erected, but there was nothing inevitable about its specific shape or features, as the contrasts between the American and British schools amply reveal. Social and historical contexts matter. A different cast of characters, with other personalities or experiences, might have come up with a rather different style of architecture. Scholars are the product of many individual influences—geographic location, family upbringing, educational opportunities, disciplinary training, thesis advisers, work history, professional successes and failures, and more—all of which may have an effect on how each person responds to the same kinds of stimuli. Entrepreneurship is driven not only by inner convictions by also by external opportunities and constraints (otherwise known as serendipity). The same questions might have been asked. But with another choice of graduate school, another mentor, or some other network of friends and colleagues, the answers and emphases might have turned out quite differently.

Likewise, other historical circumstances might well have resulted in other shared understandings about how the world works. Without the cold war, which encouraged unprecedented generosity by the United States toward its former allies and adversaries, there might have been a different understanding of how power is used in the global economy. Without new multilateral organizations like the General Agreement on Tariffs and Trade (GATT) and International Monetary Fund (IMF), we might well have thought differently about the governance of international markets. Without the memory of the Great Depression, different assumptions might have emerged about the prospects for interstate cooperation or the relationship between economics and national security.

Admittedly, these two leitmotifs, agency and contingency, are not especially novel in the study of intellectual history. Indeed, specialists in the sociology of knowledge, following the seminal work of Peter Berger and Thomas Luckmann (1966), take for granted the central role of individual action, rooted in a specific time and place, in shaping perceptions of the world. "Reality is socially defined," wrote Berger and Luckmann. "But the definitions are always *embodied*, that is, concrete individuals and groups of individuals serve as the definers of reality . . . living individuals who have concrete social locations. . . . No 'history of ideas' takes place in isolation from the blood and sweat of general history" (107, 117). In more contemporary sociology, the people I call intellectual entrepreneurs have been labeled "knowledge specialists" (Swidler and Arditi 1994).

But even if not novel, the two leitmotifs deserve to receive more attention than they have from students of IPE. Too many scholars in the field fail to recognize the degree to which their view of reality has been shaped by influential individuals working in distant times. The great economist John Maynard Keynes wrote in his *General Theory* that "madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back" (1936, 383). Today's specialists in IPE may not be frenzied, and few, I suspect, are mad. Yet in the way we approach the field, to some extent we are all responding to the voices of older scribblers. In the ongoing story of IPE, the themes of agency and contingency are particularly apt.

## The Hall of Fame

Who were these intellectual entrepreneurs? No two observers, knowledgeable about IPE, might answer the question in quite the same way. It's one thing to say, "Round up the usual suspects"; it's quite another to agree on who the suspects are. One person's idea of a creative genius may be another's model of a hack. One person's conception of a core contribution may seem to others to be peripheral or—the worst academic insult possible—merely derivative. Any attempt to establish a definitive list of names, an IPE Hall of Fame, is bound to generate dissent.

It is with no little trepidation, therefore, that I spell out my own nominees in the following pages. They are (in alphabetical order) Robert Cox, Robert Gilpin, Peter Katzenstein, Robert Keohane, Charles Kindleberger, Stephen Krasner, and Susan Strange. For me, these are the people who most influenced the construction of the field in its early years—the Hall of Fame's first team All-Stars, as it were. With a nod to the cinema, we might call them the Magnificent Seven.

A more diverse group could hardly be imagined. At first glance, my nominees would appear to share little in common. They include one Briton (Strange), one Canadian (Cox), one naturalized U.S. citizen (Katzenstein), and four native-born Americans. They include two individuals trained in economics (Kindleberger and Strange) and one in history (Cox) as well as four political scientists. They include just five who received a PhD (Gilpin, Katzenstein, Keohane, Kindleberger, and Krasner) and just one whose earliest work evinced a particular interest in political economy (Krasner). Three had lengthy careers outside academia before committing to a life of scholarship (Cox, Kindleberger, and Strange). And their birth dates ranged from as early as 1910 (Kindleberger) to as late as 1945 (Katzenstein).

Yet on a deeper, more personal level, it is as if they all came from the same mold. With all of them, three indispensable attributes stand out. One is a *broad intellectual curiosity*, which led each of the seven to look for connections between diverse literatures and intellectual traditions. These were people who preferred to build bridges across disciplinary boundaries, not find a secure academic niche in which to specialize for a lifetime. Second is a *contrarian cast of mind*, in some instances verging on outright iconoclasm, which made them all quite comfortable challenging conventional wisdom. The Magnificent Seven were not inclined to accept the status quo as gospel. And third is an *acute sensitivity to experience*, which inspired them to question ideas and theories that seemed at variance with the evidence before their own eyes. For them, the value of scholarship could be measured not by the sophistication of a model or the elegance of a technique but rather by how much it added to an understanding of the real world. They may have differed greatly in nationality, training, or career, but in essential qualities of mind and personality they are as one.

Most important, they are all united by the *durability* of their contributions. Their early work may no longer be cited regularly; some of the specific subjects they addressed, such as international "regimes" or so-called hegemonic stability theory (HST), may today be considered quite passé. But even a casual glance at the contemporary literature reveals a continuing debt to their insight and creativity, as I hope to demonstrate in the chapters to follow. The influence of the Magnificent Seven, particularly in terms of ontology, is pervasive. Their pioneering constructions were decisive in establishing the basic language of the field. More than anyone else, they shaped the way we now think about IPE.

## Consensus

Some readers will say that in limiting myself to just seven nominees, I've left out a key name or two. Others will question why so-and-so is included. And still others, certainly, can be expected to challenge the relative significance I attribute to one individual or another. No one familiar with the field is apt to be entirely satisfied with my judgments here. My only defense lies in the modesty of my ambition. This book aims to provide no more than *an* intellectual history of IPE, not *the* definitive treatment. No claim is being made that mine could possibly be considered the last word on the subject.

But neither could anyone accuse this book of being especially far outside the mainstream. While personal, my selections may plausibly be defended as neither unreasonable nor idiosyncratic. Specialists might disagree over lesser luminaries. Nevertheless, across the field as a whole, there is actually a good deal of consensus about whose stars shine the most. How do I know? Quite simply, I asked.

Shortly after the idea for this book was hatched, I conducted a private survey of some sixty-seven acquaintances, all acknowledged experts in IPE. Each person was asked to identify up to six scholars who might be thought to have been the most influential in shaping the evolution of the field. Some forty-five responses were received from a wide range of individuals around the world—junior academics as well as senior, non-Americans as well as U.S. citizens, radicals and neo-Marxists as well as more orthodox centrists or conservatives. Although my unstructured poll could hardly be regarded as scientific, the results are highly suggestive. In all, some fifty-two names were cited at least once. Overall, however, votes were clearly skewed toward a much smaller handful of popular favorites.

The top vote getter was Keohane, whose name appeared on every single return—a notable achievement. Rounding out the roster of favorites (with votes in parentheses) were Gilpin (twenty-eight), Katzenstein (twenty), Krasner (eighteen), Strange (thirteen), and Kindleberger (thirteen), all on my own All-Star list. The only name missing was Cox, who received just four votes in my survey. Cox's absence is probably explained by the fact that the majority of my respondents were Americans, who know little of his work. But I nonetheless include him in this book because his lifetime of scholarship has had an indelible impact on generations of scholars in the British school of IPE.

Indirect support for my selections was also provided by a recent, more formal survey of IR faculty at U.S. colleges and universities (Peterson, Tierney, and Maliniak 2005). Some 1,084 academics, all political scientists, listed the four scholars they felt had made the greatest impact on the study of IR in the previous twenty years (encompassing all aspects of IR, not just IPE). Among the top twenty vote getters were five of the Magnificent Seven—Keohane (again the overall favorite, with votes from 56 percent of the respondents), Gilpin (ranked tenth), Katzenstein (eleventh), Krasner (twelfth), and Cox (eighteenth). Only Kindleberger, an economist, and Strange, the Briton, were nowhere to be found in the ranking.

Each of the Magnificent Seven will be featured in the chapters to come. To fill out the story, so will a variety of other scholars who may be less familiar or more controversial. Some played key supporting roles; others were little more than walk-ons. But all deserve mention if the story is to be anywhere near complete. The choices of who to include as well as the evaluations offered are of course my responsibility alone.

## Summary

The goal of this book, in short, is to tell the story of the modern field of IPE. This is not a textbook; we already have enough of those. Nor do I mean to offer a comprehensive survey of all the relevant literature in the field; that would demand far more space than can be provided in a single slim volume. Rather, my intention here is best understood as an exercise in interpretative analysis, selective in both its coverage and emphasis. Explanations are offered;

judgments are made. But there is no pretense that my discussion will meet the fullest standards of empirical proof.

The focus here is on contributions to *theory*—the abstract concepts, principles, propositions, and conjectures that together have shaped the common language of IPE. Purely empirical research or applied policy studies are addressed only to the extent that they are informed by theory, adding to the mental constructs that help us think about the world. My principal aim is to highlight the central role of ideas as such: the vital part that theorizing and theory building have played in the construction of the IPE field.

The intended audience for the book is first and foremost the invisible college itself—the population of scholars and students dedicated to furthering our understanding of the nexus of economics and politics in global affairs. All specialists in the field can benefit from a refresher course on IPE's origins and development. Beginners will gain a greater appreciation of the effort and energy that went into building the first bridges between international economics and IR. Even seasoned veterans are likely to find new discoveries in old, seemingly familiar material.

Beyond the inner circle, researchers in related disciplines—certainly in international economics and IR, and perhaps also in other areas of inquiry—might gain fresh insight into the foundations and construction of their own academic specialties. Students of the sociology of knowledge or intellectual history should find the case of IPE instructive, possibly even illuminating. And with luck, the book might even appeal to more general readers with a particular taste for the interplay of ideas, personalities, and events.

With all these audiences in mind, the style of the book is designed to be as reader friendly as possible. Wherever feasible, jargon is shunned; where specialized language cannot be avoided, every effort is made to define or explain the terms clearly and succinctly. In hopes of sustaining readers' interest, the text departs frequently from a purely abstract analysis of ideas and theories to weave in pertinent historical context, biographical sketches of the Magnificent Seven, and even from time to time a bit of personal memoir. And above all I do not hesitate on occasion to inject humor into the mix, to liven what otherwise could easily become a somber, even drab read. I have never doubted that even the heaviest subjects can be addressed with a light touch.

In the end, the book seeks to make a plausible case for three general arguments.

First, as I noted earlier, there is the critical role of what I call intellectual entrepreneurs—pioneers like the Magnificent Seven who are prepared, even eager, to think outside the box. Though ultimately the creation and cultivation of an academic field is a collective effort—involving many hardworking scholars, not just a few whose names we still remember—there is no question that leadership is essential to provide the necessary catalyst. The agency of intellectual entrepreneurs, I contend, is indispensable.

Second, there is the equally crucial role of contingency, as also mentioned above. That does not mean that all is arbitrary. Even with different personalities or historical circumstances, the broad contours of the field might have turned out pretty much the same. But it does mean that within the natural limits set by past experience and tradition, little is predetermined. The details of content and emphasis can vary considerably, depending on little more than chance. The influence of contingency must not be underappreciated.

And third, there is the issue of diversity and what to do about it. Differences between factions, such as exist between the American and British schools, are not necessarily to be deplored. Contestation between contrasting perspectives is often the richest source of intellectual growth. But the key is engagement. To promote new knowledge, factions must expose themselves to fruitful, honest debate. The American and British schools are in many ways complementary, and have much to learn from one another. Yet they have to try. The concluding argument of the book, spelled out most clearly in the final chapter, is that much can be gained from building new bridges between scholars on the two sides of the Atlantic.

## Organization of the Book

The book is organized into seven chapters: two chapters on the birth and development of IPE in the United States and Britain, three on broad overarching themes that in my opinion have been most central to the field's construction, and two appraising present accomplishments and future prospects. Throughout, subjects are placed in historical context in order to tie intellectual developments to their roots in the real world. Most important, the text emphasizes the contributions of key members of IPE's pioneer generation—the intellectual entrepreneurs whose initiatives proved most influential in shaping the field as we know it today.

Chapter 1 begins with the birth of IPE in the United States, stressing the critical roles played by Keohane (together with his colleague Joseph Nye) and Gilpin. What accounted for the emergence of the U.S. version of the field in the early1970s, and why did its development take the course that it did? Crucial here was the relationship between the two disciplines from which the American school drew its main inspiration: economics and politics. Although a few mainstream economists played a vital role in the first years, the school's research agenda was soon seized by political scientists. Had the economics profession been more proactive at the start, IPE in the United States might well have evolved in a quite different direction. Yet in the end, ironically, economics has reclaimed a share of "ownership" of the field, at least in terms of epistemology. More and more, the predominant methodologies of the American school—how things are studied—have come to mimic the research techniques of the economics discipline—a trend that I describe as a kind of "creeping economism."

Chapter 2 traces the parallel development of IPE in Britain, dating back to a seminal paper published by Strange in 1970 titled "International Economics and International Relations: A Case of Mutual Neglect." Her clarion call for a "modern study of international political economy," a virtual manifesto, served as a source of inspiration for successive generations of scholars in Britain and other English-speaking countries, giving rise to what today is recognized as the British school of IPE. The main distinguishing characteristics of the British school are a ready acceptance of links to disciplines other than political science and economics along with a vital interest in a wide range of normative issues. These features stand in sharp contrast to the way that IPE has developed in the United States, where positivism and empiricism rule. The result of this divergence is a new case of mutual neglect that is nearly as profound as the void Strange deplored back in 1970.

In chapter 3, I take up the Really Big Question of systemic transformation, which not surprisingly was among the first issues to be addressed by the infant field. In the United States, thinking about systemic transformation was most fashionable in IPE's early years, when the horizons of inquiry seemed virtually unlimited. The main focus of debate, following the lead of Kindleberger, was HST. But in more recent years, interest in the grand theme of systemic change has largely faded in U.S. academic circles. Remarkably, this comes despite claims by many observers that today, more than ever, we may indeed be living through a truly historical transition—what has come to be known as the age of globalization. Only in the British school, owing especially to the influence of Cox, is there still much interest in the Really Big Question. Nothing better defines the differences between the American and British schools than their respective discourses on systemic transformation. From the start, each tradition constructed its own distinctive approach to the topic.

Chapter 4, in turn, addresses the issue of system governance—the widening "control gap" between state aspirations and state capabilities created by the growth of global economic interdependence. If national governments were losing control, who then would make the rules for the global system, and how would compliance with those rules be assured? For the American school the answer lies in international institutions, broadly conceived as forms of patterned cooperation among states. This discourse was set on track first by Krasner, who popularized the new concept of international regimes, and then by Keohane, who later broadened the inquiry into a more general study of institutional arrangements among governments. For the British school, by contrast, the answer is more complex, going beyond states alone to encompass a much wider array of authoritative actors. On the subject of the Control Gap, as on the Really Big Question of systemic transformation, each tradition has constructed its own distinctive approach.

In chapter 5, we come to the Mystery of the State. Most of the many differences between the American and British schools boil down to their contrasting attitudes concerning a single issue: the place of the state in formal analysis. Is the sovereign state the basic unit of interest or just one agent among many? Is public policy the main concern of IPE, or is there more to the story? Both schools acknowledge that the state is a key actor. But is the state the *most important* actor—the only really interesting focal point for analysis? That is the mystery. Scholars working in the U.S. tradition take for granted that IPE is first and foremost about states and their interactions. That does not mean traditional realism—the billiard ball model of rational, unitary states, conceived as closed "black boxes" driven solely by calculations of national interest and power. States are at the center of analysis, but they are by no means the sole actors. From early on, thanks in particular to the efforts of Katzenstein, the black box was opened to admit a much wider range of relevant actors and influences. Katzenstein's signal contribution was to encourage the addition of domestic and, later, ideational factors to the mix. As seen from the British perspective, however, a preoccupation with the state betrays the American school's early capture by political scientists like Keohane, Gilpin, and Krasner. Scholars in the British tradition prefer to follow the lead of Strange and Cox, resisting any attempt to subordinate IPE to the study of IR.

Chapter 6, reprising the field's construction, asks the question: After all these efforts, what have we learned? What do we know now that we didn't know before? If knowledge is measured by our ability to make definitive statements—to generalize without fear of dispute—the field's success may be rated as negligible at best. Many theories have been developed, from HST onward. But none is universally accepted, and disagreement persists over even the most basic issues of process and structure. On the other hand, if knowledge is measured by our ability to define the research agenda—to ask the right questions, even if we don't yet know the answers—progress has been a bit more significant, though here too the diversity of the field is well illustrated by the contrasts to be found between IPE's American and British versions. Students in the two traditions are taught to ask distinctly different sets of questions. Whereas the state-centric U.S. style is most concerned with the causes and consequences of public policy, the more inclusive British approach is inclined to encompass a rather broader range of social issues and concerns. Both schools may legitimately claim to have contributed significantly to our understanding. Yet since their agendas are so divergent, so too is what we have learned from each. The body of knowledge that has been created is large, but it is hardly tidy.

Chapter 7, finally, asks: What next? Where should the field go from here? The construction of IPE is unquestionably a major accomplishment. An elaborate edifice of concepts and theories has been put together. But the edifice is hardly complete. Though much has been learned, serious gaps remain in our understanding. Arguably, new bridges need to be built in three critical areas: between the past and the present; between rationalist and cognitive analysis; and perhaps most important, between the American and British versions of the field. For all of IPE's accomplishments to date, there is still considerable room for a new generation of intellectual entrepreneurs to follow in the footsteps of the Magnificent Seven.