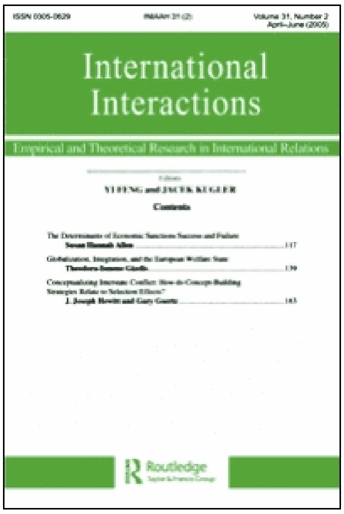

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A Grave Case of Myopia

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A Grave Case of Myopia

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Political scientists like to talk about the "median" voter—the representative voter in the middle. Suppose, analogously, we were to speak of the median scholar of international political economy (IPE) in the United States—the representative scholar in the middle of what elsewhere I have called the American school of IPE (Cohen 2008). This individual can be assumed to browse periodically through the standard literature, following current fashions in research. He or she may subscribe to mainstream U.S. journals like *International Organization* (*IO*) or *International Studies Quarterly* (*ISQ*) or buy books published by the country's better known university presses, such as Princeton or Cornell. He or she might also sit in on a few IPE panels at the annual meetings of the American Political Science Association and International Studies Association or perhaps even the new International Political Economy Society. Would this median scholar, reasonably well informed and *au courant*, have been given any reason in recent years to anticipate that we all would soon be engulfed in the worst financial crisis since the Great Depression? The answer is No. The question is: Why?

Much the same question was asked about international relations (IR) theory two decades ago after the sudden collapse of the Soviet Union. IR scholars too were caught short by a major structural shock. "The abrupt end of the Cold War," wrote the eminent historian John Lewis Gaddis (1992/93:5–6), "astonished almost everyone. . . . [We] failed to see it coming." Why was that? The consensus among IR theorists today is that the problem lay not in the quality of mainstream scholarship but in a signal failure of imagination: an inability or unwillingness to consider even the possibility of such a radical systemic change. In the words of Ted Hopf (1993: 202): "American social scientists did not look at the problem and then attack it with inappropriate theories and methods; they simply failed to look at the problem at all." Summarized Richard Ned Lebow and Thomas Risse-Kappen (1995:2) in a comprehensive retrospective: "The profession's performance was embarrassing. There was little or no debate about the underlying causes of systemic change. . . None of the existing theories of international relations recognized the possibility that the kind of change that did occur could

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occur." The challenge, they concluded, was to understand *why* IR theory was afflicted with such a grave case of "myopia."

American IPE's performance in the run-up to the current crisis has been equally embarrassing. Like the collapse of the Soviet Union, the crash of the global financial structure has all the earmarks of a genuine systemic transformation – the end of an age of vast, untrammeled market expansion and neoliberal deregulation. "The first crisis of the current era of globalization," one influential commentary has called it, a shock that has "started to reshape the global economy and shift the balance between the political and economic forces at play" (Pisani-Ferry and Santos 2009:8). Yet like IR theory of a generation back, IPE scholarship in the United States simply ignored the possibility that such a massive change could occur. Here too we failed to see it coming—another grave case of myopia. And here too our challenge is to understand why.

A DISMAL RECORD

How dismal was the profession's performance? A quick scan of articles published over the last decade in top U.S. journals (including *IO, ISQ, World Politics, American Political Science Review,* and *American Journal of Political Science*) reveals remarkably few studies that even broach the subject of financial crisis—fewer than a dozen in all—and of this handful almost all were essentially backward looking, limiting themselves mainly to explaining policy responses to banking or currency failures in the past. Some concentrated on distributional issues and the role of key interest groups. Lawrence Broz (2005), for example, undertook a detailed analysis of Congressional roll calls on financial rescues organized for Mexico and several East Asian nations in the 1990s. He found strong evidence of the impact of private-sector interests on legislative voting patterns. Similarly, Thomas Pepinsky (2008) documented the salience of varying coalitional bases in accounting for differences in adjustment policies adopted by countries like Indonesia and Malaysia during the Asian emergency of 1997–98. Others such as Hicken et al. (2005), Rosas (2006), and Keefer (2007) focused more on the part played by domestic institutions—political regime type, degrees of central-bank independence, or variations in executive accountability—in shaping governmental reactions to financial crisis. In most cases the research was thorough and insightful; none could be accused of failing to meet a high standard of scholarship. Collectively, however, there *was* a failure of imagination. Not a single one of these analyses gave even a clue that a major systemic change might be just around the corner.

Only rarely did anyone try to peer forward to anticipate possible crises in the future, and even these efforts were limited mostly to individual countries rather than to risks for the system as a whole. Illustrative was a perceptive study by David Leblang and Shanker Satyanath (2006), who focused on linkages among domestic political institutions, financial-market expectations, and the onset of currency crises. Institutional variables such as divided government or government turnover, Leblang and Satyanath argued, were likely to increase the variance of expectations among speculators and thus heighten the chance of a run on a country's money. The paper's aim was to improve on the ability of standard economic models to forecast national currency crises. But that is hardly the same thing as contemplating the possibility of radical transformation on an international or global scale.

Across the Atlantic in Britain, the story was otherwise. In my recent book, *International Political Economy: An Intellectual History* (Cohen 2008), I drew a contrast between the American school and the distinctly different style of scholarship prevalent in Britain—what I called the British school. In Britain there were actually quite a number of scholars who foresaw the possibility of a major structural crisis in finance. Many took their inspiration from Susan Strange, the doyenne of the British school, whose concerns were well articulated in two memorable books, *Casino Capitalism* (1986) and *Mad Money* (1998). The titles say it all. With the rise of largely unregulated capital markets, Strange contended, the global financial system was becoming more and more fragile, worryingly vulnerable to bouts of speculation and instability. A debilitating crash was only a matter of time. Others, such as Paul Langley (2002) and Matthew Watson (2007), developed the theme in greater detail, emphasizing the need for ameliorative actions before it was too late. So too did Mark Blyth (2003), a Scot based in the United States, who considers himself caught in the middle of British and American IPE, "a man without a country" (Blyth 2009).

This does not mean that British scholars were particularly prescient. Predictions were loosely framed and often maddingly imprecise. Few analysts foresaw the specific sequence of events that unfolded; many were downright wrong about the details; certainly none got the timing right. But even if they invited comparisons with the boy who repeatedly cried wolf, their sense of the larger picture cannot be denied. In their anxious ruminations, the threat of a looming crisis was palpable. No one reading the British literature could say that they were not warned.

The sad fact, however, was that few on the U.S. side of the Atlantic were listening. In my *Intellectual History* (Cohen 2008) I sought to draw attention to the deep and persistent gap that has developed between the American and British versions of IPE, discouraging any kind of meaningful dialogue. Over time, the two schools have evolved quite separately from one another; reinforced by divergent patterns of professional socialization, they are by now disinclined to pay much heed at all to research coming from the opposite side of the pond. The insularity has been particularly pronounced in the United States. As a result, most American scholars remained placidly unaware of the warnings of financial crisis coming from their British counterparts. In the words of Ronen Palan (2009), a leading light of the British school, it was as if the two traditions "inhabited parallel worlds: in one, the British, finance was considered all-conquering, all-encompassing, speculative and highly volatile. . . . In the other, the American, finance was a secondary sphere of activity, and by itself, was not problematized." Ignorance was bliss.

EXPERIENCE

So we were caught short. In my opinion, two factors largely explain why: experience and epistemology. U.S. scholars—myself included, no less than others—became captive to both the evidence of our eyes and the dogmas of our discipline. Each factor, in its way, constrained our ability to think outside the box.

The role of experience can be phrased in terms of a simple—but false—syllogism. The probability of a systemic crisis could be gauged by the frequency of occurrence. No systemic crisis had struck for decades. Therefore no crisis was likely.

Was this unreasonable? Not at all. Why should we *not* rely heavily on the evidence of our eyes? In the social sciences, it is only natural that thinking should be tied closely to historical context—both to things that happen and those that do not. If we see new events or trends in the world around us, understandably we want to analyze and evaluate them. What are their causes? What are their consequences? Conversely, if certain contingencies fail to make an appearance for decades, it is understandable that we might discount the likelihood of recurrence, turning our attention elsewhere. Social scientists have an obvious incentive to focus on what their experience tells them is actually going on in society. In IPE, no less than in other branches of the social sciences, our research priorities are guided by our environment.

Indeed, the very existence of IPE as a recognized academic specialty can be attributed to the impact of environment, as I stressed in my *Intellectual History* (Cohen 2008). Although the myriad links between international politics and the global economy had long been evident to many, it was not until the early 1970s that IPE began to emerge as a systematic area of scholarly inquiry, with its own networks, norms, and professional career opportunities. Many influences drove the process, but none so much as the force of events—the accumulation of evidence that fundamental changes were occurring in the world, calling for new understandings of how things work and how they might be studied. The European and Japanese economies had staged a remarkable recovery after the devastation of World War II; the postwar dominance of the United States appeared to be in irreversible decline; decolonization was heightening awareness of the challenges and dilemmas of development; and the gradual liberalization of trade was generating a new interdependence of national economies, seeming to threaten the old authority of sovereign governments. A whole host of dogs was barking, demanding scholarly attention.

Other dogs, meanwhile, remained silent, allowing priorities to shift. Among these was the threat of systemic financial crisis. Of course, most of us were familiar with Charles Kindleberger's classic *Manias, Panics, and Crashes* (1978), so we knew that the risk of another dramatic calamity could not be dismissed absolutely. Moreover, even if we were largely oblivious to warnings coming from the British school, we knew that the growth of capital mobility from the 1960s onward clearly was making global finance increasingly fragile. Yet practical experience seemed to suggest that underlying structures were now more resilient than before, thanks to the wisdom gained from fighting the Great Depression and the widespread institutional reforms that followed. Admittedly, stresses were not uncommon. In my professional lifetime, *inter alia*, I have lived through the collapse of the Bretton Woods exchange-rate system in the early 1970s, the Latin American debt crisis of the 1980s, and East Asia's banking and currency troubles in the late 1990s—each, in its day, seemingly the darkest of thunderclouds. In the end, however, each emergency was weathered reasonably well, leaving the overall system intact. And then, at the start of the new millennium, we entered into a period of exceptional calm in global finance—an era dubbed by some the Great Stability. Scholars could hardly be blamed for looking elsewhere for interesting research topics.

Scholars are by no means alone in this respect, of course. An inclination, over time, to discount the risk of potentially disastrous crises of unknown probability is in fact quite common. In the insurance business it is known as "disaster myopia"—the tendency that most people have to grow increasingly complaisant about the probability of something like a fire or flood as the interval since the last major occurrence lengthens. The more time passes without another calamitous upheaval, the more individuals behave as if it could never happen again. IPE scholars in the United States were, in that regard, unexceptional. Decades had passed since the transformative experience of the 1930s. By the turn of the twentieth century, we had all developed a bad case of disaster myopia.

EPISTEMOLOGY

But the problem also goes deeper. Reinforcing the impact of experience was the framing effect of epistemology—the methodological standards that we set for ourselves. By convention, mainstream American IPE today is biased toward mid-level theory building, focusing primarily on key relationships isolated within a broader structure whose characteristics are assumed, normally, to be given and stable. At its core, therefore, the U.S. version of the field is implicitly constructed to downplay the possibility of major systemic change. As Palan might put it, the structure itself is simply not problematized.

Epistemology, from the Greek for "knowledge," has to do with the methods and grounds of knowing. In the United States, IPE methodology tends to hew closely to the norms of conventional social science, borrowing in particular from neoclassical economics with its well-known penchant for high-powered math and formal statistical techniques. Priority is given to scientific method—what may 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. As Stephen Krasner (1996:108–109), one of the pioneers of American IPE, has put it: "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." The style is reductionist, paring messy reality down to its bare essentials. The emphasis is on technical sophistication and intellectual elegance. Formal research tools are put to work to test parsimonious hypotheses against the evidence of the real world.

Many reasons have been suggested for American IPE's love affair with scientific method—editorial control of journals, the standards applied in tenure or promotion cases, the way we teach our graduate students. But these are more symptom than cause. Underlying them all is a deeper issue, involving us and our peers in the economics profession. To be blunt: political scientists in the United States have an inferiority complex when it comes to economics—what might be described as a case of "peer-us" envy. Even such notables as Peter Katzenstein, Robert Keohane, and Krasner bow their heads, describing economics as "the reigning king of the social sciences" (Katzenstein et al. 1999:23). Whether the title is deserved or not, it is certainly true that the reductionist style of mainstream economics has come to set the standard for what passes for professionalism among U.S. social scientists. If today the most highly rated work in American IPE tends to mimic the economist's demanding hard-science model, it is largely to demonstrate that our field, for all the ambiguities of the political process, is no less capable of precise and formal rigor. IPE scholars want respect, too.

The great advantage of scientific method lies its ability to promote a broad cumulation of knowledge. Its greatest disadvantage is its tendency to shrink the horizons of scholarship. Two powerful forces are at work. First is the inherent tendency of a reductionist style to rely heavily on assumptions that set aside the possibility of broad systemic change. Attention is naturally diverted from the big to the small—toward partial (rather than general) analysis. The underlying paradigm of neoclassical economics emphasizes comparative statics rather than dangerous dynamics. The starting point is an initial equilibrium. Disturbances, when they occur, then are assumed to trigger stable processes of adjustment that ultimately lead back to a new state of equilibrium. Change, when it occurs, tends to be incremental rather than transformative. And second are the practical requirements of empiricism. By definition, a hard science model depends on the availability of reliable data. Research, accordingly, tends to become data-driven, diverted away from issues that lack the requisite base of information. In effect, scientific method plays a key role in defining *what* can be studied, automatically marginalizing grander questions that cannot be reduced to a manageable set of regressions or structured case-study analysis.

The prevalence of mid-level theory in American IPE, therefore, is no accident. An epistemology that takes the stability of underlying structures for granted is hardly likely to encourage serious theorizing about broad changes in the global political economy. Horizons have not always been so shrunken, of course. In the modern field's early days, back in the 1970s and 1980s, debates over "big" systemic questions were rife, centered in particular on the role of hegemony and potential consequences of the seeming decline of the United States. More recent decades, however, have seen a distinct loss of ambition, reflecting the gradual "hardening" of prevailing methodologies. Grand theories attempting to explain how the overall system works are no longer fashionable. Instead, research has come to focus more and more on small insights about actor behavior in specific, narrow contexts. For some, the trend represents progress—all part of the "maturing" of the field, as David Lake (2006) puts it. The more scholars limit themselves to a hard science model, the more the field approaches the respectability of "normal" science. But to my mind that assessment is altogether too kind, since it discounts the severe costs involved. The price of this kind of "progress" is measured by how much gets left out. Little room remains for holistic thinking about the system as a whole.

LESSONS

Can the field's myopia be corrected? To avoid similar failures of imagination in the future, two lessons should be taken to heart. The first has to do with the narrowness of our epistemology, which tends to preclude consideration of broader issues of structural stability and development. The second has to do with the insularity of our research, which leaves us ignorant of insights and perspectives available elsewhere.

That our epistemology has become unduly narrow is by now widely recognized. Reconsideration does not require sacrificing the rigor of a hard-science model. But it does mean according greater respectability to work that is not so highly dependent on the demands of a highly reductionist style. Technical sophistication is by no means the only measure of professionalism. Equally valid are the thoughtful insights of analysis that is more historical or institutional or interpretive in tone. The key, it would seem, lies in what Katzenstein (2009) calls “analytical eclecticism”—a pragmatic research style that is willing to borrow concepts, theories, and methods from a variety of scholarly traditions as needed to address socially important problems. We need to reward scholarship that is driven by questions, not data—especially by the kind of “big” systemic questions that were once a central part of the field’s agenda. American IPE must once again broaden its horizons.

Likewise, we would also benefit from paying more attention to work outside the mainstream of scholarship in the United States, to be found in Britain or elsewhere. Too often, U.S. scholars look with disdain on research coming from abroad that does not conform to conventional social-science norms. The loss, however, is ours, as our embarrassing failure to anticipate the current crisis amply testifies. Others across the pond saw it—or something like it—coming. We did not. We should not let ourselves be caught short again.

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