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**THE MULTIPLE TRADITIONS OF AMERICAN IPE**

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International Political Economy (IPE) came late to the United States. Although astute observers had long acknowledged the obvious linkage of economics and politics in global affairs, it was just a few short decades ago that IPE began to emerge as a formal, recognized field of scholarly inquiry with its own standards, concerns, and career opportunities. The birth of American IPE is generally dated from the late 1960s or early 1970s – the result of a conscious campaign to marry the academic specialties of international economics and international relations (IR). What emerged from those early years was by no means a monolithic orthodoxy. From the start American IPE has been characterized by multiple intellectual traditions, reflecting the hybrid nature of the field itself.

Commonalities exist, of course. In the United States, IPE has become essentially a sub-specialty of the study of IR – in effect, a branch of political science. Most scholars working in the American style take for granted that, first and foremost, IPE is about sovereign states and their interactions, as in IR more generally. In the words of one popular textbook, IPE is about “the relationship of the world economy to the power politics among nations” (Lairson and Skidmore 2003: 6). National governments are the core actors. Public policymaking is the main concern. Analysis is directed largely to understanding the sources and implications of state behavior.

Further, most scholarship in the United States tends to hew close to the norms of conventional social science. Priority is given to scientific method -- what might be called a pure or hard science model. Analysis is typically based on the twin principles of positivism and empiricism, which hold that knowledge is best accumulated through an appeal to objective observation and the systematic evaluation of evidence. Normative concerns are downplayed and grand theorizing is generally eschewed. Instead, most emphasis is placed on mid-level theory, concentrating on key relationships isolated within a broader structure whose characteristics are unquestioned and assumed, normally, to be given and unchanging. All these commonalities are distinctive enough to warrant designation of a unique “American school” of IPE, in contrast to alternative styles of inquiry that have arisen elsewhere (Cohen 2008).

Yet for all that is shared in common, the American school rarely speaks with one voice. Within the U.S.-based research community diverse camps have emerged over time, making for lively debate and a fruitful cross-fertilization of ideas. The earliest divide stemmed from natural differences between economics and political science, American IPE’s two parent disciplines. The pioneers of the field in the United States were international economists or students of IR. Though the political scientists, as it turned out, largely predominated in shaping the U.S. version of IPE, the economics profession has always remained influential, particularly in matters of methodology. Additional divides reflect paradigmatic differences, particularly among political scientists.

The aim of this essay is to explore the origins and interactions of American IPE’s multiple traditions. Three themes are stressed: the early capture of the field by political scientists, who took custody of a terrain largely abdicated by economists; the subsequent emergence of the U.S. community’s diverse camps in an ongoing dialectical process of intellectual development; and, most recently, at the level of methodology, the gradual recapture of a good part of American IPE by the research techniques of economics. The field may be young in chronological terms. But it already has a rich and diverse history.

#### **CAPTURE**

The origins of the American school go back, first and foremost, to the pioneering efforts of a remarkable generation of political scientists, led by the likes of Robert Keohane and Joseph Nye (1972, 1977), Robert Gilpin (1972, 1975), Peter Katzenstein (1976, 1978), and Stephen Krasner (1976, 1983). The historical circumstances that triggered a systematic interest in IPE are not difficult to understand. More puzzling is why economists, by and large, so conspicuously absented themselves while the field was being built.

### *Historical circumstances*

The birth of a new field of study does not take place in a vacuum. Particularly in the social sciences, intellectual developments tend to be tied to historical context – to new events and trends that make old ways of thinking inadequate. And so it was with American IPE. Fundamental changes were occurring in the world. Both the politics and the 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 mid-century, the United States had bestrode the world economy like a colossus. But with its growth rate slowing and its balance of payments mired in deficits, America 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 appeared just about over. 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, which seemed to threaten the ability of governments to manage economic affairs. Year by year, world trade was growing 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 euro-currency markets – from the late 1950s onwards. By the end of the 1960s it was evident that the spread of international economic activity had reached a critical juncture, soon to be manifest in a breakdown of the Bretton Woods monetary order, rising protectionism, oil shocks, and stagflation. Power now seemed to be slipping from states, limiting their ability to attain critical goals. Yet mainstream economics seemed incapable of providing effective solutions.

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. For years, the Cold War had 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 provided an opening to political scientists. Students of world politics could now safely divert some of their attention elsewhere – for example, to the politics of the global economy.

### ***Abdication***

The new field did absorb elements of international economics, of course. But for Keohane and Nye and others of their generation, IPE seemed most naturally a logical extension of their interest in IR. As one colleague has suggested to me in private correspondence, 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. Effectively, American IPE was captured by political scientists.

The critical question is: Why didn’t economists fight harder for “ownership” of the field? Economists were there at the creation, after all. In fact, most of the field’s earliest work in the United States was by economists, before the political scientists took over. One example, dating back to 1948, was Jacob Viner, who sought to explore the relationship between “power” and “plenty” as objectives of foreign policy. Twenty years later Richard Cooper (1968) published *The Economics of Interdependence*, highlighting the political challenges posed by the growing connections between national economies. In 1970, there was *Power and Money*, a short book by Charles Kindleberger (1970) on the growing tension between economic and political activity in an increasingly interdependent world. And in 1971 came Raymond Vernon’s memorable *Sovereignty at Bay* (1971), which heralded the arrival of the multinational corporation as a key political actor on the global 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.

Yet once the political scientists arrived on the scene, economists for the most part abdicated. Since the early 1970s, IPE has been a preoccupation only at the fringes of the economics profession. On the conservative right are scholars like Thomas Willett (1988; Willett and Vaubel 1991) who extend so-called public-choice theory to the international arena, following the early lead of Swiss economist Bruno Frey (1984). Public choice is the study of politics using the tools of economic theory; its focus is on implications that can be drawn from the underlying motivations of individuals in the political process. Both members of the general public and government leaders are analyzed as rational, self-interested actors striving to maximize some objective utility function, in a manner analogous to the behavior of individuals and firms in the marketplace. The insights of public choice make a valuable contribution in helping us to evaluate policy outcomes in terms of standard efficiency criteria. But it is also a limited contribution since it makes little serious attempt to incorporate the insights or intuitions of political science into the equation. With its methodological individualism, along with its emphasis on formal modeling, public-choice IPE amounts to little more than an application of traditional economic concepts to decision-making in the global economy. It might best be labeled the microeconomics of international relations.

Conversely, on the radical left are scholars of a more critical persuasion, best represented by the Union for Radical Political Economics (URPE), founded in 1968. URPE's origins lay in the New Left politics of the turbulent 1960s, driven especially by a revulsion with the war in Vietnam. The group's aim was to promote a new interdisciplinary approach to political economy, a fresh look at the connections between economics and politics, with particular emphasis on the dynamics and evolution of global capitalism. According to the group's website ([www.urpe.org](http://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." The value of such an approach lies in its emphasis on the broader historical structures within which political and economic activity takes place. But with its self-conscious iconoclasm and its Marxist overtones, URPE has never managed to exercise much influence on the mainstream of the U.S. economics profession.

Indeed, despite the dramatic changes in the international environment that were evident when American IPE was first getting started, the mainstream of the economics profession remained largely indifferent. The reasons were three-fold: ideological, ontological, and epistemological.

First, there was the chilling effect of postwar anti-communism. Political economy tended to be equated unthinkingly with Marxism or other unacceptable leftist doctrines. By the late 1960s détente may have been melting the ice of the Cold War, reducing tensions between the nuclear superpowers. But 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 upon 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. Apart from the youthful founders of URPE, few American economists at the time had much taste for ideas or arguments that might smack of anti-capitalist sentiment. In any attempt to integrate economic and political analysis, most of the profession saw ideological bias.

Second was a kind of intellectual myopia in the prevailing ontology of economics. 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 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? Most economists preferred to concentrate their attention on the private sphere, mainly addressing considerations of technical efficiency and economic welfare. They 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. First, the importance of institutions was discounted. In the "timeless" analytical framework favored by mainstream of the profession, political structures, if considered at all, were introduced only as a constraint on economic activity, with underlying power relationships taken more or less for granted. Conventional economics at the time 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. And second, attention was traditionally 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. Economists took that advice seriously.

Finally, there was resistance to IPE on epistemological grounds. 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? Mainstream economists were understandably hesitant to take up issues that could not be addressed comfortably using the standard toolkit of conventional economics. For a century, especially in the United States, the discipline had been growing increasingly abstract, relying ever more on deductive logic and parsimonious formal 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 economist Harry Johnson once put it (1971: 9).

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. John Kenneth Galbraith (1970: 8) derisively labeled the approach “imitative scientism” – a replication of the methods of the natural sciences “which is carried further in economics than in any other [social-science] discipline.” Highest rank in what Galbraith called the “prestige system of economics” goes to those who best mimic the reductionist epistemology of the physical sciences.

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 American IPE was first getting started: “If I can't quantify it, I'm not interested.” His remark was only partially in jest.

## DEBATES

While economists abdicated, political scientists debated. Students of IR may have agreed that IPE should be a sub-specialty of their own discipline. But they disagreed on virtually everything else, reflecting the broader dialogue that has gone on among IR theorists over the last several decades. The result is a multiplicity of intellectual traditions that can all claim a degree of legitimacy in the field as it is understood in the United States.

### *From realism to complex interdependence*

The starting point for American IPE was the classic state-centric paradigm of world politics that had long dominated the study of IR in the United States. For decades, U.S.-based scholars had been taught to think as “realists.” States were seen as the only significant actors on the global stage, conceived for analytical 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. academic circles, realism ruled.

And then came Keohane and Nye, two young scholars whose collaborative efforts, beginning at the end of the 1960s, laid the earliest foundations for the American version of IPE. 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? Disputes and tensions were on the rise; arguments over everything from tariffs and exchange-rate policy to management of the oceans were becoming more and more politicized. For Keohane and Nye, 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 interdependence of national economies. 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. 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. 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. Their effort to conceive a new vision of international relations began with an edited volume entitled *Transnational Relations and World Politics*, published in 1972, and culminated five years later in their landmark study, *Power and Interdependence* (1977). In *Power and Interdependence* they even gave their vision a name: “complex interdependence.”

Complex interdependence was defined by three main characteristics – multiple channels of communication, an absence of hierarchy among issues, and a diminished role for military force. Looking back later, Keohane and Nye (1987) expressed a degree of modesty over what they had wrought. Their purpose in developing their new vision, they claimed, simply had been to provide an alternative “ideal type” for analytical purposes. Their goal was not to discredit realism but, rather, to supplement it – to highlight dimensions of the picture that realism had overlooked. 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” (1987: 728, 730). The concept of complex interdependence itself, they insisted, was “underdeveloped” (1987: 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” (1987: 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 propositions 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 clearly 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 purposive states singlemindedly preoccupied with the “high politics” of war and peace. National governments were still at the center of analysis. But no longer could they be conceived as uniquely worthy of scholarly attention, nor could they be assumed to be preoccupied above all with the danger of military conflict. This was real value added. Keohane and Nye made scholars look at the world anew. In so doing, they facilitated the birth of a new field of study

Today American IPE takes for granted that interdependence in the world economy can be analyzed in political terms, not just as an economic phenomenon. Specialists also take for granted that patterns of interdependence can be examined by separate issue areas. Implicitly or explicitly, the U.S. version of the field is built on the ontology bequeathed by Keohane and Nye – a sense that the three characteristics of complex interdependence define the essential nature of the international system today. Without this inter-subjective understanding, systematic study of the “low politics” of trade and finance would have been more difficult, if not impossible. With it, researchers had a whole new insight into how things work. The term “complex interdependence” itself may no longer be particularly fashionable in the literature; many scholars have forgotten it completely. But the *weltanschauung* it represents is now undeniably a part of the collective unconscious of the field.

### ***Realism vs. liberalism***

Reinforcing the insights of Keohane and Nye was Katzenstein, a comparativist by training, who directly challenged the unitary part of realism's characterization of states. The realist paradigm has frequently been described as a kind of billiard-ball model of international relations, with states conceived as closed “black boxes” driven solely by abstract calculations of national interest and power. Keohane and Nye began the process of opening the black box by adding the growing crowd of transnational actors to the picture. Katzenstein pushed the process further by emphasizing the purely domestic sources of foreign economic policy. In a memorable article (1976) and subsequently in a pathbreaking edited volume entitled *Between Power and Plenty* (1978), Katzenstein demonstrated the direct salience of the political and institutional basis at home for policy preferences abroad. In effect, the notion of complex interdependence was further expanded to encompass even strictly domestic actors, whether inside or outside government, as well the institutional settings through which diverse interests were mediated and converted into policy. Yet more dimensions were highlighted that had been overlooked by realism.

None of this went uncontested, of course. Even as Keohane and Nye promoted their new vision of complex interdependence, others sought to defend the older realist tradition. Chief among them was Gilpin – “the dean of realist international political economy in the United States,” as one source puts it (Murphy 2000: 798). In a key contribution to *Transnational*

*Relations and World Politics* (Gilpin 1972), as well as in his subsequent classic, *U.S. Power and the Multinational Corporation* (1975), Gilpin explicitly confronted the issues raised by Keohane and Nye.

The emergence of transnationalism, he acknowledged, could not be denied. But that did not mean that realist theory had thus become obsolete. Quite the contrary, in fact. In insisting that a fundamental transformation was occurring in world politics, Gilpin argued in *Transnational Relations and World Politics*, 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 Gilpin, states were still the primary actors of interest and security concerns remained the crucial determinants of economic relations. In his words, “politics determines the framework of economic activity and channels it in directions which tend to serve ... political objectives” (1972: 54). Where Keohane and Nye went astray, he felt, was in failing to recognize the extent to which transnational actors and processes, ultimately, remain unavoidably dependent upon the pattern of interstate relations.

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. Realists, by contrast, 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”; one source, emphasizing the pair’s early Harvard connections, called it simply the “Harvard school of liberal international theory” (Long 1995). Gilpin, of course, was a barely reconstructed realist.

In *U.S. Power and the Multinational Corporation* (1975), Gilpin sought to respond to the new concept of transnationalism by carefully spelling out the strengths and weaknesses of each of the three approaches. His aim was to facilitate clearer and more consistent theorizing about the implications of interdependence. In so doing, he also provided 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, it has simply become an unexamined part of every specialist’s toolkit.

For the next two decades, the development of American IPE was dominated by the dueling traditions of realism (later neorealism) and neoliberal institutionalism. (The Marxist model, though influential elsewhere, has never made much headway among mainstream political scientists in the United States, for the same ideological reasons that deterred economists.) Perhaps most illustrative was the prolonged debate between realists and liberals over the issue of systemic governance, sparked by the growing threat posed by economic interdependence to the

political authority of sovereign states – the widening “control gap,” as Keohane and Nye (1972: *xxiii*) put it, between state aspirations and state capabilities. The essence of governance lies in the power to define and enforce norms for the allocation of values in a collectivity. If national governments were losing control, who then would make the rules for the global economy and how would compliance with those rules be assured?

For a realist like Gilpin, the answer lay in power politics – specifically, in a concentration of capabilities in a single state sufficient to bring stability to the system as a whole. In a Westphalian world, economic relations could be successfully managed only by one dominant power, a “hegemon,” such as the United States after World War II. Later labeled hegemonic stability theory (HST) by Keohane (1980), the idea was first broached by Gilpin in his contribution to *Transnational Relations and World Power* in 1972 and, more fully, in *U.S. Power and the Multinational Corporation* in 1975; it was also echoed in an influential article by Krasner in 1976 (Krasner 1976). By 1987, in Gilpin’s magisterial *The Political Economy of International Relations*, the theme had become grounds for an intense pessimism about the future of the global economy. America’s dominance in the post-World War II period may have been self-serving, Gilpin conceded, but it had also served the world well, suppressing protectionism and managing financial crisis. Now, however, the times were changing. By the 1980s, he averred, “American hegemonic leadership and the favorable political environment that it had provided for the liberal world economy had greatly eroded.... One must ask who or what would replace American leadership of the liberal economic order. Would it be... a collapse of the liberal world economy?” (1987: 345, 363). The outlook, he suggested, was exceedingly gloomy.

For liberals like Keohane and Nye, by contrast, there was more reason for optimism. America’s postwar dominance may have eroded, they conceded. But hegemony was not the only possible source of stabilizing governance. Alternatively, the system could be managed cooperatively through the creation of international “regimes,” embodying implicit or explicit understandings about the rules of the game in specific issue areas. The new notion of regimes could be understood as a particular form of international institution – hence the tongue-twisting sobriquet “neoliberal institutionalism.” *Power and Interdependence* (1977) was first to put regimes at the center of analysis. Regime theory was then given further impetus by a landmark collection of essays on *International Regimes* edited by Krasner (1983) and reached its apogee with Keohane’s monumental *After Hegemony*, published in 1984 (Keohane 1984). There is nothing in the logic of IR theory, Keohane declared in *After Hegemony*, that limits leadership in world politics to a single state. In principle, systemic stability could also be provided jointly through the framework of regimes, created and managed collectively. The issue was not the distribution of power but rather what conditions might best facilitate stabilizing leadership, whether by one state or several.

Effectively, the debate over HST was brought to a close by David Lake in a notable paper published in 1993 (Lake 1993). Reviewing two decades of dialogue, Lake offered a useful distinction between two different strands of HST. One was *leadership theory*, building on the logic of collective action and focused on the production of a framework for stability, redefined as the “international economic infrastructure.” The other was *hegemony theory*, which seeks to explain patterns of international economic openness by focusing on national policy preferences. Both pointed to the salience of the global economy’s political foundations -- to the critical role that politics can play in overall system governance. Power mattered not because it might be

concentrated, but rather because it is relevant to the management of economic affairs.

### *Rationalism vs. cognitive analysis*

By the 1990s, debate in IR had begun to move on. Today, Gilpin's emphasis on the role of power politics in IPE lives on in what might be termed the "new realism" of such scholars as Joanne Gowa (1994), Jonathan Kirshner (1995), and David Andrews (2006). But gradually the differences between liberalism and realism came to be seen as less crucial than their similarities – in particular, their effective convergence around what John Ruggie (1999: 215) called "neo-utilitarian precepts and premises." Both traditions shared a preference for a rationalist and materialist ontology. Actors, whether states or non-state entities, were assumed to act in pursuit of clearly defined interests, usually expressed in terms of material preferences and goals. Identities were well established and unchanging. Outcomes were the result of a careful balancing of costs and benefits of alternative paths of behavior.

The debate now was between neo-utilitarianism of any kind, on the one hand, and on the other hand so-called cognitive analysis, which focuses on the base of ideas and consensual knowledge that motivate actor behavior. As originally conceived, cognitive analysis was largely based in psychology and concerned strictly with the individual. What is in a person's mind? What is the independent influence of personal values and beliefs? Critical in bringing the insights of psychology to IR was Robert Jervis (1976), who emphasized the role that perceptions and misperceptions can play in shaping the mind sets of key decision makers. Today, the tradition is best represented in the work of Rose McDermott (2004). However, in more recent years, the cognitive approach has also spawned a second track under the label of constructivism, following the pioneering efforts of such scholars as Nicholas Onuf (1989), Alexander Wendt (1992), and Martha Finnemore (1996). The constructivist track is rather more sociological in nature, concerned more with connections between individuals – with learning, inter-subjectivity, and social knowledge. How did what is in a person's mind get there, and how do values and beliefs change over time? The spotlight of constructivism is on the independent effect of norms on behavior. "In the 1990s some of the major points of contestation [in IR] shifted," wrote Katzenstein, Keohane, and Krasner (1999: 43) in a definitive survey published at the end of the decade, stressing in particular the new prominence of the divide between rationalism and cognitive analysis.

Strikingly, to date the new debate has had only a limited impact on IPE in the United States. As Katzenstein, Keohane, and Krasner (1999: 9) conceded in their survey: "In the field of national security the discussion between rationalism (in its realist and liberal variants) and [cognitive analysis] has been more fully joined than in the field of IPE." First buds of a cognitive tradition are beginning to emerge in American IPE but are still far from a full flowering.

The reason is not difficult to fathom. Ideational factors sit uneasily in a hard science model. Like economists back when IPE was first born, U.S. specialists in the field today instinctively resist a theoretical approach that runs against the grain of accepted methodological standards. How can formal analysis account for the inherent uncertainties of the human mind? How can existing empirical methods cope with the vagueness of notions like socialization and social facts? Here too, mainstream scholars have preferred to ignore what cannot be objectively

observed or systematically tested.

Not that ideational factors were ever entirely excluded in American IPE. As early as 1979, John Odell was stressing the role of policy beliefs and subjective perceptions in the shaping of U.S. monetary behavior. “In analyses of policy change,” he argued, “beliefs should be elevated to an equal theoretical level” (1979: 80). Odell’s focus was on the individual policymaker, representative of the psychological track of cognitive analysis. His aim was to demonstrate the degree to which state behavior could be traced to the mind set of senior officials. As he expanded in a subsequent book, “behavior depends not on reality but on how reality is perceived and interpreted... Substantive ideas held by top policy makers and advisers [are] decisive or necessary elements of explanation” (1982: 58). Ideas were also at the center of Judith Goldstein’s pathmark study of U.S. trade policy (1993).

Contributions like these, however, were few and far between, failing to inspire much emulation. Even scarcer were efforts to take the sociological track, highlighting processes of social construction and the constitutive role of norms. The major exception was Ruggie, who long wandered in the desert preaching the cause of constructivism even before the movement had a name. Most memorable was a well known chapter he contributed to Krasner’s *International Regimes* volume (1983), addressing the origins of the postwar economic order. Central to his analysis was the notion of a normative framework, understood as an “intersubjective framework of meaning” (1983: 196). Regimes, he argued, are social institutions. “We know international regimes not simply by some descriptive inventory of their concrete elements, but by their generative grammar, the underlying principles of order and meaning that shape the manner of their formation and transformation” (1983: 196).

Only in the most recent years have studies of ideational factors begun to blossom in the American version of the field. Leading the way on the psychological track, once again, is Odell. In a thoughtful exploration of international economic negotiations (2000), Odell built on the insights of his earlier work to highlight the role of negotiators’ beliefs as an independent influence on bargaining strategies and outcomes. The critical issue, he suggested, is what Nobel-prize winning economist Herbert Simon called “bounded rationality” – a variant of “rational choice that takes into account the cognitive limitations of the decision maker, limitations of both knowledge and computational capacity” (Simon 1997: 291). Given the constraints of bounded rationality, negotiators typically make use of cognitive shortcuts, convenient heuristics that rely heavily on subjective beliefs to guide their behavior. To understand economic diplomacy, therefore, it is vital to understand the ideas of the diplomats. In Odell’s words (2000: 3): “If the real world is one of bounded rationality, identifying such key beliefs and their effects become a productive way to advance knowledge about, and the practice of, economic bargaining.” Along similar lines, Mark Blyth (2002) has clearly demonstrated the critical role that ideas played in shaping institutional change in the world economy over the course of the twentieth century.

Systematic studies have also begun to appear on the sociological track, albeit still at a modest pace – mostly, as it happens, on monetary or financial matters where issues of reputation and perception are obviously critical. As Jacqueline Best (2004: 404) has observed: “Financial stability depends on confidence, and confidence is a matter of faith – not simply in the efficiency of institutions, but also in the robustness and legitimacy of the norms that inform them.” One example comes from Kathleen McNamara (1998), who has carefully explored the “currency of ideas” as a driving force in the process of monetary integration in Europe. The key to

understanding the evolution of European monetary politics in the 1970s and 1980s, McNamara suggests, lay in a new “neoliberal” policy consensus that took hold among policy elites, redefining state interests. A second example comes from Jeffrey Chwieroth (2007) in a constructivist study of financial liberalization in emerging market economies in the 1980s and 1990s. Capital markets were opened, Chwieroth contends, because of a new set of neoliberal norms that were diffused through a network of knowledge-based experts once they came to positions of political authority. Other constructivist research has highlighted the social sources of financial power and monetary leadership in the global community (Barkin 2003; Seabrooke 2006).

How successful all these efforts at cognitive analysis will turn out to be remains an open question. But it is clear that the field is now increasingly open to ideational factors in addition to the more traditional neo-utilitarian approaches. American IPE may still privilege the state as the basic unit of analysis. But the field has definitely come a long way from the narrow state-centric paradigm of early realism.

### RECAPTURE

One counter-trend should be noted, however. Even as American IPE has produced a multiplicity of traditions, a striking degree of standardization has gradually taken hold at the level of methodology. Political scientists may have dominated the field from the start, setting the agenda for research. But in terms of research technique, the economics profession has been making a steady comeback. Increasingly, the style of scholarship in much of American IPE has come to resemble the reductionist approach favored by economists, featuring the same penchant for positivist analysis, formal modeling, and, where possible, systematic collection and evaluation of empirical data. More and more, what gets published in the United States – particularly in such prestigious journals as *International Organization* or the *American Political Science Review* – features the same sorts of mathematical and statistical techniques that we have come to expect in economics journals. The trend is not universal, of course. Much valuable work continues to appear that is institutional, historical, or interpretive in tone. But it would not be inaccurate to say that in epistemological terms, a good part of the field today has been recaptured by economists.

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 the 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” (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 (1999: 23) bow their heads, describing economics as “the reigning king of the social sciences.” 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 American IPE seeks to mimic the economist’s demanding hard-science model, it may be simply to demonstrate that the field, for all the uncertainties of the political process, is no less capable of formal rigor. Specialists in IPE want respect, too.

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.

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