

- GRIGORESCU, ALEXANDRU. (2008) Horizontal Accountability in Intergovernmental Organizations. *Ethics & International Affairs* 22 (3): 285–308.
- HAWKINS, DARREN G., DAVID A. LAKE, DANIEL L. NIELSON, AND MICHAEL J. TIERNEY. (2006) Delegation Under Anarchy: States, International Organizations, and Principal-Agent Theory. In *Delegation and Agency in International Organizations*, edited by Darren G. Hawkins, David A. Lake, Daniel Nielson and Michael Tierney. Cambridge, UK: Cambridge University Press.
- HOUSE OF LORDS. (2004) Strengthening OLAF, the European Anti-Fraud Office. House of Lords Paper 139, July 21. London: The Stationary Office Limited.
- KEOHANE, ROBERT O., AND JOSEPH S. NYE. (2002) The Club Model of Multilateral Cooperation and Problems of Democratic Legitimacy. In *Power and Governance in a Partially Globalized World*, edited by Robert O. Keohane. New York: Routledge.
- LIGHT, PAUL. (1993) *Monitoring Government: Inspectors General and the Search for Accountability*. Washington, DC: Brookings.
- MAJONE, GIANDOMENICO. (1998) Europe's Democratic Deficit. *European Law Journal* 4 (1): 5–28.
- MARCH, JAMES, AND JOHAN OLSEN. (1998) The Institutional Dynamics of International Political Order. *International Organization* 52 (4): 943–970.
- MARTIN, LISA. (2006) Distribution, Information, and Delegation to International Organizations: The Case of IMF Conditionality. In *Delegation and Agency in International Organizations*, edited by Darren G. Hawkins, David A. Lake, Daniel L. Nielson and Michael J. Tierney. Cambridge, UK: Cambridge University Press.
- MULGAN, RICHARD. (2000) "Accountability": An Ever-expanding Concept? *Public Administration* 78 (3): 555–573.
- NIELSON, DANIEL L., AND MICHAEL J. TIERNEY. (2003) Delegation to International Organizations: Agency Theory and World Bank Environmental Reform. *International Organization* 57 (2): 241–276.
- NYE, JOSEPH. (2001) Globalization's Democratic Deficit: How to Make International Institutions More Accountable. *Foreign Affairs* 80: 2–6.
- O'DONNELL, GUILLERMO. (1999) Horizontal Accountability in New Democracies. In *The Self-Restraining State: Power and Accountability in New Democracies*, edited by Andreas Schedler, Larry Diamond and Marc F. Plattner. Boulder: Lynne Rienner.
- O'LOUGHLIN, JOHN, MICHAEL D. WARD, COREY L. LOFDAHL, JORDIN S. COHEN, DAVID S. BROWN, DAVID REILLY, KRISTIAN S. GLEDITSCH, AND MICHAEL SHIN. (1998) The Diffusion of Democracy, 1946–1994. *Annals of the Association of American Geographers* 88 (4): 545–574.
- PAYNE, RODGER A., AND NAYEF SAMHAT. (2004) *Democratizing Global Politics: Discourse Norms, International Regimes, and Political Community*. Albany: State University of New York Press.
- PRZEWORSKI, ADAM, SUSAN STOKES, AND BERNARD MANIN, Eds. (1999) *Democracy, Accountability, and Representation*. Cambridge, UK: Cambridge University Press.
- SHANKS, CHERYL, HAROLD K. JACOBSON, AND JEFFREY H. KAPLAN. (1996) Inertia and Change in the Constellation of International Governmental Organizations, 1981–1992. *International Organization* 50 (4): 593–627.
- SIX, PERRI AND MICHAEL SHERIDAN. (1995) A World Order of Scandal and Graft. *The Independent*, May 11:15.
- STARR, HARVEY. (1991) Democratic Dominoes: Diffusion Approaches to the Spread of Democracy in the International System. *Journal of Conflict Resolution* 35 (2): 356–381.
- STARR, HARVEY, AND CHRISTINA LINDBORG. (2003) Democratic Dominoes Revisited: The Hazards of Governmental Transitions, 1974–1996. *Journal of Conflict Resolution* 47 (4): 490–519.
- UN JOINT INSPECTION UNIT. (2006) *Oversight Lacunae in the United Nations System*. New York: United Nations.
- UNION OF INTERNATIONAL ASSOCIATIONS. (1991 to 2008) *Yearbook of International Organizations*. Brussels: Union of International Association.
- VAUBEL, ROLAND. (2006) Principal-agent problems in international organizations. *The Review of International Organizations* 1 (2): 125–138.
- VAUBEL, ROLAND, AXEL DREHER, AND UĞURLU SOYLU. (2007) Staff Growth in International Organizations: A Principal-agent Problem? An empirical analysis. *Public Choice* 133 (3): 275–295.
- VOLCKER, PAUL, GUSTAVO GAVIRIA, JOHN GITHONGO, BEN W. HEINEMAN JR, WALTER VAN GERVEN, AND JOHN VEREKER. (2007) *Independent Panel Review of the World Bank Group's Department of Institutional Integrity*. Washington, DC: World Bank.
- WEJNERT, BARBARA. (2005) Diffusion, Development, and Democracy, 1800–1999. *American Sociological Review* 70 (1): 53–81.
- WOODS, NGAIRE. (2003) Holding Intergovernmental Institutions to Account. *Ethics and International Affairs* 17 (1): 69–80.

## Are IPE Journals Becoming Boring?

BENJAMIN J. COHEN

University of California at Santa Barbara

Are IPE journals becoming boring? The question is a serious one. Over the four decades or so since the modern field of International Political Economy was born, the character of what gets published in leading journals in the United States—IPE standard setters like *International Organization*, *International Studies Quarterly*, and *World Politics*—has changed dramatically. Arguably, the change has not been for the better.

To illustrate, consider a simple thought experiment. Think first of some of the memorable work published in the early years of the field—work like Keohane and Nye's special *International Organization* issue on "Transnational Relations and World Politics," published as a book in 1972; Peter Katzenstein's 1976 *IO* essay on "International Relations and Domestic Structures," which in turn led to his special issue on "Between Power and Plenty," also published as a book in 1978; or Stephen Krasner's special issue on "International Regimes," published in book form in 1983. Or think of Krasner's 1976 *World Politics* study of "State Power and the Structure of International Trade"; Peter Gourevitch's 1978 *IO* article on the second image reversed; John Ruggie's 1982 *IO* essay on embedded liberalism; or Jeff Frieden's 1991 *IO* paper on invested interests. All were seminal, foundational works—influential scholarship that is still widely read and cited.

Now compare these with anything that has appeared in mainstream journals over the last 5–10 years. A great deal of quality research has been published, much of it making use of the most rigorous and up-to-date statistical methodologies. The intellectual candlepower is impressive. But how well does this work stack up against the output of earlier years? How much can be regarded as truly path-breaking? How much is likely to be read or cited 5–10 years from now? The answers, I think, are obvious. Our major journals are full of articles that are thoroughly peer-reviewed and edited with care. With rare exceptions, research meets the highest standards of scholarship. It's just not very interesting.

### The Problem

In my judgment, this is a grave problem. As I wrote in my recent book, *International Political Economy: An Intellectual History* (Cohen 2008), somewhere along the way the field of IPE in the United States—what I call the American school—took an unfortunate turn. Increasingly, priority has come to be given to formal scientific method, a hard science model resembling nothing so much as the epistemology of neoclassical economics with its well-known penchant for formal modeling and higher mathematics. 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. Pride of place

is given to work that embodies the latest and most sophisticated quantitative or qualitative techniques. The trend is clearly confirmed in the latest TRIP survey of the international relations field (Jordan, Maliniak, Oakes, Peterson, and Tierney 2009; Maliniak and Tierney 2009), which carefully surveyed the contents of 12 leading journals since 1980.

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 appear to have an inferiority complex when it comes to economics—what I have elsewhere described as a case of peer-us envy (Cohen 2009). The parsimonious reductionism of mainstream economics has come to set the standard for what passes for professionalism in our field. If today the most highly rated work in the American school tends to mimic the economist's demanding hard-science model, it seems in large part to demonstrate that the field, for all the ambiguities of the political process, is no less capable of theoretical elegance and formal rigor. IPE scholars want respect, too. A kind of "creeping economism" has come to define what constitutes the legitimate study of our subject.

Not everyone agrees that this is a problem. For many, the trend represents progress—all part of the "maturing" of the field, as David Lake (2006) puts it. The more IPE scholars agree on a common epistemology, the more their work approaches the respectability of "normal" science. In Lake's (2009:49) words, "cacophony" yields to "Kuhnian normalcy." But at what cost? To my mind, such a happy assessment is altogether too kind, since it ignores all that is lost as a result. The price of this kind of "progress" is measured by how much now gets left out of what we have available to read.

In effect, the creep of economism has tended to shrink the horizons of scholarship. To a significant extent, this is because of 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 numbers. In effect, the approach plays a key role in defining *what* can be studied, automatically marginalizing broader questions that cannot be reduced to a manageable set of regressions or structured case-study analysis.

The consequence, as I wrote in my *Intellectual History*, has been a distinct loss of ambition in American IPE. Out are the kind of big ideas and intellectual challenges that characterized the field in its earlier years. Instead, scholars are incentivized to focus on mid-level theory. In contrast to macrotheory (or metatheory), mid-level theory eschews interpretive theory or grand visions of history and society. Rather, work tends to concentrate on narrow individual 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.) Such work is by no mean unimportant; much of it yields useful new insights. But like a steady diet of gruel, it leaves us hungry for more—more variety, more exotic ingredients, more *spice*.

### Solutions

Can the menu be spiced up? Journal editors often take the view that they are prisoners of the submission process. They too may hunger for more variety, but what can they do? They can only select from among the manuscripts that are submitted. If all that comes in is economic mid-level theory, then that is what they will publish.

To me that attitude is, to say the least, defeatist. We know that the editor's job is an unenviable one, demanding long hours and many difficult, even painful, decisions. Reviewers must be found, verdicts rendered, manuscripts edited, schedules managed. We all owe the editors of this and other journals a debt of gratitude for the profound service they render on our behalf. But that does not mean that they must therefore remain passive in the face of the field's disappointing loss of ambition. A more pro-active posture ought to be possible.

The goal is simple. Journals should aim to encourage more work that goes beyond the narrow straitjacket of a hard science model—work that dares to take on broader questions, even if that means some loss of parsimony or mathematical elegance; work that boldly poses new theory or paradigms, even if the necessary evidence may not yet be available; work, in short, that does not fear to be *interesting*.

To illustrate, consider some recent reflections from Robert Keohane (2009), who shares the discontent of many with the shrinking horizons of American IPE. A pioneer of the field, Keohane professes himself to be "disheartened" by the "new IPE" that has taken over in our journals. Much is missing, he avers—in particular, "the synthetic interpretation of change" (2009:40). More attention needs to be paid to major transformations that are going on in the world political economy, such as the emergence of China, volatility in financial and energy markets, the increasing role of global civil society, and the explosive growth of electronic technologies. In Keohane's words, we need to "let the wings of imagination spread" (2009:43).

What can journal editors do to let the wings of imagination spread? Many innovations are possible. I would stress three strategies that I think could have an impact in the relatively short term.

### Review Essays

First, I would hope that editors could be more pro-active in soliciting review essays or surveys of selected issue areas like those identified by Keohane. Today only a few journals express a willingness even to consider review essays. Fewer still actually go out and look for them. This is regrettable, since one of the consequences of American IPE's preference for mid-level theory is an increasing fragmentation of journal contents. We get isolated bits and pieces—a statistically significant regression here, a thoroughly documented case study there—but little sense of how all the pieces fit together. We would all benefit from more overarching analyses that try to tell us something about the forest, not just individual trees. Review essays provide an opportunity to survey important developments in a particular area of study, synthesize new ideas, and raise key issues for future scholarship. They give us a sense of where we are and—more importantly—where we might or should be going.

The counter-argument is familiar. Persuading scholars to undertake a review essay or survey is difficult, editors tell us. Academics are busy with teaching and their own research agendas; they just do not have the time. Worse, few see much professional payoff from such an undertaking, as compared with a solid exercise in scientific method. Review essays do not help much in gaining tenure or promotion. In the hierarchy of status among publications, they barely rank above book reviews or an op ed in the local newspaper.

But it does not have to be that way. Much depends on the standards by which such work is reviewed and edited, as well as on how prominently it is positioned in a journal's table of contents. In my own time, I have written my share of surveys and have never regretted doing so. One of my pieces has been reprinted repeatedly in edited anthologies and is still cited in the literature. I do not really think that my career was set back by the time I invested in such endeavors.

The challenge is to use the marquee power of our journals to give review essays the same cachet as standard research papers. If they put their mind to it, editors really could do much to make surveys seem a smart career move for ambitious scholars.

#### Symposia

Second, I believe that we would all benefit from more organized symposia on selected themes. Many journals already do try occasionally to provide something along this line, say by clustering together two or three articles that appear to be related in some way. But here too it seems that editors could be more pro-active. Why wait until a few papers come along that can be conveniently grouped under a single heading in the table of contents? Why not go out and do what the editor of *International Studies Quarterly* did in the case of this symposium: actively recruit contributions from an array of interested parties? Here is an opportunity for editors to really make a difference. Because of all the research they are exposed to in the submission process, editors are in a unique position to identify cutting-edge questions that we ought to be thinking about—in effect, to steer the development of the field. In my opinion, this sort of initiative should be made an integral part of their job description: a responsibility to spotlight critical problems, seek out potential authors, and encourage direct, stimulating engagement. This should be done on a regular, indeed routine, basis. The aim should be to promote constructive argument and debate—the more controversial, the better.

Again, I acknowledge that not all would agree. Why stir up controversy needlessly?, some might ask. Why risk fights that could just end up polarizing the field? Why not just let sleeping dogs lie? But as scholars, can we really afford the luxury of avoiding debate just because it might ruffle a few feathers? Contrary to those who yearn for the peacefulness of “Kuhnian normalcy,” I believe we regularly need to rouse the sleeping dogs, to acknowledge and energetically explore underlying cleavages in our field. As Peter Katzenstein (2009:130–131) has reminded us, “basic division is what constitutes the social sciences ... We cannot help but live our disagreements in cacophonous debates.” In his words, debates “remind us of the foundations of the normal work we do in our research and teaching” (2009:123). Argument is essential if we are to avoid intellectual complacency.

#### Submission Policy

Finally, I think it is time for editors to fundamentally rethink existing submission policies. Typically, the instructions posted for contributors are straightforward. What the journals want are research papers, pure and simple. Appealing qualifiers may be added—words like “theoretical,” “analytical,” “empirical,” “original,” “innovative,” even “integrative,” or “interdisciplinary.” But the basic message is clear. Preference will be given to submissions that meet the test of systematic and rigorous inquiry, backed to the extent possible by the standard methodologies of social science. Work of a more venturesome nature need not apply.

How might this change? Certainly no one wants to discourage the serious research tradition. That would mean abandoning the core mission of our journals. But must pages be reserved *exclusively* to standard research papers, at the expense of everything else? Why not reserve a portion of each issue to work of a different character, under some heading like Opinion or Commentary or New Thoughts? Journals in other disciplines do it. There seems no reason why we in IPE could not do so as well. Instructions to contributors could be easily amended

to make clear that space will be regularly reserved for more unorthodox submissions—Big Think pieces that really do let the wings of imagination spread.

Of course, there would be a price. Journals have page limits. The more space is set aside for the unconventional, the less room is left for the traditional. Fewer straight research papers would find a home. Many would regard that as an unacceptable cost. But surely it is an empirical question—in the language of my original discipline of economics, a matter of marginal cost and benefit. In my judgment, the value added would far exceed the value lost, but I concede that this is an issue on which sincere people can sincerely disagree.

In the end, the question is simple: If indeed the menu needs spicing up, what is the best way to do it? Nothing less than the future of IPE in the United States is at stake.

#### References

- COHEN, BENJAMIN J. (2008) *International Political Economy: An Intellectual History*. Princeton: Princeton University Press.
- COHEN, BENJAMIN J. (2009) A Grave Case of Myopia. *International Interactions* 35(4): 436–444.
- JORDAN, RICHARD, DANIEL MALINIAK, AMY OAKES, SUSAN PETERSON, AND MICHAEL J. TIERNEY. (2009) *One Discipline or Many? TRIP Survey of International Relations Faculty in Ten Countries*. Williamsburg: College of William and Mary.
- KATZENSTEIN, PETER J. (2009) Mid-Atlantic: Sitting on the Knife's Sharp Edge. *Review of International Political Economy* 16(1): 122–135.
- KEOHANE, ROBERT O. (2009) The Old IPE and the New. *Review of International Political Economy* 16(1): 34–46.
- LAKE, DAVID A. (2006) International Political Economy: A Maturing Interdiscipline. In *The Oxford Handbook of Political Economy*, edited by Barry R. Weingast and Donald A. Wittman. New York: Oxford University Press.
- LAKE, DAVID A. (2009) TRIPS Across the Atlantic: Theory and Epistemology in IPE. *Review of International Political Economy* 16(1): 47–57.
- MALINIAK, DANIEL, AND MICHAEL J. TIERNEY. (2009) The American School of IPE. *Review of International Political Economy* 16(1): 6–33.