

# Adding Spice to Our Scholarly Journals: The JIBS Experience

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A scientific field of inquiry is a socially constructed entity consisting of a community of scholars who share a common identity and language (Kuhn 1962). The boundaries may be more or less fuzzy, but scholars working with that field have a consensual understanding of its essential meaning. Scholarly journals are designed to introduce new research and critique existing research within a field of inquiry. As such, scholarly journals are the “eyes” through which we understand the past and anticipate the future within that field.

The editor of a scholarly journal can affect the direction and shape of a given field, and the careers of individuals working within it. Journal editors not only see the newest literature in that field, well before it reaches print form, but also have the opportunity along with reviewers to shape that literature. Editors are “guardians at the gate” (McGinty 1998) because they decide which papers are published (and, mostly, which are not) in their journals. Through their journals, editors disseminate research results and create networks of scholars working within a particular field of inquiry.

I am sympathetic with Cohen’s worry that scholarly journals in the field of inquiry of international political economy (IPE) may become boring, and that the truly path-breaking research was done in the past and not today. This concern is not a new one, however, and I suspect it may be shared by many leading scholars. In the *Journal of International Business Studies* (JIBS), for example, there have been at least three articles on this topic. The field of international business (IB) studies is seen as “running out of steam after a period of vibrancy” (Buckley 2002:365). Several view the root cause of this malaise as the lack of a “new big idea” to excite research (Buckley 2002; Peng 2004) or the lack of mid-level theory to explain issue-driven phenomena (Buckley and Lessard 2005). This suggests to me that Cohen’s concerns about boring journals may be more widespread than IPE.

In this Commentary, I address—and discard—two possible reasons for more boring journal articles: lack of research opportunities and economics envy. I then evaluate Cohen’s three ideas for spicing the IPE journals, using our experience at the *Journal of International Business Studies* (JIBS). Lastly, I argue that the key issue for scholarly journals is encouraging authors to build better theories.

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### Lack of Research Opportunities?

There is no reason for IPE journals to be boring, as we really do live in interesting times (the Chinese proverbial curse); there are lots of research opportunities. The world in which journal editors function has changed markedly due to globalization, the rise of the Internet, and the spread of information technologies. One of the major events of the 1990s, perhaps the most important, was the rapid rise of emerging and transition economies. Universities in Eastern Europe, the former Soviet Union, Asia, and Latin America are bringing many new voices “to the academic table.” Journal submissions from outside North America and Western Europe are growing, and these new voices may be less likely to follow the traditional Western models, since their institutions and cultures are often so different. Moreover, one of the clear impacts of this greater geographic diversity of scholars is that the nature of the topics that attract IPE scholars is also changing. The rise of emerging and transition economies has led to new interest in topics such as weak and missing institutions, privatization and state-owned enterprises, and cross-country comparative studies of the varieties of capitalism.

Not only have there been booms, but the global economy has also been troubled by rapid busts, ranging from banks that were “too big to fail” to international currency crises to collapses of whole economies such as Iceland and now possibly Greece. The most recent round of world trade talks ended in stalemate. Civil and interstate wars continue to wreck havoc and extend the cycle of poverty in many countries; others are on the brink of acquiring nuclear weapons. The huge oil spill by British Petroleum in 2010 is likely to permanently change not only the US coastline but also the political economy of the global energy market. The roller coaster trajectory of the global economy since the late 1980s therefore offers a rich menu of experiences from which IPE scholars should be able to develop and test new theories.

### Economics Envy?

Cohen argues that a possible cause of the IPE journals becoming boring may be that they now mimic the “economist’s demanding hard-science model,” publishing articles that exemplify “the parsimonious reductionism of mainstream economics.” He sees the problem as economics envy, causing the IPE journals to shift away from “big question” essays to data-driven research using mid-level theories. In another paper (Cohen 2010), he hypothesizes that the gap in research traditions between North American and European scholars may be partly to blame.

This critique, of course, is not new. The relative rigor of quantitative versus qualitative methods has a long and controversial history in the social sciences, manifest, for example, in the Perestroika movement in political science, which led to the founding of the journal *Perspectives on Politics*. Bennett, Barth, and Rutherford (2003) examined changes in the relative proportion of formal modeling, statistical methods, and case-based research in 2,200 articles published in 10 top political science journals, including *International Organization*, *International Studies Quarterly*, and *World Politics* (the three IPE journals Cohen mentions), between 1975 and 2000. The authors found that, over the whole period, 49% used statistics, 46% case studies, and 23% formal modeling, and that the proportions had remained fairly stable since 1975. The percentage of articles based on case studies, however, was in “steep decline in most of the top journals”, falling to 1% in 1999–2000, although the percentage remained at more than 40% of articles on international relations (Bennett et al. 2003:375).

The shift to quantitative is evidenced in our journal articles, which now almost all follow a “cookbook” format consisting of Introduction, Literature Review, Theory Development, Empirical Work, Discussion, and Conclusions. Here, JIBS is typical of other journals; a rapid scan through recent issues shows that most articles follow this format. Similarity in format, of course, makes analysis of the articles easier for authors and reviewers, reducing the transaction costs of evaluation. However, articles do not have to be written this way. Sand-Jensen (2007:723), for example, argues that rigidity of journal style encourages “consistently boring scientific literature,” turning gifted writers into dull scientists. Providing a top 10 list of causes for boring scholarly papers, he advocates for alternative writing styles and variable outlets. Even within the strictures of the scientific writing style, our articles can be less boring when authors recognize that writing is hard, and focus on ways to improve it. Both Sand-Jensen (2007) and Stimson (2010), for example, provide helpful advice to authors on improving the quality of written journal articles.

### Cohen’s Spicy Ideas: Lessons from JIBS

Cohen’s solution is that journal editors should “encourage more work that goes beyond the narrow straitjacket of a hard science model—work that dares to take on broader questions ... that boldly proposes new theory or paradigms ... work, in short, that does not fear to be *interesting*.”

I suspect that all of the editors providing comments here will say their goal is to publish journal articles that are interesting. The JIBS Statement of Editorial Policy, to speak to my own journal, says: “The goal of JIBS is to publish insightful and influential research on international business ... JIBS seeks to publish manuscripts with cutting-edge research that breaks new ground, rather than merely making an incremental contribution to international business studies.” The type of article we look for at JIBS is the “big idea” research Cohen wants to encourage. Still, of the 600 or so manuscripts JIBS receives each year, most are (not surprisingly) incremental, that is, carefully done empirical studies building on existing theories and adding marginally to our knowledge base. The articles almost all fall within the positivist tradition, using econometric techniques to test hypotheses developed within a well-known theory or paradigm.

In terms of specific advice to make our IPE journals less boring, Cohen offers three ideas for journal editors: (i) solicit review essays, (ii) organize symposia on particular themes, and (iii) add sections to their journals that encourage “more unorthodox submissions—Big Think pieces” such as Opinions or Commentaries. The idea is to “spice up” the journals by not simply taking what comes across the transom, but by actively encouraging particular types of more risky articles.

We have tested all three ideas at JIBS; most were introduced by my predecessor, Arie Lewin, after he became JIBS Editor-in-Chief in 2002. I outline some of the changes below. I believe they have been reasonably—but not unambiguously—successful. The JIBS Web of Science journal impact factors continue to rise; in 2009, the two-year score is 3.766 and five-year score is 5.727, still well below the top management journals, *Academy of Management Journal (AMJ)* and *Academy of Management Review (AMR)*. The average article published in JIBS is less incremental, has stronger theory, and is more willing to be controversial than 10 years ago. However, the proof of the additional spice will be in future “puddings”—now is perhaps too soon to tell.<sup>1</sup>

<sup>1</sup> To be clear, almost 100% of articles in JIBS go through the full double-blind review process, with about 10% being published (Eden 2009); the “spicing up” has been done within the regular review process.

*More Unorthodox Submissions/Solicit Review Essays*

In 2002, Lewin introduced a new type of article, Perspectives, which was designed to provide different perspectives, deliberately controversial or challenging to mainstream views. The Perspectives article looks to be exactly what Cohen suggests in terms of more unorthodox submissions. The category, which has its own section in JIBS, includes pieces ranging from review essays and debates to evaluation of data sources and research methods.

However, few manuscripts are submitted as Perspectives, most are by senior scholars, and perhaps maybe three or four are published a year. Reviewers clearly have more difficulty evaluating these pieces and they are harder for editors also. Despite their attractiveness (Perspectives have been among the most highly read and downloaded articles), JIBS has found it difficult to induce scholars, particularly young scholars, to invest the time in writing these more controversial pieces.<sup>2</sup> It clearly is easier to follow the standard “cookbook” approach of developing and empirically testing hypotheses within an existing theory than to go out on a limb with a controversial essay that reviewers find hard to evaluate.

A Point-Counterpoint section where scholars write on opposite sides of a debate is a second type of less orthodox publication we have tried at JIBS; these may go through either double-blind or single-blind review. For example, in JIBS 41.8, forthcoming this fall, Geert Hofstede and the GLOBE group will square off in a Point-Counterpoint, debating which group has “better” measures for cultural characteristics. Still, these are rare, and require more than normal editorial oversight if authors are to link their pieces coherently.

*Organize Symposia on Particular Themes*

Another innovation by Lewin in 2003 was the JIBS Frontiers Conference, an annual mini-conference on a “big question” with competitive paper submissions. The Academy of International Business, which owns the journal, cosponsors the conference together with a host institution. Some of the workshop papers make their way into JIBS, going through the regular double-blind review process. Some Frontiers Conferences have led directly to Special Issues of the journal, such as the JIBS 41.6 Special Issue on “Conflict, Security and Political Risk: International Business in Challenging Times”. However, most journal editors, I suspect, would see running an annual conference to be service “above and beyond” what they are willing to commit, given the enormous amount of day-to-day time already involved in running a scholarly journal. Most editors already participate in Meet the Journal Editors panels, speak at doctoral and junior faculty consortia, and run Paper Development Workshops; running an annual conference is therefore not likely to be high on their list despite the potential payoffs in terms of less conventional submissions to their journal.

Special Issues are another way to introduce spice into our journals through what are, in effect, “virtual” organized symposia. One study found that, for all but the very top journals, special issues increase journal citation rates (Conlon, Morgeson, McNamara, Wiseman, and Skilton 2006). Through Special Issues, editors can privilege certain areas of research and help address Cohen’s concerns about our journals becoming boring. For example, JIBS has an in-progress Special Issue on Qualitative Research in International Business, specifically to flag to the IB scholarly community that qualitative research can be published in JIBS. The in-progress Special Issue on Global Crises and International Business is a direct attempt to encourage IB scholarship on the current international financial crisis.

<sup>2</sup> The acceptance rate for Perspectives is probably higher than for regular submissions, but the numbers are too small for statistical accuracy. JIBS currently publishes nine issues and about 80 articles each year.

We have changed the process at JIBS for Special Issues also. In the past, Special Issues were in effect “given away” to guest editors to wholly manage, separate from the regular editorial team. We now treat Special Issues as a method by which JIBS can bring on board guest editors to work with the JIBS editorial team on topics where the guest editors have a deep knowledge base. We have, from time to time, developed a Special Issue theme and then actively sought particular scholars as guest editors; for example, Edward Mansfield joined us for the Special Issue on Conflict, Security, and Political Risk.

### **The Real Problem: Building Better Theories**

In sum, JIBS has tried, with reasonable success, all three of the ideas Cohen recommends for spicing up the IPE journals. I believe, however, that the problem is deeper and the solution needs to be more radical than what he recommends: we need to build a stronger focus on theory development into our journals. It is not the lack of review essays or symposia that make journals boring, but rather the lack of good stories built around strong theories. Our research needs to be not only insightful, but also impactful.

Many scholars will be familiar with pieces by Sutton and Staw (1995) and Weick (1989, 1995, 1999) on what theory is and is not. These readings are often assigned in doctoral seminars and consortia, and referenced in journal editorials on what constitutes good theory. Their key point is that good, strong theory development is tough work. In a world where the number of published articles is a key metric for tenure and promotion in our universities, many scholars with the intellectual capability to write insightful and impactful journal articles may find smaller, less innovative pieces an easier road to academic success.

Knowing what is and is not theory is also hard. Journal editors can help here by providing advice to authors and reviewers as to what they see as “good theory” and “big ideas.” The lead here probably belongs to the editors of AMJ and AMR, who have attempted to do just that through several years of editorials. Although their editorials are written for scholars in the management field of inquiry, they are useful reading for all social scientists interested in developing better theories. I briefly mention a few favorites here. Whetten (1989) addresses the building blocks of theory development and ways to assess theory contribution. Elsbach, Sutton, and Whetten (1999) introduce an AMR Special Issue on theory development, organized around three topics: metatheories, theories of time and process, and thick theory. Kilduff (2006) provides advice on publishing theory, recommending that authors offer big ideas, give them structure, and critique and revise before submission. Barley (2006:16) explores what makes a paper interesting, arguing that “difference is the root of all interest.” Bartunek et al. (2006) survey the AMJ Editorial Board for nominations for the most interesting articles published in AMJ and why; they find that quality, counterintuitiveness, and impact matter. Advice for junior scholars is provided in Rindova (2008).

At JIBS, my editors and I have been following in the footsteps of the AMR and AMJ editors, writing editorials relevant to international business as a field of inquiry. Topics have ranged from journal ethics to single-country studies to interdisciplinary research. These editorials, freely downloadable from the JIBS website, are designed to be less technical, broader, “big picture” pieces that we hope are useful to the wider IB scholarly community. The JIBS editors also run Paper Development Workshops at the annual AIB meetings (another Lewin innovation), where junior scholars and scholars from emerging and transition economies spend the day with JIBS editors critiquing their manuscripts and learning good publication strategies.

Anniversaries also provide an opportunity for journal editors to make a difference. Last year was JIBS's 40th anniversary. We celebrated with a Call for Papers for a Special Issue on "Innovations in International Business Theory," specifically picking the topic to encourage pure theory papers with big ideas. The issue (JIBS 41.9) is packed with interesting and insightful articles; however it will be years before we know whether they have the impact that Cohen seeks. Interestingly, while most of the anniversary articles were written by senior scholars, a couple were by young scholars early in their professional careers.

Journals can also give awards for "big idea" papers, as a way to encourage scholarly research that is less incremental, and highlight "big idea" papers. For example, JIBS gives a Decade Award for the most significant article published 10 years prior, as assessed by citations and a blue-ribbon panel. The authors are invited to write a Retrospective and other scholars write Commentaries; these are formally presented at the annual AIB meetings and later published in JIBS after single-blind review, along with the original article.

The key point is that scholars need to tell better stories, they can learn through practice how to build better theories, and they need to be encouraged to take the time and spend the intellectual effort do so. Perhaps the reason why the earlier IPE research cited by Cohen, such as Krasner's "International Regimes" Special Issue of *International Organization*, continues to resonate so well today is because the stories were so well told and controversy was deliberately included (for example, Strange's (1982) "Cave! Hic Dragones" critique of regime theory).

### Conclusions

Research in international political economy is diverse. IPE scholars tackle their research questions from a wide variety of ontological, epistemological, and methodological perspectives. Our IPE journals—the core places where IPE research appears—also reflect this diversity. Some journals encourage quantitative work that searches for general patterns across large groups of similar phenomena; others foster rich, detailed, case-based analysis. Some journals encourage activist approaches to scholarship; others are purely academic. Disciplinary boundaries artificially separate scholars, despite their overlapping substantive interests.

Despite this diversity, Cohen worries that the IPE journals are becoming boring, mimicking the scientific positivism approach of economics. He argues that the journal editors should try new ideas to spice up their journals. I agree that Cohen's concerns should not be dismissed out of hand. I believe that journal editors should encourage scholars to tackle the "big questions"—and then need to put the mechanisms in place to facilitate the translation of these big ideas into published journal articles. Adding spicy ideas to JIBS, however, has not been an unambiguous success, but rather should be seen as a work in progress. The deeper issue is how to build better, stronger theory development into our scholarly journals.

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