

## Ontology, methodology and causation in the American school of international political economy

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### ABSTRACT

This paper explores disjunctures between ontology and methodology in the American school to better understand both the limits of this approach and ways we can counter its blind spots. Tierney and Maliniak's TRIP data point to a strong elective affinity between, on the one hand, rationalist/liberal ontological assumptions and quantitative methodologies, and on the other, constructivist assumptions and qualitative methodologies. This affinity is neither natural nor obvious, as is discussed. It also raises deeper issues for the field about the nature of causation. As a variety of philosophers of science have insisted, we need to do much better in thinking about the relationship between our underlying notions of causation and the methodological tools that we employ. By so doing, we will not only be able to better build social-scientific knowledge, but also better help bridge the empirical-normative gap that Cohen identifies. More broadly, the paper suggests that by combining a more thoughtful approach to causation with a broadly pragmatist approach to the philosophy of science we can both remedy some of the defects of the American school of international political economy, and provide some pointers to the British school, too.

### KEYWORDS

Q1 American school; constructivism; methodology; ontology; rationalism.

Both Benjamin Cohen's original essay and Daniel Maliniak and Michael Tierney's response paper point to important – and for the most part under-explored – questions concerning the 'American school of international political economy' and its differences from the kinds of international political economy studied in the UK. Cohen identifies the relationship between ontology and epistemology as key to understanding the American approach to international political economy (and indeed its UK counterpart). In this

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short article, we examine a slightly different relationship – that between ontology and *methodology*. We argue that the relationship between the fundamental ontological assumptions of American scholarship on international political economy, and the methodologies that scholars use to test these assumptions is much messier than it might appear at first glance.

We explore disjunctures between ontology and methodology in the American school to better understand both the limits of this approach and ways we can counter its blind spots. Tierney and Maliniak's TRIP data point to a strong elective affinity between, on the one hand, rationalist/liberal ontological assumptions and quantitative methodologies, and on the other, constructivist assumptions and qualitative methodologies. As we argue later, this affinity is not as natural or obvious as it might seem at first glance. It also points to a deeper set of issues for the field. As a variety of philosophers of science have insisted, we need to do *much* better in thinking about the relationship between our underlying notions of causation and the methodological tools that we employ. By so doing, we will not only be able to better build social-scientific knowledge, but also better help bridge the empirical-normative gap that Cohen identifies. More broadly, we suggest that by combining a more thoughtful approach to causation with a broadly pragmatist approach to the philosophy of science we can both remedy some of the defects of the American school of international political economy, and provide some pointers to the British school too.

55 **THE RELATIONSHIP BETWEEN ONTOLOGY  
AND METHODOLOGY**

In order better to understand the relationship between ontology and methodology in the study of international political economy, we would ideally like to examine the relationship between the ontological assumptions (that is, the basic understandings of how the world works) of the two main theoretical approaches to international political economy – rational choice and constructivism – and the methodological approaches and associated methodologies – statistical or qualitative – that proponents of each ontology use to investigate the world.<sup>1</sup>

65 While we do not have space to provide a complete account of these ontologies and methodologies, we note that constructivism and rational choice accounts start from quite different accounts of how the world operates. Typically, constructivists employ ontologies that invoke mutual constitution and transformation, while rationalists, whether materialist or ideationalist, work from models in which actors' understandings of the world are complete and fixed.<sup>2</sup>

70 On the basis of non-systematic observation, we suspected that there would be a strong and obvious relationship between ontology and methodology in IPE scholarship. IPE scholars adopting a rational choice (or, in our

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proxy, liberal – see below) ontology would be more likely to use quantitative methodology, and IPE scholars using a constructivist ontology would be more likely to use qualitative methodological techniques. The TRIP dataset, which covers a broad sampling of articles from the major journals in the field, does not provide us with precisely the data one would like to test this. There is no specific code for rational choice assumptions in the data. However, we suggest that liberalism provides a reasonable proxy for rational choice: while liberal scholarship need not be rational choice,<sup>3</sup> we believe that most liberal articles in the field of international political economy invoke either soft or hard rational choice assumptions.<sup>4</sup>

Our expectation of affinities between rational choice and quantitative methods and between constructivism and qualitative methods were amply confirmed. We had originally intended to use relative risk ratios over time to illustrate the changing relationship between liberal/constructivist ontologies and quantitative/qualitative methodologies over time. However, this proved impossible; there are *zero* constructivist international political economy articles that employ quantitative techniques in the TRIP dataset (given their sampling technique, this figure has a confidence interval of +/-4.5% at the 95% level).<sup>5</sup> Even if there are a couple of articles that the sample has missed, this suggests an even stronger relationship between constructivist ontology and qualitative/case study methodology than we had first anticipated.<sup>6</sup>

When we look in contrast at the dataset's evidence regarding liberal IPE scholars' use of quantitative and qualitative techniques, we find instead that not only is there a strong contemporary emphasis on quantitative work, but that this emphasis appears to have become dramatically more pronounced in the recent past. Here, 1997 appears to have been the cusp year in which there were as many liberal articles with quantitative methodology published as with qualitative methodology (using the three year average to smooth out noise). From this point on, we see a rapidly increasing dominance of quantitative articles, so that in the three year average for 2006 there are 11 times as many articles published with a liberal ontology and quantitative methodology as there are articles with a liberal ontology and qualitative methodology, almost exactly reversing the ratio seen in 1986.

### WHY IS THIS RELATIONSHIP SO STRONG?

We doubt that this finding comes as a complete surprise to many scholars in the American school of IPE, whether rational choice or constructivist: it confirms our everyday presuppositions about the relationship between ontology and methodology. Yet, as others in this special issue argue, we do not think this relationship is natural or inevitable. Economic sociologists share many common ontological assumptions with constructivist political

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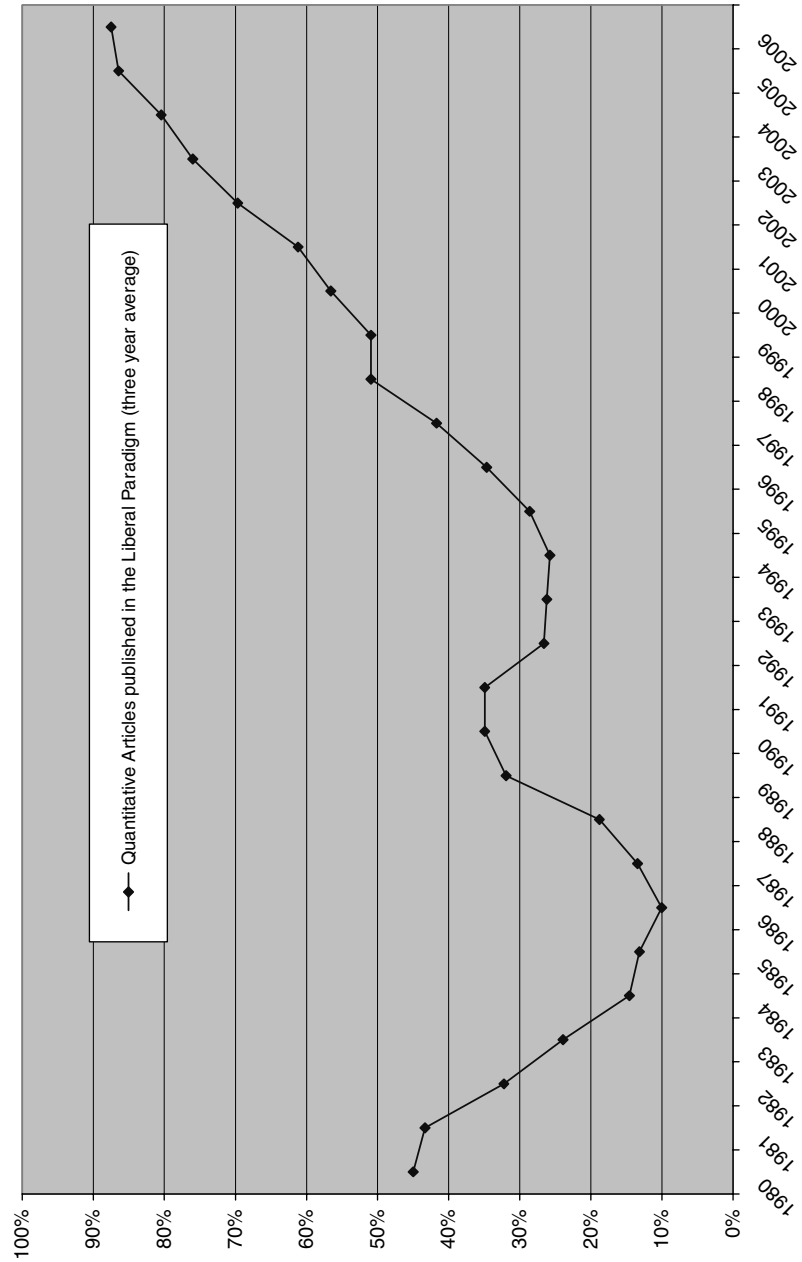


Figure 1 Quantitative articles published in the liberal paradigm of IPE as a percentage of the total of quantitative and qualitative articles (three year average).

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scientists, yet they frequently carry out quantitative empirical work to assess claims based on these assumptions.<sup>7</sup> Nor is there any reason why quantitative techniques of the kind that are usually employed in international political economy (multiple regression and its cousins) should be especially appropriate to rationalist arguments. Indeed, one might plausibly suggest that the opposite is true. Peter Hall (2003) argues that multiple regression and its variants are in fact badly suited to investigating game theoretic claims, which frequently argue that possible outcomes off the equilibrium path of play are key to explaining those equilibria that are selected. Hall makes a plausible case that qualitative evidence and process tracing provide better purchase than commonly used quantitative techniques for elucidating the kinds of causal processes that game theoretic models point towards.

Thus, we cannot assume that the very striking relationship we see between ontology and methodology flows from some underlying affinity between assumptions and research techniques. Instead, as Kate McNamara and Nicola Phillips argue in this issue, we suggest that this relationship more plausibly has its origins in the sociology of the field; that is, the social processes through which scholars are trained, come to select their fields of study, construct their disciplinary identities and decide where to submit their work and how to frame it. While the TRIP dataset can illustrate intriguing correlations, it cannot really get at the causal mechanisms that underlie those correlations. We can reasonably speculate however, regarding two possible mechanisms that we strongly suspect are important.

The first of these has to do with training. We suspect that American doctoral programs in IR disproportionately emphasize training in quantitative methods. It would hardly be surprising, then, if American IR scholars disproportionately favor these methods in designing their research. To investigate, we asked what methods courses are (a) required and (b) offered at the top IR PhD programs listed by respondents in the TRIP survey (Q22).<sup>8</sup> Based on curricula and program requirements listed on their websites, all but seven of the top programs require at least one course in quantitative methods and six of them require at least two such courses. No program lists a qualitative methods course as required for their PhD. Requirements aside, quantitative methods offerings are much richer than offerings in qualitative methods. Quantitative offerings range from three to 12 distinct courses offered over a two year period (the usual duration of coursework for a doctoral student) at top departments, with an average of 7.5 different courses on offer; qualitative offerings range from zero to 1.5 different courses over a two year period with an average of 0.64 distinct qualitative courses offered in a two year period. Rough as they are, these data support our suspicion that quantitative methods are given pride of place in US training.

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The second mechanism, as Nicola Phillips and Randy Germain also suggest, has to do with incentives for publication. Here, we have no data that would allow us to investigate the relevant causal processes, beyond occasional policy statements from editors and our assessment of the collective wisdom of the field from gossip and conversation with others. We suspect that both social processes and strategic incentives are important to explaining who submits articles where, and which articles get published. Scholars are likely to be strategic in where they submit articles and how they frame them. In particular, scholars in research-oriented departments who have not achieved tenure are highly motivated to adhere to the perceived implicit norms of major journals in order to get published, and hence improve their chances of tenure and promotion. They will be keenly interested in any information, however ambiguous, as to what criteria reviewers and editors for particular journals are likely to use in deciding whether to recommend acceptance and rejection.

This means that authors' decisions about whether to use, say, quantitative or qualitative methodology to investigate a particular research question is likely to be affected by their perceptions of how their choice of methodology might affect their chances of getting published in a particular journal or small subset of journals. If they believe that qualitative research exploring hypotheses derived from rational choice theory, or quantitative research exploring hypotheses derived from constructivism, is likely to be poorly received by reviewers and editors, they may decide not to write the article. Alternatively, where possible, they may rewrite it to change either research methodologies or ontological assumptions so that it accords better with the perceived preferences of reviewers and editors.

Authors' perceptions about what will and will not get published may thus be guided by assessments about the priorities of particular journals. They may also involve self-reinforcing information cascades. If a journal begins to publish more (or less) of a particular type of article than others, for purely random reasons, this may create a perception among potential authors that this journal is a good (or bad) venue to try to place articles of a particular kind. As more authors submit (or decline to submit) work of the relevant kind, this impression may become self-reinforcing, even if the editor and reviewers for that journal do not in fact particularly favor or disfavor articles of the relevant type. Without further data (and for obvious reasons, data about the social aspects of the reviewing process are hard to come by), it is impossible to assess how accurate these assessments are. Nonetheless, the ubiquity of gossip among international scholars about which journal is publishing who and why and the potential impact of changes in editorial teams in major journals suggests (although it certainly does not prove) that these impressions are likely to be causally important in shaping scholars' publication strategies.

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**WHY SHOULD WE CARE?**

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What implications flow from the relationship between ontology and methodology in the American approach to international political economy? Or, to put it more bluntly, why should we care about abstruse questions of metatheory? The first and most obvious reason to care is that it suggests that there are important intellectual gains to be made from exploring the underutilized combinations of ontology and methodology – that is, from the combination of constructivist ontology and quantitative methodology on the one hand, and rational choice ontology and qualitative methodology on the other. The second and more subtle reason is that if we think more carefully about the relationship between methodology and ontology, we will in turn be more likely to be careful when we talk about causation.

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If we are right that there is no *a priori* intellectual reason why a rationalist ontology should go together with quantitative methodologies, or why a constructivist ontology should be especially well suited to a qualitative methodology, several interesting implications follow. First, there are important gaps in our knowledge – it is highly likely that quantitative evidence will be helpful to constructivists in investigating and refining their arguments and qualitative information will similarly be helpful to rational choice scholars in investigating and refining theirs. Second, as a consequence, these gaps are remediable (and as we note, there are examples in related fields of inquiry which provide guidelines as to how these gaps might be filled). Third and finally, if these gaps are to be remedied quickly and properly, it will require not only effort on the part of individual scholars, but also collective changes in graduate student training and publication norms.

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Carrying out the last of these steps is perhaps the hardest. Graduate programs need to incorporate rigorous training in qualitative methods of research. The necessary resources for, and interest in, such a change clearly exist. The APSA's Qualitative Methods organized section is now the Association's second largest (as of February 2008) and the number of students attending the Institute for Qualitative and Multi-Method Research (IQMR) has roughly tripled since 2002 (from 45 to 129) (Collier and Elman, 2008).<sup>9</sup> 'Mainstreaming' qualitative methods training into students' home departments as part of the standard curriculum is clearly the next step. Changing the perceived and actual norms of publication and submission is likely to be more difficult. As a plethora of research on organizations tells us, informal norms and perceptions such as those that shape the publication process in editorial boards and professional associations, are often deeply entrenched. Some efforts to reform these norms and perceptions in the past have been at least partially successful (e.g. the steps that were taken to make the *American Political Science Review* a more friendly-seeming journal

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to qualitative approaches following criticisms from the Perestroika movement). But changes of this sort involve serious investments of time and energy, and even then their success is chancy.

We do not provide specific proposals about how this change should take place. Instead, we suggest that we need to begin a frank and open-ended conversation about (a) how we train our graduate students, (b) how we submit articles on the one hand, as working academics, and assess them on the other, as editors and reviewers, and (c) what we are missing out on (or leaving out) of our analyses, using the very helpful data that Maliniak and Tierney provide as a starting point. The disconnect between the ways that scholars talk about graduate training and publication practices in private conversations, and the ways that they publicly present their own research or assess the research of others is sometimes rather startling. As professors in public debate, we often present the processes of training and publication as relatively neutral and efficient means of furthering the dissemination of knowledge. As professors in private conversation, we sometimes obsess about how 'our' perspective is faring against those of others in the academy, or how the publishing priorities of this or that journal are shaped by the editor's idiosyncracies or perceived social connections.

The point is not that either the public or the private perspective has a monopoly on the truth: the field of international relations is both a means for gathering knowledge about the world, *and* a social institution, with all the power struggles and idiosyncracies that we expect social institutions to have. Rather, the point is that we often forget (or want to forget) the ways in which the informal social understandings of our field shape our research, training and publishing agendas. But if we do not pay attention to these understandings – and think hard about what they blind us to as well as what they conceal, we're likely to miss out on much that is interesting and important in the modern international political economy.

## WHERE WE SHOULD BE GOING

As our own initial contribution to this conversation, we would like to push scholars (a) to think more clearly about causation, and (b) to start thinking about both issues of causation and broader research agendas in a more *pragmatic* way. Our call has features in common with Peter Katzenstein's plea in this issue for more problem-driven research, but also appeals to a tradition of pragmatist claims about the purposes of social inquiry. We want to use pragmatist and causal realist arguments, to argue against *both* the casual positivism of many scholars in the American school of international political economy, *and* the post-positivist or mixed normative/positivist stance of many scholars in the British school.

Getting scholars of international political economy to think more carefully about causation is even more important than encouraging them to try



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out underexploited combinations of ontology and methodology. The lack of attention to how ontology and methodology relate goes together with a deeper disregard for problems of causation. On the one hand, 'mainstream' scholars in the American school often appear to carry out research on the basis of a not-very-well-thought-through positivism. That is, they directly or indirectly suggest that all we need to pay attention to are variables and relationships between them that are best explored either through statistical hypothesis-testing or through qualitative methods that seek to replicate the logic of statistical inquiry (King *et al.*, 1994). On the other, many scholars in the UK, Europe and (in smaller numbers) the US, adopt explicitly post-positivist ontologies, in which they either do not try to arrive at causal or generalizable findings, or else mix together normative and positivist claims so that critique and empirical claims are impossible to separate from each other.

We differ from both, although we happily recognize that scholars using both sets of assumptions have made very substantial contributions indeed to our understanding of international politics. Instead, we are more convinced by social-scientific variants of scientific realism, which stress (in contrast to post-positivists) that causation is important, but also (in contrast to positivists) that causal mechanisms exist independently of directly measurable relationships between variables.

What Daniel Little calls 'causal realism' is fully compatible with each of the major methodological approaches to international relations (Little, 1998, n.d.). We can identify constructivist (Wendt, 1999), sociological (Little, n.d.), historical institutionalist (Mahoney, 2003) and rationalist (Johnson, 2006) variants of these arguments in the theoretical and empirical literature. These various authors likely disagree, either mildly or vigorously, about the *precise mechanisms* that are most helpful to social explanation, but all of them agree that we should pay proper attention to causal mechanisms, and that we should treat them as being distinct from the observed statistical or qualitative relationships from which we infer their existence. Social scientists trying to find mechanisms are rather like physicists trying to demonstrate the existence of new fundamental particles using data about collisions in accelerators – while we cannot observe the mechanisms themselves, just as the physicists cannot observe the particles, we can nonetheless make inferences about their existence and nature from the data that we have.

We do not have space to do more than begin to sketch out what these views entail, but we do wish to point out, following James Johnson, that causal realism usefully complements a *pragmatist* account of the task of explanation in many respects (Johnson, 2006). That is, it goes hand-in-hand with accounts of scientific explanation that 'treat theory and methods as tools for solving problems, and so judge them instrumentally in terms of their consequences' (2006: 227). Rather than seeking necessarily to construct coherent bodies of law-like generalizations on the one hand, or to

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335 deny the usefulness of the scientific enterprise on the other, these accounts  
(in their social-scientific form) seek to identify relevant causal mechanisms,  
and arrive at useful, but often imperfect, judgments regarding the scope  
conditions under which these mechanisms are likely to apply. Useful causal  
explanations will invariably rest on unobservables (Mahoney, 2003). Artic-  
340 ulating the relevant mechanisms is thus, at bottom, a theoretical enterprise,  
one that must be aided by the best possible empirical research, but that  
cannot be reduced to measurement and inference. Pragmatist accounts of  
how politics works are thus more modest in their scientific ambitions than  
many forms of positivism, but also are explicitly committed to the refin-  
ing, testing and re-testing of empirical knowledge. They are notably more  
345 ambitious than positivist accounts in that they aspire to *explanation*.

Finally, pragmatists are committed to the generation of *useful* knowl-  
edge. Again, this helps to throw into relief a set of issues that are partially  
obscured by existing debates. International political economy, in both its  
American and English variants, possesses no equivalent of *International Se-*  
350 *curity* – that is, a highly prestigious journal which seeks to bridge the divide  
between scholars interested in theory, and practitioners interested in the  
generation of useful knowledge. This lacuna both reflects and reinforces a  
division between the generation of theory and the generation of practical  
knowledge. Interestingly, this division is less marked in other academic ap-  
355 proaches to the same issues (including approaches that might seem, on the  
face of it, more abstract, such as economics). When practitioners want ac-  
ademics' advice on how to reform, for example, the International Monetary  
Fund or the Appellate Body of the WTO, they typically turn to economists  
or legal scholars rather than to scholars of international political economy.

360 From the perspectives of the existing schools, this is by no means neces-  
sarily a bad thing. Scholars of the American school (even while casting the  
occasional envious glance at their colleagues in the economics department)  
may reasonably prefer to engage in the scientific enterprise without having  
to fend off the dubious enticements of consultancy fees and influence over  
365 the policy process. Scholars of the British school for their part may identify  
(with some reason) engagement with the policy process in its current form  
as tantamount to selling out to the enablers of global capitalism.<sup>10</sup>

Even so, we suspect that the dearth of connective tissue between the  
study of international political economy and the processes through which  
370 decisions are actually made in that economy weakens both approaches. On  
the one hand, this disconnect leads to a certain degree of aridity in much of  
the bread-and-butter work of the American school of international political  
economy. While the outstanding contributions of that approach provide  
deeply important insights into how the international political economy  
375 actually works, much current research is neither scientifically cumulative  
in any very convincing way, nor especially interesting on its pragmatic  
merits to non-specialists.

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On the other hand, while the best work in the British school welds together normative and empirical insights so that each reinforces the other, some of the less inspired contributions instead use normative arguments to try to patch over holes in their empirical evidence. Often, these weaker articles and monographs also fail in their own stated purpose as a result. They do not provide useful contributions to an overall critique of global capitalism that might prove helpful to those trying to change it. 380

Scholars of both approaches then might benefit from some deeper pragmatic consideration over the kinds of knowledge they are generating, and the ways in which this knowledge is likely to be useful. Furthermore, even though we do not for a moment believe that an infusion of pragmatism would dissolve the divisions between the two schools, we do suspect that it might recast the debate between them in some beneficial ways. It would furthermore help IPE scholars move forward from the often sterile 'paradigm debates' discussed by Tierney and Maliniak to a more developed understanding of the specific contribution that different combinations of ontology and methodology have to offer to the understanding of the international economy. Finally, it would help international political economy to focus on some of the 'big questions' that Keohane identifies in his contribution to this volume. A subdiscipline which purports to understand the world economy, but which fails to address the major changes in train in that economy, will have only limited ability to generate useful knowledge. 385 390 395 400

As we have argued in this short article, both ontology and methodology are important. But very often, the ways in which they relate to each other are the contingent result of the sociology of the field (and individual and collective struggles for recognition and legitimacy) rather than independently rooted complementarities between assumptions and methods. Scholars may thus build hardened identities around specific ways of relating ontology and methodology that may be less solid than they appear at first. 405

Reorienting our scholarship so that it focuses on questions of usefulness is one good way of revisiting these identities. However, it also challenges the ways that these identities relate to each other, and (as Cohen suggests) partially constitute each other. If we move from asking the question of which ontological and methodological assumptions are 'better' in some absolute sense, to the more nuanced question of which assumptions are more useful for which purposes, we will have advanced debate. There would still be substantial room for disagreement, not only over the particular merits of specific combinations, but over the questions of *useful for what* and *useful to whom*. However, a debate over these issues is more likely, we think, to be useful than the extended stand-off that we have seen over the last 20 years. 410 415 420

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NOTES

- 430 1 As we discuss further below, the Tierney Maliniak dataset doesn't specifically code for rational choice.
- 2 Standard game theoretic models require that there are 'no surprises' – i.e. that actors have complete knowledge of the game tree, and all possible outcomes, even if they do not necessarily know which part of the game tree they are on. See further, e.g. Farrell (2003).
- 435 3 See Doyle (1983a, 1983b). For overviews of different varieties of liberalism see Zacher and Matthew (1995), Doyle (1997), and Moravcsik (1997).
- 4 For discussion of the coding rules for 'liberal' see Maliniak and Tierney's discussion of codings for variables 4 and 10 (Maliniak *et al.*, : 33–8). The most obvious  
440 general exception is work on the democratic peace, which has a strong strain of work that is identifiably liberal, but descends either from idealist or classical republican thought. However, this body of literature rests outside international political economy.
- 5 More generally, of all 207 constructivist articles (international political economy and non-international political economy) coded in the dataset, only 19 use  
445 quantitative methods.
- 6 An anecdote in David Lake's contribution to this special issue neatly exemplifies how powerful this identification between quantitative methodology and liberal ontology has become; although most of Lake's work is qualitative and historical, he found himself identified to his chagrin as a 'quantitative' scholar  
450 because he adheres to a liberal and rational choice ontology (Lake, 2009, this issue). Q3
- 7 See, for example, Fligstein (1985) and Davis *et al.* (1994). Work by sociologists in the 'world polity' tradition pioneered by John Meyer and others have relied heavily on quantitative methods from its earliest days. See *inter alia* Meyer and Hannan (1979) or more recently Drori *et al.* (2006). Q4
- 455 8 For an earlier examination of these issues, see Bennett *et al.* (2003).
- 9 Summary statistics on IQMR supplied by Colin Elman, personal communication.
- 10 See Germain (2009, this issue) for relevant observations on this point. Q5

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### Queries

- Q1.** Au: Please provide one additional Keyword.
- Q2.** Au: RRH ok?
- Q3.** Au: Daniel Maliniak Amy Oaks, Susan Peterson, Michael J. Tierney, 'The International Relations Discipline, 1980-2006': please provide year and date of access.
- Q4.** Au: Please provide full bibliographic detail for this in References.
- Q5.** Au: Please provide full bibliographic detail for this in References.
- Q6.** Au: Cohen (2007): Please either cite this in text, or delete from References.
- Q7.** Au: Daniel Maliniak Amy Oaks, Susan Peterson, Michael J. Tierney, 'The International Relations Discipline, 1980-2006': please provide year and date of access.