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CHAPTER

## The Welcome Return of Political Economy to International Political Economy Scholarship: States, Markets, and Governance

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### Abstract

This chapter argues that methodological pluralism and innovation, combined with new data sources and research technologies, have steadily eroded the divisions observed in the “Cohen debate” on the UK versus US schools of (International) Political Economy. A problem-solving approach that nonetheless addresses big questions of global and normative importance has reduced the influence of “gatekeeper” approaches such as critical theory or Open Economy Politics, integrating many of their insights on the way. Understanding the observable variation in state-market-governance relationships remains central to the contemporary field: a focus on the interface between what people do as they go about their material life, and the many collective forms of governance and organization, private or public, formal or informal, local or cross-border, that emerge as a result.

**Keywords:** (international) political economy, governance, state-market relationship, methodological pluralism, problem-solving, critical theory, Open Economy Politics

**Subject:** Political Economy, Political Institutions, Politics

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This chapter begins with the observations of the editors in their Introduction: that International Political Economy (IPE) as a field has drifted into a state of constructive, problem-solving non-engagement that is more positive than otherwise. The assessment here is cast more broadly than in the Introduction, and also focuses on what is new. To the standoff between “critical theory” and the rest,<sup>1</sup> one should add other approaches that have proven deliberately resistant to cross-fertilization, such as Open Economy Politics (OPE), the residue of Marxism and dependency approaches, or other “grand theories” based on broad and unverifiable, system-level generalities. The methodological and other divisions observed in the “Cohen debate”<sup>2</sup> have proven unstable, and a healthy, cross-fertilization dynamic has taken its place, albeit with lingering exceptions. This chapter will advance three interrelated arguments in defense of this position.<sup>3</sup>

Firstly, the key developments since Cohen wisely stirred things up are threefold: (1) a long-fused end to the methodological and “conflicting approaches” conflicts of the 1990s and earlier 2000s wherein methodological innovation is now part of a renewed, if still fragmented openness; (2) enhanced data availability; and (3) evolving research technologies, frequently imported from other disciplines, permitting new forms of enquiry. The discipline has expanded as well, numerically and in terms of intellectual terrain. Many new scholars with reduced historical baggage and better methodological training have entered the field. Many of these have training and/or professional experience on both sides of the Atlantic or in Asia. While subfield, PhD background, and empirical specialization naturally remain and produce differences, there has been a pragmatic abandonment of the IPE-Comparative Political Economy (CPE) “levels of analysis” distinction and a variegated commitment to understanding levels-interlinkages in terms of national, regional, and historical context. The understanding of governance, public and private, has long since superseded any earlier excessive focus on what states and formal institutions of government do. Pragmatic, plural, and productive: the pragmatism of the problem-solvers is having a highly integrative effect on IPE scholarship, with lots of critical argument in between. While this is indeed an optimistic take with which some colleagues may disagree, the point is that the enemy is not diversity or fragmentation, but the practice of exclusionary club-formation and careerist, intellectual rent-seeking.

Secondly, this means that those approaches to the field that subsist on boundary and gatekeeping exercises, for example the OEP or “critical theory” schools, have become over time more isolated from a range of important insights accepted as part of the IPE toolkit by the ever-broadening (if diverse) mainstream of the field. The economist-imitator OEP leadership may be observed as steadily abandoning the clubhouse for more open ground (Lake 2009, 2010, 2021). For the “critical theory” school, economics and much mainstream IPE remains both monolithic and largely a negative influence, whereas crucial insights from critical theory continue to be absorbed by other scholars, arguably placing at risk the critical enterprise itself (Jahn 2021). “Mainstream” IPE is addressing the normative concerns of the “critical” school efficiently with well-grounded and innovative empirical evidence and has returned to its classical political economy origins as *the* integrated “science of society,” and has “swamped” OEP approaches in the process. For the problem-solvers, the level of economic expertise has likewise vastly improved. To varying degrees depending on who and where, the problem-solvers are eagerly interacting with and innovatively adopting insights from specialists in economics, political science, political theory and philosophy, history, socio-economics and other social sciences, and, increasingly, the natural sciences. Solving problems through this pragmatic and plural attitude to scholarship is likely to take us far. All of these threads share an ancestry that pre-dates the calcified if ultimately unstable divisions observed by Cohen.

Thirdly, while recent bibliometric researches (Seabrooke and Young 2017; Clift, Kristensen, and Rosamond 2022) focus on boundary and intellectual community detection exercises, we should adopt a substantive definition when we seek to define (International) Political Economy or (I)PE as a field of enquiry. Arguably the core issue remains the intersection of states and markets, from which stems a range of specialized enquiries. Given the classical heritage and wealth of evidence, one should put this with greater nuance: a focus on the interface between what people do as they go about their material life, and the many collective forms of governance and organization, private or public, formal or informal, local or cross-border, that emerge as a result (Ostrom 1990; Leeson 2007; Skarbek 2020).

There are historical reasons for this focus (Underhill 2000), but given contemporary developments it remains relevant simply because of the sheer amount of empirically-observable variation in forms of market and governance, the problems we face, and the related cross-disciplinary intellectual borrowing that goes on around the study of these phenomena. If the field depends on how it is defined by its own specialist practitioners, important as it is to understand these emerging patterns, this likely yields a less-than-helpful definition that focuses on boundaries and gatekeeping. More helpful is to observe how, despite a welcome diversity, this substantive definition unites rather than divides scholarly effort across the

“natural” boundaries of specialization. As long as the problem-solvers engage in an open, dynamic, and plural version of “niche proliferation” (Seabrooke and Young 2017; Young 2021), the disagreements need not fix sterile boundaries. A field that prides itself on interdisciplinarity and openness loses these characteristics as soon as boundaries are set. Neither Smith nor Ricardo would have agreed to that.

These arguments can be tied together by pointing out that it is through specific attention to real-world problems that theoretical and methodological progress has been made in our field. Phenomena that have always intrigued or eluded satisfactory explanation push us to look at what other disciplines are doing. This applies directly to work in political economy that explicitly explores the governance-market relationship in some way or other, which is most of it. This concern for concrete issues that plague our societies has led to much of the methodological and interdisciplinary engagement of individual and groups of (I)PE scholars, and to much innovation when it comes to new sources and uses of data. The pragmatism associated with problem-solving thus has a great deal to do with an iterative bridging of the gap between (I)PE approaches and other fields of enquiry, simply because the problems we identify involve normative and ideational big-question foundations that no group of scholars can ignore for too long. All of this requires the constant fuel of a strong dose of critical thinking that consists of being most skeptical of ideas and arguments that fall in our personal comfort zones, and being most open to those ideas that make us most uncomfortable.

The remainder of this chapter starts by briefly examining the residue of the Cohen debate, an important punctuation point in the emergence of the contemporary field of (I)PE. The second section examines how real-world events and generational change helped to “bridge the gap” between the divided “non-engagement” fragments of a diverse field of enquiry. “Big Questions” of states and markets simply refused to abandon the stage. A growing acceptance of the complementarity of quantitative and qualitative methods, as well as the increasing ubiquity and sophistication of the former, revealed how problem-solving could explore them in new ways that complemented some and undermined other more macro approaches. Problem-solving could be, in critical and pragmatic hands, an inherently integrative form of enquiry. Problem-centered enquiry into the state-market relationship can and indeed has produced important theoretical and empirical research agendas that take us away from boundaries and toward the open skies, while simultaneously linking the contemporary field to its deep roots in historical scholarship and philosophical enquiry. The fit is imperfect: the incentive to distinguish one’s “ism” has not disappeared, some research remains necessarily specialized and focused, but these developments arguably return (I)PE to the substantive state-market-society agenda and leave scope for theory too, as long as this pluralism remains “engaged” (Young 2021). This depends on individual scholars; those with strong commitments to particular approaches, methods, or ontologies are less likely to engage than the more pragmatic. The chapter concludes with a forward look at what is new as a result of these changes.

## The Cohen Debate: Punctuation and Turning Point

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The most fundamental schism in political economy over time was the emergence during the nineteenth century “marginal revolution” of neoclassical and formal economics as separate from the broader social and historical concerns of the classical political economists and their successors. Nevertheless, “minority” economists kept (international) political economy alive through “trespassers” from development economics such as Albert Hirschmann (1970, 1981), Raul Prebisch (1950), or Amartya Sen (1991), and also by the interwar emergence of institutional economics (Commons 1924, 1932, 1950; Coase 1937) followed by its more recent revival (Williamson 1975, 1985; North 1990, 2005; Acemoglu, Johnson and Robinson 2005; Greif 2006; North Wallis and Weingast 2009). The public choice scholars (Buchanan and Tullock 1962; Buchanan 1987)<sup>4</sup> and the vast range of work by Barry Eichengreen<sup>5</sup> could be supplemented by the scholarship that emerged from business and strategic management as a field where the political, institutional, and private influence elements could not be ignored (Vernon 1966, 1971; Dicken 1986;<sup>6</sup> Stopford and Strange 1991; Dunning 1988; Baron 1995, 2001, 2014). This highly incomplete tour testifies to a far more plural set of theoretical and methodological toolkits than in “standard” economics, and the innovation has continued with, among others, behavioral (Smith 1991) and experimental (Kahneman 2011) economics.

The punch line is that (I)PE scholars have borrowed extensively both from “standard” and these more eclectic contributions from economists, while the reverse has almost never happened (Jones 2020, 54). Although Economics is too often presented as both rival and enemy to (I)PE and anathema to some, the field continues to absorb insights from Economics pragmatically to its benefit.

This hybrid richness of (I)PE, cross-fertilized by contributions from dissenting economists, is well illustrated by two surveys in *International Affairs* (Underhill 2000; Dickins 2006). Like Cohen’s book, these are accounts of the postwar “revival” (Underhill 2000, 794) of the older, broader field of study rooted in the classical tradition of the Enlightenment that subsequently fragmented into the specialized social science approaches we know today: (economic, political, other varieties of) sociology, (neoclassical and Keynesian) economics, political science and several subfields like international relations, law, human geography, and so on. Both pieces emphasized the breadth and common historical origins of the contemporary field, yet also the conflictual state of affairs in terms of methodological, normative, and theoretical differences. These divisions were represented by the competing but overlapping insights of Dickins’ species *Ratiosaurus Rex* (interstate competition and cooperation tradition in IPE, stemming from International Relations (IR) and the OEP school) versus genus *Querimonia* (states–markets interaction tradition). Each article made a case for synthesis based on less querulous interpretations of the field and its possibilities—a synthesis that this chapter argues is now taking place in a decentralized way.

If these articles and a range of textbooks exposed the divisions, Jerry Cohen (2008) sounded an alarm. Cohen (2007, 2008, 3–5, Chs. 1–2), employing what he admitted was a somewhat risky simplification of observable diversity, organized these divisions analytically into the “US School” (professional, empirically systematic and rigorous, yet arguably conventional and increasingly narrow “small bite” focused) versus the “British School” (socially critical and interpretive, historical-institutional, still asking big questions, yet arguably less than systematic analysis). Each had a different “take” on our understanding of the state-market relationship, and Cohen in turn sparked a debate encapsulated in two special issues of key journals (RIPE 2009; NPE 2009). The constructive mischief Cohen intended was successfully stirred up and this trouble became a turning point in our understanding of states and markets as well. While some argued that Cohen’s typology was too crude or just wrong, many on either “side” argued that, simplification aside, it was all too true and the divide augured badly for the future of (I)PE and other social sciences more generally (e.g., Mearsheimer and Walt 2013)—after all, Cohen had reserved his harshest criticism for the narrowness of the US School.<sup>7</sup>

Methodology and the use of data were key points that sustained the divide: the “economics imitationists” of the OEP/US School insisted on the primacy of powerful new quantitative methodologies and the exploitation of related data that was being rapidly developed, running the risk that data availability drove the research agenda. UK-school acolytes, sometimes doggedly, defended the primacy of qualitative methods grounded in traditional historical and normative social science and contextual expertise, and some (see Cameron and Palan 2009) seemed inclined to dispense with the need for empirical verification and data altogether. Cohen’s book exposed and lamented the division between the two schools, looking back on an earlier age of plural “invisible college” transatlantic engagement now marred by the splendid yet regrettable isolation of each.

## Bridging the Gap

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Cohen and the two special issues drew attention to and elaborated on the sources, motivations, and negative results of the divide. A particular lament was the disappearance of the once-fruitful cross-fertilization of the (I)PE postwar revival and its focus on the big-question nature of states and markets. The field had moved from an attitude of critical enquiry, constructive difference, and engagement to a world of club-goods scholarship wherein “approaches” also defined what scholars must do to remain credible and legitimate in the eyes of “selectorate” peers, and this infected journal editing and reviewing as well.<sup>8</sup> To stylize the point at the risk of injustice, OEP laid out specific elements of ontology and methodology required for graduate training and publication in top journals, while senior scholar reviewers actively defended the turf; in turn, critical theory approaches were no longer about an attitude toward understanding, one of questioning established ways of thinking, but arguably likewise became a set of things one must do and cite in order to remain credible as a member of the club. Scholars retreated to their respective self-made ghettos.

Bridging the gap seemed a tall order unlikely to materialize with despatch, given the way in which careers and peer review might depend on the choice of one school over the other.<sup>9</sup> However, the divide proved, as predicted (Underhill 2009), unstable. The ubiquity of computer technologies; online-available numerical, textual, and survey data exploitation; and the ultimate mutual compatibility of qualitative and quantitative methodologies, the very elements that seemed so divisive, in time turned out to be the source of an eventual, decentralized integration across the field to the current dynamic status quo. Many senior scholars in the US took Cohen seriously (Lake 2010; Mearsheimer and Walt 2013).

Real-world events also intervened. The financial crash of 2007–2008 and subsequent Arab Spring put big state-market social and political economy issues squarely at the center of the agenda on both sides: inequalities; the balance between the open economy and social protection; the state-market and private-public authority balance in (international) economic governance; the relationship between development, authoritarianism, and democracy. This was fertile ground for the problem-solvers but was also the terrain of those posing big questions. The monumental work of Thomas Piketty (2014) on the long-run dynamics of inequality; Robert Gordon’s reignition of the debate around growth, productivity, and innovation (2016); or Thomas Philippon on market competition (2019), each demonstrated that economists, too, were back in the big question, (I)PE economy business. Political economists were not to be outdone: Ansell and Samuels (2014), draw on enlightenment philosophy and data to rethink the historical relationships between development, elite competition, rising inequality, and democratization. Carles Boix in *Democracy and Redistribution* (2003) employed new data to theorize political transitions in relation to the nature and distribution of economic assets and the balance of power among competing social groups. The same author later (2015) challenges institutional economists, offering novel data sets and a “productionist” argument focusing on military technologies and competition.

There were enough individuals and informal networks across the divides who had an interest in overcoming divisions whenever the need arose, and who managed to work around the obstacles to publication.<sup>10</sup> Indeed, there were already examples of “bridging the gap,” including my own cross-disciplinary work with economists. Claessens, Underhill and Zhang (2008) argued through qualitative process-tracing that the Basel II global supervisory standards of the Basle Committee on Banking Supervision, though intended to apply to a wide range of financial institutions in developed and developing economies, advanced the interests of powerful market players with less regard for smaller, less sophisticated banks, especially in developing and emerging-market economies. The article went on to establish through quantitative methodologies the extent to which this was indeed the case: borrowers in poor countries faced higher supervisory charges and thus costs as their economies sought to catch up. The cost imposed was considerable when banks used the data of external ratings agencies to measure risk, yet less when banks used the more sensitive “internal ratings” approach to assessing the risks of lending to clients in developing economies. Banks had finer and better knowledge of their client risks than did the ratings agencies. The point is simple: these findings would not have been possible without the simultaneous use of each of the “rival” methodologies coveted by so many who reinforced the US-UK school divide. One might add that engagement with economists was far from the toxic danger of which so many in more critical (I)PE warned.

Others aided and abetted this nascent cross-fertilization. Burgoon (2001, 2009, 2012, 2013; Burgoon, Oliver, and Trubowitz 2017) pursued a systematic exploration of the relationships between policies of economic openness, national-level labor market and welfare-state regimes, and sociopolitical support for a liberal economic order. Eichengreen and Leblang (2008) in turn engaged in a sophisticated and refreshing use of data and quantitative techniques that demonstrated the mutually constitutive relationship over time between the multiple dimensions of global market integration, “globalization” for short, and the emergence of democracy.<sup>11</sup> The institutional economists North, Wallis, and Weingast (2009, 140ff) provided a more traditional discursive argument concerning the “new logic of collective action” as to how and why democracy might generate political economy dynamics that reinforced the openness of economies. These responses to big questions employed a combination of methodologies, and scholars were leaving little out.

Perhaps most importantly, bastions of UK-school traditionalism with strong and historically grounded allegiances to quantitative approaches, such as IR and politics at Oxford, were over time totally transformed in terms of methodological foundations, PhD training, and faculty hiring. The quality and methodological breadth of a number of prominent PhD programs in the UK and Europe both improved in quality and embraced plural methodologies, frequently more easily than US institutions. Postgraduate programs on UK-school territory and in neighboring Europe now formed scholars more systematically. What emerged were centers of hybrid and heterodox scholarship such as the Copenhagen Business School, the Graduate Institute in Geneva, the Amsterdam Institute for Social Science, the EUI in Florence, or SAIS Washington and Bologna wherein scholars with particular attributes could easily cross-fertilize and collaborate. There was much transatlantic trade in research teams and academic personnel, with a good deal of the bridging of the gap coming from the UK and Europe. Mixed and combined methodologies increasingly became a mainstay of postgraduate curricula and PhD programs and faculty hiring. While divisions remained stark in some US universities, methods wars eventually simmered to a truce at worst and collaboration and cross-fertilization began to take hold. This was fruitful ground for the innovative use of new forms of data and modeling technologies, for problem-solving, and for answering big questions.

In short, what had been most divisive, a methodology-centered conflict between two splendid isolations, evolved into a matter of pragmatic, problem-solving choice based on research question, data availability, and what we already knew and still had yet to find out. Cohen’s book appeared at the right moment for IPE and helped to catalyze what was already, ever so gingerly, taking place. The intersection of states and markets remained a central, arguably *the* central, preoccupation of (I)PE.

These anecdotal claims above are insufficient to support the argument. Articles using the TRIP data referred to in the Introduction to this volume and more bespoke datasets (Seabrooke and Young 2017; Clift et al. 2022) have examined the field as a whole, or at least a large part of it, and reached similar conclusions. Yet how does this work, in research practice on states and markets? A more focused examination of several articles that represent the trend I argue has emerged will provide further support. The argument is that pluralistic methodological developments have permitted a systematic exploration of state–market, or market–governance interactions, and the embeddedness of both in society across local, national, and transnational levels of institutions. Given the enormous growth in the volume of publications since Cohen’s great divide emerged, the choice of articles may appear arbitrary, yet I rely on material assembled for postgraduate teaching across the master and doctoral levels during the past 15 or so years, often developed conjointly with colleagues. The argument in favor of such an approach is that the choices were made from a very wide range of available literature, and that they were far from arbitrary, designed as they were to make available to students the central arguments of this article about the evolution of (I)PE over time.

## Methodology unites

Let us begin with methodology in order to demonstrate how the debate has moved from an emphasis on the superiority of one to the complementarity of quantitative, qualitative, and a range of new forms of computer–based modeling and simulation techniques. The debate on methodology and the divisions Cohen observed ran across the social sciences, and so the examples are not limited to specific (I)PE research. Two articles in dialogue with each other illustrate the point. Evan Liebermann (2005)<sup>12</sup> elaborated the concept of “nested analysis” in mixed–methods strategies. His argument, not per se unique but for the first time systematically developed for application, was that Large–N, quantitative data–based analyses and case–based Small–N case study analyses were synergistic in a particular way. Large–N studies would reveal the broader patterns of statistical relationships; these relationships should then guide case selection wherein Small–N studies explore and test the plausibility of the relationships the data revealed. Case studies would also allow scholars to develop further theoretical insights through, for example, outlier or “most different” cases. A strategy integrating the two approaches makes up for the weaknesses of each: firstly, Large–N studies might have “insufficient data to assess statistical relationships” or “the nature of causal order could not be confidently inferred” (Liebermann 2005, 440). Secondly, “Because causal inference in the nested approach does not rely solely on the Small–N portion, the standard pitfalls of selection bias are less likely to lead to faulty inferences” (446).

Rolfing<sup>13</sup> (2008) responded: Not so fast! It is more complicated than that: “the specific methodological problems of nested designs have not been fully appreciated” because “ontological misspecification has methodological implications that are difficult, if not impossible, to detect in a nested analysis in its current form” (1493). Large–N statistical analysis may establish convincing yet misleading causal pathways, and nested analysis may lead to greater losses than gains wherein cross–validation may be more apparent than real. Rolfing strongly supported the mixed–methods approach and went on to suggest alterations to Liebermann’s nested analysis framework that, “renders nested analysis even more demanding, given the increasing degree of sophistication of quantitative and qualitative methods” alike, but rendering the combination more valid as well (1511).

The methods debate across the social sciences (if less so in Economics) has, then, for some time been firmly focused less on which of quant or qual is better, but on when each works best and how to combine them most advantageously. Advances in statistics and modeling have been complemented by a burgeoning literature in and beyond (I)PE on case studies and process–tracing qualitative techniques (Bennett and Checkel 2014; Checkel in this volume). Greater rigor and understanding have emerged on both sides of the erstwhile, now less divided, methodological landscape.



## Big questions on states and markets, problem-solving, and (I)PE research

The analysis now turns to three articles that help us understand how these methodological shifts have opened up problem-solving approaches to big questions in the field of state-market interaction. These were selected because each is arguably in the “problem-solving” basket, they each respond to big questions about the core (I)PE focus on the cross-border state-market-society nexus, and each requires a commitment to methodological pluralism and innovation. The first (Eichengreen and Leblang 2008) is a bravado piece of statistical analysis that opened up new frontiers for our exploration of the relationship between forms of governance and deep-going market integration.

Harvard IPE development specialist Dani Rodrik wrote several articles and books (e.g., 2000) in which he argued persuasively and discursively that deep cross-border market integration had gone too far: “The political trilemma of the world economy is that international economic integration, the nation-state, and mass politics cannot co-exist. We have to pick two out of three.” In short, if we wish to preserve national democracy, deep market integration has to go.

Yet how do we know if he is right? Well, we can see that people do not always like it, and generations ago Polanyi (1944) warned of the tensions between radically liberal markets and political and social stability. The evidence of market-driven disruption of societies and politics is clear and we can pile it up. Yet to this author the argument seemed unlikely: years later, neither states nor open markets seem to be going anywhere, though market adjustment presented plenty of challenges as usual, the financial crisis among them. Eichengreen and Leblang (2008) tested the proposition in ways that others had not, using a much longer and richer historical dataset that combined both trade and financial integration data and various historical waves of market (dis-)integration from 1815 onward. Their principal finding was that the relationship between globalization and democracy was a positive one, and that the causal pathways over time went in both directions, suggesting positive feedback loops between the two. That said, they openly accepted (shades of Rolfing) that the analysis only demonstrated general patterns and *not* the causal interactions at any particular time and place (319). This raises the possibility that democracy (echoes of Polanyi) shapes and drives market integration in ways that undermine the trilemma argument (see also North, Wallis, and Weingast 2009, 141ff). This opens up a vast terrain for (I)PE state-market problem-solvers to explore the context and cases of time and place, requiring specialist, qualitative understandings as well as statistical methods. Chwioroth and Walter (2019) filled in an important part of this space in the domain of finance. Krapohl, Ocelík, and Walentek (2021) have used Python software coding to re-open and advance the work of Axelrod on evolutionary game theory as a tool of analysis in (I)PE research, in this case looking at the (in-)stability of the liberal trading order.

A second article by Pagliari and Young (2016) was the last of a series in their successful mixed-methods collaboration on the question of private sector influence in financial governance. Many (including this author) had argued on the basis of qualitative case research that policy capture and private preferences had unduly informed global regulatory and supervisory standards in the financial sector, potentially undermining financial stability and producing crisis (Underhill 1995; Baker 2010). The argument was likely sound in relation to the cases examined, but was there something more general going on? However, the financial sector does not always get its way (Young 2012). Is policy capture and private preference a problem in the financial sector more generally? Pagliari and Young addressed this question step by step in their work, concluding in 2016 that interest group pluralism is particularly low across the “interest ecology” of national financial sectors, while mobilized dissent is in turn weak and disjointed. Financial policymaking across country cases is dominated by financial interests (312). Apparently, there is something different and worrisome about the financial sector after all. Methodological innovation was central to their findings: their “analysis takes strong cues from existing ‘population ecology’ approaches to the study of interest groups” (311), examining through textual analysis and qualitatively analysing huge numbers of interest group

responses to regulatory issues in financial governance, measuring *and* assessing qualitatively “what groups speak up, and what they say” (315). This big question problem seems settled until further notice.

A third article again employs theoretical, methodological, and modeling innovation to open up genuinely new horizons in our understanding of states and markets, this time reviving an old question of the nature US structural power and the validity of arguments about US decline (Wincoff 2020).<sup>14</sup> Here the long-standing concept of structural power and the persistent myth of lost US hegemony (Strange 1987) is reconfigured as a network phenomenon, linking it methodologically to complex network science. The work replaces the typical “attribute- or capability-based approach” (212) to structural power by adopting the “fitness plus preferential attachment” model (210, 212–17) to demonstrate the ongoing “high centrality” of the US across several (213) the domains of finance, trade, security, and knowledge networks. Complex network science permits an understanding of the evolutionary dynamics of interdependence and the endogenous, feedback-based patterns and “nodes” of power and network performance that we do not observe in traditional data. “Because the system is not just the sum of its parts, it should be studied holistically—not just the units but also the connections between them—and modern network science provides an advanced conceptual, analytical, and methodological toolkit for doing so” (214). This research design likewise permits a far better understanding than traditional methodologies of the interactions in complex interdependence, a key concept in (I)PE that has always been as difficult to operationalize as (structural) power, wherein the inference is based on an observed, historical outcome (216). Complex network science permits a proper interactive modeling of structure, agency, and change in a holistic framework.

The findings are intriguing. “There are reasons to believe that the geopolitical advantages the United States has enjoyed in the post-war era would not have obtained absent network prominence. Will this continue?” (242) Furthermore, as Strange argued so long ago, reports of US decline and the relative rise of China are overstated: “if emerging markets continue to pursue development by integrating into the economic and political networks that the United States maintains ... then American prominence is likely to be further reinforced by their growth” (243). Wincoff concludes that shifts in the global economy and security structures have had more negative effects on Europe than the US.

This brief survey of recent literature demonstrates that state-market problem-solvers are addressing big questions, and that methodology is no longer the source of division that it once was. The emergence of younger scholars trained across the panoply of methodologies has truncated the standoff. On the contrary, methodology has become a source of synthesis across the comparative-IPE divide and also a source of new insights and ways of seeing. Problem-solvers are delving into and responding to big state-market questions that matter to national and international political economy. Secondly, while there clearly is problem-solving driven fragmentation across the field—after all, the world is a complex place such that specialization and plurality should not be viewed in negative terms—there is also constructive engagement with long-standing theoretical and more problem-oriented debates. The more dogged adherents to intellectual clubs are simply being left behind. Lastly, these articles also show that the relationship between states and markets, or market exchange and governance, or “state-society complexes” (Cox 1986), however one prefers to phrase it—is still very much at the core of the (I)PE intellectual agenda and binds it together. These questions and the answers we find matter to the future of humankind and the planet.

This state-market agenda is also unlikely to dissipate any time soon, simply because it is at the core of the human dilemma. This admits of a simple, bottom-up, and agent-centric explanation that provides a good metaphor for the developments I have argued took place. As Buchanan and Tullock argued some time ago (1962), we cannot accomplish much of what is dear to ourselves, our most self-interested preferences, without engaging others. This “dilemma of interdependence” soon constrains and condemns us to resolve (or not!) the tensions we experience with those around us, producing formal or informal norms and “rules of the game.” In short, this dilemma of mutual dependence generates patterns of coordination and

collective action. We call this “organization,” and, at scale, institutions and governance, such that political economy dynamics lie at the very roots of society. Violence remains one solution (as we are reminded persistently), but it is not very efficient. If we opt for a more appealing solution, we discover that one cannot have organization without resources and a division of labor, and one cannot have a division of labor and deployment of resources without organization. We continue to differ amongst ourselves about how this should be done, and about the distribution of the costs and benefits of the collective choices we make. Global integration has rendered this increasingly complex and variegated in terms of institutional form and patterns of interaction. Some scholars focus more on the “international” (or “global” if one prefers), others on the more national or local, others on specific areas of production, exchange, or finance, but the levels of analysis distinction so dear to IR remains an irrelevance in (I)PE.

## Conclusion: So What Is New?

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The conclusion to the previous section implies that this long-standing, core state-market agenda is always an open field. Human interaction in the face of distributional and normative tensions is seldom static for long. A good deal of innovative recent work has already been cited in this article, yet there is plenty more. A changing world as it confronts our human curiosity is one source of “newness” — an issue-driven agenda. New methodologies and modeling techniques also means that we have new ways to deal with state-market-integration complexity: new theoretical horizons that may provide a better explanation and understanding of change.

There is far too much research being produced in this vein to attempt an exhaustive or even representative account.<sup>15</sup> Let me therefore focus on examples of work that combines methodological and theoretical innovation with an eye on better understanding the state-market problems and dynamics that we face as we approach the mid-twenty-first century. First up is a new focus on interdependence that takes issue with the OEP approach, arguably launched by Farrell and Newman as the “new politics of interdependence” (2014, 2019). Thomas Oatley (2019) has introduced complexity theories, ecology, and information theory to the concept, and this complements the evolutionary biology approach cited above (Krapohl et al. 2021). As the work by Pagliari and Young discussed above implies, global interdependence and the hydra of global finance continues to keep state-market scholars busy: Heather Ba (2021); Oatley *et al* (2013), Winecoff (2015), Chwieroth and Walter (2019). There has been a revival of work on corporate elites with new methodological flair (Valeeva, Heemskerk, and Takes 2020; Valeeva, Takes, and Heemskerk 2022; Fichtner et al. in this volume) and on labor markets and workers’ rights (Lim, Moseley, and Prakash 2015; Moseley 2017; Moseley and Malesky 2018). One should not forget recent work on rising powers: see Chen (2021) on the implications of China’s Belt and Road initiative, and Otero-Iglesias and Weissenegger (2019) on China’s foreign investment (and many others). One must not forget climate change and the Anthropocene (e.g., Bernstein 2020). I have not yet said anything about the rise of populism against market integration, or the political economy of pandemics!

Lastly, what is waiting to be born? The dilemma as always is that specialization leads some scholars to focus on the “big picture” macro or systemic dimension of the (I)PE of states and markets. As I argued above, this picture needs to tally with a well-fashioned aggregation story. In parallel, the demands of complexity mean that many scholars rightly focus on specific “bite-sized” issues and analytical frameworks in our contemporary (I)PE, attempting to identify cause and effect relationships between the multiple facets of what we observe around us. Both may be problem-solving, asking big questions, and methodologically challenging. Yet more difficult is ensuring that scholars pay sufficient attention to how these two different sorts of enquiries fit together. These two ways of seeing research lay behind Cohen’s great divide. This article has argued that they need not do so, and that there is much more integration and engagement than before.

Yet “big picture” and specialized, issue-focused scholars too often do not mix and may still claim the superiority of one over the other. This article is an invocation for scholars to make the effort to understand each other across this ever-potential divide: to understand why they should fit together. We cannot understand our world and its problems without focused data and specialized methods, and the nitty-gritty of research frequently involves narrow hypothesis-testing. Yet issue-focused scholars can and *should* link their research agendas to big picture approaches.<sup>16</sup> Put polemically, stacking up the findings and calling that coherence is insufficient. Likewise, big picture questions cannot be answered through vague, macro generalities invoking “the nature of the system.” If the claim is that free-market capitalism (or state intervention) is bad (or good), one must demonstrate systematically the various negative/positive effects that different variants of markets and governance produce, *and* why they are good/bad. This necessarily involves a normative dimension or at least starting point, and once again the aggregation problem is daunting. The issue is one of moving from micro- to macro-level, and back again, while dealing with a high degree of heterogeneity and endogenous dynamic relationships: the “everything is related to everything else” problem. Too frequently, the two approaches remain separate because linking them is hugely challenging. The ongoing tensions between micro- and macro-economists illustrate the point.

What might help? Mathematicians will tell us any real-world phenomenon may be modeled. Simulation or game theory and agent-based modeling techniques can reveal bottom-up complex adaptive systems as one solution, and have been explored in political science and economics.<sup>17</sup> This is highly promising if still a niche pursuit; as with all modeling, much still depends on choices of simplifying assumptions, agent motivation, and the design of feedback loops. There is another possibility. As the core of the field, the (I)PE of states and markets, with its constant and dynamic variability producing enormous complexity in terms of interactive relations among heterogeneous agents and an almost endless multiplicity of possible variables, is arguably appropriate to quantum approaches taken from particle physics. The field is full of “quantum uncertainty” and open-ended outcome possibilities, and thinking of ourselves as “objects” has long been recognized as unhelpful.

Alexander Wendt has made serious attempts to do this in a more general IR and social sciences context (2015, 2018), and there was a forum on his work in *Journal for the Theory of Social Behaviour*.<sup>18</sup> Another recent IR contribution came from Chengxin Pan (2018, 2020), with reference to the “relational turn” in IR and IPE (e.g., Nordin et al. 2019). The aim of such approaches is to deal with the “whole and its parts” and structure and agency problems, often thought of as chicken-and-egg dilemmas. There is debate about whether the approach should be “scientific realist” as adopted by Wendt, or more by way of analogy (Pan 2020, 20–1; Donald 2018). Taking such an approach seriously would require some re-tooling for most social scientists, whose understanding of the quantum world, like my own, is typically limited. Yet could this help scholars solve the big-picture-bite-sized research problem, system wholeness, the parts, and order referred to above?

A serious attempt to adapt this initiative to state-market (I)PE would be a substantive companion to complex network theory, which also attempts to focus on relations and their dynamics across systems, yielding a better explanation of change over time. The goal would be to move (I)PE from a classical Newtonian, “parts add up to system” approach to a more sophisticated world of interactive quantum theorizing, because political economy, like the broader social world, is always “becoming,” involving the simultaneous interaction of individuals, collectivities, ideas and consciousness, and systemic parameters yet which are, somehow, all part of the same thing.

Challenge is what the field is all about. Someone should take this up.

## Notes

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- 1 A standoff for which there is no inherent logic, and that not all critical theorists embrace (Jahn 2021).
- 2 See Cohen (2008), and special issues of *Review of International Political Economy* (2009) and *New Political Economy* (2009).
- 3 The author was, from 2018–2021, the Associate Editor (IPE) and Editor-in-Chief of the *European Journal of International Relations*. This vantage point was particularly useful for observing the development of IPE in relation to other subsets of Political Economy writ large.
- 4 Wherein political and economic exchange are considered essentially the same: (Buchanan and Tulloch 1962 Ch. 3); also “Politics is a structure of complex exchange among Individuals, a structure within which persons seek to secure collectively their own privately defined objectives that cannot be efficiently secured through simple market exchanges” (Buchanan 1987, 246).
- 5 Too numerous to do him justice, but see Eichengreen (1992) or (2018) to start with.
- 6 At least six editions of this work have been published.
- 7 Maliniak and Tierney (2009), in their article in the *RIPE* (2009) special issue, analyzed data from their journal article data base and from two surveys of IR scholars in the US and Canada. Their findings largely confirmed the trend identified by Cohen.
- 8 The problem is arguably still very much present in some journals on either side of the divide.
- 9 For many PhD candidates, there was not much choice at all; junior scholars aiming high frequently had to eschew a personal tendency to eclecticism and deliver what supervisors, and above all editors and the reviewers they selected, expected. There was a cost to ploughing one’s own furrow, while one must learn much in the first place before that can occur.
- 10 Some journals thankfully remained more open than others. That said, the numerical growth of the field combined with the pressure from universities for impact factor and ratings made publication just plain difficult for all.
- 11 More on this article later.
- 12 A comparativist at Yale, Princeton, and then MIT focusing on developing Africa.
- 13 Professor at Bremen and latterly Cologne whose work is on methods and party competition.
- 14 Winecoff is by no means alone in embracing complex network approaches to (I)PE; see also Cooley and Nexon (2013); Heemskerk and Takes (2016); Goddard (2018).
- 15 The selection that follows was made largely on the basis of recent issues of the *European Journal of International Relations*, a highly-rated generalist IR journal with about 25% (I)PE content. There is no pretence that this is a proper representative sample, with apologies to rival editors, but it serves the purpose here. Together the collection of articles in this volume will offer more comprehensive coverage of new developments.
- 16 Of course, many do indeed do so.
- 17 See the highly accessible discussions by Benoit, also Cederman in (2001) *The Political Methodologist*; in Economics, a recent example is Gerdes et al. (2022).
- 18 Vol. 48 Issue 2 (June 2018); this includes some robust pushback from the world of physics (Donald 2018).

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