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Political Economy, the 'US School', and the Manifest Destiny of Everyone Else

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SPECIAL SYMPOSIUM

Political Economy, the ‘US School’, and the Manifest Destiny of Everyone Else

GEOFFREY R.D. UNDERHILL

The take-off growth phase of political economy (PE) as a discipline,¹ which began in the early 1980s, is over, and a ‘crisis of maturity’ involving opposing US and UK schools has been stylised by Benjamin J. Cohen in his thoughtful book. It is now incumbent upon scholars in the discipline on both sides of the Atlantic and elsewhere to ponder how the differences might be bridged and how the discipline might recapture the benefits of both the theoretical and disciplinary cross-fertilisation identified with the ‘Magnificent Seven’ and other pioneers. This essay addresses the future of the ‘British school’ first by observing an implicit Anglo-American centrism to Cohen’s UK/US-school analysis and demonstrating the very oecumenical and *European* origins of both, a debt which Cohen recognises but underplays. A second section analyses the UK school in the same critical and European light, arguing that it has apparently abandoned historical moorings of sound, case-based *empirical* analysis which was (to a fault) rather disinterested in theory. These moorings have been replaced in the worst cases by ‘template theorising’ which pays insufficient attention to the high evidentiary standards required of theoretical claims. This feature is not fully explored by Cohen, and is a problem which the UK ‘school’ can and must address, because the key to bridging Cohen’s divide lies in the UK/European tradition in the first place. The final section will develop a number of arguments as to why Cohen is perhaps overly pessimistic, pointing to how the divide between rationalist and cognitive modes of analysis might be bridged.

If these arguments are at all convincing, Professor Cohen would most likely welcome such a conclusion. These arguments are presented in the context of a scholarly career of over two decades of research and writing on the political economy of trade, money and finance across levels of analysis, a career firmly anchored in what Cohen labels as ‘second-generation scholarship’ and by a scholar who is neither British nor from the US, but from Canada, and whose

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appointment is at the University of Amsterdam in Europe. The work of the first generation and the giants on whose shoulders they stood was the bread and butter here. Their scholarship did not have such a national flavour (though provenance was not unimportant) and it also crossed *language* barriers in a relatively easy way because of the language skills of a number of scholars involved, and Cohen's competing styles were not yet definitively set. This perspective is important for section three in particular. I argue that the second generation and some key figures of the third remain well placed to build the necessary bridges.

A (yet more) oecumenical story: the US–UK schools of IPE (and everyone else)

As Cohen argues, the US–UK distinctions have emerged and settled over time, yielding a US 'school' characterised by the methodological rigour of Galbraith's 'imitative scientism', and largely a branch-plant enterprise of state-centric international relations (IR). This is in contrast to a British school which is wide-open in terms of actors and levels of analysis, makes broad and often normative claims in terms of social constructivism and 'critical theory', is at best eclectic in terms of methodology, and often emphasises historical relativism. Yet Cohen's analysis also reveals the highly heterodox origins of each, including the interaction of the early and take-off phases.

The central point here is briefly to remind ourselves of the highly oecumenical origins of the contemporary US–UK schools of (I)PE. To Cohen's account should be added the influence of European social science and access to language skills on *both*. The methodological rigour and attention to systematic empiricism of the US school is surely a legacy of German and other European immigrant scholars as much as an inheritance of British utilitarian traditions (which were widely adopted in Europe), and the German historical school is surely the major influence behind the US focus on realism, balance of power and state-centric analysis. Hans Morgenthau, father of US post-war realism, was German to say the least, and the post-war realism of Raymond Aron (France) and Stanley Hoffman (US citizen, born in Vienna and educated in France) was likewise influential. Neither Karl Deutsch (Czech-Austrian) nor Albert Hirschmann (born in Berlin, with degrees from Paris, London and Trieste), who influenced so many US political economists, were American by birth; nor was Peter Katzenstein, one of Cohen's 'Magnificent Seven'. The tide of European political economists emigrating to the US, from Joseph Schumpeter on down, was enormous. The influence on US social science traditions of the functionalist and structuralist cross-currents (*and* the latter's eventual refutation via post-modernism/social constructivism) associated with such figures as Émile Durkheim, Max Weber, Bronislaw Malinowski and Claude Levi-Strauss was considerable. Johan Olsen of 'new institutionalism' and path-dependency fame was Norwegian. The utilitarianism of the Scottish enlightenment and English philosophers is a prime source of rationalist analytical rigour, which was clearly imported into the US as much by immigrant Europeans as by British scholars.

As Robert Cox also emphasises in this issue, the influences on the 'British' school were of similarly eclectic and heterodox origins, and would include all of

the above in different proportions, as well as interaction with the US. Extremism and persecution in Europe lay behind much of the emigration: Europe's loss, UK and US (and, incidentally, Canada and Australia's) gain and one of the most important reasons why the intellectual traditions of each are historically so rich and varied. In terms of direct influences on (I)PE, Karl Polanyi was Hungarian, educated in Vienna, and he emigrated briefly to the US and eventually to Canada. Much of the British school's encounter with post-war and earlier Marxism came via Europe: Louis Althusser, Nicos Poulantzas, the Frankfurt School and offshoots, the French 'Regulation School' led by Michel Aglietta and Robert Boyer, and of course Antonio Gramsci and Fernand Braudel, who, combined with Polanyi, eventually yielded Robert Cox's 'neo-Gramscian' synthesis. Postmodernism came out of the 'post-structuralist' intellectual ferment of France, with Michel Foucault and Pierre Bourdieu as founding figures. To round off this highly global picture and to emphasise a point raised by Craig Murphy in this issue, let us not forget Latin American influences such as Raúl Prebisch or Fernando Henrique Cardoso and Enzo Faletto, who also had an impact on US and UK thinking about underdevelopment. Finally, not yet mentioned is the tremendous influence of the study of European integration on both the US and UK traditions in (I)PE, and in this field the Europe-US-UK debates about functionalism, (social) constructivism and institutionalism appear to have remained perhaps more interlinked than in the case of (I)PE, despite 'national' trends. This was accompanied by considerable attention to the comparative political economy of welfare (-state) provision and economic openness which characterised much earlier work of Peter Katzenstein, one of Cohen's 'Magnificent Seven', and this focus still forms (as Helge Hveem also points out in this volume) a bridge of sorts between the two schools.²

If one accepts this 'more oecumenical origins and linkages' story, three points are at stake. Firstly, the British 'school' is in fact part of something much larger, historically embedded and shared. There is a broader shared world in Europe (Scandinavia, the Low Countries, Berlin), Latin America, parts of Asia. It is only in the US where these linkages and broader concerns have become so constrained. European, Asian, Canadian, Antipodean and UK scholars readily relate to each others' work and often and necessarily refer to US scholarship. So really it is the US school versus everyone else, a point echoed by Eric Helleiner in this issue. Secondly, many European and UK scholars always have aspired to methodological rigour and soundness (having invented the idea in the first place), while still asking the broader questions. Thirdly, the origins of *both* of Cohen's schools are heterodox and, as European scholarship migrated to the UK/US over time, much was always shared, and this process of cross-fertilisation has not entirely stopped. (I)PE is still part of a broad European political economy tradition in which the distinction between the national/comparative and the international of (I)PE was more a question of empirical focus than importance. These historical linkages are arguably more durable than is perhaps apparent, embedded transatlantically through scholars who cut across the two approaches.

But Cohen's claim is still essentially correct: there *has* emerged a dialogue of the deaf between US scholars and the rest. The next section looks critically at contemporary UK scholarship in IPE as it has emerged and how the weaknesses identified by Cohen might be shored up by a better combination of the historical

strengths of the British empirical tradition tempered by versions of European–US–UK–utilitarian analytical and theoretical rigour.

The UK school: what it lost and may yet regain

If the US school is hostage to a parsimonious economism and state-centrism, shaped by a ‘methodological nationalism’ and which reduces enquiry to neat and testable hypotheses, I concur with Cohen’s assessment that the UK school has become increasingly focused on the wide-open question of systemic globalisation in a sometimes rather undisciplined way (see also Blyth in this issue). But I am far less convinced that this agenda is overly broad and that, implicitly, one should abandon the notion of political economy as the study of wider social whole (Cohen 2008: 168). That would be to abandon Adam Smith, David Ricardo, Karl Marx, Karl Polanyi and the whole grand tradition of European political economy, from its origins as a study of the difficult social, political, and ethical questions related to the social whole, about how governance, economic transactions and social constituencies interact over time (See Krätke and Underhill 2006). If these scholars could do it, we are all capable of seeing further by standing on their giant shoulders. We should of course welcome sectoral, area/regional and other forms of empirical specialisation, but that does not diminish the possibility of a holistic approach to theory.

There is nevertheless a growing problem in the British school which affects the capacity of scholars to address the questions being posed. Some of course have joined the ‘other side’ (along with colleagues in Europe as well) by demonstrating a rather slavish devotion to the US-school preference for economic data analysis and the testing of narrow and unoriginal hypotheses. On the other hand, in Cohen’s British school the lack of analytical rigour is if anything becoming worse, deteriorating into declaratory theoretical posturing essentially devoid of empirical underpinnings and operating at a high level of systemic abstraction. Uncomfortably close to the quality median, especially among some of Cohen’s ‘third-generation’ British-school scholars heavily shaped by post-modernism and a sloppy understanding of neo-Gramscian approaches, an increasingly postured adherence to ‘critical theory’ does little more than revive an inchoate range of concepts taken from more traditional Marxism, often mixed uneasily with post-modern relativism. This deteriorates into a sort of template oppositionalism to global capitalism, the *scholarly* content of which is difficult to discern. The critical ‘attitude’, opposition to established ‘neoliberal’ order and the identification of the pressures on the vulnerable and the inequities which are undoubtedly an ongoing part of global market integration become virtually an indulgent end in themselves.

Too little attention is then paid to demonstrating that the claims of this ‘template theorising’ are justified in terms of the emerging (mix of) available data and a rigorous analysis thereof, or that the theoretical template provides a better explanation than alternative approaches. Template theorising ‘discovers’ stylised facts, just like worst of the ‘enemy’ economists. To repeat a prescient quote employed by Mark Blyth in this issue: ‘[the US school understands] that data and observation are so unproblematic we can accept them as real . . . the British

that data and observation are so problematic that we must dispense with them altogether' (Cameron and Palan 2009:123).

This is a new departure relative to the long-standing intellectual traditions of British political science and international relations scholarship. British scholars were once almost irritatingly content to lay out the facts in an orderly and largely chronological fashion. The story told itself, prompting Roger Tooze to comment in 1985 that 'the British "historical" approach could benefit from a more coherent, comprehensive, and articulated conceptual basis' (cited in Cohen 2008: 141). Quite so, and Cohen rightly complains that Susan Strange never fully developed her theoretical ideas, being not terribly interested in theory as such, despite her valuable contribution thereto. But there was great virtue in this unstructured British historical empiricism: it set the facts straight and often turned up a range of counter-intuitive truths which then found their way to supporting theoretical musings. This virtue is sadly absent from the postured template theorising of too much contemporary 'Global PE'.

Indeed, Strange's observations of the decline of sterling and of international monetary relations under the Bretton Woods 'system' lay behind her core theoretical insights. The short-lived (1959–71) Bretton Woods system never functioned as it was supposed to, and the IMF never played its designated central role; instead, central bank cooperation based in Basle did the job, yielding a dollar-centric structure featuring the US economy as the world's banker where the US government and private actors could get away with what the others could only hope for. This led to *instability*, not the stability usually attributed to Bretton Woods. The rise of the Eurodollar markets and the role played by international bankers was the key to understanding its collapse, not (just or even most importantly) the US payments deficit.³ While states were important, non-state actors were as well. Structural power was a crucial concept in (I)PE, this structural power was exercised by a variety of actors, and the theory of hegemonic stability was a nonsense because it was a state-centric version of affairs which is not how US power worked and, anyway, the period of most obvious US hegemony did not correspond to a period of monetary stability as the theory predicted.

Many British-school scholars likewise came to theory via expert case knowledge, coupled with posing innovative and even 'grand' questions. This concern with history was as true of UK (versus, for example, French) 'mainstream' Marxists, whatever that came to be,⁴ in considerable contrast to the contemporary UK school propensity to prioritise theory. Asking grand questions is a virtue, but a weak command of the empirical terrain (which takes time and is not an instant gratification exercise like 'theoretical correctness') is inexcusable. So is a failure to explore appropriate methodologies for the different aspects of the analysis (wherein methodology is a *means* at the service of the questions we pose; using a *variety* of means is nearly always better than remaining limited because it will deliver *more* and *different sorts* of data), and a failure to demonstrate empirically if/how a broader model plays out in a case-specific context. The British tradition has lost its empirical moorings and jumped from a rather unstructured focus on the finer details of the story to a rather unstructured preoccupation with 'correct' theorising at its worst combined with a disdain for data.

Additionally, too much the British school claim to ‘critical thinking’ is little more than declaratory posture. The best scholars in this tradition do come up with new questions and responses thereto, but ‘critical’ has come to mean ‘correct’, some variant of radical posture which links tired and familiar questions to equally tired template answers. My last two visits to the annual conference of the British International Studies Association provided anecdotal evidence of the general predicament, and an experience which is reflected in Blyth’s parallel conference experience related in this issue. Too many papers demonstrated a severe allergy to the basic findings of economics and other empirical literature outside the template theory approach. Effort was being poorly expended to collect and stylise what had already been well done elsewhere if one bothered to look. The reply to questioning was essentially that the correct ‘critical’ and/or ‘financialisation’ spin was missing from the ‘other’ literature and thus the latter was not relevant. Knowing one’s (basic) economic concepts and statistics was apparently unnecessary. I was informed (after two decades of my own writing on power and conflict in the political economy of trade and finance) that it was all about power (in a particular way of course) and that the discipline was not about these ‘technical’ questions. How on earth would I have known this had I not happened on this happy gathering with such unique insights into the mysterious workings of the world?

Furthermore, the idea that economists or number-crunchers might actually *disagree* with each other in the literature, and sometimes viciously, was apparently lost, and the idea that there are economists with interesting and important ideas about institutions and their role in society as a whole was *unthought* of. Interacting with scholarship outside a defined theoretical terrain was loudly ruled out in favour of template theoretical correctness, perhaps illustrating that the UK third generation is less interdisciplinary than Cohen’s account implies: the questions may be broad, the approach not always so.

In my view, critical thinking involves questioning *tous azimuths*, in particular questioning what makes *ourselves* as individual or ‘clubs’ of scholars feel most comfortable. The whole point about scholarship is that one’s sentiment and intuition is dangerous to the truth and is *bound* to be for something, for someone (as Cox famously argued), indeed for oneself and one’s sympathisers and no one else. This form of disciple assemblage, which has its unfortunate functional equivalent in the US school or for that matter in Europe, is antithetical to the scholarly enterprise. If economic ‘science’ makes political economists uncomfortable, then at least one should find out what it is about and what has been written. And one is also acutely aware that there are economists pushing at the bounds of their own discipline and who appreciate the fresh air which can be brought in by (I)PE and the other disciplines.

Clearly, the best scholars of the British school need to reclaim the ‘critical’ ground in terms of theory and data analysis. A disdain for the limitations of positivism untempered by neither empirically grounded analysis nor genuinely critical enquiry does not a school make. Yet Cohen’s observations concerning analytical lassitude need not be true of a ‘British’ (I)PE tradition which rightly aspires to responding to grand questions. Section one established that there is plenty in the broad European/UK intellectual traditions that have to do with theoretical,

analytical and methodological rigour in combination with an understanding of political economy as being about the social whole. A properly critical approach and openness must certainly be preserved, but attention to evidentiary standards and systematically establishing the explanatory 'value-added' of specific theoretical claims, however ambitious and 'world-order' oriented, must be reclaimed by rediscovering the UK historical tradition itself. Given UK school origins, this is certainly no concession to the US school, if that is what the worry might be. This would also partially restore a certain balance to the long-run complementarity of the British and US approaches. To bridge the gap, however, something also needs to happen on the US-school side of the equation, but the common European-British origins of both leaves room for optimism here too, though the process may take time.

By way of example: from complementarity to bridging the gap?

Arguably, Cohen's book and the above analysis indicate that the US school may be in bigger trouble than its 'British' equivalent. The British school may have lost its cautious empirical moorings, but it remains well integrated with its broader shared European and transatlantic- (which includes Canada and Latin America) plus antipodean-oecumenical heritage and with contemporary scholarship across a range of national and regional traditions in Asia, Europe and elsewhere. Repairing the damage only concerns the worst, not the best, of British-school scholarship. On the other hand, the isolation of the US school relative to (I)PE scholarship in Europe, the UK and Asia may be splendid and may represent the best of what the US approach has to offer, but like so much else in the US which has developed over the past two decades or so, it is isolation nonetheless. There are of course reasons why there remains (limited) interaction between the schools, but the reign of overly abstract rationalist modelling based on quantitative methodologies testing narrow, micro hypotheses will be difficult to break in US (I)PE.⁵ The fact that the quality is as high as it is observed to be is part of the problem.

Yet this section is dedicated to arguing that Cohen is overly pessimistic in terms of the prospects of bridging the gap. The key lies in the British school and its European and other cousins. As argued above, a range of UK and European scholars have enthusiastically and perhaps slavishly embraced the US-school obsession with methodology. Already there is potential if insufficient condition for interaction. Still too many qualitative-methods British-school scholars remain highly sceptical about the net worth of employing quantitative methodologies *tout court*, and at its worst this has become an ideological stumbling block antithetical to a critical scholarly concern for discovery. While Cohen's own principled objections concerning the limitations of rational choice and quantitative methods clearly hold water, this is certainly no argument in favour of ignoring these altogether and Cohen never meant it as such. I strongly suspect that the postured and allergic opposition to quantity lies as much in self-interest as principle: in a weak capacity of many scholars to employ quantitative methodologies at a competitive level. This derives from a long-run blindness in the training of undergraduates and postgraduates combined with a failure to explore the

economics and (I)PE literature which uses such methods – a point echoed by Cox and Weaver in this issue. Those who ‘cannot’, so to speak, have defended their incapacity as a principled objection. I do not mind admitting that I was too old to have received the required methodological training at the undergraduate level (that came at the MA stage in the 1970s in many universities in both the US and Canada), and then I went to Oxford where at the time there was combined an absence of postgraduate attention to methodology with a too-frequent, proud if misplaced opposition to standards of theoretical and analytical rigour in general. Thus to complete my thesis I of necessity gave myself a self-taught crash-course in industrial economics, international trade, the use of and problems of aggregate comparative national accounts and other statistics, and in the cost structures and the terms of competition among developed and developing country business enterprises in various sectors. If most of the literature available was in economics, my questions were very much associated with political economy. These various excursions have in the eyes of referees over the years neither hurt my ability to infuse my very qualitative methodological approach with a serious discussion of quantity, nor to focus on issues of sociopolitical conflict and power, nor to deal with theory and ask the ‘big’ questions, including the normative ones.

An important point is that while a high-level mastery of multivariate statistical analysis requires considerable devotion and specialisation, the basics are not overly difficult. They should simply be learned and students should be exposed to the range of methods available and their possible uses as means to varied sorts of enquiry. Method does *indeed* constrain what questions might be either asked or answered, but that is a particularly elementary point and it applies to *all* methodologies, not just rational choice or quantitative approaches. This implies flexibly deploying a range of methodologies appropriate to the theoretical and empirical context involved, including the availability of different sorts of data. This of course implies that US-school methods teaching should commensurately be broadened so as to alert students to the appropriate role of both qualitative and quantitative approaches, and in a number of places this has always been the case anyway. The British tradition of pragmatism could nonetheless show the way here. US-school methodological die-hards would have a lot less to be smug about if British-school scholars more consistently demonstrated the requisite attention to evidentiary and methodological standards. It is worth finding out why the ‘others’ think the way they do, and it can only add to, not detract from, one’s own scholarly and *critical* capacity, a point echoed by Catherine Weaver in this issue. If political economy is by nature interdisciplinary, as Strange and other pioneers correctly argued, then an interdisciplinary and inter-methodological capacity can only prove positive.

There are additional reasons to resist Cohenian pessimism. The first is the very deep roots of the shared European heritage and the ‘complex interdependence’ of ongoing transatlantic intellectual traffic which cuts across social science disciplines. These roots have not yet been pulled up in the US school. The very nature of (I)PE as a discipline tends to yield such a situation. Secondly, and no doubt as a result, there *is* frustration in the US and there is an audience for Cohen’s argument about the costs to be born the more the US school pursues

the current track. There are also institutional economists, as argued above, who are pushing at the bounds of their discipline by asking political economy questions, particularly in development economics. The emphasis of research funding councils on interdisciplinarity certainly helps and this should be pushed yet harder. Financial crisis has also helped discredit market fundamentalism and the methodologies which accompanied its rise, and has clearly opened the book on normative issues of the legitimacy and sustainability of market-driven processes and their governance at national and global levels. Most important is the ongoing presence of transatlantic 'bridging scholars' of the first, second and even third generations who are strongly rooted in the grand tradition of political economy yet care greatly for intellectual rigour (Cerny, Murphy, Katzenstein, Gourevitch, Pauly, Hiscox, Stasavage, Boix, Burgoon, Scharpf . . . the list could go on and Cohen's does). The current gulf identified by Cohen is unlikely to remain static.

Conclusion

One might conclude here by pointing out that Cohen's analysis also points to a certain relationship between the questions asked in (I)PE scholarship, and the state of global affairs (for example the differences during and post-Cold War). Scholars are naturally and rightly drawn to what is going on around them. If this is the case, then one might note that the financial crisis and seismic shifts in the global economy which have gone with it have once more placed us in 'interesting' times. These are world order questions. Just as the United States itself will likely be drawn by dramatic world events out of its current phase of unilateralist introspection, so American scholarship in political economy will need to reconnect with its broader heritage. If US scholars fail to ask the larger order questions, then this time around others may provide answers for them. The rest of the world can show them how this might be done.

Notes

1. As Cohen demonstrates, the levels of analysis distinction became crucial for the US 'school', yet this was and remains much less the case for the UK school. Part of the argument in this piece is that 'PE' is the appropriate label to build the necessary bridges. This article recognises the levels of analysis tension within the discipline by employing the abbreviation '(I)PE' in place of Cohen's (and the more typical) 'IPE' formula.
2. See, for example, the (sometimes co-authored) work of (Australian) Michael Hiscox at Harvard and (American) Brian Burgoon at the University of Amsterdam, who have published frequently in the pages of *International Organization* and the *Review of International Political Economy*, among other outlets.
3. Few bother to read Strange's longest and best work, *International Monetary Relations* (1976).
4. See, for example, the avowedly Marxist but essentially empirical works by Peter Burnham (1990, 2003).
5. As Cox and Weaver argue separately in this volume, this may have a deal to do with the nature of US postgraduate training programmes.

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