

Historical International Political Economy

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Abstract: The field of International Political Economy (IPE) is steeped in history; and most IPE scholarship today remains historical in a broad sense. Yet, IPE scholars have never been more diverse in their modes of engagement with history, in the influence they ascribe to the weight of history, or in their beliefs about the potential for human progress. This chapter analyzes each of these developments. First, it considers the field’s major modes of engaging history: holistic, comparative, and deep. This includes the use of both old and new historical methods, techniques, and approaches. The chapter then surveys the broad range of views scholars take on the influence of the past on subsequent outcomes, from accounts of strong path dependency to those of perennial change. Last, this chapter discusses the evergreen debates about whether, and how, studying the past can lead to our emancipation from it. Throughout, the chapter highlights opportunities for more “constructive engagement” across distinct bodies of scholarship.

Keywords: history, historical, progress, historical methods, path dependency, trans-Atlantic divide

Introduction: We Are All Historians Now; and We Always Have Been So

The field of International Political Economy (IPE) was born of history. This is true in a formal sense as several of IPE’s “intellectual entrepreneurs” emerged from history departments (Cohen 2008, Introduction). But it is also true in a deeper sense. The great puzzles in those early days, concerning the evolving structures of the global political economy, were overtly historical (Cox 1983; Kindleberger 1986; Krasner 1976; Strange 1987). This is hardly surprising given that each of IPE’s major paradigms—liberalism, mercantilism, and Marxism—is historical in its own way. Historical analysis transcends the classic “rationalism-constructivism debate.” Several of the central concepts

in neoliberal institutionalism—repeated play, the shadow of the future, and reputation—are temporal in nature. In this approach, actors’ decisions at any given point must be analyzed within the longer-run “historical” context of the dynamics that preceded this moment and the consequences that were expected to follow (Axelrod and Keohane 1985; Keohane 1984; Oye 1986; Snidal 1985). So, too, with actors’ identities, ideas, and interests—the construction of which is a historical process (Goldstein 1993; McNamara 1998).

Conventionally, “historical method” connotes analyzing evidence from the archives in narrative form. But what is an “archive”? What counts as “historical evidence”? And what is a “narrative”? If all the vital facts were preserved within ministerial “structure[s] of glittering white concrete,” we should have our answers easily (Orwell 1961, 4). But that is less true now than ever before.

Indeed, historical IPE spans all methodological cleavages as scholars pursue knowledge and understanding in ever more places using a growing set of tools. Consider two recent publications in the *Review of International Political Economy*: Sahasrabuddhe’s analysis of federal currency swaps during the Global Financial Crisis; and Jamal and Milner’s survey experiment on Tunisians’ support for globalization. Initially, it seems that the articles have little in common. But, from a broader view, both relay narratives/findings of how actors/respondents reacted to a shock/treatment. Both do this toward the end of teasing out broader lessons/implications about the role of ideational and material variables, even as both acknowledge the limitations of generalizing from their limited cases/samples. The principal difference lies in the nature of the experiment analyzed—natural versus contrived (Jamal and Milner 2019; Sahasrabuddhe 2019). Yet, once the experiments have run their course, both are history.

Thus, in a most banal sense, historical IPE is simply the study of IPE “in the past.” But empirics are always things that have happened “in the past,” whether that “past” was a few millennia or a few moments ago. These days, there is rather little in IPE that is devoid of empirics. But to say that “we are all [historians] now,” is not to suggest that we all “do history” in the same

way (Friedman 1966; “We Are All Keynesians Now” 1965). Here, there is “constructive non-engagement” among different groups of scholars who grapple with history in different ways, who see history as operating on different scales, and who engage history for different purposes.

How We Engage History

Broadly, scholars take three approaches to history in IPE. There are those who consider the history of the whole global political economy as such. A second group compares the paths of particular units within that system. Finally, there are those who trade breadth for depth.

Holistic IPE

Following Marx, Andre Gunder Frank famously argued that global political economy can only be understood “holistical[ly]”—“in terms of the whole world economy/system” (2007, 39). Frank combatted the traditional “stadial” theories of development—dating back to Adam Smith—in which countries progress through a “succession of capitalist stages” with “today’s underdeveloped countries . . . still in the stage, sometimes depicted as an original stage, of history to which the now developed countries passed long ago” (Frank 2014, 106). Instead, Frank argued, “contemporary underdevelopment is in large part the historical product of past and continuing . . . relations between the satellite underdeveloped and the now developed metropolitan countries” (2014, 106).

Wallerstein distilled Frank’s perspective into World-systems Theory. “Capitalism,” Wallerstein explained, “was from the beginning an affair of the world-economy and not of nation-states” (1974, 401). For him, the “basic unit of observation” was “a concept of world-systems that have structural parts and evolving stages.” Only “within such a frame-work,” can one “fruitfully make comparative analyses—of the wholes and of parts of the whole” (1974, 415).

The Hegemonic Stability Theorists were quintessentially holistic in their approach. They explained the overall structure of the international system with reference to the “distribution of potential economic power” among the systems’ states. For them, a given state’s foreign economic

policies could not be understood apart from that state's position within the structure at the time (Krasner 1976).

While Hegemonic Stability Theory is now seen as passé, holism continues to thrive among many of today's critical histories of the global political economy. In particular, Trotsky's infamous rendering of history as the story of "uneven and combined development" has found new life among those scholars seeking to take the history of the global political economy beyond "Eurocentric" tropes (Anievas and Martin 2016). Even those who have complicated World-systems Theorists' "homogenous" reductions similarly describe "a system that was simultaneously both intensely connected and deeply divided." These views are not less—but more—holistic. "Global modernity," Buzan and Lawson argue, "pulled the world into a *single* system." The "global transformation" of the nineteenth century "foster[ed] the emergence of a *full international system*."ⁱⁱ In each of these holistic views, the international system has not only a history but also a trajectory. But where, precisely, are things going? All the major permutations have been predicted. The Hegemonic Stability Theorists—particularly Gilpin—saw a cyclical tendency (Gilpin 1987, xiii). Frank, by contrast, hailed "the return of the world's economic centre to Asia, where it had always been before the past period of temporary Western ascendance" (2007, 52). Wallerstein predicted something like chaos. "We have now entered into the post-American era, but also the post-liberal era," he proclaimed. It "promises to be a time of great world disorder, greater probably than the world disorder between 1914 and 1945 . . .," he warned (1993, 4). Buzan and Lawson, by contrast, see history unfolding along a certain path but not necessarily toward any knowable endpoint. They see the twentieth and twenty-first centuries as "close kin" to the nineteenth century; but we have yet to see the full realization of the dynamics unleashed in that previous "global transformation" (2015, 5).

Comparative history

Those writing comparative history reject what they see as an over-emphasis on the global setting of particular elements and episodes of foreign economic policy.ⁱⁱⁱ They do *not* suggest that "the

international system”—however understood—is irrelevant. But, for them, the international is just one of the several “levels” from which influences emanate. They compare distinct experiences and moments both macro- and microscopically. This increases the number of observations and, at its best, enables the formulation of generalizable lessons.

These days, comparative history is the most prevalent approach to history in IPE, and there is no shortage of exemplary work. It might be most useful to highlight the underpinnings of this approach by examining two of the pathbreaking pieces that have exerted an enduring influence on the discipline.

First, Rogowski’s classic work on trade and domestic political cleavages was theory-driven. From the start, he acknowledged that his “preliminary survey of the evidence” was “sketchy” (1987, 1127). But even his more extensive case analyses were brief. For instance, he wrote, “The details of the US experience are too familiar to require extensive discussion . . . workers joined owners in support of a triumphant, highly protectionist, and increasingly imperialist Republican party” (1989, 43–4). And, he conceded, “the present theory can offer no explanation whatever for the acute class conflict that characterized Britain in the interwar years” (1989, 82).

Second, Simmons’s classic analysis of interwar foreign economic policy squared off directly against the central accounts offered by the Hegemonic Stability Theorists. Like the holists, she recognized that states’ different positions within the global economic structure shape the constraints and opportunities that their policymakers face. Yet, she highlighted, this was insufficient to explain why similarly situated countries reacted to their circumstances differently. It was thus necessary to “supplement” “systemic international relations theory” with historical analysis of the “*variations* in policies over the two decades and from country to country.”^{iv} She concluded, “the ability to comply with internationally accepted adjustment norms may be conditioned by the domestic political and institutional variables that influence a country’s macroeconomic tastes more generally” (1994, 46).

Deep history

A third category of IPE scholarship emphasizes the cross-temporal and cross-sectional variation that marks the global political economy. It celebrates the “messiness,” as Phillips and Sharman put it, of “the heterogenous international systems that have predominated for most of world history”—“international systems,” plural (2015, 3–4). It revels in the distinctiveness of particular cases and contexts rather than pressing their comparability. This can lead to mere “story telling” in which we learn more and more about less and less. But deep history trades generalizability for analytic precision and theoretical parsimony for empirical robustness.

Whereas comparative history is often theory-driven, deep history typically begins with the history—the empirics. Consider how Schonhardt-Bailey casts the puzzle in her landmark analysis of the repeal of the Corn Laws in 1846:

This is a book about an anomaly of spectacular proportions. For more than 150 years, researchers have sought to unravel the puzzle of repeal. Indeed, this anomalous case continues to spark the curiosity of leading political scientists on both sides of the Atlantic into the twenty-first century. (Schonhardt-Bailey 2006, 2)

Schonhardt-Bailey was intrigued by the analytical challenge presented by the elusive puzzle. She renounced “the attractive . . . simplicity” of an account based exclusively on “economic interests” and instead grappled with the “thicket of overlapping and intertwined interests, ideas, and institutions” (Schonhardt-Bailey 2006, Preface, 2).

But it is not just that Schonhardt-Bailey developed a multi-causal explanation. Her deep history *emphasized* the complexity of the course of events:

Britain’s bold move to free trade in 1846 . . . violated the core protectionist ideology of the Conservative party while simultaneously undercutting the economic interests of the ruling landed aristocracy. The Conservatives entered government in 1841 with a strong and . . . unified commitment to protecting agriculture, and yet . . . Prime Minister . . . Peel, completely reversed this stance within five years. (Schonhardt-Bailey 2006, 1)

This is nothing like the “clearly defined” “stable class cleavages” described by Hiscox (2002, 71). Nor was it reducible to Rogowski’s just-so story in which “capitalists and labor . . . unite[d] effectively in the Liberal party and forced an expanded suffrage and curtailment of (still principally landowning) aristocratic power” (1987, 1125). Schonhardt-Bailey shows that these simplified explanations are manifestly inaccurate. And they gloss over the far more profound puzzle. “Rarely,” she notes with delight, “do we find a case in which political representatives acted seemingly against not only their own economic and political interests but also against the mandate on which many of them were elected” (2006, 1). And, yet, this is precisely what happened in 1846. Schonhardt-Bailey resolves the puzzle only by working through thousands of pages of primary source material, analyzing them with a mix of traditional and cutting-edge methods across hundreds of pages.^{vi} Staying with the UK for illustrative purposes, deep histories reveal the same types of limitations with comparative histories of the interwar period as well. Simmons’s “explanatory domestic political variables” include regime type, the political orientation of party in power, labor unrest, government instability, and central bank independence (1994, 42–6). Such a general typology is necessary for useful comparison across contexts; but the fit with specific cases is always subject to contention.

For instance, Simmons scores the Bank of England—the “most independent of central banks”—as fully independent across this period (1994, 230, Appendix II). But deep histories show a more complicated story. During and after the First World War, Bank Governors Cunliffe and Cokayne broadly deferred to the Treasury in setting monetary policy. Montagu Norman came to power in 1920 determined to reverse course. He spent the next two and a half decades establishing the Bank as the ultimate arbiter of macroeconomic policy. This reached a crescendo in 1931, when the Bank dictated fiscal (as well as monetary) policy to the government in its misguided campaign to save the gold standard. Thus, the Bank’s “independence” was hardly invariant. More importantly, the Bank’s securing of its “independence” is itself a key variable to explain.^{vii} On regime type, Simmons counts the UK among those powers “that remained democratic for the entire period”

(1994, 293). Yet, the UK's "regime-type" was itself transformed within this period. Prior to 1918, all women and poor men (42 percent of the total male population) were disenfranchised. The Representation of the People Act 1918, granted wealthy women the right to vote and removed the property requirements for men, expanding the total suffrage from eight to twenty-one million. It was only with the 1928 Equal Franchise Act that women were given voting rights on the same basis as men. One hesitates to count a country a "democracy" when one-third of the adult female population is denied the right to vote.^{viii} Substantively, this matters because those most negatively affected by the foreign economic policies of the interwar period were those poor men and women whose voting rights were most restricted. Moreover, as Teele shows, these changes in the suffrage themselves interacted with the broader struggle between the working class and the Conservative and Liberal elites (2014).

This oversight is part of an oversimplification of the domestic politics that defined the UK's response to its changing position in the international system. The typical story has been that the UK left the gold standard in 1931 in the context of "labour unrest" and an "unstable", "Left-wing Government" (Simmons 1994, 281). Indeed, the Labor government—supported by the Liberals—was so "unstable" that it fell entirely just prior to the suspension of gold convertibility. But it was replaced with an effectively *Conservative* government. The "unrest" at that time was nothing compared to the strikes in 1919 and 1921, let alone the 1926 General Strike—the largest strike in Western European history (Shefftz 1967, 387). No doubt, this was due to the Parliamentary Act severely *restricting* labor rights in 1927. Even prior to this, however, the "unrest" did nothing to thwart the return to gold or to "reform" *laissez-faire*. Indeed, the 1931 General Election saw the Conservatives win the largest electoral mandate in modern British history by promising to save the gold standard. They did not deliver but, instead, introduced protection, after it was "no longer necessary" economically (Keynes 1931). More broadly, the leading parties' orientation on the major issues—trade and the gold standard—shifted considerably across the interwar period.^{ix} This revolution followed from both the "unintended experiments" of elites and the "everyday discourses

constructed by mass public agents” (Morrison 2016; Seabrooke 2007, 795). In all of these ways, the history of political-economic reality defies the elegance of generalized IPE theory.

Specialize and trade for richer histories

It is worth considering the UK’s lead toward, and away from, the liberal international order for several reasons. First, the UK has had an outsized effect on the trajectory of IPE across the last several centuries. For better and for worse, the decisions made in London substantially altered the constraints and opportunities for many other actors, from Dublin to Delhi. Perhaps for this reason, the UK has assumed an altogether outsized importance in the field. From the Hegemonic Stability Theorists to the pathbreaking comparativists, the UK has been IPE’s indispensable case. And despite that it is also the most heavily conceptualized case, new methods and new perspectives continue to grant new purchase on even this well-trod ground.

So, it is worth understanding the dynamics that gave rise to those world-changing shifts emanating from the world’s “core” political-economic centers. Such deep histories might evade the sin of Eurocentrism by disposition, as they interrogate how some powers assumed such influence at these pivotal moments. But they do not counter it. The field needs more such deep—and deeply informed—historical analyses of other places, like those offered by Pepinsky (2007), Naqvi (2018), and Dafe (2019) in recent years. Such new histories provide fertile ground for building and testing theory in less familiar contexts. So, too, does the field want serious engagement with non-Western theories of IPE as such, like Chey’s (2019) exemplary analysis of the history of Korean IPE thought. Taken together, such work offers starting points for crafting “connected histories” within IPE (Subrahmanyam 2004a; Subrahmanyam 2004b).

But whether even a multitude of rich case studies offers generalizable lessons can only be determined by studying history comparatively and globally. Ultimately, IPE benefits from historical work done in all three modes: holistic, comparative, and deep. As Pevehouse and Seabrooke (2020) remind us, it is more important than ever that scholars bridge such divides. The oldest lesson of IPE

remains true here as elsewhere: we all gain when we specialize according to our comparative advantages and then trade the fruits of our labor.^x

How Much Does History Matter?

Originating in the subfields of American and Comparative Politics, *historical institutionalism* has become increasingly central to the analyses of IR and IPE. Broadly, there is agreement about the pre-eminent *ways in which* the past shapes the present. “That tradition’s core,” Fioretos (2011, 370) explains, “includ[es] the role of founding moments in shaping later developments, how institutional legacies affect the degree to which power resources can be harnessed, and the ways in which varied patterns of incremental adaptation shape institutions over time.”^{xi} Yet, there is little consensus about *the extent to which* history exerts its influence. From Brexit to the rise of Trumpism, even the most surprising shifts in global political economy of the recent past are subject to the timeless debate over the relative weight of structure and agency, inevitability and contingency (Peterson 2018; Thompson 2017). This section will review exemplary work from both perspectives.

History’s heavy hands

When asked to judge the consequences of the French Revolution, Chinese Premier Zhou Enlai is reputed to have replied, “It is too soon to tell.” While apocryphal, the quip epitomizes his long-run perspective. Such is the heavy hand of history that its effects must be gauged across at least centuries—perhaps millennia.

Some of the most prominent historical work in IPE emphasizes the ways in which history compounds the effects of small differences over the (very) long run. Most famously, Acemoglu, Johnson, and Robinson identified colonial-era institutions as among “the fundamental causes of the large differences in income per capita across countries” today (2001, 1369). In the decades since, subsequent analyses have enriched our understanding: by showing that such initial differences may produce complex dynamics that evolve over time (Stasavage 2014); by exploring the origins of such

key institutional changes (North et al. 2009); and by revealing that such divergences may go back even further—and affect more today—than previously recognized (Gaikwad 2020).

Similarly, many of our most important ideas and identities prove remarkably stubborn. This is true for leading individuals, most of whom refuse to “learn” even as new facts come to light (Blyth 2013; Chwieroth 2009; Nelson 2017). But this is also true for states and international organizations. Consider Zarakol’s recasting of “reputation” as *stigma*, “a label of difference imposed” on, and internalized by, those actors judged as “backward” by the “modern” state system. Even as countries’ circumstances shift radically—and regimes become “zombies”—perceptions, and behavior, can remain much the same (Gray 2018; Zarakol 2011, 4).

Today, leading international regimes theorists see history as continuing to loom large. While he has acknowledged the importance of critical junctures and founding moments, Ikenberry contends that “the old American-led hegemonic system” has proven “durab[le]” even in the face of “remarkable upheavals in the global system—the emergence of new powers, financial crises, a global recession, and bitter disputes among allies over American unipolar ambitions” (Ikenberry 2011, 6). Such accounts are partly explicable in terms of path dependency: any such entrenched order is unlikely to be overturned easily. But the American-led order maintains its inertia today partly because it has demonstrated (repeatedly) that it “works” (Drezner 2014, Ch. 2).

For the global order’s critics, it still “works” for the wrong sorts. Initially, there were hopes that the Global Financial Crisis might signal the “last gasp of [the] old-fashioned concert of great powers” (Woods 2010, 51). In the years since, it seems that Strange’s decades-old warning was prescient. The “persistent myth of lost [American] hegemony” has only fanned the flames of America First and American ego-centrism, even while the US has remained vital to the structures of the global economic order (1987). For Frank, even the resurgence of China, one of the largest potential destabilizers, is intelligible only in terms of the “long historical continuity” of the processes of the “Rise of the West” and “[t]he (cyclical) decline of Asian economies” (2007, 39–40).

But is the global political-economic order really so obdurate?

Making history

“The power to become habituated to his surroundings is a marked characteristic of mankind,” Keynes wrote in the wake of the First World War. Yet, he warned, “[v]ery few of us realise . . . the intensely unusual, unstable, complicated, unreliable, temporary nature of the economic organisation by which Western Europe has lived for the last half century” (Keynes 1920, 3). Frieden repeats the warning: “Today’s international economic order also seems secure, but in historical perspective it may be only a brief interlude. The historical forces that shaped the world economy in the twentieth century continue to determine today’s version of globalization and will decide its fate” (2007, xvii). In such accounts, the structure of international politics is supple and the stream of time, mercurial.

Several histories of the Anglo-American postwar order paint images of perennial change, complete with regular moves by the leading powers to exit and re-make the principal institutions that comprise this regime complex. Gowa (1983) showed this in the case of the International Monetary Fund (IMF) in her classic study. Barton et al. (2006) have told an analogous story in their history of the trade regime. The story is not as plain in the case of the World Bank. But, as Allan (2019) shows, the US steadily advanced a “neoclassical growth imperative” across the postwar period, even as the content of that vision evolved over time. Regardless of the direction of change, such accounts remind us that change is the norm, that our multilateral economic institutions are always contested and always evolving, as both Seddon (2017) and Kaya (2015) describe.

So, too, with actors’ all-important reputations. Analyzing sovereign debt across three centuries, Tomz (2012) shows that one’s “reputation” is far more nuanced—and mutable—than we have traditionally assumed. One way to alter a reputation, Gray (2013) finds, is by changing the “company states keep.” It seems that Benjamin Franklin’s aphorism that good reputations are hard-won and easily lost applies to IPE as well.^{xiii} Moreover, not all of the challenges we face are familiar. Climate change is without precedent and, arguably, without even analogy. But “the *idea* of climate emerged from a process of co-production in which both state imperatives and scientific knowledge

shaped and reshaped the form and content of the climate.” It was *constructed* as an “epistemic” “governance object”—which is to say that there is a history to our understanding of, and approach to, the climate crisis.^{xiii} More broadly, all “[c]rises”—and the “appropriate” responses to them—“. . . are constituted in and through narrative” (Hay 1996, 254).

Emancipation(,) from History

At its best, historical IPE is emancipatory. Studies of trade sometimes read like so-called “Whig histories,” with their notion that “cooperation” is “Pareto optimal”—or, at least, that trade war is typically suboptimal (Krasner 1976, 318). Such narratives tend to elucidate those factors that have enabled liberalization—read: liberation—from primitive protectionism.^{xiv}

In other areas of IPE, the multiple equilibria are more ambiguously ranked. Scholars of money are acutely aware that the story of international monetary order across the last several centuries includes at least as many steps backward as forward. This history proves invaluable for putting the present into proper perspective (Helleiner 2003). So, whether we are considering the “future of money” (in general) or just the future of particular challengers to dollar hegemony, historical analysis is a necessary precursor, even if only to reveal the limits of precedent (Cohen 2006; Matthijs and Blyth 2015; McNally and Grun 2017, 603).

The field of migration is darker still, as the work of Peters and Watson shows. At first blush, their treatments appear to be a world apart—emblematic of the “trans-Atlantic divide” (Cohen 2007). Peters uses a positivist, mixed-methods approach to analyze the empirical evolution of the international migration regime across two centuries; while Watson offers a reflexive account of the use of *Robinson Crusoe* in economics textbooks to “construct” a particular “theory of market exchange” (Peters 2017; Watson 2018, 547). Yet, Peters’s “policymakers” and Watson’s *Crusoe* (as lawgiver) both overtly treated the subjects of rule as “nothing more than a combination of latent labouring possibilities” (Watson 2018, 550). In both analyses, we see that international migration regimes depend upon the broader social, political, and economic structures in which they are generated. The disappointing conclusion is that anyone seeking to understand the regime governing,

say, North African migrants in Europe today, must recognize both that these individuals are evaluated in a “raced market frame” and that their “rights” are determined by a complex of international arrangements that govern how capital is mixed with labor and how goods are brought to consumers.

The same is true with other frames—such as gender—and across other issue areas within IPE, as Elias and Roberts (2018), among others, show. In all of these areas, exploring how things became the way they are is a necessary preliminary to advancing alternatives. But whether the mode is excavation or deconstruction, laying bare the foundations of the present is a preliminary to renovation.

For others, history is invaluable as a means to revisit roads not taken—and to reassess our shared narratives about the roads we have taken. Chang’s brief but provocative book employs “the historical approach in the study of economic development” both as a source for “new” ideas and to furnish empirical evidence in support of the proposals that follow there from (Chang 2002, 8). This is, itself, reminiscent of the German Historical School’s infamous rehabilitation of mercantilism in Imperial Germany (Schmoller 1896). And so, too, is the response. Just as Viner challenged the historical veracity of Schmoller’s account, so too has Irwin challenged Chang (Irwin 2004; Viner 1948).

As ever, our understandings of the past shape our perceptions of the present—and vice versa. This is why we always have done—and always shall do—history in IPE.

Conclusion: The Future of Historical IPE

From “Make America Great Again” and Brexit in the West to the textbook controversies in the East, the “culture wars” over the rendering and meaning of history have never loomed larger (Bell and Vucetic 2019; Shin and Sneider 2011; Watson 2019). IPE is uniquely positioned to shape those debates. Its deep, expansive roots in historical analysis offer a wealth of wisdom. Its appreciation of the political and economic consequences of narrative ensures an earnestness too seldom supplied in

an age of politics-by-Tweet. Its commitment to inclusion across sectional divides promises a diverse array of perspectives, a powerful toolkit, and a global sensibility. We have much to offer, indeed.

But realizing that potential requires more “constructive engagement” across our traditional cleavages. Our history is too rich and our understanding of it, too important, to leave it to any particular method of analysis, to any single mode of engagement, or to any specific locale of scholarship. We have always been historical, and we shall always be so. But the histories of the future need not look strictly like the histories of the past. Let us couple the tried and true modes, traditional methods, and familiar foci with new approaches, modern methods, and an ever more diverse cast of characters and contexts. This will make our accounts more inclusive, more interesting, and—crucially—more accurate. By deliberately bridging these divides, we will see more of our world—and see all of it more richly.

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Notes

- i The classic liberals believed in the promise of progress–improvement over time. Locke (1967) *Second Treatise*, Ch 5 §§ 39–51 (Hume 1987); Smith (1976) Bk III. Marxists contend that the march of history is resisted by entrenched elites defending their interests (Marx 1977, 176–98). The mercantilists used history to expose liberals’ hypocrisy (Schmoller 1896).
- ii Emphasis mine. Buzan and Lawson (2015, 2–3, 9).
- iii Rogowski, for instance, quite deliberately followed Barrington Moore’s “comparative sociology” Rogowski (1989, 33).
- iv Simmons (1994, 3, 13), emphasis in original.
- v Cf. Rogowski’s suggestion that “Britain” “at the middle of the nineteenth century” “can stand as the surrogate for the advanced and labor-rich economies of northwest Europe generally” Rogowski (1987, 1128).
- vi Dean (2016) has similarly highlighted key problems in these comparative historical accounts of US trade policy in the late nineteenth century.
- vii Moggridge’s several books and Sayers’s 3-volume history work through these questions. Moggridge (1969, 1972); Sayers (1976).
- viii UK parliament (2021). The UK’s Polity IV score was an eight from 1901 through 1922, when it then jumped to a ten (Marshall et al. 2018). In 2016, it reverted to an eight (Marshall et al. 2018). Of course, Polity scores are not calculated based strictly on the size of the suffrage in national elections. But given the massive changes in the rights to vote in 1918 and 1928, it is hard to understand the reasons for these codings. This author has raised these concerns with the authors of the Polity IV dataset, but they have not responded at the time of publication.
- ix To his credit, Hiscox noted the “substantial decline in party cohesion” in this period. But, surprisingly, he explains these changes without any reference to the gold standard (Hiscox

2002, 79). Williamson explores at length the centrality of the gold standard to these other shifts (Williamson 2003).

- x Blyth made this point to great effect in his roundtable presentation at the IPE Society meetings at the University of Pennsylvania, November 15, 2008.
- xi See also Fioretos et al. (2016).
- xii By leaving the gold standard in 1931, the UK liquidated the credibility it had built up by maintaining a stable gold value of the pound (almost without exception) for more than two centuries (Morrison 2016).
- xiii Allan (2017, 154–7), emphasis added.
- xiv Morrison (2012) is an extreme example. But other work within that literature has a similar flavor (Bailey et al. 1997; Hiscox 1999; Irwin et al. 2008).