OCCLUDING THE GLOBAL: ANALYTIC BIFURCATION, CAUSAL SCIENTISM, AND ALTERNATIVES IN HISTORICAL SOCIOLOGY

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Historical sociology has not been as global as it might be, instead remaining tied to various forms of state-centrism. This paper explains why and suggests some strategies for redressing the problem. Focusing mostly upon 'second wave' historical sociology, it argues that historical sociology's occlusion of global and transnational forms, dynamics, and processes lies in its analytic infrastructure which analytically bifurcates social relations across space and emphasizes a variable-based causal scientism. Overcoming the occlusion requires rescaling the objects of study and seeking descriptive assemblages of global and transnational forms, dynamics, and processes.

Keywords: *historical sociology, state-centrism, transnational forms, analytic methodology, analytic bifurcation, causal scientism, world-systems theory, global processes.*

Historical sociology in the USA has produced novel insights on various dynamics, forms, and processes (Adams, Clemens, and Orloff 2005). It has illuminated state-formation, revolutions, class-formation, political cultures, the state, the development of the professions, and transformations in economic systems. It has also offered innovative theoretical and methodological insights regarding the role of historical sequence, path dependency, the place of narrative in social life and the use of narrative as an analytic methodology. But there is at least one area about which historical sociology has been comparably silent: global and transnational forms and processes. The one exception is the early work of Immanuel Wallerstein and the subsequent school of world-systems analysis (Manning and Gills 2011; Wallerstein 1974, 1980, 2011a, 2011b). World-systems studies are global and transnational to be sure (Chase-Dunn and Babones 2006; Chase-Dunn and Hall 1997, 2012; Hall 1998; Hall and Chase-Dunn 2006). But this is the exception that proves the rule. Institutionally, world-systems analysis has branched off from historical sociology. Bracketing world-systems analysis, therefore, it is not unfair to say that conventional historical sociology has remained comparably uninterested in global and transnational processes. While there is now an emerging and promising strand of the 'third wave' of historical sociology that has globalized its focus and orientation, much more needs to be done (Adams, Clemens, and Orloff 2005: 57–63; Magubane 2005).[1]

The occlusion of the global and transnational is counter-intuitive from a certain standpoint. Global and transnational processes *should* be the objects of interest to historical sociologists. As a regime of thought as well as a disciplinary formation, histori-

cal sociology was founded upon an interest in the emergence, constitution and social complexities of modernity – or as Adams, Clemens, and Orloff put it (2005: 2), in 'how people and societies became modern or not'. And we know, not least from world-systems analyses, that modernity has never been a national phenomenon. It has been a transnational and global development, occurring on scales higher than national states. If historical sociology is interested in modernity, it is not unreasonable to think that it might also be interested in a larger project of illuminating the emergence, construction, and dynamics of modernity on transnational and global scales. Besides, historical sociology's colleagues in history have already globalized their discipline: 'global history' and 'transnational history'. And Presidents of the American Sociological Association like Michael Burawoy and European theorists like Ulrich Beck have called for sociologies that are more global in method, theory and conceptualization (Beck 2006; Burawoy 2008), thereby joining the ongoing calls of world-systems analyses. In this, historical sociology lags behind.

The issue is *not* that comparative historical sociology has narrowed its lens to Europe or the United States. As historical sociologists themselves defend, non-European parts of the world are on the agenda already (Mahoney 2010). Nor is it a question of looking at 'inter-*national*' issues. Historical sociologists have looked at the international system already, taking it to consist mainly of *national* states. The issue is that, for too long, comparative historical sociology has failed to look beyond, through, or across national processes and inter-national systems to explore *trans*national and global dynamics: that is, connections, relations, and processes that traverse conventional state boundaries. Note the main themes of the 'second wave' of historical sociology as developed by leaders like Theda Skocpol or Charles Tilly. They were largely about class-formation, revolution, political regimes, the welfare state and state-formation, collective action, and related matters. In any case these were all about *national* states or processes *within* national states. They also sometimes assumed, implicitly or explicitly, that the state and the social aligned. And while the 'international' sometimes appeared onto the second wave's analytic radar, it did so fleetingly at best. Even then, the key dynamics and dimensions of the global were not adequately theorized. Instead second-wavers tended to offer only an impoverished conception of the 'international' (Hobden 1999).

So why has historical sociology, for so long, avoided the global and transnational dynamics, dimensions and dialectics of modernity? The intuitive answer seems simple enough: lack of interest. If we are interested in the French revolution, or English state-formation, we are already interested in a national process, and global or transnational processes are supposedly irrelevant. Yet, the argument of this essay is that the answer is not as simple as that. This essay reconsiders the 'second wave' of historical sociology, related social theories of European modernity, and studies of British industrialization to show that the occlusion of the extra-national processes and forms lies in a deeper analytic infrastructure that has (mis)guided our conventional studies. The culprit is two-fold: *analytic bifurcation* and *causal scientism*. It is by recognizing this infrastructure that a forward advance can be made, for unless it is recognized and hence

dismantled, historical sociology cannot be properly globalized. It is for this reason that this essay, to forge ahead, first looks back at the second wave. Accordingly, I conclude the essay by highlighting some ways that a global historical sociology might proceed.

Bifurcating Relations

One part of the infrastructure that has served to occlude the global is *analytic bifurcation*. What is 'analytic bifurcation'? The critique of 'state-centric' thought by Wallerstein (2001) and related general critiques of 'methodological nationalism' is a good place to start. In these critiques, social science has operated from the problematic assumption that the boundaries of state, society, and state territory overlap; that social relations are contained by state boundaries; and the related myth inherited from Westphalia that the world consists essentially of sovereign states (Chernilo 2006; Taylor 1996, 2000; Wallerstein 2001).

Historical sociology would also fall under the rubric of this critique, and this might explain why the second wave overlooked global and transnational processes: its categories and objects are simply part and parcel of state-centric structures of thought in social science. Barrington Moore's work, for instance, was taken as exemplary by Theda Skocpol for its latent Millsian methodology, but both scholars took the national state as the primary unit: Moore studied political forms in different nations (dictatorship or democracy), while Skocpol famously explained revolutions in different states (Moore 1966; Skocpol 1979, 1984). There was no sense of interaction between them, no theorization of the wider global environment in which they operated, and no sense of trasnational flows of social influence or transnational social relations (Hobden 1999). Andre Gunder Frank's dependency approach was probably among the few that created categories for circumventing the confines of methodological nationalism: for Frank, the world economy could be thought of chains of satellites and metropoles, and the boundaries of these units did not always align with nation-states. Frank stated explicitly that satellites and metropoles could also be thought of regional spaces or intra-national areas (like urban areas *vs*. rural areas) (Frank 1967). Still, the problem is not with Frank's theory; it is how Frank's theory taken up by the second wave of historical sociology. It was taken as a theory about dependency between nation-states. Or it was not taken up at all (Manning 2006; Manning and Gills 2011).[2]

In short, there is indeed something about state-centric thinking that accounts for the orientation of second-wave of historical sociology. The literature on revolutions studied the causes of revolution in one country or another but state-centrism had them overlook transnational flows of influence and inspiration. Similarly, social movement theory and research fruitfully explained national-level crises or conditions by which the US Civil Rights movement or the anti-apartheid movements could flourish, but few if any considered the connections between them; nor thought of exploring the transnational connections amongst all such movements and the waves of decolonizing movements in Africa or Asia. Scholars studied state policies, welfare regimes, or other state forms but rarely if ever the transnational organizations that national states confronted, the transnational networks of ideas or experts that influenced state policies, or the potentially symbiotic or otherwise countervailing logics of imperialism overseas. The states theorized

in this work were always *national* states – rarely if ever empire-states, city-states, or regional associations. After all, one of the founding themes of the second-wave was 'bringing the *state* back in', by which was meant the *national* state and which carried the implicit notion that state, society, and territory easily overlapped.

Still, while state-centric thought has been a fetter, so too has a related structure of thought that I call *analytic bifurcation*. By this I mean the tendency to conceptually slice or divide relations into categorical essences that are not in fact essences. Postcolonial theory alerts us to this by critiquing Eurocentric knowledge's tendency to separate metropole from colony (Bhambra 2007; Magubane 2004). In historical sociology this sort of bifurcation is also seen. Theory and research in historical sociology's second wave divides not only metropole from colony but also 'East' from 'West', 'Europe' from the Rest, the 'inside' of nations from the 'outside' of nations, or 'the domestic' from the 'foreign'.

To better understand this, let us first take an example from an influential theorist whom many historical sociologists have adopted as their own: Michel Foucault. In *Discipline and Punish*, Foucault argues that the spectacle attendant with punishment in the *ancien regime* 'disappears' and is replaced by the prison (Foucault 1979: 7–8). Foucault restricts this 'transformation' (in his words) to Europe, but the realities of imperial history upend his characterization and this reflective spatial qualifier. The British colonial state in India did not respond to the 'Indian Mutiny' with a panopticon but with public brutality that involved executions, 'hangings and floggings' and spectacles such as 'blowing rebels from the cannon's mouth' (Connell 2006: 261). France's colonies from Saigon to Senegal to Algeria saw spectacular violence too. As Rosalind Morris points out, 'if it is true that the "slackening of the hold on the body" and the "decline of spectacle" marked the nineteenth and twentieth centuries in Europe… it remained profoundly central to colonial regimes' (Morris 2002: 265).

Perhaps to defend himself against such criticisms, Foucault qualifies his narrative spatially to Europe. But this is exactly the 'slicing' that is questionable, for by this means Foucault arbitrarily cuts 'Europe' off from its colonies – as if colonies of the French empire were not also, by virtue of being subject to the sovereignty of the French state, part of 'Europe' in that sense; as if imperial and colonial history *were not also Europe's history*. Such is the work of analytic bifurcation which impedes a more global analysis.

We can now turn to a classic work of second-wave historical sociology and see more clearly how analytic bifurcation works and how it occludes a more global analysis: Charles Tilly's *Coercion, Capital, and European States* (Tilly 1990). This is an exemplary work in historical sociology, for it seeks, as the best historical sociology does, to explain key aspects of modernity; in this case, the formation of the nation-state or, as he calls them 'national states', and their rise to dominance. We would think that this book would make transnational and global relations a key part of the analysis. After all, the book seeks to explain, rather than take for granted, the national-state form. There should be little threat of falling prey methodological nationalism. Furthermore, when we think about European states from AD 990–1992, surely European *empires* would come to the foreground; and empires were transnational phenomenon through and through. Not only did they expand globally and interact on a global stage, they were

themselves complex transnational formations that bled over, and across, different political spaces. Indeed, empires *should* be central to Tilly's analysis. The book's entire point is to explain how the national-state came to become the dominant form over other possible sociopolitical forms, including city-states and – yes indeed – *empires*![3]

But where is the global? Where is empire? Some critics have charged this work for falling short because it focuses upon 'European' states rather than other states, but this is really not the problem in my view. The problem is how those so-called European states are conceptualized in the first place. Tilly defines states as 'coercion-wielding organizations that are distinct from household and kinship groups and exercise clear priority in some respects over all other organizations within substantial territories' (Tilly 1990: 1). He defines *national* states as 'states governing contiguous regions and their cities by means of centralized, differentiated, and autonomous structures' (*Ibid.:* 2). All good so far: we anticipate, given this conceptual scheme, that Tilly will tell a story of how it is that these national states came to dominate Europe and win out over other possible forms. Hence, would expect him to show us how, around the mid-twentieth century, national states in Europe emerged from the ashes of European empire. Why? Because for most of the historical period Tilly covers, European states like Britain and France – which Tilly refers to as exemplary of national states – were *not* coercion-wielding organizations 'governing contiguous regions and their cities by means of centralized, differentiated and autonomous structures'. They were *empire-states*; coercion wielding organizations governing expansive regions and cities with a hierarchy of citizen/subject at the core of the system. In the 1920s and 1930s, the British empire-state was at its territorial highpoint, encompassing more than 33 million miles of territory around the world, structured by various hierarchical political divisions and fragmented sovereignties. The French empire encompassed over 12 million miles around the same time. These states only became truly national states later, after World War II.

Yet remarkably, this is not Tilly's story. Tilly instead sees the 'national state' winning out over 'city-states, empires, theocracies, and many other forms of government' a century earlier, in the nineteenth century. 'Full-fledged empires flourished into the seventeenth century, and the last zones of fragmented sovereignty only consolidated into national states late in the nineteenth' (Tilly 1990: 23). How can this be? The problem lies in the bifurcation effected by Tilly's understanding of states. He notes, for instance, that just as national states in Europe were emerging, they were also 'creating empires *beyond* Europe, in the Americas, Africa, Asia and the Pacific' (Tilly 1990: 167). He refers to these as 'external empires' (*Ibid.:* 167). In other words, Tilly's theory posits an 'internal' national state 'inside' Europe and its 'external' empire 'outside Europe'. In Tilly's model, there is a 'European' national state and then there is imperialism and an overseas 'empire'; there is a national state *in* Europe, exerting sovereignty over parts of Europe, and then there is, *over there*, an 'empire'; as if the latter were an appendage irrelevant to the constitution of the former, as if the model of sovereignty had not been already forged in and by interactions with the periphery *out there*; as if there could realistically be such an easy distinction between 'inside' and 'outside'. But, of course, national states did not develop their ideas and practices about sovereignty first *in Europe* and then transpose them outward; they developed first amidst sixteenth cen-

tury colonial claims and disputes between empires about overseas territory (Branch 2012). And the so-called 'external' colonies of Britain were not 'outside' Britain: they *were* British. They were declared subject to the sovereignty of Britain, just as France's so-called 'external' colonies were subject to the sovereignty of France – hence fully inside it. This is why the English crown fought, so hard and so often, to keep colonies within itself, suppressing the American revolution in the 1770s or, for that matter, violently suppressing the Mau-Mau rebellion in the 1950s. And France's colonies likewise were not 'outside' of France: they *were* French. Hence France fought the bloody Algerian war in the 1950s to 'keep Algeria French'. That *was* the mantra after all.

In short, Tilly's model analytically bifurcates into distinct domains the 'national state' and 'empire' –'internal' and 'external', 'inside' and 'outside' – that were never really separated in practice.[4] In so doing, his model by its very categorical elision occludes imperial relations and hence a more global analysis. This would be an analysis that would not be restricted 'to Europe', if only because empires never were so restricted. It would be an analysis, instead, that would track the global by tracking the imperial – taking us from, say, London to Calcutta down to Nairobi over to Suva in Fiji, and the relations and connections throughout. This would also be an analysis, by the same token, which could track not just now the national state form came to dominate over other forms like empires, but how it emerged *from* the dynamics of empires; how global space, in the wake of decolonization, came to *appear* nationalized; that is cut up into distinct units called 'national states', and how it did so by the very dynamics of coercion and capital within and between empires which Tilly already pinpoints but which he arbitrarily restricts to a regional (European) phenomenon alone. It would be a global analysis that does not presume a sovereign state that then extends itself overseas or contracts and retreats back home but whose very policies, practices, and forms were forged in and through its interactions in transnational and global space in the first place. Because of the analytic bifurcations endemic to his approach, Tilly's model occludes any such analysis. Instead, the 'global' in his analysis boils down to a diffusionist story whereby a 'national state' that ostensibly emerged in Europe then diffuses to the rest of the world.[5] The global is treated not as a constitutive force but instead merely a blank slate onto which our Eurocentric historical sociologies are etched.

'Hunting' for Variables[6]

Besides analytic bifurcation, the other way in which the global has been occluded is through comparative-historical sociology's tendencies towards causal scientism. By this I mean the way in which historical sociology, in an effort to legitimate itself as scientific and to differentiate itself from disciplinary history, has aimed for causal explanation as the goal of research and has treated causal explanation as a matter of 'variable hunting'. Calhoun (1996) noted long ago that the second-wave served to 'domesticate' the intellectual potential of historical sociology by seeking to legitimate itself through the use of ostensibly scientific methods (Calhoun 1996). Charles Tilly 'emphasized the operationalization of quantitative sociological research and analytic methods' while Theda Skocpol and others 'mobilized John Stuart Mill to distinguish between parallel demonstration of theory, contrast of contexts, and their favored combination of the two:

macrocausal analysis' to the effect of neglecting historicity (*Ibid*.: 309). I suggest that causal scientism has had another effect: rather than only serving to occlude historicity, the hunt for variable-based causality has occluded the global, thereby 'domesticating' historical sociology in the sense of keeping the analytic focus on the 'domestic' rather than the transnational.[7]

In one of the few works discussing global historical sociology, Magubane (2005) points out that the assumption of comparative historical sociology of unit independence (or as statisticians might call it, 'Galton's problem') assumes that transnational and global relations are irrelevant. I am arguing here that it is the search for variables in the first place which is the problem. We all know that in historical sociology the search for causes usually takes place within the context of so-called 'small N' comparative work. When we, historical sociologists, pursue causes, we tend towards comparison, because we know that if we are to establish causation we need variation. And we get variation from comparing across cases. In order to find the causes of social revolutions, we need to look not just as France but also China, or China and Russia, and so on. In order to the find what *really* caused the industrial revolution in England, we need to see what was *not* happening in France, China or India, *etc*. The problem is that this sort of comparative approach is forced to reduce the transnational or global field into a 'variable'. And as such, it is most often present across all of the different cases such that it becomes invisible. Its presence is effectually erased because it does not appear as a so-called 'sufficient' cause; even if it could very well be, and often is, a *necessary* cause.

Take the simple example of the Arab Spring. Because this happened so recently and we all read about it and followed it in the news, we all know that there were important so-called 'global' factors to all of the individual revolutions: surely it would sound silly to us if, for example, someone said that diffusion effects were not at play here; that is, that the revolution in Tunisia did not in turn shape or give inspiration to those in Egypt, Yemen, and Libya. In either of these scenarios just mentioned, the 'global' or at least in this case the regional or international, would be seen by us as important for any meaningful account.

But let us fast forward fifteen years from now and imagine that an aspirational young historical sociologist wants to write about the revolutions. Because she is aspirational, this young historical sociologist will likely be compelled – or advised – to think about the revolutions causally – that is to write a dissertation that includes at least some significant bit on *why* the revolutions occurred. And, because she is aspirational, wants to get a job in a good department and publish in mainstream sociology journals, she will adopt some typical method like Mills' method of agreement or difference – the standard method for assessing causality in comparative-historical research. So she might employ Mills' method of difference to assess causes: she will list all of the countries that had revolution and those that did not and try to find the factors that the former shared that the latter lacked. And if she did so, of course, her analysis will reveal that diffusion was *not* a cause; that the 'global' did *not* matter. She will find that in all of the countries after the Tunisian revolution, all had televisions, newspapers or access to the internet that would have let the inhabitants of those countries hear about the Tunisian revolution. In all of the 'cases', then, there must have been diffusion; in other words, all

the countries were enmeshed in a transnational circuit of information about revolution. According to Mills' logic of difference, therefore, the transnational should *not* be part of our causal story, for if the transnational is constant it cannot explain variation: *all* of the cases were exposed to the Tunisian revolution, yet only in some of them did a revolution occur. So our young historical sociologist might instead look to national conditions or factors that better explain the variation and, as a result, the 'transnational' or 'global' recedes from view. Of course we today would find such a claim counter-intuitive. While none of us would at all argue that intra-regional ideational influence was the sole 'cause' for revolutions, we would recognize that it was at least perhaps it was a *necessary* (but not sufficient) one. Yet even this more minimal claim would not appear in our young historical sociologists' analysis; and our young historical sociologist will produce a study where regional influence plays little to no part in the story.

Another example is the debate over whether or not European development was positively impacted by global factors – not least by overseas imperialism beginning in the long sixteenth century onward. World-systems and dependency schools of thought, along with postcolonial studies drawing upon those schools, have long contended that European development cannot be understood without recognizing the advantages obtained from overseas imperialism (Wallerstein 1974: 128). Some argue that in fact Western 'take-off' was itself due to the economic surpluses produced from imperialism rather than due to, say, Weber's Protestant ethic or some other factor internal to Europe or individual European countries (Blaut 1993). But there is still a debate: critics assert that imperialism was *not* important for causing or contributing to European development and instead that various internal factors have more causal power (Usami 2011).

On what grounds? There are two arguments. The first argument is qualitative. O'Brien points out that all of the principal powers in Europe in fact remained undeveloped from around 1415–1815 except Britain and arguably Holland, despite the fact that they all had imperial acquisitions. Portugal and Spain, for instance, had massive overseas empires yet neither overtook England and instead suffered from economic stagnation up through their entire periods as empires. If imperialism is connected to economic growth, then these countries should have been as economically successful as Britain (O'Brien 2005: 77; O'Brien and Escosura 1999). This argument implicitly enlists Mills' method of difference and we see the same issue as with the Arab Spring: the potentially independent 'variable' is a global factor – imperialism – and it cannot explain variations in developmental outcomes because it is a *constant*. By implication, imperialism and hence the global does not matter. Once trapped in the logic of variable-based causation using standard comparative-historical methods, accounts of European development could very well ignore global factors and focus on factors internal to the different countries to explain variation among them. Imperialism does not matter for accounting for European economic development.

The second argument for why imperialism is not important for European economic development is quantitative. O'Brien collects data to show that fortunes from overseas commerce between 1450 and 1750 were miniscule and did not represent any significant increase from previous (presumably non-imperialist) periods; and that, even in the late 1700s, European exports and imports to the periphery amounted to only a per cent and two-four per cent total economic output respectively (as late as the 1840s, total exports

and imports never amounted to more than 15 per cent). O'Brien makes similar points about bullion extraction. We can immediately see, then, that this argument occludes the global by reducing the global to a quantitative variable (amount of trade) (O'Brien 2005: 77). The implication is that imperialism can be taken in or out of an equation without effecting the whole – which is exactly how variables are treated in linear probabilistic causal models. Presumably, without the one per cent or four per cent of colonial trade, the economic outcomes would be exactly the same.

The problem, of course, is that colonial trade is not a variable in this sense. Profits from trade may have been small but if they were used to fund a critical sector of economic development, then its importance is much greater than a per cent would represent. In other words, even if we wanted to treat the global as a quantitative variable, so-called 'interaction effects' must be taken into account. And yet, even then, the global recedes from analytic view. Why? We are forced to reduce it to one variable among others, even if its interaction effects can be modelled. In short, once we go down the path of causal scientism, and hence turning the global into a variable it becomes increasingly difficult to keep the global in sight.

How to do Global Historical Sociology

How, then, to proceed? The solutions should come from the diagnoses. First, to overcome analytic bifurcation, we should radically shift – or rather rescale – our objects of analyses. For much of our existing analytic objects in historical sociology – the nation, the state, state-formation, the capitalist class, discipline, revolutions – have for too long been embedded in state-centric thinking that analytic bifurcations are almost built-in to their very conceptualization. So we should rethink those categories if not surmount or even excise them altogether, *making transnational and global social relations and processes our categories of analysis*.

This is the virtue and radical innovation of world-systems theory: to rescale our unit from nations or societies to the world-system as a whole. But the logic can also be applied more broadly. For example, instead of focusing on the national state – such a common object in the historical sociology of the state – we might track the 'imperial state', which for many of the states we study is exactly what those states *were* – or, perhaps, imperial relations or processes. In place of a sociology of the state, in other words, we might do a sociology of empires, colonialism and imperialism; borrowing from world-systems analysis, we might do a sociology of imperial formations and relations (Chase-Dunn and Hall 1997). Or relatedly, rather than taking the boundaries of sovereignty for granted, we should problematize them, analyze them for their historical construction and reconstruction and the peripheral places or spaces of their enactment. Rather than probing state-formation, we might instead explore boundary-formation (see, *e.g.*, Hall 2009). Or, as another example, rather than tracking revolutionaries in France in the nineteenth century to understand the French revolution, we should track the larger ideational or social *networks* that crisscrossed the French state and beyond; the transnational networks in which those revolutionaries were embedded (Go 2012). Instead of focusing on French revolutionaries in France alone, we might look at French masons and hence follow them and their associates in Cuba, Haiti, Spain or Boston. Or why not make religious diasporas our object of historical analysis? Or commodity chains? Or pirates? In short, I am suggesting we shift our analyses from standard sociological *objects* ab-

stracted from space to transnational or global *relations*. This is the spirit of world-systems analysis after all. It is also the guiding spirit of our colleagues in global and transnational history. Historical sociologists can learn something here.

It is true that parts of the so-called 'third wave' of historical sociology has already begun to rescale their studies (Adams, Clemens, and Orloff 2005). But the other issue also has to be dealt with: the limitations of variable hunting. How to transcend causal scientism? One strategy would be to retain the interest in causal explanation without conflating causal explanation with variable hunting. We should not throw out the baby with the bathwater; causal explanation should be part of the agenda. The point rather is that we should transcend variable hunting with other methods and epistemologies. Strands of critical realism, for instance, urge us to attend to causal *mechanisms* rather than only focusing upon causal variables (Steinmetz 1998). Meanwhile, methods such as Qualitative Comparative Analysis, case-oriented analysis, and related 'INUS' views of causation (that emphasize necessary rather than only sufficient conditions) would also pertain (Goertz and Starr 2003; Ragin 2004).[8] These sorts of approaches would not exclude global factors. To the contrary, they would urge us to better explore causal processes and relations. Once we recognize the global as a necessary cause, for instance, we can explore – and indeed better *describe* – the causal mechanisms and paths that unfold from the 'variable' of the global to the 'outcome' without dismissing the 'variable' on the grounds that is *only* a necessary but not sufficient cause.

Another strategy would be to suspend if not suppress our emphasis on variable-based causation altogether. The global is not a variable. True, we could try to disaggregate the global to more specific variables that might then appear in our comparative analyses. But this would just reproduce the problem. The problem is not that our global variables are ill-conceptualized but rather than we think in terms of variables at all. A case can be made here for descriptive sociology (Abbott 2001; Savage 2009). Our colleagues who do ethnography do this sort of description all the time. Rather than fetishizing causes – that is 'why'? – they are more interested in unearthing social contexts, relations, and how parts are connected to wholes. If they are concerned with causation at all, it is often in the sense of richly describing causal processes or causal mechanisms: not 'why?' but 'how?' questions.[9] It is fitting here that when world-systems analysis first emerged – which as we know is one bright exception to sociology's occlusion of the global – it was not primarily about hunting for variables (Wallerstein 1974). It was not even primarily about conventional causal explanation. It was about richly and systematically describing the relations constituting the world-system, the transformations of that system over time, and connecting parts to wholes. Causal models could be derived from that work, and causal language is evident in the work too. But the overarching goal was never about locating explanatory variables. It was about understanding the shape, internal constitution, and historical unfolding of social forms and systems.

More recent work drawing upon world-systems analysis also engages in significant descriptive work. For instance, studies of the world-system sometimes pinpoint cyclical trends, such as recurrent phases of financialization (Arrighi 1994), frontier dynamics in the world-system (Hall 2009), or phases of hegemony and imperialism (Go 2011). The identification of these trends demands description. So too does an understanding of

exactly what might be similar or different across identified historical time periods. In the late nineteenth century, the world-system entered a stage of multipolarity and hegemonic decline. This is similar to the late twentieth century. In both cases, the declining hegemony engaged in heightened imperialistic aggression. The US in some ways reproduced Britain's imperialism of the late nineteenth century. But the imperialistic aggression of the US also had distinct forms: it was more about temporary military occupation than direct territorial colonization (Go 2007, 2011). The point is that these comparisons require serious description first and foremost rather than searching for variable causes.

In short, for globalizing historical sociology, it seems to me that serious descriptions would be a good place to start. Even if we are ultimately interested in causation, descriptions are a necessary first step. Rather than worry about why the public sphere emerged in England rather than, say France or Vienna – which also had coffeehouses, by the way – we might explore how coffeehouses were but the end point in a larger chain extending through the continent over to Yemen. In other words, rather than asking *why* the public sphere emerged in England we can ask *how* the public sphere in England was connected to peasant cultivation and warlord power in Ethiopia. Or, rather than seeking the causes of imperialism we might explore the different modalities and forms by which imperial power is exercised across expansive space. Not *why* certain groups or organizations rather than others are endowed with more power than others but *how* they were able to obtain that power and *what* they do with it. And rather than use Millsian methods to trace out why the French revolution occurred, we can trace the actor-networks that connected the French revolution to the Haitian revolution. All of these approaches, it seems to me, would still fulfil the mission of historical sociology of tracking relations and processes in time and space, but it would do so without occluding the global – without, that is, restricting our analyses of historical social relations and processes to a single national state that presumably contains the social.

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NOTES

[1] See, for instance, Adams (2007), Barkey (2008), Go (2008, 2011), Magubane (2004), Steinmetz (2007). Some insightful programmatic statements include Adams, Clemens, and Orloff (2005) and Magubane (2005). For promising guides from UK scholars, see Lawson (2007) and Bhambra (2010).

[2] On historical sociology's approach to world-systems and dependency see Adams, Clemens, and Orloff (2005: 57–58).

[3] See, for instance, McNeill (1986).

[4] And while Tilly places much emphasis on the role of 'war-making' for state-making, most of the wars he pinpoints as critical were imperial wars or wars of conquest overseas, occurring this either outside 'Europe' or as wars *for* territory outside 'Europe' (see Tilly 1990: 165–181).

[5] For informative critiques of the privileging of the national state in social science and the associated overemphasis upon 'modern' states, see Hall (1998) and McNeill (1986).

[6] I take this idea of 'hunting' for variables from Krause (2010).

[7] Part of the problem, of course, is that quantitative data is national, and hence automatically state-centric. But my critique here applies to qualitative work as well.

[8] 'INUS' refers to 'insufficient but non-redundant parts of a condition which is itself unnecessary but sufficient for the occurrence of the effect' (Mackie 1988).

[9] Similarly, many of us in historical sociology are fond of Foucault but we less often imitate his methodological approach. This is an approach that was not about finding causes but rather offering genealogical description, conceptual elaboration (cataloguing and describing, for instance, different modalities of power in different historical moments or in different places), or excavating conceptual landscapes (the larger linguistic system in which certain ideas obtain for instance). We all read Foucault but then we go and deploy Popper or Mill (cf. Magubane 2005). Why? Why not follow Foucault's own methods and eschew altogether our automatic attachment to causal models?

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