How to Write the History of the World

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In one of Jorge Luis Borges’s short stories a royal mapmaker is asked to fashion increasingly accurate maps of the kingdom until, finally, he covers the kingdom with a map. This parable is a warning to all historians but especially to world historians, who may struggle more than others with pressures simply to “cover” time and territory. Reaching beyond mere coverage is crucial to the field’s development and to its status within the profession. How can we write the history of the world in a way that is not just broad but also broadly influential?

This question poses itself at a time when world history has already arrived as a serious research enterprise. Once the nearly exclusive realm of a few—William McNeill, Philip Curtin, Alfred Crosby, and some others—the field now draws scholars who no longer consider an association with world history as a mark of hubris, a paean to mass marketing, or evidence (God forbid) of a preoccupation with undergraduate teaching. The field has its own journal, the Journal of World History, which has seen the quality of its articles rise steadily and now routinely publishes both original research, much of it done by junior scholars, and important synthetic pieces. Meanwhile scholarly interest in the topic of globalization has helped to forge an interdisciplinary audience with an interest in the longue, longue durée.

Yet questions remain about the sorts of methodologies that aspiring world historians might embrace and promote. Aiming for comprehensiveness and relying on older narrative techniques are not serious options. Without a conceptual framework the data threaten to overwhelm argument. Otherwise we could sensibly choose to produce a five-thousand-book series, each title evoking John E. Wills’s recent book 1688: A World History; that is, we could write the history of the world one year at a time.

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Whatever else its virtues world history has not produced a significant volume of methodologically thoughtful discussions or theoretically influential studies. There are, to be sure, discernible methodological patterns and debates in the literature of world history, and some of these do contain lessons for other subfields. Following the title of Donald R. Wright’s well-crafted book from 1997, *The World and a Very Small Place in Africa*, one approach involves alternating attention between global processes and local experiences. This methodology informs a number of prominent world-history initiatives, including efforts to place formerly insular national histories in global perspective. But the approach may ultimately prove less important to world historical writing and to fields seeking a connection to world history than two other common strategies.

One of these approaches we might label circulationist. Its objects of study are the movements around the globe of—in no particular order—commodities, capital, ideas, people, germs, and ways of marking ethnic and religious difference. Like so many tops spinning these circuits together comprise what C. A. Bayly has called “archaic globalization” in the early modern period and what observers of the contemporary scene call simply “globalization” (forgetting, sometimes, that it has a history). By shifting our gaze from one sphere of circulation to another we simulate a perception of the whole of global interconnectedness. Following Arjun Appadurai we can give these circuits names—either his unwieldy labels of ethnoscape, bioscape, financescape, and so on or the more traditional Latinate categories we already associate with established areas of study, such as migration, diffusion, or expansion.

Another approach to globalization in its early and late forms is less familiar but just as important. Rooted in comparisons it purports to uncover the structural similarities of polities that may be distant in place and time. Here the historian finds globalizing influences by surmise and by arithmetic; so many similarities in so many places suggest common connections to forces crossing borders and oceans. When done well this technique reveals hidden continuities. It focuses our eyes not on global circulation but on its imprint, origins, and contexts: for example, status and class distinctions, strategies of resistance, institutions of rule, and nationalism.

Circulationist projects appear to be in much greater supply. In part this is because of a certain transparency of social theoretical constructs related to the movement of people, commodities, and ideas. For example the concept of “networks” has worked its way into mainstream historical studies and has provided a vocabulary for historical writing on topics as diverse as European migration, Third World urbanization, and the transnational diffusion of
ideas. In part the proliferation of circulationist studies reflects the institutionalization of regional historical studies and their logical development. For example the acceptance of Atlantic history as a bona fide area of research provides legitimacy—and professional cover—for scholars wishing to map Atlantic circuits and follow them wherever they might lead, even if this means overstepping the boundaries of an already expanded Atlantic world.

Yet it is also true that many circulationist projects remain relatively undeveloped. Thirty years after Pierre Chaunu mapped in Braudelian fashion the movement of shipping from and to Seville, we still do not know enough about regional or global circuits of people and goods (let alone microbes). As Alan Karras’s study of New World Scottish “sojourning,” Karen Barkey’s work on Ottoman qadis, Alison Games’s research on cosmopolitan English colonists, and other complex migration stories remind us, patterns and understandings of long-distance movements are much more varied than historians of a generation ago believed. Many of these circuits are still in need of documentation, including the movement of both official and nonofficial personnel within and across empires. As for commodities scholarly interest in consumption has played an important role in widening and deepening the analysis of global trade. But here again one has the clear impression that we are at the edge of a vast and varied area of study with much more to explore and explain besides commodification, symbolic capital, and circuits of silver.

As developed and promising as circulationist world history may be, structural approaches to world historical analysis are newer still—and in combination with studies of movement perhaps potentially more revealing. This is somewhat paradoxical because comparative world history has some of its roots in a familiar, old-fashioned comparative approach. As David Armitage has pointed out in surveying trends within Atlantic history, an older comparative history juxtaposed different civilizational areas and sought explanations for their diverging trajectories. This kind of comparison is still with us, as we have been reminded by new attention in the work of Bernard Lewis and others to the old question of where the Islamic world has “gone wrong.” We also find it in the continuing debate about the timing of European-versus-Asian economic development that has seized the attention of the so-called California school of economic historians.

There is another strand of comparative history, though, with roots that are better established in historical sociology than in sociological history. This approach analyzes multiple cases involving broadly similar historical processes in order to advance generalizations about “big history.” In sociology we think of Charles Tilly, Jeffrey Paige, and Theda Skocpol as prominent
comparativists in this vein; in history, exemplary works include Michael Adas’s early book on millenarian movements, Philip Curtin’s study of trade diasporas in world history, or Patricia Seed’s flawed but interesting comparison of European ceremonies of possession. Rather than comparing trajectories and tallying up the factors “needed” for historical change of a certain kind such comparative studies examine the structural logic of conflicts or processes in particular places. Global patterns are seen as emerging out of the repetition and replication of similar social tensions and practices, while these are in turn understood as influenced by familiar global circulationist currents. The methodology has the advantages of privileging the kind of careful case analysis that historians claim as their strength and of placing social conflicts and discourse, broadly defined, at the heart of the problem of defining international order.

I began with this approach in conducting research for my book Law and Colonial Cultures: Legal Regimes in World History, 1400–1900 (2002). The noticeable dearth of historical studies treating law as a global phenomenon has no doubt had something to do with the imperfect fit between traditional legal history and circulationist models. The book, departing from both, analyzes the ways in which law constituted an element of global ordering before the emergence of international law and the interstate order. In early modern empires, including European overseas empires, and in other sorts of polities, too, legal orders were characterized first and foremost by jurisdictional complexity. Religious minorities, communities of traders, and subject populations were expected to exercise limited legal authority over their own community members. The claims of states did not include a monopoly over law, and membership in a legal community was only sometimes defined territorially. This dynamic of multicentric law was both rooted in particular places and so widespread as to constitute an element of international ordering. To give just one example Portuguese agents arriving in West Africa in the fifteenth century were aided in setting up trading posts by the homology that existed between their understanding of their limited jurisdiction over Christian subjects and Africans’ acceptance of the legal authority of diasporic traders over their own community affairs.

Jurisdictional complexity produced both discernible institutional patterns and also sometimes transformative conflicts. Legal pluralism established rules that were there to be broken or changed, and legal actors at all levels of the colonial order proved to be adept at maneuvering through and in the process altering the legal order. One of the interesting conclusions of colonial legal histories is that pressure for the creation of colonial states came sometimes
from indigenous actors rather than from the metropole, which in many cases labored to limit its own jurisdictional claims and minimize administrative costs. Over the course of the long nineteenth century institutional configurations shifted—gradually and sometimes only partially—in the direction of state claims to legal supremacy. In this way the emergence of a global interstate order was the product of politics in particular places, rather than the result of metropolitan or Western designs or of some incontrovertible systemic logic.

This example shows that comparative analysis need not propose a model or experience (of capitalist development, state formation, or modernity) to be used as a benchmark for the study of divergent trajectories. It is also important to note that global circuits of labor, capital, and ideas are not irrelevant to patterned social conflicts but also do not necessarily hold the key to their understanding. In some ways this sort of comparative approach builds on the same strengths that make historians so good at placing local histories in global context. Attention to the local *is* indispensable to the ability to generalize about the global. Yet the technique of juxtaposing broad trends with the history of any “very small place” cannot by itself confirm broad insights about global shifts and their origins.

Comparisons of this type, it turns out, may be surprisingly compatible with approaches influenced by postmodern perspectives. Both postcolonial histories and a recent strain of scholarship in British imperial history have analyzed iterative structures within various arenas of discourse on imperialism. Here the echoing effect of structural similarities occurs not across a range of cases but within different facets of a global enterprise. As Nicholas Thomas said nearly a decade ago, a version of world history can be rendered vertically, as the study of “projects” stretching from centers of rule to imperial borderlands.

Both horizontal and vertical variations of comparative world history present theoretical and practical challenges. Comparing structures across many cases may suggest functionalism if one is not careful to emphasize the contingency of outcomes in all cases. And asserting the essential similarities of various unconnected arenas of discourse may border on the banal, as when David Cannadine in *Ornamentalism* promotes “hierarchy” as an organizing trope of British imperialism. Although a combination of care and flair may provide an escape from these shortcomings, there is also no question that we are describing merely a comparative perspective, not a theoretical answer to the problems of writing global history. At the same time we can affirm that world history may be written with the express purpose of producing theoretical insights and methodological innovations. Coverage is dead; long live theory.
Regarding practical challenges the central problem may become one of sheer effort. Mastering the complexities of conflicts or discourse in a range of places or cultural milieux requires a great deal of time, expertise, and travel to collections, not to mention mastery of multiple languages. Yet these obstacles may appear less formidable as multisited research becomes more accepted by funding agencies and as the boundaries of regional subdisciplines continue to be eroded by the circulationists.

Despite these and other obstacles there are compelling intellectual reasons for making comparative history at least as common as circulationist projects in world history. The approach lends itself to the study of a wide range of social, cultural, and political conflicts and their local-global interconnections. This translates into an opportunity to expand world historical inquiry from its more established base in economic history and its more recent, sometimes disturbingly seductive move toward biological-environmental narratives. Institutions should also be objects of study for world historians—not just transnational institutions, which operated fitfully if at all in most historical periods but global institutional regimes that have emerged out of common cultural practices and patterned political conflicts. And for those who think institutions are a bore no matter how they are discussed, there are plenty of other topics that do not always lend themselves to fruitful study through a circulationist approach. Aesthetic practices and sensibilities, for example, may be widespread without having come to be so through processes of diffusion.

I anticipate—and hope—that the better-established methods of world history-writing will stay with us. We need more well-crafted studies analyzing specific local-global interconnections and also more research into the circulation of people, commodities, ideas, discourse, and, yes, microbes. I also hope that these efforts will be joined by the multiplication of studies that build on the best kinds of comparative analysis, moving beyond questions about different developmental trajectories and probing unlikely elements of global order and disorder.

Unlike Borges’s mapmaker we will not have to cover the world with a map in order to understand it. Nor will we be limited to other mapping exercises, such as projecting small-scale studies onto a global plane. Instead of cartography the relevant scientific analogy might turn out to be contemporary astrophysics. Its preoccupation with multiple, unseen dimensions in universes we can only imagine offers the combination of precise analysis and broad conjecture to which world historians might now aspire. And then there’s the lure, however remote, of a grand, unified theory—nothing less than a theoretically compelling history of the world.