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Marc Egnal. *Divergent Paths: How Culture and Institutions Have Shaped North American Growth.* New York: Oxford University Press, 1996. xvi + 300 pp. $45.00 (paper), ISBN 978-0-19-510906-1; $111.00 (cloth), ISBN 978-0-19-509866-2.

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Scholars like George Beer and Charles M. Andrews, several generations ago, investigated the ways in which government in London affected the development of political culture and institutions in North America and vice-versa. Their work also considered, at least on some level, the economics of running an empire, and how colonial economies grew over time. Exploring the ways in which government and commercial life functioned, such imperial studies only rarely took into account the experiences of "real people"–who regularly faced the ramifications of policy decisions made elsewhere. Now deemed "macro-history," these studies are often used to provide background context for queries of smaller scale (though, not necessarily smaller import).

The "new" social history of the 1960s and 1970s transformed historical fields of vision. No longer did many scholars examine politics and trade, on either side of the Atlantic. Instead, the profession began a prolonged archival binge, focusing on documents that might tell us something of the lives of those whose voices we had not previously heard. The experiences of women, Africans, and those who worked hard for a meager subsistence were recaptured; our collective understanding of the past and its meaning became more inclusive, more detailed, more complete. As places like Dedham, Massachusetts, and tidewater Virginia entered our historical viewfinders, so too did a different kind of comparative history. We began to contrast the experiences of similar groups of colonials in one society with those in another. No longer was America a single entity tied to its metropole through policy; America consisted of a variety of disparate places. Such "microhistorical" studies continue to be important building-blocks for narrative history, even as they went a good way towards exposing the shortcomings of the very narratives which informed them.

More recently, some scholarship has attempted to move away from a purely archival social-historical method. Raising broad thematic and comparative questions, such work focuses largely on processes–migration, racial classification, colonization, revolution, to name just a few. Such work too often sits on the margins of early American scholarship; it simply does not mesh well with much social history because it does not strictly adhere to many of the geographical and chronological boundaries that academics slowly internalized in the 60s and 70s. Though a few scholars now remind us to adopt an "Atlantic world" perspective, they have a limited British Atlantic–Britain and those of its American colonies that declared independence in 1776–in mind. For many of them, the American Revolution looms like a great continental divide: on one side lay British tyranny; on the other reside liberty, equality, and, of course, The Constitution.

Comparative history of the sort required by an "Atlantic world" is eminently desirable for many reasons–one of them that this was the place in which modern European colonialism developed, in which anti-colonial revolts took place, and in which local populations regularly interacted with each other. Historians paid attention to this in 1992, with the Columbian Quincentennial, but there does not seem to have been either a deep or a sustained interest in such broadly-conceived questions. The British West Indies continue to remain virtually ignored (despite a lot of nodding these days to their importance); few early Americanists know much about what went on in eighteenth-century Grenada or Jamaica. Along similar lines, the colonies of other European states–part of the very same Atlantic world that Andrews and Beer described–are even more rarely conjured. The lives of Quebecois, Cubans, Mexicans, Surinamese, Brazilians all have been maddeningly excluded from scholarship about early America. (To be fair, much of the scholarship dealing with these places excludes the mainland North American colonies.)

The reasons that more inclusive history needs to be written seem fairly obvious, and hark back to the criticism that the social historians leveled at the imperial historians. If imperial history ignored people of lower rank,

than social history has led to fragmentation and compartmentalization of the discipline. The two need desperately to be put back together.

It is, then, uncommon for a book to address macro-historical questions in a way that is sensitive to the conditions of those at "the bottom" of a society. It is unusual for a history to move beyond national boundaries to explore multiple societies. It is anomalous for an author successfully to integrate various subfields of historical research (social, political, cultural, and economic) into a complex argument. This state of affairs is easily understood. Not only will the author's imagination be tested rather severely but a colleague's scrutiny of the footnotes will almost certainly reveal an archive that went unvisited, a source that did not get used, or a fact that contradicts a generalization.

Divergent Paths, Marc Egnal's recent comparative history, deserves a loud round of applause from all historians, simply because it takes a risk and grabs hold of a subject–economic growth–that has long been purely the province of the economists and economic historians and treats it as a topic in cultural and comparative history. The book examines three distinct societies, at least as Egnal defines them, and argues that "culture and institutions" (vi) explain regional growth or the lack thereof. Books with such ambition appear far too infrequently. *Divergent Paths* is, therefore, a bold book. Egnal deserves our thanks for attempting to move early American history in a new and ultimately necessary direction. Still, *Divergent Paths* demonstrates a danger of comparative history: the utilization of assumptions that derive from one of the societies being compared.

Divergent Paths takes a long view; it begins around 1750 and ends in the current decade. The first two-thirds of this 211-page book (not including notes) cover the period from the 1750s to the 1850s while the last third of the book moves the story forward into the 1990s. Looking at the five southern colonies (along with other states that practiced slavery in the nineteenth century) as one unit and comparing it to two others–the eight northern colonies (along with, later, the upper midwest) and Quebec (briefly under French and mostly under British rule), the book strives to explain why both Quebec and the American south did not grow economically as much or as quickly as did the north. It proposes both familiar reasons, like geography and demographics, and somewhat less familiar, though no less convincing, ones like culture. Egnal concludes that the American south had cultural attitudes and institutions that hindered its rate of economic growth, especially when compared to the north. So too did Quebec, though its conservative culture was rooted more in French Catholicism than it was in slavery. The book's title appropriately indicates its conclusions: conflicting cultural institutions placed the northern tier of mainland states on a divergent trajectory from that of French-speaking Canada and the south. As Egnal says in his conclusion, "Sustained-growth must be home grown and come from an educated, entrepreneurial local population" (202). The argument is valid, and it gets to all the issues of colonialism, cultural and economic, political economy, and values with which world historians regularly grapple.

The book is, nevertheless, problematic because its assumptions do not always serve it well. There is, for example, an assumption that because Britain industrialized, and the northern states followed suit, growth should be measured in terms of industrial product. In other words, there is really insufficient attention paid to the global agricultural markets and to how people in non-industrialized regions evaluated their choices. The South built a large number of institutions to protect its labor force and separate the races into positions of inferiority and superiority. Egnal is cognizant of this fact, yet he does not seem to recognize that these institutions served to guarantee the profitability of slavery to those who wrote the laws. It did not matter to them that free labor could well have provided a higher rate of return on investments. What mattered was preserving the social order while, simultaneously, enjoying the standard of living to which they had become accustomed. When slavery ended, the same could be said. So long as the society remained an ordered one, and so long as those on top earned enough, there was little incentive to change the economy. So, too, in Quebec, where cultural institutions shaped people's attitudes towards economic progress. Not going the way of the north was a choice that not just Quebec and the south made, but a choice that was made by many other societies, from 1750 onwards. That many of these societies are now part of the "developing world" suggests something about the domination of industrialization that could not possibly have been foreseen at moments when choices needed to be made. In short, the book asks comparative questions that derive from studying Britain and the northern colonies, and applying these questions to the other regions. Another approach would have been to derive new questions and ask them of all regions, building a narrative in that way. It should be possible to ask what the relationship of the economic elite was to the global marketplace and how the elite constructed institutions to balance that relationship against local populations (who had, presum-

ably, far less power). The narrative that Egnal tells could then have been smoother, as well as more imaginative–and paradigm-shifting.

One wonders what audience Egnal had in mind. The brevity, typical regional division, and placement of the notes in the back of the book all suggest readers in an upper-division undergraduate course. By the same token, the many references to historians in the text itself, and several point-by-point refutations of their arguments, suggest a professional audience–for whom the brevity becomes a problem (for example, see the section on migration, pp. 70–72). *Divergent Paths* is certainly eloquent, yet there are several sections and passages that are not. Perhaps most irritating is the author's tendency to describe, physically, many of the writers whom he quotes, as if their physical appearance had something to do with their thought; these phrases do nothing more than disrupt otherwise fluid prose (see pp. 78 and 84 for examples).

On balance, the broad outlines of *Divergent Paths* are almost certainly accurate. The book would have been stronger had its author reconceptualized his questions by challenging his assumptions–about the definition of growth, about the relationship between government and economic decisions, about the ways in which regional identities are formed, and about the ways in which culture is defined from both above and from below–before he began writing the narrative. Had Egnal truly linked disparate historiographical fields, he would have focused much more on the processes and much less on comparing regions using a set of questions that derived from only one of them. After all, the social historians told us that "winners write history." The best way to rewrite that history is to look at the problem from another perspective. The path to which Egnal points us is dandy; it is up to the rest of our profession to push much harder at formulating new questions. Such exertions will enable us more nearly to reconstruct a world that was, itself, far more connected than all but a few of us have made it out to be.

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