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Featured Review Article

Strange parallels: Southeast Asia in global context, c. 800–1830. Volume 2: mainland mirrors: Europe, Japan, China, South Asia, and the islands

By Victor Lieberman. Cambridge: Cambridge University Press, 2010. Pp. xxv + 947. 25 b/w illustrations, 10 maps, 1 table. Paperback £26.99, ISBN 978-0-521-53036-1.

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Charles Darwin once lamented the unrelenting focus that had seized the operation of his brain: 'my mind seems to have become a kind of machine for grinding general laws out of large collections of facts'.[1] Everything had become grist to the mill of his attempt to find some larger pattern of meaning in the profusion of life on earth, otherwise a mass of irreducibly *sui generis* entities. This was a complaint, but as a consolation he did end up producing *On the origin of species*. When reading Victor Lieberman's vast work of comparative Eurasian history one is moved by a sense of an equivalent unity of purpose, a possibly obsessive labour driven by a determination that the world be called to order. Luckily, the result places him much closer to Darwin than to Edward Casaubon.[2]

Historians are increasingly prompted to raise their heads above the sub-disciplinary walls of regional specialization and consider their work in a more global light. However, given that the tendency over the last generation or more has been for grand narratives of all kinds to be junked, for Marx and Weber to be bundled into retirement, for historical sociology and history to become somewhat estranged, and for large-scale causation to be eschewed in favour of synchronic recreation or the excavation of the subjective, it has not always been evident whether historians have the conceptual tools truly to profit from these expanded horizons.[3] One fruitful response has been to follow particular movements that connected up otherwise diverse parts of the globe. Yet the very act of considering connections is likely to invite comparison: we might want to understand how local conditions differed in order to appreciate how any given new idea, migrating elite, or item of technology was translated and incorporated. Ultimately, then, the intellectual globalization of history is likely to breed historians with a greater tolerance for the generalizations that comparative analysis requires.

The *Strange parallels* project, encompassing the first volume on mainland Southeast Asia as well as this 947-page second volume on what would become France, Russia, Japan, China, India, and island Southeast Asia, is an extraordinary achievement that will have a lasting influence on how we think about global history. It does, however, demand sustained digestive work from the reader. The purpose of this review article is to explore certain aspects of this unusually complete and original intellectual world. I do not mean to suggest that it is so exotic as to require an ethnographer – indeed it comes complete with exemplary self-exegesis – but it does refer to the Dutch as 'White Inner Asians'.

1 Charles Darwin, *Autobiography of Charles Darwin*, London: Watts and Co, 1929 (first published 1887), p. 74.

2 An alarming image of doomed monomaniacal scholarship in George Elliott's *Middlemarch*.

3 See R. Bin Wong, 'Causation', in Ulinka Rublack, ed., *A concise companion to history*, Oxford: Oxford University Press, 2011, pp. 27–56, on a loss of confidence in grand narrative and causation.

This article also sets out to test some of the strengths and weaknesses of Lieberman's analysis, with a particular focus on how productively the analysis handles ethnicity and religion.

At several points in the book, Lieberman carefully summarizes his argument (indeed the whole of Chapter One is an overview of both volumes), but in the interests of not merely repeating the author I shall represent his main findings a little differently. The patterns he describes are given form through:

1. Cycles. The societies that Lieberman considers are presented as developing in cycles or bursts of expansion and disintegration, in the sense that the forces that drive the former may also precipitate the latter. At the same time, there is an emphasis on a linear movement underlying the rise and fall of political structures, as we shall see.
2. Holism. Development appears to work in holistic fashion, by which I mean that Lieberman's main indices of comparison – of economic and demographic expansion, territorial consolidation (where a region is resolved into fewer and fewer polities), political centralization, and cultural homogenization – are related in important ways and often seem to proceed hand in hand.
3. Rhythm. The gaps between these developmental bursts tend to become shorter over time.
4. Coordination. These cyclical movements are strangely coordinated across Eurasia, and increasingly so as time goes on.
5. Protection. While the above factors draw out commonalities across all his cases, Lieberman invests just as much energy in showing how the histories of European, Japanese, and mainland Southeast Asian cases lying in the 'protected zone' of Eurasia differed from China and India in the exposed zone. The former were protected from the empire-building feats of Inner Asian conquest elites, whereas the latter were not.

All of these findings demand historical explanation. For example, why should there be any sort of coordination among such diverse regions in their experience of expansion and crisis? Why does there seem to be an association between political and cultural integration? What difference did subjection to foreign conquest elites really make? Such questions invite us to suppress inherited geographical reflexes. Contrasts between 'Asia' and 'Europe', for example, are largely swamped by other terms of comparison.

Teleology

In the crises or interregna that punctuate periods of expansion, anachronistic arrangements are presented as subject to creative destruction: 'a Darwinian flight from earlier weaknesses' (p. 60). However, emergent polities also succeed in building upon earlier administrative legacies and tapping into more subterranean currents of cohesive potential. These less visible currents are explored in great detail. They include an increasingly integrated market, a commercialized and monetized economy, growing attachment to an increasingly preponderant capital city, and a tendency for peripheral regions and lower orders to be drawn into the languages, literatures, and religious movements characteristic of the capital. As the regions advance along all these dimensions of integration, the severity and length of their crises becomes shorter each time.[4] If Victor Lieberman's vision of history is cyclical, his cycles are not fixed in place, like Catherine wheels, but are more like the wheels of vehicles that go places.

But hold on – where have we arrived now? A sense of coherent *movement* in history – of *forward* movement no less? Some vague discomfort may already be gathering in the bowels of the professional historian; fingers may already be reaching out for well-worn academic panic buttons labelled 'teleology' or 'Whiggishness'. For what is the end result of this forward momentum? In the protected zone it is something approaching the nation-state. Lieberman thus ends up with France, Russia, Japan, Burma, Thailand, and Vietnam; and even in the exposed zone we see some of the ligaments that would allow China and India to be pulled into being in the modern era. They were all increasingly 'coherent' realms in many dimensions before the arrival of the modern ideology of nationalism, or (in the case of the Asian regions) before the advent of European modernity in the shape of colonialism. Yet the thrust of many of recent works of world history has been to break free from 'France, Russia, Japan, Burma, Thailand, and Vietnam': that is, to refuse the nation as a self-evident category of analysis for the revelation of currents of change and myriad interactions connecting up much broader

4 See Victor Lieberman, *Strange parallels: Southeast Asia in global context c.800–1380. Volume 2: mainland mirrors: Europe, Japan, China, South Asia, and the islands*, Cambridge: Cambridge University Press, 2010, pp. 75, 270.

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regions.[5] And in trying to explain this forward propulsion, in identifying the workings of its hidden motors, does he not thereby reinforce some sense of its inevitability?

As always with criticisms of 'teleology', there are, in fact, two kinds of objection here, and the first is trivial: that Lieberman has set out to answer a politically or intellectually conservative question, which is frankly a matter of taste. While previous answers may have been couched in an objectionably nationalistic, triumphalist, or simplistic idiom, this says nothing about the validity of the question itself. Moreover, Lieberman's approach acquires a radical quality from its global nature: there have been few serious comparative attempts to look at these very long-term developments outside the West and to take Ayudhya as seriously as Bourbon France. Furthermore, the *really* Whiggish approach would be to assume that the increasing 'coherence' of these regions is simply a function of the direction of history itself and can therefore be taken for granted. Lieberman, by contrast, refuses to assume that the coalescences that he discerns are natural and therefore uninteresting; he is puzzled by them; he asks us to step back and see them as the result of particular historical forces, as part of previously obscure, more global patterns.

The second objection might be that Lieberman has been drawn into anachronism in over-emphasizing the ways in which features of the modern world were progressively anticipated in earlier periods, perhaps because he has been misled by outdated models or been too blithe in ignoring the increasing complexity of recent scholarship. A simple glance at the footnotes should help allay the latter concern. More fundamentally, however, one must consider the great height at which he has set up his observation station. From this altitude certain revisionist movements against older narratives are simply going to seem less explosive, even if they hardly disappear. For example, for many years references to European 'absolutism' or the homogenizing and disciplining power of the Reformation have given ground to explorations of how these movements were thwarted, redirected, or appropriated on the ground. But in nonetheless emphasizing these phases as intensifications of what went before – in terms of political centralization in the former, and religious discipline in the latter – Lieberman cannot be accused of simply repackaging dusty Whiggish nostrums. He not only acknowledges regional revisionisms but is even content to bring them into alignment. While discussing what Mughal state-building had in common with contemporaneous regimes in Europe, Japan, and mainland Southeast Asia, he lists the various ways in which central authority was enhanced (improved infrastructure, efforts at legal standardization, systematized legal records, and so forth) and then finishes a paragraph with the un-anxious comment that 'In each case, an early historiographic insistence on central imposition has yielded to a greater appreciation of local powers of negotiation and resistance' (p. 651).

Why un-anxious? This is partly because his model is of such long duration that while emphasizing the 'achievements' of any one phase he is ready to acknowledge their limitations because he is already anticipating, in cyclical fashion, the next collapse – in this case, the early eighteenth-century collapse of the Mughals – and the rise of a superior avatar. But also on display is the comparativist's confidence that he is simply asking a somewhat different question from the regional specialists. He does not need to prove that the early modern French kings were 'absolutist' in the conventional sense, but something more modest – that French kings *circa* 1750 were more powerful than in 1500. Out of such modest building blocks are his grander claims constructed.

Furthermore, as with all good comparative history, Lieberman's approach demystifies national histories by rendering them variations on broader patterns, and by refusing to succumb to purely internal explanations of historical change. Indeed, while some may charge him with reifying the nation-state, Lieberman may also face criticism that he has refused to take national distinctiveness seriously, insofar as he has suppressed crucial differences between his cases in his bid for commonalities, that he has boiled them down into some sort of mono-flavoured stock. However, not only is he scrupulous in explaining relevant contrasts between his cases at great length, but his most important comparative claims are not so much about the *nature* of changes as about their *rhythms*. Arguing that, say, Toungoo Burma can be placed in the same category as sixteenth-century France by virtue of the way in which their capital cities were successful in subordinating

5 See Micol Seigel, 'Beyond compare: comparative method after the transnational turn', *Radical History Review*, 91, 2005, pp. 62–90, for a critique of comparative history as sympathetic to the nation-state as a unit of analysis, as opposed to the more deconstructive 'transnational' history.

peripheral regions, is not to assume that this had the same implications in France as it did in Burma. He is not setting up a big universal measuring stick and lining up his candidates alongside it in the hope that they will be roughly the same height at the same times. Instead he furnishes each candidate with its own measuring stick and watches how they all develop. It is the direction, dynamics, and timing of growth that is important for his argument, rather than their extent in absolute terms. Hence he claims 'that within each region *judged by local standards*, political and cultural cohesion in 1830 exceeded that in 1600, which exceeded that in 1400 and so forth' (p. 53, emphasis in original; see also p. 124). Sometimes the universal measuring stick *is* brought out, and then we are liable to be shown how diverse the assembled candidates are.[6]

Synchronicity

Lieberman sets out to show how integration – and particularly the economic and demographic growth on which political and cultural forces depend – became increasingly coordinated over his millennium. In Southeast Asia, Europe, and China, for example, economic and demographic expansion was particularly notable from 800/900 to 1270, 1470/1500 to 1640, and 1700 to 1830.[7] Explaining such synchronicity is one of the major tasks of the book, although it is also where the author is ultimately at his most tentative (pp. 77–84). Much of the weight of the argument is assembled in favour of variations in climate as the principal underlying determinant, but in the end Lieberman pulls back from rendering it the key role, noting an 'uncertain interplay' with social and technological mediators of climate.

At first sight, Japan appears to be a major breach in Lieberman's argument for synchronicity, for its chronology looks so different from all the other cases: little long-term growth in population and agrarian output from 730 to *c.*1280, while subsequent expansion was accompanied in the political dimension by 'glacial devolution' from the late 1300s that finally led to the breakdown of central authority in 1467. Lieberman uses this as the 'exception to prove the rule' about which coordinating factors were most potent: climate change and disease exposure. The other main reason why the Heian order was simply allowed to decay without being plunged earlier into a decisive phase of creative destruction earlier is Japan's relative isolation. Indeed, Japan emerges as hyper-protected. However, with the reunification of Japan by 1603, it appears as 'back on track' with other protected-zone realms. Why should this be? Part of the reason is that Japan was simply becoming less isolated, being pulled into maritime trade, turning itself into a great silver producer, and rapidly taking advantage of that technological boon to centralizers, firearms.[8]

This illustrates a more general point, which is Lieberman's contention that synchronicity becomes more apparent as the second millennium wears on. The 'charter' phase for the protected zone is really a rather loosely coordinated one, but with the resurrection of new polities from the mid fifteenth century onwards the 'strange parallels' of the title gain greater weight. This is plausibly connected to the fact that Eurasia itself was increasingly linked by more extensive flows of people, technologies, trade, and so forth. If this thesis of 'early modern' synchronization is to stand up to the kind of scrutiny it deserves, then it should provide substantial support for a claim that is often made (indeed, is fast becoming disciplinary common sense) but that is far more difficult to back up with convincing evidence: that the 'early modern world' deserves that title because we see a step change in the connectedness of all societies – that we see, in short, something like a first globalization, whose participants and implications go far beyond the normal narrative of European discoveries. It should indicate that manmade

6 For example, sixteenth-century France and Burma may be analogous in their deployment of 'viceregal plenipotentiaries sent by the crown to replace hereditary dynasts in newly annexed areas' (Lieberman, *Strange parallels*, vol. 2, p. 252), but Lieberman then goes on to list six major differences in the nature of administration from the monetization of bureaucracy to the fact that France had one royal appointee for every 45 square kilometres, while the Burmese empire had one for every 470–700 square kilometres.

7 *Ibid.*, p. 548. South Asia also experienced long-term increases in cultivation, population, and trade *c.*850–1300 and *c.*1500–1700, although thereafter it diverged somewhat (p. 703).

8 See Lieberman, *Strange parallels*, vol. 2, pp. 55–6, 376–80, 416.

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connective forces were indeed making a decisive impact on the very tempo of Eurasian history, rendering the more ‘natural’ factors of climate and disease less important. While Lieberman is most striking as a purveyor of an unusually pure form of comparative history, his concern with synchronicity has involved a serious contribution to connected history.

The analytical implications of protection/exposure: empires, politicized ethnicity, and state efficacy

The concept of geographical protection from Inner Asian domination clearly has a major place in Lieberman’s project, and yet on further reflection it may not be quite clear how the distinction is related to his major arguments. In his preface, Lieberman says that he considered foregrounding the distinction even more, by dividing the book into two sections along the protected/exposed fault line but ‘ultimately declined to do so for fear that by masking overlapping similarities’ he would end up with a ‘no less deceptively reified bifurcation’ (xii). It is possible, then, that he initially imagined that the distinction could be made to do more analytical heavy lifting, but that his extensive reading on East and South Asia illuminated too many interesting parallels that cut across this division. The principle of geopolitical protection does not by itself predict conformity to particular chronological rhythms, for we saw that Japan was furthest removed from any of the protected or exposed zone cases, and nor does it predict how territorially consolidated a wider region may be: between 1750 and 1830, Japan remained just one unit, Southeast Asia went from nine to three, while Europe moved from 370 to 57 states (p. 274). Nor does it by itself predict the presence of politicized ethnicity, which Lieberman argues to be weak in Japan and pre-sixteenth-century island Southeast Asia, or whether such states would survive into the modern world in roughly recognizable form, given that China and India are with us today as nation-states just as much Japan or Russia. This is presumably why he describes the protected zone as forming only a ‘modestly distinct complex’ (p. 48).

Lieberman presents us with the following three distinctive features of the protected zone. The first is almost a restatement of its definition: it was not occupied for any substantial period by Inner Asians owing to distance from their heartlands, topographical barriers, or inadequate pastureland (p. 114). This means that the most characteristic political form of early modern Eurasia, the great multi-ethnic agrarian empires ruled by elites of Mongol, Turkic, Afghan, Mughal, Manchu, and Jurchen origin, could find no purchase in these areas. The second feature concerns the timing of ‘charter states’,[9] which, in the protected zone, all arose in the latter half of the first millennium by importing from much older centres a package of ‘civilization’, including technological, political, and cultural legacies. Conversely, all the earliest civilizations lay in the ‘exposed zone’.[10] Thus civilizational precocity and subsequent vulnerability to Inner Asian prowess were linked: both depended on an openness to the intellectual and material exchanges that flowed through the main conduits of Eurasia.[11]

However, we are still left asking what impact these two factors had on the processes that Lieberman has in his sights, which is where his third distinction comes in: ‘In most protected-zone realms, modest scale joined sustained interstate competition to favor cultural integration more readily than across India and accelerating administrative centralization more readily than in China and India’ (p. 114). This is carefully worded and requires a great deal of unpacking, which follows.

First, it would seem that it was not the presence of Inner Asians themselves that was critical but rather the presence of large empires *per se* – hence Iberians, Dutch, and British can be considered ‘White Inner Asians’ for their early exploits in the archipelago and India. Moreover, it appears that exposure to *external* elite agency is not critical either, for imperialisms can bloom from within

9 These are the first major indigenous states in any given area, from which subsequent states trace their origins.

10 These include the primary civilizations of the Indus valley, southern Mesopotamia, and the Nile valley, and the secondary states that developed and expanded from these areas as Maurya, Gupta, Han, and Rome (Lieberman, *Strange parallels*, vol. 2, p. 108).

11 See *ibid*., 109, 902.

regions as well as without them.[12] So Lieberman's point has to be something different. It seems, ultimately, to be a question of size. At this point, I think it may be helpful if we introduce a definition of empire. I shall propose that empire happens when a state structure maintains a certain tension between the unity and heterogeneity of its constituent parts, in both political and cultural dimensions. That is to say, if all empires tend to deploy processes of political and cultural integration in order to bind their parts together, they only stay as empires to the extent that they nonetheless maintain some diversity beneath the ruling elite.[13] Returning to Lieberman's protected zone, we might say that here state expansion was likely to be limited to areas in which the integrative forces that pre-modern states had at their disposal could get to work effectively enough to dissolve that heterogeneity.[14] Whether they were foreign dynasties assuming the throne or peripheral regions brought under it, new elements were usually absorbed by one dominant culture and administrative system.

One consequence of this rough ceiling in spatial expansion was a multistate environment (except in Japan): a number of states of medium scale, each able to promote cultural integration and growing attachment to an authoritative centre, and yet also sharpening their self-awareness through military conflict and competition with each other. The result of both these conditions was 'politicized ethnicity'.

Appreciating the consequences of South Asia's exposed status allows Lieberman to stand well back from the influential and sophisticated historiography of this region and identify what is particular – indeed peculiar – about it. In *c.*900–1300, South Asia is presented as roughly analogous to Europe in the scale of the regional kingdoms that were emerging. But then we see the region enter an oscillation between Turkic- or British-led imperial formations and regional resurgences, which thus 'prevented regional cultures from entering into the sort of centuries-long continuous synergy with medium-sized polities that we find' in the protected zone (p. 713). This helps to explain why ethnicity seems to have had such limited historical import.[15] Strikingly, if we agree with Chris Bayly that the eighteenth-century states did cultivate something akin to 'regional patriotisms',[16] they were rather novel affairs that were generally unwilling or unable to locate legitimizing precedent further back than 1560.

All this can help to clarify the sort of confused apprehension that the scholar of Sri Lankan history may have towards Indian history. It is only natural to locate Sri Lankan research within the wider field of 'South Asian' (essentially Indian) scholarship. And yet, for all the island's intimate ties with the subcontinent, there is something about the world described by this scholarship that can remain stubbornly alien. Lieberman's work seems to indicate that the source of this sensation lies in the more profoundly cosmopolitan and discontinuous nature of Indian politics as a function of its more 'exposed' situation. Sri Lanka, which I have suggested was akin to island Southeast Asia in remaining relatively protected before the sixteenth century, saw an unusually long-lived coalescence of language, religion, origin stories, historical memory, and image of political unity.[17] All Sinhalese polities saw themselves as the latest in a line of Sinhala-speaking Buddhist states that stretched back to the genesis of the

12 In fact, Lieberman appreciates that all pre-modern polities have an imperial quality in their predisposition to expand over diverse peoples and polities; hence sixteenth-century France, Russia, and the mainland Southeast Asian realms are at one point referred to as 'polyglot empires' (p. 206).

13 F. Cooper and J. Burbank, *Empires in world history: power and the politics of difference*, Princeton, NJ: Princeton University Press, 2010) are helpful here, but their biggest theoretical lacuna is in distinguishing 'empires' from 'kingdoms'. This is what Lieberman's approach illuminates by focusing on the dynamic of integration.

14 Conversely, in the exposed zone it was possible for political formations to be assembled of such size and diversity that they placed limits on the capacities of 'imperial' centres to incorporate and homogenize them.

15 As emphasized by Sheldon Pollock, *The language of the gods in the world of men: Sanskrit, culture, and power in premodern India*, Berkeley, CA: University of California Press, 2006, pp. 474, 476, 509–11, 715, for example.

16 See C. A. Bayly, *Origins of nationality in South Asia: patriotism and ethical government in the making of modern India*, Oxford: Oxford University Press, 1998.

17 Alan Strathern, 'Sri Lanka in the long early modern period: its place in a comparative theory of second millennium Eurasian history', *Modern Asian Studies*, 43, 4, 2009, pp. 809–64.

Anuradhapura civilization in the centuries BCE.[18] It is only by taking up Lieberman's geopolitical tools that we can pare Sri Lanka away from its common-sense assimilation to 'South Asia'.[19] Here, again, we find that received geographies are productively called into question.

However, strictly speaking Lieberman's argument is not about ethnicity *per se* but about the relationship between ethnicity and the state. It was only in the protected zone that common projects of politicized ethnicity uniting ruling elite and the mass of subjects around stable political centres were to be found over the *longue durée*. What difference does it make, however, that the integrative sentiments in one polity had an ethnic or ancestral quality and in another were cultural?[20] Cultural forces can produce homogeneity and define belonging too, and the cultural and the ethnic usually blur into and inform each other. For all that the Qing pursued a classic form of imperial cultural politics in distinguishing the Manchu elites from the Han Chinese as just one among several ethnic blocs, they were nevertheless agents of cultural integration within China proper, overseeing the extension of Neo-Confucian norms and forms down the social scale.[21] If the cultural dimension of 'Chineseness' was actively promoted by its foreign rulers, while its ethnic dimension remained alive to be readily politicized in times of crisis and strain, one might ask how much it ultimately mattered that Chinese ethnicity was cut off from the imperial court. Politicized ethnicity may be assembled against empire or despite it, as well as within more discrete entities.[22]

So much for cultural and ethnic integration, but what about administrative centralization? What are the consequences of being 'protected' for the efficacy of state structures? The argument sometimes seems to be leading to the suggestion that a common project of politicized ethnicity across the ruling/ruled divide conferred a Darwinian advantage, but in fact Lieberman explicitly says that 'neither Manchu nor Mughal experience supports the assumption, basic to modern nationalism, that ethnic and religious solidarity between rulers and ruled was a necessary precondition for political effectiveness' (p. 105). Indeed, 'in terms of territorial conquest, internal stability, cultural circulation, and economic output, the Qing may have been the world's most successful early modern dynasty' (pp. 597–8). Inner Asians thus drove forward many of the dimensions of progression that concern Lieberman, territorial and economic expansion perhaps above all.

So does the protected/exposed distinction make no difference here? A clue is provided in the preface, which refers to the 'smaller-scale, *more manageable* demographic and political units' of the protected zone (p. xxii, emphasis added).[23] Once again, then, it is a question of sheer size. The point here is not that the administrative systems put in place by these foreign conquest elites were any the less innovative, sophisticated, or anticipatory of modernity. It is rather that their very ability to assemble such far-flung dominions presented insuperable limits to centralization for pre-modern systems and technologies.[24]

18 In this sense, Sri Lanka's early charter-state genesis does not fit with a 'protected zone' categorization, as the Indian Ocean seems to have been more a conduit of civilizational communication than a barrier.

19 This is not to deny the importance of links between the island and the subcontinent, or the continuing importance of south Indian political forms and waves of immigration.

20 The terminology of cultural 'coherence' or 'integration' may seem perverse to scholars of later, more specialized, more 'complex', and interconnected societies and has occasioned criticism in this way from Mary Elizabeth Berry (see her 'Public life in authoritarian Japan', *Daedalus*, 127, 3, 1998, pp. 133–65). However, Lieberman's argument is not that 'diversity' of whatever kind goes away – rather, it shifts its nature. New forms of diversity are played out on a greater field of common language, learning, religion, and loyalty.

21 Lieberman sees this: in the third feature of the protected zone above, note the absence of China in the first clause: 'favor cultural integration more readily than across India and accelerating administrative centralization more readily than in China and India'.

22 Again, it should be clarified that none of this deviates from Lieberman's argument, strictly speaking; it merely places some limits on the power of the protected/exposed distinction to generate predictions about the strength of ethnic sentiment considered more widely.

23 And recall that the list of Qing achievements quoted above does not include political consolidation.

24 The consequences of this for India and China were, however, quite different. In India it meant that imperial powers were always subject to centrifugal entropy, that political evolution was far less continuous than in the protected zone. In China it meant that the

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Recall also that, for Lieberman, the decreasing length and severity of interregnal periods is a result of enhanced integration across his various indices as reflected in a 'psychology of interdependence' (p. 270; see also p. 75). To be consistent with his model, there should then be implications for their long-term durability, in the sense that a greater burden of integration is thrown onto cultural mechanisms. Sometimes we do see a potential implication of the absence of a politicized ethnicity overarching elites and subjects. For South Asia, Lieberman tugs on the shirttails of the current consensus, which focuses on the lasting appeal of Mughal authority and the far-reaching attractions of its generally tolerant Perso-Islamic high culture, by suggesting that the inability of the Mughals to create either an ethnic or a religious basis for elite unity eventually told for them in the face of regional resurgence and non-Muslim revolts.[25]

Religion and discipline

Cultural essentialisms, Weberian summations of civilizational wholes, and triumphant celebrations of national genius are all given short shrift by Lieberman's method. As he says, 'explanations of local change framed entirely in terms of idiosyncratic cultural or social traits become prima facie suspect' (p. xxii). And yet Lieberman is unusual among those engaging with the grand questions of global history in taking culture as seriously as any other field of life.[26] How exactly is cultural change lent agency as a propeller of the grand dynamic of coherence and not just an epiphenomenon of it? Here we shall scrutinize the role played by religion and 'disciplinary revolutions'. While Joseph Fletcher, one of the first scholars to imagine a global early modernity, saw patterning as no less credible in the field of religion than in any other sphere, historians have largely left it alone since then.[27] Contained within *Strange parallels* is the most ambitious argument about the role of religion in global early modernity to have emerged thus far. For the moment, this crude sketch will have to suffice.

Religion in the societies of charter-era Europe, Japan, and mainland Southeast Asia is presented as fractured between a 'culturally encapsulated' elite world centred on the capital and a more unfathomable world of peasant religion. The former might have had cosmopolitan and promiscuous spiritual appetites but sought to establish grandeur through its patronage of great establishments devoted to a world religion – monastic centres, temples, cathedrals, and so on. These tended to look outwards to other royal and religious centres within the wider civilizational ecumene (for example, Sanskrit, Pali, Latin Christianity, etc.), rather than to concern themselves too much with the diverse local and popular cults of the rural hinterland. The latter sustained largely illiterate populations, which were less able to participate in the *imaginaire* created by sacred texts.

Following the charter era, the realm of religion 'cohered' in a number of ways, which I would summarize thus: first, the population under political control came to participate more fully in religious practices and beliefs common to the whole territory. Purely local saints and gods might be inflated to a national level or sidelined and delegitimized, or incorporated into a single hierarchy. As always, this worked both horizontally, drawing in peripheral regions, and vertically, plunging deeper into the peasantry. Second, religion itself displayed a greater emphasis on coherence: literacy, schooling, and doctrinal understanding were advanced; boundaries and orthodoxies were lent greater weight. Third, religious institutions not only expanded and deepened their presence among the population at large but were subject to a form of centralization that both mirrored that of the state and then became increasingly subjugated to the interests of the state

state rarely felt in a position to subject its regions to intensive taxation.

25 Lieberman is careful to distinguish his position from that of older nationalist or communalist historiography. It has long been recognized that the Mughal throne retained an aura of legitimacy well into the nineteenth century, so few historians today would argue for the general friability of the Mughal cultural project. Lieberman's claim is smaller than this.

26 C. A. Bayly, *The birth of the modern world, 1780–1914*, Oxford: Blackwell Publishing, 2004, is another exception.

27 Joseph Fletcher, 'Integrative history: parallels and interconnections in the early modern period, 1500–1800', *Journal of Turkish Studies*, 9, 1985, pp. 37–57. An exception as a textbook is Merry E. Wiesner-Hanks, *Religious transformations in the early modern world: a brief history with documents*, Boston, MA: Bedford/St Martin's, 2009.

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(as the great monastic centres, for example, lost their autonomy). Fourth, these increasingly shared discourses of religious truth described and enhanced rulers' legitimacy and fused in powerful ways with ethnic and political loyalties. In other words, they might lend a moral and 'transcendental' charge to identification with the political centre. Once again, these generalizations are geared towards the experiences of protected-zone societies, with looser and less complete analogies in the exposed zone (and least applicability, perhaps, to the Indian subcontinent).

This is where Lieberman's comparative method may prove most indigestible to regional specialists: scholars of the subjective realms of religion and thought are often the most redoubtable particularists. One must, for example, note fundamental differences in how religious boundaries were conceived between the monotheistic cases and the Indic and East Asian. Occasionally, Lieberman does seem to strain too hard for equivalence between hugely divergent religious cultures. He seems most sympathetic to the arguments of Jean Delumeau that Europe was 'Christianized' in the era of early modern reform, following a medieval period in which the laity languished in animist behaviours beneath a thin veneer of Christian forms.[28] This image of a pre-Christian medieval Europe is more readily comparable to charter-era mainland Southeast Asia, where many rural areas were yet to be even superficially introduced to Theravada Buddhism. However, most scholars have considered Delumeau to have pushed his claims too far with respect to a medieval Church whose rituals provided the rhythm of life for many peasant communities, and whose eschatological significance was widely felt. There were meaningful ways in which the Church overarched divides between capital and regions, town and country, and elite and lower orders. In fact, however, Lieberman has really only come down too heavily on one side of a viable debate rather than claiming anything utterly outlandish by the standards of European historiography. More importantly, as always it is the direction of change revealed by the *relative* measuring sticks that is ultimately at issue, and here it makes little difference whether rural France was much more profoundly Christianized than rural Siam, Cambodia, and Burma were Buddhicized; the point is that in both areas the early modern period saw more determined efforts to shape the religious lives of the masses than previously – and to note that this chronology in some broad sense tallied with the growing claims of political centres.

In general, Lieberman is close to his fellow Southeast Asianist Anthony Reid in arguing for a strong connection between the adoption, control, and dissemination of 'universal' religions and the construction of more powerful and far-reaching states.[29] Lieberman does not suggest that religious change was simply a design of the state: both were dependent on common developments (for example, literacy, printing, and urbanization), while neither are given clear priority in terms of cause and effect.[30] Indeed, Lieberman advances an audacious argument for religion's causative power that takes its inspiration from the work of the historical sociologist Philip Gorski.[31] Gorski drew upon insights from Weber, Elias, and Foucault to argue that early modern European states were not simply imposed from above but were enabled by social changes welling up from below. Religious change created institutions and patterns of behaviour that had the side effect of making the realm more governed and governable; it improved the human material upon which effective states could be built by cultivating discipline at the most intimate level of psychology – yielding more pacific, virtue-conscious, obedient individuals – and the social level of communal surveillance.

Gorski himself was unabashed about his contention that Calvinism was the most effective disciplinary force and that this explained why, for example, the Dutch republic ascended so quickly to a degree of success apparently out of proportion with its observable state apparatus. It could immediately punch above its weight because of the virtues and consistories of its Calvinist population. Gorski's book has not always been welcomed with open arms by historians. Critics of the 'confessionalization' paradigm argue that the relationship between state-building and religious change was far too

28 Jean Delumeau, *Le catholicisme entre Luther et Voltaire*, Paris: Presses Universitaires de France, 1971.

29 Anthony Reid, *Southeast Asia in the Age of Commerce, 1450–1680, volume 2: expansion and crisis*, New Haven, CT: Yale University Press, 1993.

30 See Lieberman, *Strange parallels*, vol. 2, p. 359.

31 Philip S. Gorski, *The disciplinary revolution: Calvinism and the rise of the state in early modern Europe*, Chicago, IL: University of Chicago Press, 2003.

ambiguous and chaotic to fall quietly into such a pattern, while adherents of confessionalization see little point in giving up their central contention that the consequences of reformism crossed all confessional divides. Gorski could even be seen to be reverting to an antique historiographic mode of unreconstructed Protestant self-congratulation. A more substantive criticism has been to ask how much weight one can accord the 'disciplinary revolution' in determining state efficacy given that some of the strongest examples of state-building were, in fact, the Catholic realms of Habsburg Spain and France.[32]

Nevertheless, this instance illustrates how Lieberman's comparative concerns allow him to make use of model-building scholarship without falling prey to the empirical criticisms that have been put its way. He is quite at liberty to note:

> But as Gorski concedes, Calvinist reform was merely the most insistent version of a wider early modern shift in European sensibility affecting Catholic and Lutheran lands as well. Thus counter-reformation France experienced a tightening of clerical regulation, the spread of popular confraternities and sacraments, more demanding systems of poor relief, expanded lay and ecclesiastical roles for women, and a notable expansion in education, all of which served to align personal salvation with public discipline, to instil respect for hierarchy without a direct outlay of resources, to curb physical assaults, and thus to make governance more feasible. (p. 72)[33]

Gorski is thus dragged back towards the consensus historiography of pan-Christian developments, but not at the expense of mislaying what may be of profound value in his method. If Gorski's argument thus seems watered down with regard to his European claims, it is also greatly inflated in the process of being refashioned as a tool for doing global history. In the process, Lieberman advances a major new argument of comparative history about the relationship between psychological, cultural, and political change that could be extracted from the book and made the basis of a monograph in its own right.

This would, however, have to address some frighteningly large questions. Is the natural condition of man otherwise a form of Hobbesian violence (i.e. what do 'non-disciplined' societies really look like)? Are disciplinary revolutions not on some level equivalent to the forces of socialization that allow human communities of all kinds (including small-scale hunter-gatherer groups) to function? If so, then perhaps the disciplinary revolutions in question are distinguished by being particularly suitable for more large-scale, urbanized, and mobile populations in which face-to-face interactions, kinship ties, and local cults have lost their traction? To what extent do more peaceable populations really give state centralizers an advantage? Do waves of cultural-psychological disciplining necessarily accompany successful pre-modern state construction?

Obviously, religious ideologies (and particularly, I would add, those born out of 'Axial Age' breakthroughs, with all their finely tuned machinery for emotional and moral transformation) are likely to be the most natural candidates. But Lieberman takes a wide range of cultural forces as capable of creating more state-friendly communities: other European examples include courtliness and Renaissance humanism, while he suggests that the increasing hold of caste principles over most of India from the seventeenth century may have advanced the state's interests in pacification and control.[34] Lieberman clearly sees Theravada Buddhism as potentially having a similar function, but (perhaps simply because Gorski's work came out too late to be considered for volume 1) we are not given many details as to how the extension of monasteries, literacy, and so forth led to state-friendly behaviour among the laity at large.[35]

While the argument may have originated in a European context, Lieberman is surely right to contend that it may find its most fruitful application in the case of China. Certainly Chinese imperial governments had long understood the advantages of moulding a far-flung population with minimal state apparatus through the cultivation of moral behaviour and obedience. Neo-Confucianism could

32 See the review by R. Po Chia Hsia in *Central European History*, 38, 2, 2005, pp. 280–2.

33 See also Lieberman, *Strange parallels*, vol. 2, p. 72, n. 86: 'However impressive Prussian and Dutch performances, it is worth remembering that the two most successful European states c. 1550 to 1750 were Catholic Spain and Catholic France.'

34 See *ibid*., p. 743.

35 See *ibid*., pp. 39, 72, 284–5, 359. See also vol. 1, p. 137, for a reference to a teetotal movement in Upper Burma.

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be considered the 'disciplinary revolution' par excellence. In the Neo-Confucian scholar class, the empire had at its disposal a class of literate non-military elites distributed throughout the regions but able to respond to the moral imperative issuing from the centre, and who would work towards the common good at a local level without necessarily demanding too much in the way of recompense and official status. I have already referred to the way in which the Neo-Confucian norms then percolated into rural classes below the gentry in the Qing period.[36] China may thus have benefitted from unusually powerful integrating cultural forces, allowing the state to spread itself so thinly across such great distances and yet hold together despite the decline of political initiative at the Qing centre and all the ructions and colonial predations of the nineteenth century.

The social power of religion is largely discussed in terms of its ability to integrate populations, but of course it may also fragment them into smaller, harder groups who refuse to dissolve themselves into state containers. If ruling elites are able to appeal to matters of salvation, ultimate truth, and universal ethics that are shared by the common mass of their subjects, their claims to political pre-eminence may become more palatable and compelling. However, such matters have an unfortunate tendency to divide opinion. Their very 'transcendence' over mundane politics establishes their unrivalled ability to legitimize it; yet it can also mean that mundane political loyalties may pale beside the pursuit of much higher ends. In short, if the masses of a number of 'early modern' societies were indeed entering more fully into the salvific projects of the world religions, then they formed 'human material' that could explode the pretensions of states as well as uphold them. Europeanists might wonder whether Lieberman has produced confessionalization writ large, and confessionalization theory in the classic sense has come under attack for failing to address how disruptive the passions unleashed by the Reformation could be, how they inflamed local communities or sects that sought to resist or evade the state.

The French wars of religion are therefore something of an anomaly in Lieberman's scheme: they appear as an unusual twist on the late sixteenth-century crisis ('extremism followed a peculiar internal logic' (p. 267)). He does not shy away from describing the communal violence of the St Bartholomew's Day massacre, for example, nor fail to identify its source in 'eschatological anguish' (p. 268). It is merely that episodes such as these emerge out of the blue rather than issuing organically out of his overarching theory. Perhaps they might be included as a more paradoxical feature of the strengthening grip of salvationism, much in the same way that economic growth could both fundamentally promote and yet also come to undermine state centralization. At any rate, this is not to impugn his line of argument; indeed one might comment that it is all the more remarkable that central authority was so firmly re-established in France, and sacral Catholic monarchy buttressed in newly explicit ways, given that its late sixteenth-century fracture was compounded by soteriological fault lines. Moreover, there is no substantial analogy to the Wars of Religion in the non-European case studies, even if Japan's long 'interregnum' of 1467–1603 was exacerbated by autonomous monastic institutions drawing on mass salvationism and an offshoot of confessional traumas in the form of Christianizing *daimyo* (territorial lords). Religious difference is naturally acknowledged in the cultural politics of the exposed zone, as we saw above.

Modernity and early modernity

Anyone who has attempted to work seriously with a term such as 'early modern' will know how quickly one runs into conceptual and empirical quicksand. One problem is that modernization theory has been roundly discredited as economically resurgent Asian societies have failed to 'converge' into a single form based on Western characteristics. At the same time, however, the shake-up of the world order has led scholars to reclaim indigenous potential for contributing to modernity independent of Western initiative. What to do? Attempt to stipulate the features of modernity and one risks being tarred with the brush of out-of-date or Eurocentric sociology. Yet, without any attempt to specify its contents, the concept obviously remains vacuous. This is the dilemma that has given rise to such terminological contortions as 'multiple modernities'.[37] The reduction of

36 See Lieberman, *Strange parallels*, vol. 2, pp. 474, 608–9.

37 It should be pointed out that Shmuel N. Eisenstadt and Wolfgang Schulchter,

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modernization to globalization and the consequent search for interconnectedness has been one response. But most scholars clearly itch to say something more profound than this. Another major problem with the 'early modern debate' has been that its generalizations about the era have rarely proceeded from a medieval baseline against which 'early modern' characteristics can be shown to diverge. Ironically, this is exactly the same point that provoked 'early modernists' into action against the airy generalizations about (often colonial) modernity made by theorists in the 1980s and '90s: that they rarely paused to think properly about what exactly the pre-modern was like before they rushed off to show how it was transformed.

Lieberman's method entirely avoids the latter problem, of course, in that his millennium-long vision allows for systematic contrasts between different eras, while the former is met head on. He sees clearly that, if one is going to use the term, 'early modern' must both be pinned down in specific characteristics:

> The combination of accelerating post-1450 administrative centralization, firearms-based warfare, rising literacy and textuality, more popular literatures, more encompassing ethnicities, wider money use, increased market specialization, and more complex international linkages, marks the period from the late 15th or early 16th century to the late 18th or mid-19th century as a more or less coherent period in each of the six realms under review. (p. 76)

As a result, his method discloses a vaguely paradoxical insight that is commonly applied to modern globalization but much less often pursued into the preceding epoch: that just as diverse societies found themselves every more interconnected so they set to work in shaping and defining themselves.

The main development of early modernity in the realm of ideas and feelings is 'politicized ethnicity'. In what sense can this be understood as a precursor of nationalism? The book presents an unusually sane account of continuity and change in the history of group emotions, in which the contrast with high modernity is always borne in mind. Lieberman is at pains to list ways in which politicized ethnicity differs from nationalism – whether it be the limits presented by earlier communications technologies or the almost complete prevalence of hierarchicalist, universalist, dynastic, and religious modes of legitimizing political power. The category of 'early modern' often looks most convincing when one can show how an aspect of the period appears to 'look both ways': where it seems to contains the beginnings of the familiar modern within the forms of the unfamiliar pre-modern.[38] Religion often serves as a defamiliarizer, as emerges from Lieberman's account of religio-political identity in the protected states. These tended to evolve a sense of political community broader than the dynastic allegiance or elite rhetoric through a sense of common sacred destiny as a people endowed with a mission to protect righteousness, truth, or correct spiritual relations.[39] Rulers might have been promoting ever grander visions of their sacred magnificence, and insisting on their authority as bestowed from on high, but in the meantime they were also helping shape an image of a much broader moral community. In this way, religion may have woven the emotional fabric of group solidarity and responsibility that secular nationalism then cut for its own purposes.[40]

But what exactly do we see as distinctive about modernity: the development of mass solidarities, or their secular expression? Indeed, what is the real relationship between these politicized ethnicities and the nationalisms that so transformed the world in the twentieth century, and in whose frame we still

'Introduction: paths to early modernities – a comparative view', *Daedalus*, 127, 3, 1998, pp. 1–18, who helped initiate the notion of 'multiple modernities', were, however, concerned to give content to the concept. See also *American Historical Review*, 116, 3, 2011, on the vexed problem of 'modernity'.

38 See Markus Vink, 'Between profit and power: the Dutch East India Company and institutional early modernities in the age of mercantilism', in C. Parker and J. Bentley, eds., *Between the Middle Ages and modernity*, Lanham, MD: Rowman & Littlefield Publishing Group, 2007, pp. 285–306.

39 This line of analysis has much in common with the work of A. D. Smith (e.g. *The ethnic origin of nations*, Oxford: Blackwell Publishers, 1986) and Adrian Hastings (e.g. *The construction of nationhood: ethnicity, religion and nationalism*, Cambridge: Cambridge University Press, 1997).

40 Lieberman, *Strange parallels*, vol. 2, p. 359.

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make out our lives? We are, at this point, in the distressing position of charging the author of one of the largest-scale works of serious analytical history ever produced with not stretching even further to include the post-1830 world. Nevertheless, an ultimately satisfying case for retaining the terminology of 'early modernity' would show how exactly all the integrative developments that Lieberman outlines contributed to the world as we know it today. Beyond observing how pre-1830 forms seem to anticipate our world or start to look increasingly familiar, we might want a sense of their role as *causes*.[41] We would thereby be able to weigh up their causal contribution against that of two strangely powerful ideological strains, modern secular nationalism and the self-conscious aspiration to modernity itself. To what extent did these viruses, incubated in Europe and rapidly disseminated globally, entirely reorder the political imagination and set to work almost regardless of the historical legacies that they encountered? Would Lieberman's method allow us to consider, for example, whether the Meiji restoration was a cataclysmic rupture or a plausible extrapolation from Tokugawa trends? Ultimately, such questions hinge upon one's prior definition of 'modernity'.

Arbitrariness

Generalists' appreciation of *Strange parallels* will only get us so far. Each of the case-study chapters now deserves scrutiny from specialists in order to determine how far Lieberman has twisted the arm of his regional protagonists to bring them into line with his preoccupations. One of their tasks should be to consider how far his determination of the timings of administrative cycles and periods of crisis has an arbitrary quality outside the demands of his model. Rather scrupulously, the footnotes do investigate other ways of chopping up time.[42] For example, he ponders whether the Merovingian period (*c.*500–720) should be seen as the first phase of a charter cycle (*c.*500–890) rather than as an administrative cycle in its own right, or whether the upheavals of the Fronde (1648–53) should be conceived as a state breakdown.[43] Too much of this and the rhetorical spell cast by the book may begin to dissipate.

The same question of arbitrariness could be investigated with regard to geographical selection too: does it matter that case studies have been chosen that we know turned into nation-states and whose coalescence was therefore preordained?[44] For the unprotected zone, there are many other possibilities (such as the Ottomans, dealt with in a long footnote). But the main question here relates to the selection of European regions. On the one hand, Lieberman is clear that France and Russia are representative cases that can stand in for the rest of Europe ('I could have used England, Spain Portugal, Sweden' (pp. 49, 207)). On the other hand, he says that France was selected because it had a 'political chronology eerily similar to mainland Southeast Asia' (p. 49). Most notably, these cases shared a late sixteenth-century collapse, but Lieberman goes on to suggest that these were early manifestations of crises afflicting a host of other European states in the 1590–1650 period. England's civil war of 1642–51 thereby becomes assimilated to the French wars of religion of 1562–98: chronologies inevitably become stretched. In his occasional pauses to survey other European countries, his biggest challenge lies with what to do with the stubbornly localized/lightly imperialized regions that would only resolve into Germany and Italy much later. One might ask how well his method copes with the political messiness that lasting dynasticism engendered in early modern Europe, with its composite monarchies and imperial agglomerations. It is characteristic that 'Spain' is taken as the unit for comparison and not the Habsburg monarchy. It may be that the question of arbitrariness, particularly with regard to its chronological dimension, can only be resolved by someone who has the patience to assess whether his patterns bear statistical analyses of their significance, and this in turn may require an appetite for turning the stuff of history into numbers.

41 At one point Lieberman does suggest that (with the long-delayed stimulus of alterity in the shape of American and European threats) the rare success of Japan's modernization in the Meiji restoration was facilitated by 'a pan-Japanese identity rooted in cultural distinctiveness and loyalty to the emperor' (*ibid.*, p. 490).

42 See *ibid.*, pp. 54–5, and 94, n. 131.

43 *Ibid.*, p. 55.

44 Lieberman deals with this at *ibid.*, p. 53, where he argues that focusing on the 'losing' states would also indicate territorial consolidation.

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Conclusion

There are moments when one feels that *Strange parallels* is a book ahead of its time, or perhaps that it represents a new genre of history, one that pushes the discipline further in the direction of the social sciences and somehow succeeds in combining a deeply historical sensibility with the scientific urge to explain and order. It should at the very least provide a bridge to historical sociology across which many from either side will be emboldened to travel. For the 'global turn' in historiography has prompted an urge to make comparisons without necessarily providing us with the conceptual apparatus for doing so. Few historians have been able to create an analytical language that renders comparative work truly meaningful. Yet there are considerable payoffs for stabilizing analytical terms across regional sub-disciplines, not least because it is only against a common backdrop that the really particular and indigenous will stand out. What emerges from this lofty perspective will not necessarily be what appears most vital to those working at a regional level – but for comparative history to be worth its salt it has to do more than merely reflect the current consensus positions within each national historiography. A number of times we have seen how Lieberman's analysis rows back on the significance of recent revisionist movements, which have quite properly sought to question older clichés. Japan, for example, does end up looking rather 'exceptional' – but only insofar as its unusual isolation shaped its developmental rhythms. *Strange parallels* allows us some relief from the politicized concerns, scholarly disagreements, and tides of fashion that wash across all national and regional historiographies. It is therefore emphatically not the kind of book whose value rests on contemporary taste or the validity of any one of its overarching theses.

This has been a long review, but it has still had grossly to simplify Lieberman's arguments at times. Readers should not conclude that the generalizing tone adopted here reflects the experience of reading Lieberman's text, which is much more grounded in the nuances of the secondary literature. Indeed Lieberman must be unrivalled in his ability to reach so deeply into the historiographies of so many parts of the world. Apart from anything else, his book has enormous potential for teaching: great tracts of historiography have been sifted for the most meaningful conclusions, issues, and insights. It is difficult to see how it might be imitated in the future, given the sheer amount of reading involved and the even more remarkable consistency of analytical control to which it has been subjected. The arguments are expressed in clear and precise language, the judgements are consistently clear-headed and wise. In short, it may be the most profound work of large-scale comparative history yet written. Undoubtedly there will be readers who refuse to extend the principle of charity to this sort of exercise, and specialists who find it difficult to witness such Olympian appropriation of their subject matter. Nevertheless, at some point we may all find ourselves confronting the question: do we want history to explain or merely to recount? Do we want it to tell stories – ever richer, more complex stories – or do we want to see if it is possible to peel back some of the surface chaos and reveal the patterns that have shaped our existence? For those of the latter persuasion, world history has rarely made so much sense.