NATION, REGION, AND GLOBE ALTERNATIVE DEFINITIONS OF PLACE IN WORLD HISTORY

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The paper begins in the recognition of the importance of 'world history' and considers some of the current challenges this field faces. It considers several important contributions to the field that illuminate the value of fresh approaches: James Scott's construction of 'Zomia', Emmanuel Todd's historicization of 'France' as a nation, Bin Wong and Kenneth Pomeranz's new approach to Eurasian economic history, and Victor Lieberman's analysis of the strange synchrony between Southeast Asia and Western Europe over a millennium of political development. The essay concludes with several historiographical maxims: avoid eurocentrism, expect variation, look for mechanisms of inter-connection, avoid capture by 'nation-state' concepts, and pay attention to different schemes of historical time.

Keywords: *world history, eurocentrism, Southeast Asia, Western Europe, nation, region, historiographical maxims.*

Global Historiography

A question that arises in historiography and the philosophy of history is that of the status of the notion of 'global history'. This issue is important in contemporary debates about world history – for example, when economic historians make the case for Eurasian history rather than French history or Japanese history. There the view is that expanding the scope of vision from the separate nation states of Europe or Asia to the broader panoply of multiple peoples, cultures, and structures is helpful when it comes to understanding the past four hundred years. But what are some of the more general concerns that make thinking about global history an interesting or important topic?

One important reason for thinking globally as an historian is the fact that the history discipline – since the Greeks! – has tended to be eurocentric in its choice of topics, framing assumptions, and methods. Economic and political history, for example, often privileges the industrial revolution in England and the creation of the modern bureaucratic state in France, Britain, and Germany, as being exemplars of 'modern' development in economics and politics. This has led to a tendency to look at other countries' development as non-standard or stunted. So global history is, in part, a framework within which the historian *avoids privileging one regional center* as primary and others as secondary or peripheral. Bin Wong makes this point very strongly in *China Transformed* (Wong 1997).

Second is the apparent fact that when Western historical thinkers – for example, Hegel, Malthus, Montesquieu – have turned their attention to Asia, they have often engaged in *a high degree of stereotyping* without much factual historical knowledge.

The ideas of Oriental despotism, Asian overpopulation, and Chinese stagnation have encouraged a cartoonish replacement of the intricate and diverse processes of development of different parts of Asia by a single-dimensional and reductive set of simplifying frameworks of thought. This is one of the points of Edward Said's critique of orientalism (Said 1978). So doing 'global' history means paying rigorous attention to the specificities of social, political, and cultural arrangements in other parts of the world besides Europe.

So a global history can be expected to be more agnostic about patterns of development, and more open to *discovery of surprising patterns, twists, and variations* in the experiences of India (and its many regional differences), China, Indochina, the Arab world, the Ottoman Empire, and Sub-Saharan Africa. Variation and complexity are what we should expect, not stereotyped simplicity. (Geertz's historical reconstruction of the 'theatre state' of Bali is a case in point – he uncovers a complex system of governance, symbol, value, and hierarchy that represents a substantially different structure of politics than the models derived from the emergence of bureaucratic states in early modern Europe [Geertz 1980].) A global history needs to free itself from eurocentrism.

This step away from eurocentrism in outlook should also be accompanied by a *broadening of the geographical range* of what is historically interesting. So a global history ought to be global and trans-national in its selection of topics – even while recognizing the fact that all historical research is selective. A globally oriented historian will recognize that the political systems of classical India are as interesting and complex as the organization of the Roman Republic.

Another aspect of global history falls more on the side of how some historians have thought about historical structures and causes since the 1960s. History itself is a 'global' process, in which *events and systems occur that involve activities in many parts of the world simultaneously.* Immanuel Wallerstein is first among these, with his framework of 'world systems' (Wallerstein 1974). Wallerstein's prologue to the 2011 edition of the book is a very useful reflection on criticisms and reception of the book in its original version (Wallerstein 2011). But the basic idea is a compelling one. An effort to explain the English industrial revolution by only referring to factors, influences, and experiences that occur within England or on its edges (Western Europe) is inadequate on its face. International trade, the flow of technologies from Asia to Europe, and the flows of ideas and peoples from Asia, Africa, and the Americas have plain consequences for the domestic economy of England in 1800 and the development of machine and power technologies. And a 'globally minded' historian will pay close attention to these trans-national influences and interdependencies. This aspect of the interest of global history falls within the area of thinking about the scope of the causal factors that influence more local developments.

An important current underlying much work in global history is the *reality of colonialism* through the nineteenth and twentieth centuries, and the equally important *reality of anti-colonial struggles and nation building* in the 1960s and 1970s. 'The world' was important in the capitals of Great Britain, France, Germany, and Belgium because those nations exerted colonial rule in various parts of Africa, Asia, and South America. So there was a specific interest in gaining certain kinds of knowledge about those societies –

in order to better govern them and exploit them. And post-colonial states had a symmetrical interest in supporting global historiography in their own universities and knowledge systems, in order to better understand and better critique the forming relations of the past.

Then there is the issue of climate and climate change. The 'little ice age' had major consequences for population, nutrition, trade, and economic activity in Western Europe; but the same climate processes also affected life in other quarters of the globe. So to have a good understanding of the timing and pace of historical change, we often need to know some fairly detailed facts about the global environment (Fagan 2000).

A final way in which history needs to become 'global' is to incorporate the *perspectives and historical traditions of historians in non-western countries* into the mainstream of discussion of major world developments. Indian and Chinese historians have their own intellectual traditions in conducting historical research and explanation; a global history is one that pays attention to the insights and arguments of these traditions.

So global history has to do with

- a broadened definition of the arena of historical change to include Europe, Asia, Africa, the Middle East, and the Americas;
- a recognition of the complexity and sophistication of institutions and systems in many parts of the world;
- a recognition of the trans-national interrelatedness that has existed among continents for at least four centuries;
- a recognition of the complexity and distinctiveness of different national traditions of historiography.

Dominic Sachsenmaier provides a significant recent discussion of some of these issues in *Global Perspectives on Global History: Theories and Approaches in a Connected World* (Sachsenmaier 2011). Sachsenmaier devotes much of his attention to the last point mentioned here, the 'multiple global perspectives' point. He wants to take this idea seriously and try to discover some of the implications of different national traditions of academic historiography. More than half his book is devoted to case studies of global historical research traditions and foci in three distinct national contexts – Germany, the United States, and China. How do historians trained and en-disciplined in these three traditions think about the core problems of transnational, global history? Sachsenmaier believes that these differences are real, and that they can be productive of future historical insights through more sustained dialogue. But he also believes there are conceptual and methodological barriers to these dialogues, somewhat akin to the 'paradigm incommensurability' ideas that Thomas Kuhn advanced for the physical sciences. And he does a good job of articulating what some of these conceptual barriers involve:

> Certain hierarchies of knowledge became deeply engrained in the conceptual worlds of modern historiography. Approaching the realities and further possibilities of alternative approaches to global history thus requires us to critically examine changing dynamics and lasting hierarchies which typify historiography as a global professional environment… It will become quite clear that in

> European societies the question of historiographical traditions tended to be answered in ways that were profoundly different from most academic communities in other parts of the world (Sachsenmaier 2011: 17).

So Sachsenmaier's attention is directed largely to the conceptual issues and disciplinary frameworks that are pertinent when we consider how different national traditions have done history. What he has to say here is very useful and original. But he also makes several of the points mentioned above as well – the need to select different definitions of geography in doing history, the need to put aside the stereotypes of eurocentrism, and the value in understanding in depth the alternative traditions of historical understanding that exist in the world.

Here I want to look at some of the specific historiographic issues that have delayed, but sometimes furthered, the development of a more truly global history.

Methodological Nationalism

Are there logical divisions within the global whole of social interactions and systems that permit us to focus on a limited, bounded social reality? Is there a stable level of social aggregation that might provide an answer to the 'units of analysis' question in the social sciences? This is a question that has recurred frequently in several areas of the social sciences – on regions, on levels of analysis, and on world systems. Here I will focus on the nation-state as one such system of demarcation.

We can start with a very compelling recent critique of current definitions of the social sciences. Andreas Wimmer and Nina Glick Schiller offer an intriguing analysis of social science conceptual schemes in 'Methodological nationalism and beyond: nation-state building, migration and the social sciences' (Wimmer and Schiller 2002). The core idea is the notion that the social sciences have tended to conceptualize social phenomena around the boundaries of the nation-state. And, these authors contend, this assumption creates a set of blinders for the social sciences that makes it difficult to capture some crucially important forms of social interaction and structure.

Their view is a complex one. They think that the social sciences have been trapped behind a kind of conceptual blindness, according to which the concepts of nation and state structure our perception of social reality but disappear as objects of critical inquiry. Second, they argue that there were real processes of nation and state building that created this blindness – from nineteenth century nation building to twentieth century colonialism. And third, they suggest that the framework of methodological nationalism itself contributed to the concrete shaping of the history of nation and state building. So it is a three-way relationship between knowledge and the social world.

'Nationalism' has several different connotations. First, it implies that peoples fall into 'nations', and that 'nations' are somewhat inevitable and compact social realities. France is a nation. But closer examination reveals that France is a social-historical construct, not a uniform or natural social whole. (We will consider Emmanuel Todd's version of this argument in the next section.) Alsatians, Bretons, and Basques are part of the French nation; and yet they are communities with distinct identities, histories, and affinities. So forging France as a nation was a political effort, and it is an unfinished project.

Second, nationalism refers to movements based on mobilization of political identities. Hindu nationalists have sought power in India through the Bharatiya Janata Party (BJP) on the basis of a constructed, mobilized (and in various ways fictional) Hindu identity. The struggle over the Babri Mosque, and the political use to which this symbol was put in BJP mobilization, illustrates this point. But 'nationalist politics' also possess a social reality. It is all too evident that even fictive 'national identities' can be powerful sources of political motivation. So nationalist politics in the twentieth century were a key part of many historical processes. (Michael Mann's *The Dark Side of Democracy: Explaining Ethnic Cleansing* illustrates this point [Mann 2005].) And, of course, there may be multiple national identities within a given region; so the 'nation' consists of multiple 'nationalist' groups. Ben Anderson's *Imagined Communities: Reflections on the Origin and Spread of Nationalism* (Anderson 1983) provides an extensive development of the political and constructed nature of ethnic and national identities. Also relevant here are (Frank 1998), (McNeill 1986), and (Hall and Fenelon 2008).

What about the other pole of the 'nation-state' conjunction – the state? Here the idea is that the state is the seat of sovereign authority; the origin and enforcement of legal institutions; and the holder of a monopoly of coercive power in a region. A state does not inevitably correspond to a nation; so when we hyphenate the conjunction we make a further substantive assumption – that nations grow into states, and that states cultivate national identities.

The fundamental criticism that Wimmer and Schiller express – the fundamental defect of methodological nationalism – is that it limits the ability of social scientists and historians to perceive processes that are above or below the level of the nation-state. Trans-national processes (they offer migration as an example) and sub-national processes (we might refer to the kinds of violent mobilization studied by Michael Mann in the *Dark Side of Democracy* [Mann 2005]) are either invisible or unimportant, from the point of view of methodological nationalism. So the methodology occludes social phenomena that are actually of great importance to understanding the contemporary world.

Wimmer and Schiller seem to point in a direction that we find in Saskia Sassen's work as well: the idea that it is necessary for the social sciences to invent a new vocabulary that does a better job of capturing the idea of the interconnectedness of social activity and social systems (Sassen 2007). The old metaphors of 'levels' of social life organized on an ascending spatial basis does not seem to work well today when we try to deal with topics like global cities, diasporic communities, or transnational protest movements. And each of these critiques makes a convincing case that these non-national phenomena are influential all the way down into the 'national' orders singled out by traditional classification schemes.

France as a Nation?

The idea of 'nation' has been tested in many settings. One is the case of France. Is France one nation? What makes it so? And what are the large socio-cultural factors that led to modern France? These are the questions that Emmanuel Todd raises in *The Making of Modern France: Ideology, Politics and Culture* (Todd 1991). Todd is one of this generation's leading historians in France, and his conception of the challenge of history is worth studying. He is a 'macro-historian', in that he is interested in large processes of change over extended stretches of space (for example, the extension of industry across the map of France from 1850 to 1970, or the patterns of religious dissent from the twelfth to the twentieth centuries), and he singles out characteristics of family structure, demography, literacy, and religion as a set of causal factors that explain the patterns of historical change that he uncovers.

Todd's starting point seems exactly right: the 'nation' is not a particularly salient level of analysis for making sense of large historical change in the case of France. Social, economic, and political developments should not be presumed to unfold at the level of the nation. Todd puts forward a simple but apt criterion for choosing a level of analysis for historical inquiry: 'one has to observe the social and economic behaviour of the human beings in question and discover their scale in order to define closed and homogeneous groups which then can be called society X or economy Y' (Todd 1991: 7). And in fact, he argues that 'France' is better understood as a configuration of regions and zones than as an integrated national system. As he puts the point, 'one can represent France as a heterogeneous and open area in which social, economic and political forces emerge, spread and establish themselves quite independently of the central power and of the overall national structure' *(Ibid.:* 8). And: 'Notions of "French society", "French economy", "French industry", "French working class" are to some extent myths' (*Ibid.:* 7). (It is interesting to observe that this is one of G. William Skinner's central insights into Chinese history as well, especially in his analysis of the historical relevance of 'macroregions' in China [Skinner 1977].)

So what are the patterns and causal factors that have given rise to 'modern France' in Todd's reckoning? Crudely, Todd argues that there are large regional patterns of culture, demography, and property that created distinct dynamics of change across eight centuries of French history. The southern half of France is characterized by complex family systems with several generations in the same household and a low rate of reproduction, in contrast to the nuclear families of the north and their higher rate of reproduction. The family values of the southern region gave greater importance to literacy and education than the nuclear (and larger) families of the north. And family structure, patterns of inheritance, and land tenure are in turn highly relevant to the formation of large patterns of ideology. (A similar logic is expressed in another of Todd's books, *The Explanation of Ideology: Family Structure and Social Systems* [Todd 1985].)

The central analytical device in Todd's argument is a fascinating series of maps of France coding the 90 *départements* of France by such variables as the percent of women holding the *baccalauréat*, the percentage of priests accepting the *serment constitutionnel* (revolutionary loyalty oath) in 1791, or the percentage of workers in a given industrial sector. The maps display striking geographical patterns documenting Todd's interpretation of the large historical patterns and their underlying anthropological and geographical causes. At the largest scale, he argues for three axes of historical causation: a north-south axis defined by family structure that creates differentials of literacy and population growth; an east-west axis defined by the diffusion of industry from northern Europe into eastern France and across the map from east to west; and a political pattern different from both of these, extending from Paris at the political center to the periphery in all directions. The following is a great example; Todd is interested in observing the degree of ‘religiosity’ across France around the time of the Revolution, and he uses the percentage of priests who accepted the oath of allegiance demanded by the Revolutionary government as a measure. The resulting map reveals conspicuous patterns; the periphery and the south stand out as non-conformist.

Todd also argues that there is a causal order among the large social factors he singles out. Family structure is causally relevant to literacy and education level; literacy is relevant to religious dissent and the emergence of Cathars, Waldensians, and Protestants; family structure is relevant to reproductive rates which are in turn relevant to the spread of industry; and traditions of inheritance are relevant to a region's receptiveness to the ideology of the Revolution. And the patterns created by these causal processes are very persistent; so the southern belt of high-literacy *départements* of the twelfth century coincides almost exactly with the pattern of high incidence of *baccalauréats* and doctors in the late twentieth century.

A particularly interesting part of Todd's analysis is his effort to map out the agrarian regimes of pre-revolutionary France (the *ancien régime*). He observes that this has not been done by existing studies of French rural society, and that there is no suitable statistical data on the basis of which to do so for the eighteenth century in any case. However, he makes use of the first census in 1851 to infer back a century in order to arrive at an analysis into four categories: large estates with hired labor, peasant proprietorship, tenant farming, and share-cropping. And using the mid-nineteenth century census data he constructs a map of France that indicates the distribution of agrarian property regimes across the territory (Todd 1991: 60).

The large estates are concentrated in the center of France, including Paris; while peasant proprietorship (sometimes combined with share-cropping) predominates in the southern tier. Note as well how closely these patterns conform to the distribution of family structure and fertility at the top of the posting. And Todd argues that these patterns showed substantial continuity before and after the Revolution (*Ibid.:* 61). In other words, there is a very substantial overlap between agrarian regimes and the anthropological-demographic patterns discussed earlier. Todd then uses these geographical patterns to explain something different: the pattern of de-christianization that took place over the century following the Revolution. Basically, de-christianization is associated with the regions involving a large number of landless workers, whereas this cultural process was least virulent in regions of peasant proprietorship. In other words, he offers an explanation of ideology and religion in terms of a set of demographic and social characteristics that are distributed differentially across regions.

I have not touched on the dynamics of politics at all here, which is an important piece of Todd's work. But these comments suffice to illustrate the pattern of historical thinking represented by Todd's work. It is striking for its effort to cross genres, incorporating geography, anthropology, and sociology into the formation of large interpretations of French history. And it is striking for the scale of the canvas that he attempts to paint.

Beyond Divergence

Let us turn now to another of the key challenges of global history, the effort to eliminate eurocentrism from historical analysis. There has been a major debate in economic history in the past twenty years about what to make of the contrasts between economic development trajectories in Western Europe and East Asia since 1600. There had been a received view, tracing to Adam Smith and Thomas Malthus, that European 'breakthrough' was the norm and Asian 'stagnation' or 'involution' were the dysfunctional cases. E. L. Jones represents this view among recent comparative economic historians (Jones 1981). Then Kenneth Pomeranz and Bin Wong challenged this received view in a couple of important books. Pomeranz argued in *The Great Divergence: China, Europe, and the Making of the Modern World Economy* that the premises were wrong (Pomeranz 2000). He argued that Chinese productivity and standard of living were roughly comparable to those of England up to roughly 1800, so China's economy was not backward. And he argued against the received view's main theories of Europe's breakthrough – the idea that European economic institutions and property rights were superior, or the idea that Europe had a normative or ideological advantage over China. Instead, he argued that Europe – Britain, to be precise – had contingent and situational advantages over Asia that permitted rapid growth and industrialization around the end of the eighteenth century. These advantages included large and accessible coal deposits – crucial for modern steam technology – and access to low cost labor in the Americas (hidden acreage). Bin Wong made complementary arguments in *China Transformed: Historical Change and the Limits of European Experience* (Wong 1997), where he addressed the parallel processes of development of political and economic institutions in the two sets of polities. Wong's most fundamental insight was that both processes were complex, and that balanced comparison between them is valuable.

Now the debate has taken a new turn with the publication of R. Bin Wong and Jean-Laurent Rosenthal's *Before and Beyond Divergence: The Politics of Economic Change in China and Europe* (Rosenthal and Wong 2011). Rosenthal is an accomplished historian of European economic development, and Wong is an expert on Chinese economic, social, and political history. So their collaboration permits this book to bring together into one argument the full expertise available on both ends of Eurasia. The book aims to unsettle the debate in fundamental ways. Wong and Rosenthal take issue with a point that is methodologically central to Pomeranz, concerning the units of comparison. Pomerantz wants to compare England with the lower Yangzi region in China, and he gives what are to me convincing arguments for why this makes sense. The authors want to compare Europe with China, making England a special case. And they too have good reasons for their choice.

Second, they disagree with the temporal framing that has generally been accepted within this debate, where economic historians have generally focused their research on the early modern period (1600–1900). Against this, they argue that the causes of divergence between Europe and China must be much earlier. They set their clock to the year 1000, and they examine the large features of political and economic development that started around that time.

Finally, they offer crippling objections to a number of standard hypotheses about Imperial China as a place to do business. They show that there were alternative credit institutions available in Ming and Qing China. They show that the Chinese state was sensitive to levels of taxation, and kept taxes low (generally comparable to European levels). And they show that Imperial social spending (the granary system, for example) was generally effective and well managed, contributing to economic prosperity. So the traditional explanations for Chinese 'stagnation' do not work as causal explanations.

They find one major difference between Europe and Asia during the first part of the second millennium that seems to matter. That is the multiplicity of competing states in Europe and a largely hegemonic Imperial state in China and the scale of the relevant zones of political and economic activity. Chapter 4, 'Warfare, Location of Manufacturing, and Economic Growth in China and Europe', lays out this argument. The competing states of Europe were frequently drawn into conflict; and conflict often resulted in warfare. The authors argue that this fact of competition had a fateful unintended consequence. It made fortified cities much safer places than open countryside. And this in turn changed the calculation about where 'manufacture' could occur at lowest cost. Labor costs were higher in cities, so absent warfare, producers were well advised to pursue a putting-out system involving peasant workers (proto-industrialization). But with the threat of marauding armies, European producers were pushed into urban locations. And this in turn gave them incentives to develop labor-saving, capital-intensive techniques. Putting the point bluntly: China did not have an industrial revolution because it was too safe an environment for labor-intensive production.

These debates about how best to position the comparison of different aspects of Eurasian economic and political development provide very important impetus to a better version of global history. There is a very vibrant field of work underway with this trans-Eurasian perspective (see also Arrighi 2007 and Beckwith 2009).

Zomia

Now let us consider a particularly interesting challenge to methodological nationalism, James Scott's recent theorizing of Zomia in *The Art of Not Being Governed: An Anarchist History of Upland Southeast Asia* (Scott 2009). Scott opens this most recent book with quotations from frustrated pre-modern administrators and missionaries whose territories included the peoples of inaccessible highland regions – Guizhou, highland Burma, and Appalachia. Scott finds that the geographical circumstances of highland peoples mark them apart from the political organizations of the valleys; states could control agriculture, surplus, and labor in the lowlands, but were almost entirely incapable of exerting sustained rule in the highlands. And he finds that highland cultures and systems are more or less deliberately shaped to elude the grasp of the state; linguistic variety, swidden agriculture, and ethnic opacity all work to make the art of rational administration all but impossible. The book is a significant contribution to the social and political analysis of very large swatches of the world

Scott makes use of the concept of 'Zomia' to capture the highland peoples of Southeast Asia. Scott estimates the population of the minority peoples of Zomia at 80–100 million. What is intriguing about this definition of space and social reality is that it is *not* defined by nation-state boundaries and jurisdiction, by linguistic groupings, or by ethnic and national identities. Scott emphasizes the enormous linguistic and ethnic variation that occurs across this expanse of space. 'In the space of a hundred kilometers in the hills one can find more cultural variation – in language, dress, settlement pattern, ethnic identification, economic activity, and religious practices – than one would ever find in the lowland river valleys' (chapter 1; Kindle location 343).

Two central arguments take up much of Scott's attention in the book. One is an argument about the logistics of state power in a pre-modern agrarian society and the agency of 'fugitive' peoples. Essentially he argues that pre-modern agrarian societies were only able to impose their rule over a tight radius of perhaps 300 kilometers, when it came to collecting taxes, grain, and manpower. Moreover, this radius of power reduced significantly when population was distributed over mountainous country. So as a practical matter, the pre-modern states of Burma, Thailand, and Cambodia were river-valley states, and the peoples of the highlands were rarely subject to central rule. This argument resonates with Michael Mann's analysis of pre-modern state power in *The Sources of Social Power: Volume 1, A History of Power from the Beginning to AD 1760* (Mann 1986). On this scale, the Kingdom of Chicago would barely be able to exert its will over the peasants of Peoria or Milwaukee; and Indianapolis would be a distant and irrelevant place.

And, he argues, the peoples of the highlands deliberately organized their activities in ways that made the power of the state least effective.

> Virtually everything about these people's livelihoods, social organization, ideologies, and (more controversially) even their largely oral cultures, can be read as strategic positionings designed to keep the state at arm's length (Kindle loc 26).

The other central theoretical argument that Scott offers concerns the question of ethnicity and identity. Like Ben Anderson (1983), Scott believes that the identities of Burman, Mon, Khmer, Tai, or Shan are constructed identities, not essential or ancient.

> Identity at the core was a political project designed to weld together the diverse peoples assembled there. Bondsmen of allied strongmen, slaves captured in warfare or raids, cultivators and merchants enticed by agricultural and commercial possibilities: they were in every case a polyglot population (Kindle loc 1166).
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> The central plain of what would become Siam was, in the thirteenth century, a complex mix of Mon, Khmer, and Tai populations who were an 'ethnicity-in-the-process-of-becoming' Siamese (Kindle loc 1172).

The book takes up the argument that Scott began in *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed*: that a central task of the state it to render its territory and population 'legible' (Scott 1998). The state needs to be able to regiment and identify its subjects, if it is to collect taxes and raise armies; so sedentary, mobile, peripheral peoples are antithetical to the needs of the state. This argument begins in *Seeing Like a State*; and it gains substantial elaboration here. And it is a fundamental call for a different approach to conceptualizing and studying the cultures and populations of Southeast Asia: not by ethnic group, not by national boundaries, but rather by the common circumstances of material and political life in high, rugged terrain.

Scott's work almost always takes the form of an imaginative re-framing of problems that we thought we had understood. But once looking at the facts from Scott's point of view, we find that the social phenomena are both more complex and perhaps more obscure than they initially appear to be. And the Zomia concept seems to force us to rethink the way we partition social space and the concept of ethnicity – highly responsive to the complaints against methodological nationalism.

Zomia Reconsidered

So what about Zomia? How does this concept hold up when considered by other experts on Southeast Asia? As noted, Scott turns in his usual creative, imaginative, and innovative treatment of the subject matter; the book is an absolutely captivating argument about the push and pull between states and fugitive peoples. As such, it suggests the possibility of bringing some of the central ideas and analyses to bear on other geographies as well. But how accurate is Scott's reading of the primary historical experience of these parts of Southeast Asia – Burma, Thailand, Vietnam, China, Cambodia, and Bangladesh?

This is the question posed by a recent issue of the *Journal of Global History*, with essays by C. Patterson Giersch, Magnus Fiskesjo, Sarah Turner, Sara Shneiderman, Bernard Formoso, and Victor Lieberman. All the essays are fascinating, including the editorial introduction by Jean Michaud. But particularly important is Lieberman's essay. Lieberman is one of the leading contemporary historians of Southeast Asia, and he is a very fertile and imaginative thinker himself. So his responses to Scott's arguments are worth looking at closely. (His recent two-volume work, *Strange Parallels: Volume 1, Integration on the Mainland: Southeast Asia in Global Context, c. 800–1830* [Lieberman 2003], is directly relevant to Scott's analysis.)

Lieberman begins by establishing the territory on which he agrees with Scott. First, he accepts the fact of a growing separation between lowland and highland peoples in Southeast Asia during early modern times, and he agrees about the importance of analyzing this pan-Southeast Asian phenomenon. Another point of agreement is the fact of highlander agency. Lieberman agrees with Scott's insistence that highland peoples throughout Southeast Asia crafted their own social worlds in response to the political and natural environments that faced them. So Lieberman acknowledges the importance and boldness of Scott's effort at providing a comprehensive historical study of Zomia. But Lieberman offers a series of important criticisms of Scott's historical case.

First, he finds Scott's documentation to be weak, in that it makes little use of Burmese-language sources. This has led, in Lieberman's opinion, to a number of errors of fact, some more significant than others. He cites estimates of literacy, for example; Scott says less than 1 percent of people were literate in Southeast Asia, and Lieberman documents 50 per cent for Burma in 1800.

More significantly, Lieberman believes Scott over-estimates the importance of manpower as a determinant of military success in the region. The degree of maritime commerce was equally important, he argues. And this is critical to Scott's argument, since competition for manpower is one of the primary reasons Scott cites for the efforts of lowland states to attempt to dominate the highlands.

Finally, and most important, Lieberman argues that there is little documentary evidence for significant population flight from lowland to highland (Lieberman 2003: 339). This is key to Scott's interpretation, and Lieberman argues the evidence is not there to support the claim. After reviewing Scott's own evidence and some additional data of his own, he argues that Scott may have over-estimated 'flight'. Moreover, Lieberman argues that Scott's interpretation of the highlands becomes so dependent on one causal factor, state oppression, that it neglects the processes of development that were internal to the highland societies themselves. 'Ecological and cultural conditions that were intrinsic to the hills and that were substantially or completely divorced from the valleys receive little or no attention' (Lieberman 2003: 343).

This point is more important when we consider an example not included in Scott's analysis – the highland peoples of Borneo/Kalimantan. Lieberman argues that these tribes had virtually all the characteristics of culture and agriculture displayed by Zomians, including swidden cultivation and a proliferation of local languages, and Scott interprets these traits as deeply defensive. Yet these features of highland life emerged in Borneo without the pressure if a surrounding predatory lowland state (*Ibid.:* 345). And this casts serious doubt on Scott's anarchist, anti-statist interpretation of Zomia.

Lieberman's point is not that Scott's interpretation of Zomia is unsupportable. Rather, his point is that it is a bold and substantive interpretation of a complex historical domain, and it requires serious, fact-based consideration. And this is exactly what the essays in this special volume of *Global History* promise to do.

This debate is interesting and important, in part, because it sheds light on the practical empirical research challenges that arise when we consider bold new interpretations of social data. A bold hypothesis is advanced, purporting to pull together the processes of development observed in a variety of places; and then there is the practical question of evaluating whether the hypothesis is born out when we do the detailed, local historical research needed to test its basic assertions. In this case, Lieberman is suggesting that several of the components of the theory are found wanting when applied to highland Burma.

Strange Parallels

Let us close by considering Lieberman's own way of recasting traditional ways of parsing the world in his recent work. Lieberman uses the phrase, 'strange parallels', as the title for his two-volume study of Southeast Asian history (*Strange Parallels: Volume 1, Integration on the Mainland: Southeast Asia in Global Context, c. 800–1830*) (Lieberman 1999). Besides offering a highly expert history of Burma and its many kingdoms between 800 and 1830, Lieberman poses a fascinating and novel question: how can we explain the substantial historical parallels that existed between Burma and various parts of Europe, including especially France and Russia? He writes:

> In fact, in mainland Southeast Asia as well as in France, the late 18th and early 19th centuries ended the third and inaugurated the last of four roughly synchronized cycles of political consolidation that together spanned the better part of a millennium (Lieberman 1999: 2).

The figure that Lieberman provides illustrates the kind of synchrony that Lieberman is highlighting – over a sweep of some thousand years, there is a rough-and-ready correspondence in the patterns of territorial consolidation that existed in Burma and France.

Lieberman's current work broadens the canvas by looking at broad temporal patterns of consolidation and turmoil across the full expanse of Eurasia, including Russia, France, Japan, China, and Southeast Asia. In two volumes of *Strange Parallels* he documents a degree of synchrony among widely separated polities that demands explanation. Here is how the pulsing of consolidation and disintegration looked in Southeast Asia:

> In sum – in lieu of four modest charter polities in 1240, 23 kingdoms in 1340, and 9 or 10 kingdoms in 1540 – mainland Southeast Asia by the second quarter of the 19th century contained three unprecedentedly grand territorial assemblages; those of Burma, Siam, and Vietnam (Kindle loc 799).

Lieberman defines consolidation as a broadening of scope of a polity, including territory, population, war-making capacity, and fiscal reach. And he notes that each of the world polities he studies shows a sequence of consolidation, followed by periods of turmoil and breakdown. And this was true as much in Burma as it was in seventeenth and eighteenth century France. Moreover, and this is his key point, these periods show a remarkable degree of synchrony, from Kiev to Paris to Burma. So here is the central question: what kinds of global triggers or events could have created this synchrony?

Lieberman poses the crucial historical question in these terms: 'Why should distant regions, with no obvious religious or material links, have experienced more or less coordinated cycles? If we discount coincidence, what hitherto invisible ties could have spanned the continents?' (Lieberman 2003: 2) To further complicate the picture, Lieberman points out that there were other regions of the world where these patterns of consolidation did not occur, or did so on a very different timeline. So we can exclude the idea that there was some common global cause leading to simultaneous pulses of consolidation; rather, Southeast Asia and Western Europe were synchronized, but India was not.

Lieberman's explanation of this observed historical synchrony goes along these lines. He believes that both internalist and externalist approaches have a role to play. The internal historical dynamics of the state systems in Burma and Western Europe were governed by particular local factors. But they each created a tendency towards consolidation of land and power. And external factors provided periodic 'pulses' that served to synchronize these internal patterns of development. So the effects of an external factor – maritime trade – pushed both Western Europe and Burma into extended periods of state formation and consolidation. This story combines several ideas about causation: local processes that are developing according to their own imperatives, and occasional system-wide pulses that bring these local processes into synchrony. And the explanation allows Lieberman to place the intellectual frameworks of both Tilly and Wallerstein into the story.

Here are a few candidates that Lieberman considers as possible mechanisms of synchrony. For the tenth – thirteenth century, he considers the effects of global climate fluctuation, disease, Viking invasions, and the predations of Mongol armies from Inner Asia. And for the seventeenth and eighteenth centuries he considers the expansion of Eurasian trade, modern arms, and monetary uses of silver in Europe and Asia (Kindle loc 8745).

Internal to each polity are factors that appear to be local in their effects: population change, agricultural improvements, new organizational forms in governance, military, and taxation, and the diffusion of literacy and national culture. But the logic of these processes does not imply any sort of global synchrony; so, once again, what would serve to link consolidation and disorder in France and Burma?

This is world history you can get your teeth into. It is detailed, making use of the best available sources for each of the regions and polities considered. And it is bold in its effort to arrive at trans-continental, even global causes of these local developments. Lieberman's approach is important for debates about history and the social sciences because it leads us to ask different questions about historical causation and historical time. And it provides important new thinking about how to approach the nexus between regional, national, and global history.

Conclusion

World history is more timely today than ever. 'Globalization' is almost a cliché, from 'The world is flat' to 'the homogenization of cultures' to the 'commodification of place'. Everyone now recognizes the fact of globalization in the contemporary world. But we need to understand the many ways in which many parts of the world were deeply and systemically interconnected long before the post-World War II wave of revolutions in communications networks, rapid travel, containerized shipping, and military power contributed to the current interconnectedness of most countries and peoples. We need a strong historiography for the global world.

To be most productive, however, we need to approach the tasks of global history with some fresh thinking. There are several key points that have emerged as fundamental. The first is to be vigilant about making Eurocentric assumptions about development and change. Whether in the domains of politics, economics, or culture, it is crucial to avoid the assumption that Europe set the model for developments in key areas of historical change. New historiography of Eurasian economic development illustrates the power of an approach that avoids Eurocentrism, including Bin Wong, Ken Pomerantz, and Prasannan Parthasarath (Parthasarath 2011).

A second is to expect variation rather than convergence. There are many ways that human societies have found to solve crucial problems of coordination, order, production, and the exercise of power. Global historians need to be alert to the development of alternative institutions of politics, economics, culture, or social cohesion in different locales. In particular, it is important to take note of divergences as well as parallels in the political and economic development of great civilizations like those of India, China, Southeast Asia, or West Africa.

Third, it is important to avoid being captured by the conceptual schemes of nationalism and states. 'France', 'Indonesia', and 'India' are places with diversity and internal variation, and they each followed distinct rhythms of consolidation as states and nations. It is often more revealing to look to regions that cross the boundaries of existing states; we learn much by looking at the dynamics of change in regions that are smaller than nation-states (the American South, for example, as an economic and racial regime that had little in common with Northern cities); and it is sometimes the case that we are best off considering the histories of dispersed peoples and activities (Zomia, diasporic histories, bandits).

Fourth, the way in which we consider historical time sometimes needs more critical reflection. Lieberman's focus on the punctuated patterns of consolidation that took place from Burma to Kiev is one aspect of this reflection; the world's clock was synchronized in a pattern that was quite distinct from the internal patterns of change in each of the affected countries. And the historian needs to be attentive to both clocks. Likewise, world historians need to be open to considering temporality on a range of scales – from the months of the Terror to the decades of contention that preceded and followed the French Revolution, to the century and a half that separated the French Revolution from the Chinese Revolution.

Fifth, the global impact of environmental factors needs to be given the emphasis it deserves. Climate change, exhaustion of woodlands, extension of mining and extraction – all these processes and factors influence human activity at a range of levels, and their impact needs to be assessed carefully on the basis of historical and physical data.

Finally, world historians need to pay particular attention to the mechanisms of influence through which places exchanged cultural and economic material in the long centuries from the development of substantial Mediterranean trade in the ancient world to the shipping lanes of the contemporary world. Trade, the diffusion of ideas through cultural contact and migration, the effects of the book trade, the military logic of colonialism, the advent of organized long-distance communication and travel, the creation of international governance institutions – these mechanisms of social exchange constitute many of the pathways through which global integration occurs, and their dynamics are worthy of close attention by historians.

Significantly, almost all these factors find their way into the work of many recent historians who are taking on the challenge of making sense of the history of the modern world. World historiography is on a very promising path.

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