Comparing comparisons: ancient East and West

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Walter Scheidel

Stanford University

Abstract: What is comparative history good for? Does it pose special challenges? In our time of accelerating globalization, are we ready to embrace a new inter-discipline, Comparative Classics?

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In the study of History, comparative analysis remains rare. Explicit reflection on the uses, methodology and problems of historical comparison is rarer still. In this respect, the divide between History as an academic discipline that has at least occasionally been counted among the Social Sciences and fields such as Economics, Political Science and Sociology is as wide as it can be. I have decided to focus on the ‘how’ and ‘why’ of comparative history rather than present a specific case study. This decision is in part motivated by what have turned into years of lingering anxiety about the ‘proper way’ to conduct comparative history, a concern that I suspect may well be shared by many others. In my case, these doubts have been heightened by my own efforts to encourage comparative interests in others – a case of the one-eyed leading the blind? I am above all keen to learn what others think about these issues, and hope that these cursory remarks will stimulate fruitful reflection and discussion. In my experience there is always a temptation to ‘get on with it’ – plunge into a discussion of specific case studies – and I want to encourage some soul-searching on why we think a comparative perspective is worth adopting, and more importantly on how to go about applying it in practice and developing it to greater maturity.

The key questions are, what is comparative history good for; how should it be done; and how has it been done (or not) so far? First of all, is it worth it? Comparison combats hyperspecialization, the great bane of modern professional scholarship. Neither Classics nor East Asian Studies have displayed much resistance to this particular affliction. Vasunia 2011:224 notes that comparisons “generate inferences … that speak to the concerns of other times and places” – surely a welcome bonus feature for historians of early periods who may sometimes find themselves at the margins of their discipline. Comparison defamiliarizes the deceptively familiar. By observing alternatives, the characteristics of one’s “own” case becomes less self-evident, and appreciation of what is possible increases accordingly. Geoffrey Lloyd and Nathan Sivin (2002:8) even regard this as the principal benefit: “The chief prize is a way out of parochialism.” Comparison improves our understanding of X as it took different forms in different societies, whereas “[s]cholars whose work is confined within a single cultural area easily suppose that its ways are natural and inevitable.” Could it be that this is more of a problem in intellectual history – one ‘philosophy’, ‘science’, ‘medicine’ – than in other areas of History? Or should we take this observation to mean that any complacency about the “natural and inevitable” interferes, or seemingly obviates the need for, explanation of observed traits?

This brings us to our conveners’ statement of purpose. Qiaosheng Dong and Jenny Zhao observe that only through a comparative approach “can the distinctive features and commonalities between these two civilisations [i.e., Greece and China] be identified.” Once this has been accomplished, the task at hand is to explain observed differences – in philosophy, science, medicine, historiography, etc. (To which one might add that observed transcultural commonalities, as long as they are non-trivial, are likewise in need of explication, as they cannot readily be taken for granted.) Yet I wonder if the purpose of comparative history is to explain difference (why is A not like B): at a more basic level, it may turn out to be the best means of explaining the properties of each case (why is A like A). The underlying objective is causal explanation of features and developments of any one case, which may be more difficult or perhaps even impossible in the absence of a comparative approach.

Comparative history contributes to the understanding of any given case. This is true in a fundamental way: how can we move from description to explanation except by contrast? When Max Weber asked why capitalism arose in Europe, the question was also, Why did it not arise elsewhere? (Haupt & Kocka 1996). I was delighted by Jeremy Tanner’s (2009:89) opening shot against classicists’ wariness of comparison due to the perceived incomparability of “the Classical” – for how can anything be established as “classical” if not by comparison?

This paper was prepared for the conference Comparing ancient worlds: Greece and China held in Cambridge (UK) in January 2013 in honor of the eightieth birthday of Sir Geoffrey Lloyd. I have left the colloquial character of this presentation unchanged.

1 For relevant reflections in the latter fields, see esp. Bonnell 1980; Skocpol & Somers 1980; Tilly 1984; Ragin 1987; Mahoney & Rueschemeyer 2003. For History, Haupt & Kocka 1996 is central.
In the worst case, ‘single-case historians’ resemble the drunk who looks for his lost keys under a street light – not because that’s where he lost them but because that’s where he can see. Put more academically, “Analyses that are confined to single cases … cannot deal effectively with factors that are largely or completely held constant within the boundaries of the case (or are simply less visible in that structural or cultural context). This is the reason why going beyond the boundaries of a single case can put into question seemingly well-established causal accounts and generate new problems and insights.” (Rueschemeyer 2003:332). More specifically, ‘single-case’ studies may be misled by the nature of the sources (and/or established scholarship, hardly an independent variable): if the authorities (of either kind) emphasize A, it is difficult to realize that B, which is given short shrift, might have been critical in producing observed outcomes. Comparison offers a way out of this common trap. (For A, read “Confucianism” or “Mediterranean”, depending on your field.)

Heuristically, comparative history helps us identify problems and questions that would not be clear without comparison. When Marc Bloch sought to explain similar agrarian developments in England and France he looked for French equivalents of English enclosure, something ‘French-only’ historians would not have done (Haupt & Kocka 1996). Descriptively, it allows us to identify particular cases as unusual (e.g., the Greek polis). But the greatest gain lies in the ability to explain. “Comparative historical inquiry is fundamentally concerned with explanation and the identification of causal configurations that produce major outcomes of interest.” (Mahoney & Rueschemeyer 2003:11). Causal argument is central to this type of analysis, which must focus on processes over time.

Analytically, comparison allows us to delineate chains of development and in the process to critique established ‘local’ explanations (which may turn out to be pseudo-explanations). For example, consideration of shared input that is correlated with common outcomes in different places helps us supersede strictly local narratives of causation. (On the grandest scale, climate change or pandemics are suitable candidates: see below). Comparative analysis may also be employed to contest or reject generalizing (pseudo-)explanations, such as expectations or ‘norms’ derived from too few cases (A always leads to B), which may turn out not be true.

This latter issue may strike us as a less than pressing concern, given that the identification of normative causal relationships is not normally regarded as a principal task of the historian. This is why I will stop here, well short of the ambitions of the Social Sciences: to use comparative historical analysis to test a theory, or to create typologies. I have summarized elsewhere historical sociologists’ methodological discussions of “parallel demonstration of theory”, where comparison is meant to verify theory, and macro-causal analysis that seeks to generate new theory (and is therefore the more reliable the more different cases are involved) (Bonnell 1980; Skocpol & Somers 1980; cf. Scheidel (ed.) 2009:5-6). There is no need to reiterate this here as it would lead us beyond what historians usually seek to do.

It is however worth noting that this is precisely the area that has attracted the most explicit engagement with procedural and epistemological questions. The more modest applications of comparative analysis favored by historians have suffered from neglect. There is no manual on how to do comparative history. As an approach, it is not well conceptualized, formalized, let alone theorized. I have not even been able to find a general introduction to Comparative History (contrast Crossley 2008, “What is Global History?”). Where social scientists are eager to fret, historians generally tend not to. This deficit of self-examination need not be an insuperable hurdle to the success of comparative history, but it can hardly be considered helpful. At the very least it poses the risk of having to reinvent the wheel every single time we get down to business.

Comparison is perhaps best defined as a perspective or an approach rather than a formal method. Two basic principles merit attention (Haupt & Kocka 1996). First, comparison is about similarities and differences, not about connections per se (although they may of course affect observed outcomes). This is important because it shows how a comparative approach has the potential to liberate us from conventional constraints of time (as in the timeless question asked of historians, “what is your period?”) and space. Comparanda do not have to be spatially adjacent or contemporaneous. In a sense, the less close and connected they are, the better. Distance suppresses interaction effects, thereby simplifying causal analysis. (Do “Silk Road Studies” qualify as comparative history?) Comparison between “East” and “West” is
inter-cultural rather than intra-cultural. In many contexts – say, for Europeanists – it is often hard to distinguish between these categories, but this is a very straightforward matter for students of the ancient Mediterranean and early China. In practice, inter-cultural comparison tends to be limited to a few, often just two, sharply profiled cases (Osterhammel 1996). Transcultural comparison, by contrast, tends to focus on a potentially universal repertoire of possible forms of features and processes (power, production, socialization, cultural symbolism, etc). Historians are more likely to privilege the former approach, historically minded social scientists the latter.

Second, comparison cannot be an end in itself but has to be a means to an end. This may seem trivial but undoubtedly bears emphasizing, especially in view of the limitations of some of the work that has been attempted so far (see below). The two basic types of comparative reasoning – contrast and generalization – go back at least as far as John Stuart Mill’s “method of difference” and “method of agreement”, which have repeatedly been taken up by modern comparativists (e.g., Theda Skocpol and Charles Tilly’s distinction between “contrasting” and “universalizing” types of comparison). The Millian methods serve to eliminate potential necessary and sufficient causes, thereby narrowing our choice of putatively significant causal factors (Mahoney 2003). The method of agreement focuses on equivalent outcomes in different cases: if some cause is only present in some of these cases, it cannot be necessary to produce this particular outcome. According to the method of difference, if outcomes differ in different cases, shared causes cannot be deemed sufficient to produce equivalent outcomes. Basic as this may seem, this logic provides a good way to judge and set aside rival explanatory hypotheses. In practical terms, the underlying rules may have to be relaxed in a probabilistic fashion to “to permit causes that are “usually” or “almost always” necessary or sufficient” (Mahoney 2003:344). This takes account of the messiness of history, specifically of measurement problems and related issues that make patterns of association difficult to identify in the record (334-7).

The search for causation favors analysis structured around discrete variables. This is a common approach in the Social Sciences but probably less so among historians. More generally, History and the Social Sciences have differed in terms of practitioners’ expectations regarding the nature of comparison. Three characteristics of comparative work are specific to historical scholarship (Haupt & Kocka 1996). (1) The Enlightenment notion that historical research ought to be close to the sources in order to count as professional and authentic has imposed disciplinary standards that encourage skepticism against generalizations and are strictly applicable only to certain kinds of endeavors, most notably specialized studies – but less so, if at all, to larger syntheses. For comparative analysis to become feasible, such standards must not be over-prioritized at the expense of alternatives that allow an appreciation of broader patterns. Concerns over disciplinary standards are closely linked to questions of expertise, which I will discuss below. (2) Historians are by definition interested in change over time; the entire field is characterized by a special relationship with the dimension of time. Analysis progresses from older to newer without severing connections over time. History is not seen a sum of cases from which general principles can be abstracted; instead, individualistic models (about A or A vs B) dominate. (3) Historical processes are seen as deeply embedded: any element of the historical experience is considered hard (if not impossible) to understand outside the context provided by other elements. The underlying expectation is that it is necessary to understand the whole in order to understand elements thereof. In as much as this principle prevails, and history is viewed as deriving meaning from synchronic and diachronic contextualization, a focus on discrete variables might seem constricting, unprofessional, or worse. Moreover, the ceteris paribus condition implicit in variable-centered approaches is in practice rarely met.

(1-3) create tensions with the principles of comparative analysis. The more cases are involved, the less proximity to the sources can be attained (due to language problems and greater reliance on secondary scholarship); yet multi-case studies may produce more robust findings. Cases have to be defined and hence to some extent isolated in order to be subjected to comparison, and to be related to each other as individual cases. This process breaks continuities: the focus shifts from change over time to similarities

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2 See Mahoney 2003:348-53 for a response to criticisms of Millian methods.
and differences. Most importantly, comparison is predicated on selectivity: cases tend to be decontextualized and stripped down, especially in multi-case comparisons.

Various strategies are available to cope with historians’ reservations arising from these tensions. (1) Comparison may be limited to a few, ideally just two, cases, in order to reduce the need for simplification and second-hand scholarship. (2) Abstraction is limited by retaining as much context as possible. Emphasis is placed on contrast rather than generalization of commonalities. (3) A focus on processes helps preserve the link to change over time. Much of the work being done on ancient East/West comparisons is fairly representative of this approach. Yet it is important to realize that there is no silver bullet. Emphasis on richly textured narratives that strive to preserve as much as nuance as possible is a problematic solution: the more of the original context of each case is retained, the less systematically comparative the resulting study will be. Inevitable trade-offs appear built into the comparativist venture, regardless of individual idiosyncrasies of perspective and approach. Moreover, the palliative strategies outlined here in turn raise new problems. Historians would do well to engage more with social scientists’ concerns about what is known as the “small-N problem” that threatens to undermine comparisons between only two (or few) cases: “the combination of many factors assumed to be causally relevant with evidence from only a small number of comparable cases”, which may leave us with “too few cases chasing too many causal factors” (Rueschemeyer 2003:305, 325). Although worries about the formal (statistical) significance of putatively necessary or sufficient conditions in small-N studies may seem of little relevance to qualitatively oriented historians, the underlying problem is the same (Mahoney 2003:350): how can we be confident that the factors that are thought to account for meaningful differences between two cases (say, ancient Greece and early China) are in fact critical to these observed outcomes if our analysis is confined to just those two cases?

On a more positive note, we must remember that issues of selectivity, perspective and construction are inherent in any kind of historical study. Comparative history is special only in so far as it renders them particularly conspicuous. This should be considered a gain, as it compels historians to acknowledge more explicitly what it is they are trying to accomplish, and how. Comparative history is intellectually demanding because it requires continuous reflection. What are the appropriate units of comparison? There is no single answer: our choice depends entirely on the questions we wish to ask. What is to be compared with what? Once again, this is determined by our questions: apples and oranges make for fine comparanda if our focus is on fruit.

This leaves us with one of the biggest elephants in the room, the problem of professional expertise. Jeremy Tanner might miss an important factor when he muses that classicists’ wariness of comparison may be linked to ideological reasons and concerns about its usefulness: insularity can also be engendered by self-imposed disciplinary standards that severely constrain scholars’ ability to move beyond their own field of specialization. Virtually every time I disclose my own comparative interests and projects to classicists, the first question is, “So are you learning Chinese”? If Classics is seen as predicated on the mastery of classical philology, ‘Comparative Classics’ logically ought to entail mastery of two separate philologies. Strictly applied, this premise would either exclude most ordinary mortals from comparative endeavors or latently discredit comparative work undertaken by such lesser beings. Neither one of these reactions can reasonably be expected to sustain a viable program of inter-cultural comparative research. Just as the perfect is the enemy of the good, insistence of conventional disciplinary standards is hard to reconcile with the demands of comparative history, especially once it transcends one-on-one comparisons and covers multiple cases. From a traditionalist’s standpoint, ‘serious’ comparative history might well be impossible.

What are the solutions? Subject matter is relevant. Research questions that require close engagement with textual sources would seem impossible to pursue without appropriate linguistic skills. Different agenda might accommodate a relaxation of this premise: the nature of the questions we are asking is vital in determining the means necessary to address them. Although Classics and Sinology/East Asian Studies may count as especially text-centered fields, the challenge of inter-cultural expertise is well recognized by comparative historians of other periods. Social scientists tend to side-step the problem by
focusing more on ‘big’ questions, where reliance on secondary scholarship is both acceptable and inevitable.

Not coincidentally, similar problems apply to the field of Comparative Literature: the need to privilege theoretical sophistication over language competence, and concurrent charges of dilettantism. In my very limited understanding, Comparative Literature has moved from something like comparative history – comparing discrete cases, often in the context of nation states – to something more akin (in terms of outlook though not method) to Social Science, transcending traditional divisions and approaching world literature as a quarry to explore specific questions. No comparable Comparative History has yet developed.

Even so, avoidance strategies that prevent comparative historians to engage with the full range of the evidence (and often language-specific secondary scholarship) can only lead to a very impoverished research agenda. Collaboration may be the only feasible solution (e.g., Meier 2003:266). If one scholar cannot master all the required skills, two or more of them need to pool their complementary resources. This conflicts with the largely solitary character of much Humanities scholarship, the result of tradition, personal inclination, and entrenched academic incentive structures. Participation in conventional edited volumes or big editions cannot count as valid exceptions to this principle. Thanks to its overlap with the sciences, archaeology offers a more promising paradigm of collaboration (not only among specialists in different areas but also transnationally) – yet historical research resolutely remains more individualistically organized.

Given the considerable hurdles of moving between Greco-Roman and early Chinese sources and scholarship, the near-absence of genuine inter-area collaboration is indeed striking. Tanner’s marvelous bibliography of Sino-Hellenic Studies (2009:106-9) reveals hardly anything at all. The only exceptions are – you guessed it – Lloyd & Sivin 2002, plus Shankman & Durrant’s 2000 monograph and 2002 edited volume. (Hall and Ames – 1995, 1998 – are both China scholars, which does not count for my present purposes.) Who has been behind these rare collaborations? The balance of the Shankman & Durrant collaboration is already signaled by the reversal of the authors/editor’s surnames. In her review of their 2000 book, Yiqun Zhou notes that “it is evident that the book is to a much greater extent informed of the personal mission of Shankman the classicist than that of Durrant the sinologist” (Zhou 2000:175).

This meshes well with my own experience in running Stanford’s “Ancient Chinese and Mediterranean Empires Comparative History Project” (ACME 2005-12). I suspect that the relative heft and maturity of the two fields account for this imbalance. ‘Classics’ has produced around one million publications since 1900 and is relatively well represented in academia as a legacy function of its privileged position in the age of global European hegemony. Several thousand scholars attend the annual meetings of the North American Classics association; over 500 Greco-Roman historians hold faculty positions at Anglophone universities (Scheidel 1997, 1999). Whatever the corresponding numbers for early China studies (in the West), they are bound to be much smaller. In some ways, early China scholars operate in a context that is reminiscent of Classics a century or more ago – with fundamental texts being edited for the first time, much of the existing sources unavailable in translation, and archaeology rapidly expanding the body of knowledge. All this may concentrate minds on the more essential tasks at hand. But this is merely a conjecture: I would greatly welcome feedback from China scholars on their field’s incentives and disincentives to inter-cultural collaborative research. Scholars of the Greco-Roman world, it must be said, in any case lack any pragmatic excuses for their failure to instigate more comparative work.

What has in fact been done? The trend is upward, even as the overall volume of relevant work remains deplorably minuscule.
Comparative ancient East/West publications
(counting edited volumes as single items)

Tanner’s pioneering survey of 2009 dealt with the liveliest area, that of comparative intellectual history – “history of science and medicine … literature and historiography, philosophy, religion, law, cultural history”, which coincides with the principal interests of this group. This relieves me of the obligation to cover familiar ground. Tanner 2009 also very helpfully furnishes me with a template for comparing comparisons: how does existing work in comparative intellectual history compare to that in other areas?

Tanner (2009:90, focusing on Lloyd 1994, 1996) describes Lloyd’s method as contextualizing knowledge production in socio-political conditions, such as civic institutions that favored persuasion and use of evidence in Greece and monarchical arrangements in China. Lloyd’s rejection of holistic generalization (Greece here, China there) is consistent with an interest in factors that can be causally linked to observed outcomes (92). This places his approach in proximity to the variable-based and causation-driven comparisons favored by social-science-friendly historians. “Explanations of cultural difference” (98) are a key objective.

Mutschler (Tanner 2009:103) has followed a similar approach in his writings on historiography, contextualizing and explaining divergent traits with reference to the social circumstances of text production. Kim 2009, in a comparative study of representations of ethnicity, contrasts the Greeks – on the margins of a larger civilization/empire – with the more centrally situated Chinese. His goal is “to determine the historical, political and cultural factors that determined the Greek and also Chinese perception of foreigners”. His main concern are “causal factors” that account for specific outcomes (2009:2). Differences are traced back to specific contexts: while the polarizing Greek-barbarian divide is interpreted as the result of conflict with Persia that prompted anxious Greeks to play up their martial and phenotypical superiority, the early imperial Chinese tradition stresses differences in material culture (2009:144). Imperial inclusiveness emerges as a significant variable: from a metropolitan vantage point, Sima Qian portrays nomads as radically different but is prepared to incorporate other (assimilable) sedentary cultures as (faux-)Chinese, whereas Herodotus more flexibly switches between Greek/barbarian and civilized/uncivilized dichotomies as his perspective warrants (145). All this comes across as a fairly straightforward variable-centered approach: specific differences in context are held responsible for observed differences in outcome. The question remains, however, to what extent inter-cultural comparison is required to establish these connections.

Zhou 2010 finds that social solidarity was pursued within different institutions – festivals, symposia or gymnasia in Greece; ancestral sacrifice, family banquets or communal drinking parties in China – highlighting a contrast between peer-group- and kinship-centered activities. While the Greek
tradition emphasizes extrafamilial homosocial bonds, the Chinese tradition revolves around patrilineal family and kinship. This contrast is complemented by differences in the portrayed nature of mother-son bonds and female homosocial ties. Causation is traced back to the dynamic but divisive environment fostered by the principles of equality and competition in Greece and the perceived centrality of hierarchy (instead of gender) in early China. This amounts to a causal model of differences in traits that stem from differences in socio-political context and structure, similar to Lloyd’s approach.

Students of comparative institutional history are likely to be comfortable with this line of reasoning. They share both a preoccupation with causation – the question of how and why divergent forms emerge – and an interest in how they are maintained or modified over time. For instance, my own work on ancient Mediterranean and East Asian coinage employs the same procedure (Scheidel 2009). In this case, a clearly defined phenomenon – the creation of coined metal money – independently occurred only twice in world history. Discrete factors are identified to account for differences in outcomes: resource endowments (sustaining large-scale precious-metal coinage in western Eurasia); political ecology (heavily fragmented polities in the Aegean that relied on full-bodied issues and larger imperial formations in China that were able to accommodate a greater degree of nominalism and rely on base-metal issues for fiscal circulation); military ecology (the uneven demand for military pay especially in the Hellenistic and Warring States contexts); and the long-term consequences of path-dependence, as traditions crystallized over time (with later Chinese regimes reverting to the original norms even as contextual conditions changed). The focus is on variable-based causal explanation from an extended developmental perspective.

The single largest effort in the area of East/West comparison concerns what has become known as the “Great Divergence” (after Pomeranz 2000), the divergent economic development of ‘the West’ and China since around 1800. The literature is enormous and cannot be referenced here. One-on-one comparisons between the relevant parts of Europe and China suffer from a particularly extreme version of the “small-N problem”: a whole array of possible causes has been proposed to explain divergent outcomes in no more than two cases. On one end of the spectrum, geographical-ecological factors almost obviate the need for specific inter-cultural analysis: if Europe enjoyed geographical advantages (Diamond 1999) or was closer to the New World (Morris 2010) or farther from the Eurasian steppe (Turchin 2009), its eventual ascendency might be considered a long-term lock-in. If we assign greater significance to institutions, as most historians would do, the picture becomes more complex and ancient historians potentially have a more important role to play. For example, if persistent political polycentrism is regarded as critical to particular ‘Western’ outcomes, the post-Roman non-reconstitution of universal empire in Europe takes center stage. Once again, a number of highly diverse variables may account for long-term differences in European and East Asian state formation, from steppe exposure (Turchin 2009) and sheer spatial spread (Hui 2005) to the configuration of the main sources of social power (Zhao forthcoming), fiscal regimes (Wickham 1994; Scheidel 2011), religion, and so on. Once more, the small-N problem causes observed outcomes to be heavily overdetermined (as multiple causes may all contribute to the same difference in outcomes). Multi-case comparison, pitting one case against many, that allows more systematic discrimination between variables may be the only way out of this conundrum, as one-on-one comparisons on that scale may simply not be capable of identifying the principal causal associations. In other words, more comparison may be the best remedy for overly narrow comparative history.

Another option is exemplified by Hui’s 2005 study of why the Warring States environment resulted in universal empire in China whereas intense inter-state competition in early modern Europe failed to do so. Applying the “uncommon foundations” method advocated by McAdam, Tarrow & Tilly 2001:81-4, Hui tracks different combinations of often shared causal mechanisms with varying initial and environmental conditions that generated different outcomes (2005:8). It is telling that this pioneering study across periods was produced by a political scientist, not a historian; that historians’ reactions have prioritized points of historical detail instead of engaging with the merits of the overall thesis or approach; and that historians have more generally shied away from this kind of problem-centered comparative analysis that transcends conventional periodization.
It is only in certain cases that synchronicity is a crucial element of historical comparison. World-systems approaches come to mind, not so much in the radical version that seeks to identify effects of supposedly interactive world systems thousands of years ago (Frank & Gills 1993; Frank & Thompson 2005, 2006) but in the appreciation of exogenous inputs such as climatic variation that may be causally linked to equivalent outcomes in otherwise separated societies (e.g., Chase-Dunn, Hall & Turchin 2007). Teggart’s notorious 1939 study of interaction effects between eastern and western Eurasia was an early, if inept, example of this approach. Conversely, asynchronous comparison challenges the intellectually lazy notion that absolute chronology is the most obvious way of structuring comparative analysis (whereas it requires at the very least explicit justification: e.g., Scheidel (ed.) 2009). To name just one example, the study of economic efflorescences in imperial contexts invites comparison between Rome and Song China, which as Morris 2010 suggests had reached comparable levels of social development but a millennium apart.

For now, however, we have to make do with what is available. Several more conventional comparative studies have made significant contributions to our understanding of case-specific outcomes. Hsing I-tien 1980, which has unfortunately remained unpublished and therefore completely ignored, analyzes the role of the army in imperial succession in the Roman Principate and the Western Han period. He identifies three key differences: a well-established principle of closed dynastic succession in China, very much unlike in Rome; close contact between rulers and the military in Rome but not in China; and the structure of the armed forces, which were better able to develop corporate interests in the Roman context. Hsing I-tien carefully distinguishes between factors that merely provided opportunities for military intervention in the political process (the first two) and the one factor that crucially accounted for different incentives for it (the third one) (1980:212). This raises a new question, why the two empires ended up with different types of army (1980:214-9). This anticipated Lloyd’s call for ‘deparochialization’ and demonstrates very clearly how a variable-centered approach can not only improve historical explanations for individual cases but also helps identify the most important questions.

Custers 2008 is in its entirety focused on variables and their configuration in addressing problems of long-term imperial stability in Rome and China. Rosenstein 2009 links divergent trends in state formation in the Roman Republic and Warring States China to the nature of competition: as Rome experienced a relaxation of foreign threats (compared to China), it embarked on an alternative trajectory (2009:49-50). As noted by Vasunia 2011:227, this entails an implicitly counterfactual claim of anticipated commonality had contextual circumstances been more similar. However, Tan 2011 arrives at congruent conclusions by explaining the absence of coercion-extraction cycles in Republican Rome that were typical of early and mature modern European states under military pressure.

At the same time, a number of existing works falls short of the demands of comparative historical analysis. For instance, Motomura 1991 assumes that comparanda need to be very similar for comparisons to be feasible. Gizewski 1994 is primarily concerned with identifying parallel processes in ancient eastern and western Eurasia without addressing the question of their causal underpinnings (cf. Scheidel (ed.) 2009:13-14). Adshead (2000:ch.1; 2004:20-9), in a series of admirably lucid comparative observations, is content with producing what are essentially laundry-lists of observed differences between the Roman empire and Han or Tang China: the ultimate purpose of this exercise remains unclear. A step forward in a similar vein is Morris’s 2010 long-term comparative appraisal of East and West through the lens of a ‘social development index’ based on the four variables of energy capture, urbanism as a proxy of organizational capacity, information processing, and war-making capabilities (2010:148-9). The end result is a kind of score card on who was ‘on top’ at any given time.

Mutschler & Mittag 2008, in a well-attended project, favor a somewhat idiosyncratic form of hands-off comparison, whereby “the comparative aspect” … [is] addressed, if only indirectly, in the pairing of papers for each topic, thus freeing contributors from drawing explicit comparisons, and hence from forays into unfamiliar fields of specialized knowledge” (2008:421; also XVI). It is perhaps not entirely obvious why, if experts for particular topics had already been found and paired, they could not be encouraged to collaborate – perhaps even co-author – in the service of sustained comparative analysis. It is also unclear why the reader is supposed to do all the hard work (“Readers themselves must … have
noticed many parallels and differences”). And in any case, comparative history is not primarily a matter of ‘noticing parallels and differences’ but of trying to explain and understand commonalities and differences. Their “Epilogue” (2008:421-47) addresses this deficit only in part by belatedly pulling some loose ends together.

Parallel exposition likewise dominates the chapter on “Imperial rule in Rome and China” in Burbank & Cooper 2010:23-59. The ostensible question of what made both polities successful is not well served by the avoidance of any causal comparative analysis. Only the question of post-ancient divergence prompts brief comparative considerations (2010:54-8) but only a single variable – direct vs indirect managerial strategies in Qin/Han China and Rome, respectively – is proffered without exploring alternative explanations.

Finally, I am left wondering about the insights meant to emerge from Brennan & Hsing I-tien 2010, an erudite and entertaining speculation about the fictional experiences of travelers to ancient Chang’an and Rome. The only causal observation I was able to discover refers to the impact of the republican regime on Rome’s cityscape (2010:200). This is unfortunate, as urbanism invites and indeed richly rewards more systematic comparison. In two separate contributions to Scheidel forthcoming, Mark Lewis and Carlos Noreña contextualize differences in Roman and Han urban features much more fully and explicitly. Lewis forthcoming focuses on the public sphere and its public spaces, contrasting the ‘city of the Romans’ with its deep historical texture and the multiple capitals of Chinese dynasties that manifested a de-historicized and well-ordered environment (that among other things eschewed military monuments, thereby decentering the military) in which the absence of public spaces served to equate the public sphere with the state. The regional cities of the Roman world were run by locally autonomous elites whereas the Chinese district centers sought to replicate the principal metropolis and its state apparatus on a smaller scale, as loci of government presence and control. Noreña forthcoming likewise causally associates differences in cityscapes with differences in state formation: whereas Han cities served as instruments of social control, Roman civic autonomy rested on independent bases of localized social power. Noreña tentatively recasts what Roman historians are parochially used to seeing as the success of Roman-style city-based governance as a sign of state weakness, in contrast to Han arrangements, and considers the consequences for later imperial reconstitution.

It is comforting to be able to end this very brief and superficial survey on an optimistic note. Yet its very brevity, and the noted widespread absence of explicit engagement with problems of approach and process (to avoid the lofty term ‘method’), are cause for serious concern. By any standard, the comparative history of the ancient world is still a very immature area of scholarly activity. It is too promising to be left to ‘outsiders’ (although their potential contribution should by no means be underrated). Yet unless professional historians on both sides embrace comparative perspectives and its vital corollary, collaboration, little will change. I am offering these pages in the hope they will provoke debate and dissent, and encourage us to reflect more deeply on our shared interest in historical comparison.

3 A very substantial comparative history of the Roman period and early China was rejected by several university presses primarily because the author, a very well-read senior academic at an elite US institution and native Mandarin speaker, was not a professional historian. A massive causal analysis of Chinese state formation by a historical sociologist is now in danger of being considered not historical enough by historians and too historical by social scientists. The list will no doubt keep growing.

4 I expect that my relentless preoccupation with causal explanation will raise some hackles.
References


