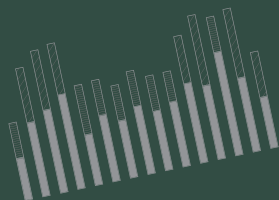




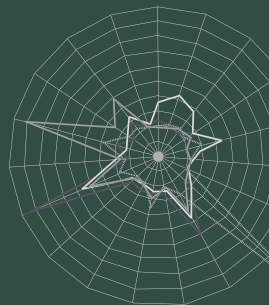
Methods in
World History
A Critical Approach



Arne Jarrick, Janken Myrdal &
Maria Wallenberg Bondesson (eds.)



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METHODS IN WORLD HISTORY



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A Critical Approach

Edited by
Arne Jarrick, Janken Myrdal
& Maria Wallenberg Bondesson

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Globalization and world history

An introduction to studies of methods

Arne Jarrick, Janken Myrdal & Maria Wallenberg Bondesson

Globalization – a long-term process

Globalization may be considered a process in which the network of human interaction gradually widens and takes on new and more complex forms. We would venture to say that each step of these deeper and more inclusive interconnections has unique characteristics. For instance, during the time of the great empires at the beginning of the Common Era (CE), the flow of materials and intellectual influences reached a higher level than ever before. Another important step was taken in the sixteenth century, involving the merging of the two worlds, America and Afro-Eurasia. These steps presented new challenges to populations all over the world, in the spiritual sense no less than in the material sense. Such challenges permeated the encounters between people and peoples who previously never met, and who found one another alien and perhaps even less than human. And those people, confronted with completely new geophysical circumstances, carried with them disease, which would prove yet another fateful challenge.

Globalization can neither be understood as a consciously intended process, nor as the irreversible goal or end-point of history. It is not the final outcome of some ancient master plan. Likewise, from our vantage point we cannot be sure that the worldwide web of human encounters will never fall apart, destroyed by, for example, pandemics, devastating wars, climate collapse, or something else that we cannot even envision. However, the fact that globalization has most often evolved as the unintended consequence of intended action has not prevented it from having a certain direction as it drives the ever-increasing connectedness of people around the world.

Obviously, globalization has not been the same throughout history. It has appeared in different guises at different periods in time. What are the distinguishing features of our own age of globalization, then? Trade? No, the late nineteenth century saw the establishment of bulk trade with steamers and railways, a much more pervasive change in trade than we see today. Migration? Only if we talk about shorter translocations – if we are talking about mass migrations then other periods are far more important. But there is one specific feature that is uniquely contemporary: the immediate and worldwide transfer of information. That has never happened before: what does it imply for globalization?

Globalization is multifaceted, permeating almost all aspects of human life, from the production of material objects to the production of ideas, from social conflict to ideational clashes. Ideas have spread worldwide, so that similar and fundamental concerns have seeped into people's minds, whether explicitly discussed or not. Indeed, today we are witnessing a rapid convergence in what people around the world are discussing, not just geographically but also temporally, be it a truly worldwide conversation in the global agora or a series of local exchanges resembling each other.

Of course, even in our time, there are a wide variety of issues, dealt with separately in the different regions and nations of the world, but they are increasingly converging into a more limited number of core issues of common concern to all humankind. Worldwide access to modern Internet media is the technological motor of this change, together with computerization, which leads to a similarity in intellectual tools and approaches all over the world. However, aside from the opportunities provided by new and fast-expanding technologies, there is also a societal and environmental base for the heated arguments found in almost all corners of the world. Democracy, women's rights, environmental problems, and not least comparisons between cultures: for good reasons, these issues worry us wherever we happen to live, as they are the basis for sustaining our lives and societies, and require globally agreed solutions.

For instance, patriarchs in Central Asia and feminists in New York are not only equally preoccupied with gender issues, but also

largely familiar with one another's discourses to which they also react, so that advances in women's rights in one place may trigger a conservative, patriarchal reaction in another. Environmental issues are linked to questions of power and responsibility not only between nations, but between continents, in a continuation of a centuries-long trend towards increased rights for previously repressed groups being offset by counter-reactions from individuals and groups who see their power diminishing.

Self-evidently, this intense global discussion does not necessarily mean that we are witnessing an increasingly unified culture. Quite the contrary, we are rather experiencing the continued competition between world religions and ideologies, and the strengthening of some aspects of regional cultures, perhaps in reaction to the pressure applied by globalization. However, those religions, ideologies and regional cultures increasingly revolve around common core issues.

It is precisely here that world historians come in. Indeed, the growing field of world history research is itself part and parcel of that globally converging agenda. And in every discussion that is relevant for the future direction of humankind, in every discussion with ideological connotations and implications, history plays a role. History is the storage chamber from which arguments are fetched, whether complete myths or solid facts. Potentially, this gives the writing of world history a specific and crucial role as globalization unfolds. For this reason it is reassuring that a globalizing trend is clearly discernible in current historical research.

World historians

It is clear that the scientific tide has turned many times before, and historical research is no exception. Thus it is no surprise that over the last two millennia there have been a number of synthesizing waves, when intellectuals from different schools have made attempts at formulating grand, general ideas about the forces and destiny of world history. From earlier periods we might mention Ssu-Ma-Chien or Ibn Khaldun, from recent history Karl Marx and Max Weber (for an overview and a number of presentations

of world historians, see Galtung & Inayatullah 1997), and more recently Immanuel Wallerstein, Jared Diamond and David Christian (Wallerstein 1974–1989; Diamond 1997; Christian 2004). Today we see world history studies evolving into a movement, a genre in its own right, with specific journals, international conferences, and increasing numbers of scholars leading the way (Collins 1999; Bentley 2011).

On the surface, the current boom in world history studies resembles a similar boom in national history in the nineteenth century. In every country, new journals and associations were founded and large groups of historians published on national history in monographs and textbooks. But the similarity goes deeper than that. Generally speaking, the peoples of Europe and their politicians in the nineteenth century embraced nationalism. Thus, from the nineteenth century onwards, professional historians were “drafted” into the nationalist project of providing their nation-states with a glorious past. This, said in passing, has been repeated in nationalist and sometimes anti-colonial historiography in other parts of the world since the Second World War. Since nationalism was an all-embracing ideology, nationalistic inclinations often harmonized with the attitudes of the historians. However, gradually some of them became annoyed by the lack of scientific distance to certain of these myths, and took steps to professionalize and at the same time improve historical research. As professionals they reacted against the role that history was supposed to play in the formation of national self-awareness – the forming of nations as collective units.

Citizens were taught that they had a shared history, a kind of unifying experience. It was normally charged with pride at being a citizen of the nation to which they belonged. We are all aware of the fact that the development of national self-awareness can have disastrous results. After all much of the ideological basis of German expansionism was to be found in how history was written. At the same time, history as a discipline became more and more professionalized. History was one of the core subjects at the universities, and when the humanities underwent a profound methodological transformation – the introduction of detailed and critical description – history was in the front line.

Source criticism developed into a key method, with certain criteria for how the double-checking of primary sources should be implemented. Plain forgery was the first to be weeded out, with the help of indicators such as writing style, age of paper and deletions. Another criterion was proximity to event and place. The nearer in time and space, the greater the credibility of the information gained. A most important check was purpose. Every source created by human beings embodies an intention. Most often, those who produce the source (whether written, painted or created by other means) want to portray themselves in a good light (when describing a war, for instance), or to gain some advantage (in a conflict over property, say). Source criticism could be used as a powerful weapon against exaggerated national self-esteem, with its counterpart in today's sensitivity to Eurocentrism and other self-blind biases. Source criticism certainly does not offer complete protection, however. Thus the country where the method was first developed, Germany, was also the country where national pride, or even "race pride" based on counterfeit historiography, took on horrific proportions, with the most disastrous effects. The Nazis had strong popular support, largely thanks to historical mythmaking.

This process can be described as bi-directional. Thus professionalization, inspired by nation-building, could *also* provide the tools by which the myths and misinterpretations in nation-building historiography could be undermined. Professionalization, in the sense that scientific methods are developed, is potentially a self-healing process during which facts will be established under increasingly intense scrutiny, reviewed by peers, while the individual scholar simultaneously exerts self-control when interpreting sources.

Improving methods

What is the lesson to be learnt from this? Historians have a responsibility for their presentation of knowledge of the past, but also for trying to avoid undesirable use of their findings. One important way to minimize the risk of abuses of historical knowledge is to expose the results to stringent tests, as well as to gather and systematize knowledge with a mind as "clinically" detached as possible.

An essential step is to open up a thorough discussion of what the production of historical knowledge can and should imply for us in the methodological sense. However, to date such methodological issues have seldom been discussed. For example, the just-published *A Companion to World History* (Northrop 2015) has over thirty interesting chapters, but almost none of them are occupied with critical methodological perspectives (for the one rare exception, see Adas in Northrop 2015). The present volume is thus an attempt to redress this sort of relative deficiency.

Today's world historians need to reflect systematically on the methods they apply in order to improve and develop their craft. We are fully aware that this brings to mind a wide variety of issues, of which only a small number and specific perspectives will be particularly addressed in this volume. Our take on the matter is as distinctively or narrowly methodological as the overarching questions are quite simple. The first question concerns how to gather information; the second, how to make sure that the information gathered and utilized is reasonably reliable. The questions are operationalized into a number of different issues, all aiming at the improvement of the craft of world history. They range from an encouragement to utilize new, non-textual sources, through calls to improve source criticism using systematic examination of secondary sources and the different degrees of resolution of data to be compared, to methods for improving our ability to understand and compare seemingly unintelligible sources divided by wide cultural distances, and, finally, to methods for measuring long-term economic relations between countries and regions.

For quite a few global historians, the major methodological mission is different from ours. It is to find ways to resist ideological tendencies and temptations – varieties of Eurocentrism being seemingly the most important and pressing one. Quite frequently, Western historians have accused other – mainly Western – historians of treating Europe as a model for the rest of the world. This criticism appears in two guises: as an accusation of diffusionist bias or as what could be called a topical bias. The former type of criticism has been frequently repeated since at least the end of the Second World War and is well known to all who are familiar with global

or world history. Ironically as it might seem, the latter criticism has been evoked by attempts to respond constructively to the first type of criticism. As a response to charges of naïve diffusionism, historians have taken pains to show that certain social, institutional and economic processes, such as advanced trade networks, were established in many parts of the world independently, prior to corresponding processes in Europe, instead of being spread to peripheries from a Western center of origin (for example, Abu-Lughod 1993; Lieberman 2009). In turn, the critical repercussion has been that it is now regarded as Eurocentric to identify essential aspects of societal development with processes once thought as Western or European, although perceived as evolving independently of the West itself. Why focus on phenomena so closely linked to the development of Western capitalism, whether developed independently or not (Conrad 2013)?

One may wonder if it is possible to imagine any approach that would not be viewed as Eurocentric – one way or another. Certainly, it is always important to cultivate a sensitivity to one's own potential biases. And obviously, historians have had a tendency to present their own region as the bearer of specific and perhaps superior qualities. This is clearly a problem that must be addressed by all world historians with an ambition to provide critical and comparative analyses. Yet, the rejoinder may be a case of over-sensitivity, prompting anxious scholars to circumvent all kinds of globally oriented historical comparisons.

Equally, it is far-fetched to consider the use of certain concepts and theories as Eurocentric simply because they originated in Europe. It would be as strange to regard certain concepts as "Afrocentric" only because they were invented somewhere in Africa. This is to conflate narrow-minded part-blindness with the universal character and usefulness of certain analytical tools and theories offered to everyone wherever they happen to live.

Another danger, especially to historians, is politically or commercially driven expectations to present a distorted picture of long-term environmental change. This might be disastrous for our chances of solving future problems. Similarly, if certain idiosyncrasies concerning women in patriarchal cultures are to decide how women's

contribution to human intellectual and material culture is to be described, this could hamper the process of women winning more rights, not just for decades but for centuries to come.

This volume

Biases such as these constitute a threat to societally relevant research. Being of profound importance to science and society, these issues also feature large in this volume. This includes a deliberate suspicion of one's own non-scientific idiosyncrasies as much as of other scholars'. In other words, to us it is obvious that criticism of ideology-driven research in itself has to be as non-ideological as possible in order to be, and thus appear, reliable. Indeed, double-directed awareness of this kind is evident in much of this volume, especially in Janken Myrdal's plea for better historical source criticism, Mats Widgren's criticism of the myths of agrarian development in the world, Eva Myrdal's discussion of the data asymmetry between Sweden and Sri Lanka, and Rikard Warlenius' diligent attempt to measure value and exchange relations in a non-Eurocentric manner.

However, although present in most of the chapters, critique of ideology is not at the core of our methodological approach to world history. The same applies to theoretical and conceptual issues, despite the irrefutable fact that they likewise are relevant to matters of method.

As said, our overarching ambition is more limited. But even with a limited ambition it is beyond our reach to exhaust the issue. A great many other questions could have been included. Yet, since we see this as a starting point for a new way of approaching methodological problems in world history, we expect other scholars to join in and supply what is missing here.

Historical research has normally been based on textual sources, and it still is, a fact which is reflected in most of the chapters in this volume. This is unfortunate. Historians should be ready to approach any aspect of the past that could be of potential use for of any kind of information that helps in tracing bygone processes leading up to the present – be it a text, a physical object or a chemical process. This is precisely the issue at stake in John McNeill's contribution. McNeill mentions additional historical sources such as tree rings, ice cores,

mineral deposits in caves, fossil pollen, marine corals, et cetera. He states that the increasing interest in such sources is due to a “new surge of research into climate history”, in its turn reflecting concerns about the presently ongoing potentially devastating change in the climatic conditions for human life. If historians continue sticking to their age-old textual tradition, they will gradually become marginalized, miss out on crucial debates, and much needed historical knowledge will never be produced. Acquiring new methods is, however, not an easy task which is why historians, according to McNeill, may wish to collaborate with microbiologists, geneticists, chemists, and other experts on methods that so far have been rather alien to the historical sciences.

However, despite the coming sea change in the informational conditions for historical research – McNeill calls it a revolution – historians are still preoccupied with textual sources. This is also why the discussions in this volume mainly address the problems of tackling textual remains from the past. Our take on the matter is positive as well as negative, welcoming the huge potential of text-based comparative research, yet also warning against too sanguine an attitude to the problems intrinsically tied to it.

In his plea for sharpened source criticism, Janken Myrdal recommends that historians check up on a few indicators that are globally represented. Such studies could be based on a combination of sources, primary, secondary (literature), and tertiary (literature referring to other literature). Any such combination may result in new and solid knowledge, as long as the study is arranged so as to make it sincerely possible to refute its results. With the aid of empirical examples from his own research Myrdal then addresses a pair of essential methodological problems which he then applies to the testing of the so-called axial age theory.

Myrdal touches upon the problem that world history to a substantial degree has to be based on secondary or even tertiary sources. But he leaves a closer treatment of the issue to Mats Widgren. Widgren is presently engaged in a project to summarize and assess existing knowledge of global agrarian systems in the last millennium. In this project, the researchers have had to rely heavily on secondary sources. Here, they have been struck by how often scholars ignore empirically tested generalizations for untested “commonly accepted

assumptions". Widgren devotes his chapter to a critical discussion of four myths based on such assumptions: the myth of empty land in areas that were actually populated and used in different ways; the myth that current foraging systems are representative of their assumed counterparts in prehistory; the myth of agrarian inertia in the past; and the myth of environmental determinism. Along with his critical discussion of these myths, Widgren suggests how best to distinguish between reliable and unreliable secondary sources.

Like Janken Myrdal and Mats Widgren, Eva Myrdal addresses some of the problems with sources. Her particular aim is to raise warnings against a reliance on comparisons between regions or countries where data are really not on par due to their different degrees of resolution. She illustrates the general problem by charting how to achieve comparability of certain aspects of the long-term economic development of Sweden and Sri Lanka. The ultimate goal is to overcome such imbalances and thus establish a more solid ground for globally oriented comparative research.

All these chapters address a series of methodological shortcomings in the field of world history research. In contrast, Arne Jarrick's and Maria Wallenberg Bondesson's purpose is instead to argue that the methodological obstacles are not as non-negotiable as is often claimed and lamented. With a number of examples from their ongoing comparative research into the long-term history of law-making worldwide, Jarrick and Wallenberg Bondesson show that it is indeed possible to make intelligible – and so comparable and contextualized – texts from cultures at huge temporal and geographical distances from one another. Basing their research on primary sources, they also give an outline of the particular tools needed to come to grips with tough but nonetheless digestible matter of this kind.

Continuing the theme of comparisons, both of the two concluding chapters deal with different aspects of comparative economic history. Drawing on Immanuel Wallerstein's world system theory and ecological economics, Rikard Warlenius takes a structural-ecological approach to shed some light on the long-standing dispute over whether the early modern world economy centered on Europe or China. The case of the eighteenth-century tea and iron trade between Sweden and China is the subject of Warlenius' analysis, which he

carries out using time-space appropriation (TSA), in which the land and labor embodied in the commodities exchanged are calculated and compared. Since prices are considered highly cultural, and are not simply an outcome of supply and demand, this approach lays bare the power relations beneath what on the surface looks like equal exchanges. Warlenius demonstrates how to use the method, developed at the Human Ecology Department at Lund, making it possible for others to apply.

Like the other contributors to this volume, Rodney Edvinsson combines the identification of a methodological problem with suggestions of how to find a solution to it; in this case, how to estimate the long-term development of the GDP of different countries in order to make them reasonably comparable. Such work could be immensely time-consuming, and the challenge is how to find a shortcut which, being good enough, could become widely accepted – and far more valid than Angus Maddison’s heavily criticized “time series” which spans the entire era from 1 CE to the present. After addressing a number of potential pitfalls, Edvinsson ends by proposing what he calls the expenditure approach.

In conclusion, the intention of this volume is to serve as a starting point for constructive developments in the field of global history research. We are well aware that there are many more methodological issues that need to be addressed than those discussed here, and our aim is to mark a baseline for this most vital discussion about world history.

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Historians, superhistory, and climate change

J.R. McNeill

That we are now in an age of rapid climate change is disputed in only a few places, and there mainly for political purposes. Concern over what changing climate might mean has motivated a surge of research into climate history, using all manner of proxy evidence to inquire into temperature conditions, droughts and floods, the frequency of hurricanes and other major storms, El Niño events, and much else besides.

Only a small proportion of this new evidence on climate history comes from textual sources, the familiar terrain of the historian. Instead it comes from tree rings, ice cores, speleothems (mineral deposits in caves), fossil pollen, marine corals, varves (layers of silt or clay on the seafloor) among other places. Climate history is undergoing a renaissance thanks to all these new data. At the same time, as I will explain below, new evidence of other sorts, pertaining to other sorts of history, is also cascading forth.

Fifty or sixty years ago, under the influence of Fernand Braudel and his friends, professional history took a turn toward the social sciences. Thirty years ago, under the influence of other French scholars, no friends of Braudel, professional history took a “linguistic turn”. Now it looks to be taking a “natural science turn”. Historians seem to be rather like windmills, turning this way and that in response to the prevailing winds of other intellectual disciplines. There is nothing wrong with that. Surely it is often preferable to adjust one’s research methods in response to innovations, whether they come from physics or from literary studies, rather than to remain resolutely unaffected by changes in intellectual life.

In this chapter, I will try to explain some of the opportunities and challenges presented by the volcano-like eruption of new historical data from the natural sciences, with special attention to climate data and what some prominent historians have thought about climate. I will also raise the question of how the new data might affect the choices historians make about the scales on which they pursue their research, in particular the logic of selecting a global scale.

Superhistory and why historians need to overcome the text fetish

The past, always a foreign country, is growing more foreign to text-based historians.

If historians wish to improve their – our – ability to address puzzles from the past (and for that matter to remain central in the study of history), they – we – need to embrace what is fast becoming superhistory. Superhistory amounts to a methodological revolution by which textual evidence jostles together with that of ice cores, marine sediments, peat bogs, stable isotopic ratios, and the human genome – and a few other genomes as well. The revolution takes historians to new terrain, to geo-archives and bio-archives, as well as to the more familiar archives containing old documents. Climate history is part of this revolution, so far perhaps the biggest part.

While careful analysis of documentary texts is the bread and butter of the historical method, historians for at least a century have found ways to use other sources such as art, literature, and the findings of archeologists. For those times and places at which abundant texts, art, and archeology overlap, such as the Roman Mediterranean of the first and second centuries CE, the interplay among specialists using different sorts of sources is a long tradition and a fruitful one. Such melding of sources has yielded information and insights rarely matched for times and places with fewer texts, less surviving art, or low appeal to archeologists. So superhistory has its precedents, and perhaps should be regarded as an expansion upon a methodological tradition (Myrdal 2012).

Superhistory nudges us, and perhaps drives us, in the direction of global history. Texts come in languages, and those languages some-

times correspond to political structures such as states and nations. Japanese is spoken in Japan and almost nowhere else; similarly with Danish and Denmark. Thus text-based historians who know these languages are tempted to write their histories on the national scale or smaller. Moreover, much textual documentation has been produced by bureaucrats employed by states and concerns the business of states. Thus textual history – what we still call history – exhibits a strong bias toward the affairs of nations and states. It encourages historians to accept nations and states as appropriate units of analysis, which for some questions they are and for others they are not. This tendency towards national-scale history has probably weakened in recent decades; the advent of superhistory will weaken it further.

The evidence of superhistory bears much thinner relationships to nations and states, and encourages historians to play around with other units of analysis, both larger and smaller. Of course, textual historians for centuries have worked on a variety of scales. Some attempted global history or thematically defined subsets of global history, such as warfare, marriage, or agriculture. The new torrents of climate evidence and the genomic data easily lend themselves to global-scale analyses. Thus the evidence from the natural sciences that is now spewing forth invites a new generation of historians to adopt world-historical perspectives. And those scholars already employing world history perspectives probably find the new evidence of superhistory more interesting, exciting, and compatible with their ambitions than do other historians.¹

Superhistory bears a cousinly resemblance to Big History. Scholars such as David Christian (in Australia) and Fred Spier (in the Netherlands) have spearheaded a very long-term perspective on human affairs, which they call Big History. It involves situating the human story inside the story of life on Earth, inside the story of Earth, inside the story of the solar system, our galaxy, our Universe. At this scale, mind-boggling for most historians, Christian and Spier find patterns that are not readily visible on the brief time scales familiar to historians and archeologists. They see, for example, recurrent stories of energy capture and advancing complexity, in celestial bodies and civilizations alike, over time (Christian 2005; Spier 2010).

Big History, moreover, necessarily requires a plurality of sources,

disciplines, and perspectives. Big historians such as Christian (a historian of imperial Russia by background) and Spier (an anthropologist with an undergraduate degree in biochemistry), must wrestle with cosmology, astronomy, earth science, evolutionary biology, paleoanthropology, archeology, as well as history as conventionally understood. Big History is stunning in its ambition, and requires an Aristotelian reach on the part of its practitioners. No wonder there are comparatively few who practice the art.

Superhistory calls for something less. First of all, it is not concerned with the origins of the Universe, stars, planets, galaxies, solar systems, or life. Nor is it concerned with much of the first four billion years of life on Earth. Rather, it is concerned with the human experience and only the human experience. It is a few notches less ambitious, and less demanding, than Big History. But it shares with Big History a recognition of the value of approaches to the human past through multiple disciplines and multiple types of sources. In this chapter I maintain that historians, like it or not, would be well advised to accept the implications of superhistory and acquaint themselves, where appropriate to their subjects, with the evidence from the natural sciences. But I do not claim that they need to become practitioners, or devotees, of Big History. While I have the greatest respect for the achievement of Christian (2005) and Spier (2010), and am among those fascinated by the larger patterns they identify, I do not yet see that their elegant nesting of human history inside so many other histories necessarily changes the way historians should see their subjects. In a sense, Big History changes everything and changes nothing. It tells us that our species' story conforms to a larger pattern. But it does not change our species story. In a nutshell, superhistory represents a revolution in historical methods but no change in subject; Big History is a revolution in subject, perspective, and method.

Before going any further, let me turn to some of the dark sides of superhistory. Among the risks one runs in globalizing history is the temptation to seek simple explanations for things outside one's area of expertise. This temptation operates within history itself, and *a fortiori* with respect to scholars venturing beyond its traditional confines and dabbling in superhistory. So, historians of modern or late imperial

China for example, who would never accept a simple explanation of the fall of the Qing dynasty, are tempted to accept one for the fall of the Maya city-states or for the empire of Mali. This is only human: it would take time and effort to educate oneself in the complexities of Yucatan and Guatemala in the eighth to the tenth century, or West Africa in the fourteenth and fifteenth. Who can justify the time?

Historians who forage in other disciplines to enrich their sense of the past run a still greater risk. Climatology, genetics, historical linguistics, isotopic analysis, and other specialist realms, of course have their complexities and controversies, and informed assessment of them requires some considerable education in fields most historians have steadfastly avoided since secondary school. Hence the powerful temptation to accept simplistic explanations – or, what is probably worse, to ignore novel sorts of evidence and pretend that texts are all that matters.

That last position, while comforting to those of us trained to examine texts, is increasingly naïve and intellectually unsustainable. By fetishizing documents, professional historians for nearly 200 years have presented caricatures of the past based on what were often accidents: what happened to get written down, and, of that, what happened to be kept, and, of that, what happened to survive rot, fires, floods, ravenous insects, important people trying to hide a sin or two, or any of the several other hazards to which paper, papyrus, clay tablets, or oracle bones might be subject. As a result, a great deal remained hidden to historians, many of whom found it congenial and comforting to pretend it therefore did not happen.²

Confining oneself to data present in texts is now a worse method than ever before. Nowadays the chemically-inclined physical anthropologists can tell us, by examining strontium-calcium ratios in bone and teeth, where the food was grown that nourished any particular body. They can tell which alpine valleys Ötzi the Iceman frequented during his childhood, and, if given a tooth, could tell us whether or not President Obama was raised in Hawaii and Java as opposed to Kenya, as a remarkable proportion of Americans believe (not that strontium-calcium ratios would likely change their opinions in the matter!). They can tell us, from chemical analysis of human remains, that Italians in 900 CE ate almost

no seafood unless they lived on the coast, but that by 1300 even inland Italians ate it routinely.

The microbiologists can tell us which antibodies prevailed in which populations, providing indications of past experience of bubonic plague or malaria. From teeth, they can tell us which skeletons in London cemeteries from 1348 belonged to bubonic plague victims and which did not. They can map the geography of the 1918 influenza epidemic's intensity at least as well as can historians working with texts. The geneticists can tell us that the founding mothers of the Icelandic population were overwhelmingly from Britain and Ireland, presumably abducted or purchased by Norsemen en route to settlement in Iceland in the decades after 874 CE, something on which the Icelandic texts are mute and archeology unhelpful.

Microbiologists and geneticists have also resolved what for Americans was a detail of some importance: that Thomas Jefferson did indeed father children by a slave woman named Sally Hemings. Jefferson's paternity of Hemings' children, once widely doubted, is now denied only by his fiercest apologists. It is not merely the distant past that new methods illuminate, although their value is indeed greater in that very foreign country because of the paucity of texts.

It may be disconcerting for most of us, but history is in the throes of a methodological revolution or two, one for which none of us are trained and few of us prepared. To the extent that we shy away from it, we will be shunted further to the margins in some of the important debates of our time, such as that over the significance of climate change, and – incredible as it may seem to historians – perhaps also in some discussions of the past. To the extent that we embrace it, we will have a voice in all these conversations.

I will finish this discussion with a cheerful example of the promise of superhistory drawn from my own experience. While a doctoral student in the early 1980s, I grew interested in the history of yellow fever in the Caribbean. At that time, no one knew if yellow fever was originally an American or an African disease, no one knew whether its vector, a particular species of warmth-loving mosquito, was American or African, and no one knew why the texts seemed to show a stronger prevalence of the disease in the eighteenth and nineteenth centuries than before. (I should also say that as far as I

could tell no one other than myself seemed to care about that last point.) I wrote an amateurish paper on yellow fever in the Caribbean, thinking I would follow up with more research soon.

Fortunately for me, I let life intervene for a quarter century before I returned to the subject. In the interim, geneticists had shown that the yellow fever virus circulating in the Americas is of African origin; and that the vector mosquito is also a migrant from Africa. These new data help explain the immunological basis for a racist discourse of difference between Africans and others in the Caribbean, one that claimed Africans were more fit than others for labor in the torrid zone. (Being African or black was widely thought to be important but in fact was irrelevant; disease resistance to yellow fever was based on whether or not one spent one's childhood in an endemic yellow fever zone such as most of West Africa – and perhaps whether one's ancestors over hundreds of generations had done so. Many Africans and many blacks had neither acquired nor heritable resistance to yellow fever.) But many did carry immunity to yellow fever, and resistance to malaria as well. And so once these diseases became established and endemic in the West Indies (by about 1650), it seemed to most observers (or, more precisely, to those whose opinions happened to get written down and preserved) that African bodies were by nature suited for labor in the Caribbean.

Meanwhile, historical climatologists studying the chemistry of the shells of sea creatures had shown that the warming at the end of the Little Ice Age (already known as a European phenomenon in my student days) extended to the Caribbean. As the Caribbean got warmer, conditions improved for the yellow fever mosquito. "Vector abundance", as specialists put it, is crucial to the prevalence of mosquito-borne diseases, so climate's suitability from the mosquito point of view was an important factor affecting the risk presented by yellow fever. Climate scientists, in the interim, also had constructed a database of El Niño events over the centuries, allowing respectable hypotheses about drought frequency and varying conditions for mosquito breeding.

So, by 2005, without having done any of the research myself – which indeed I was not competent to do, having sidestepped microbiology and climate science in my education – I was in a

much stronger position to make sense of the fragmentary textual record concerning yellow fever outbreaks available in a handful of archives. All I had to do was read the recent work of a handful of scientists. And the book I wrote on these subjects was more convincing than it could have been in 1985, thanks to the emergence of new data from the natural sciences – convenient bits of superhistory (McNeill 2010).

Natural scientists provided these convenient bits of superhistory not because I wanted them, but because such research suited their own agendas. Had it been up to me, I would have asked for research on some other issues pertaining to yellow fever as well (particularly the unresolved issue of whether or not there is any heritable resistance or even immunity to the disease). Unfortunately for me, I was not in a position to direct the efforts of microbiologists and geneticists. Few historians, if any, will ever be in that position. This, then, is a limitation of superhistory: the findings pouring forth result from research agendas that historians do not shape, and generally do not even influence in the slightest. But this is only a limitation. And if historians join interdisciplinary research teams before those teams fully set their research agendas, the odds of affecting those agendas improve dramatically. This limitation is not reason enough to scorn the data of superhistory.

Why historians need to elbow their way into the climate change debates

A big part of the new superhistory, and the only part I will deal with henceforth, concerns the history of climate. So far the archeologists and paleo-anthropologists have gone further than historians in taking historical climate change seriously. There are several possible reasons for their eagerness to embrace climate change in their work. One is that they live off research grants to a larger extent than do historians, and to elbow their way to that feed trough amidst the legions of cancer researchers and hunters of the Higgs boson, they need to make plausible claims to be doing relevant and useful science. And among the few routes open to them is to offer results that speak to societies' experience with climate change. A second

possible reason is that because they typically deal with sparse evidence – a few bones and potsherds – any new evidence looms large for their interpretations.

Historians, at least most of us, do not compete at the same trough as natural scientists. Our survival is not so directly tied to providing useful science, so our quests for funding do not propel us toward the issue of climate change. And most of us, at least, do not thirst for new kinds of information. We have enough of the old sort to keep us occupied. There is no shortage of texts awaiting examination or re-examination in light of new concerns. But all that, like the limitations mentioned above, is not reason enough to shy away from the new data offered by natural scientists, especially on climate.

Scientific American, an excellent popular science magazine, recently printed a fine overview article on current climate change and its implications. The author, a distinguished earth scientist from one of the world's foremost research universities, makes several trenchant arguments for the importance of the ongoing pulse of climate change. Then he writes:

Human civilization is also at risk. Consider the Mayans. Even before Europeans arrived, the Mayan civilization had begun to collapse thanks to relatively minor climate changes. The Mayans had not developed enough resilience to weather small reductions in rainfall. The Mayans are not alone as examples of civilizations that failed to adapt to climate change (Caldeira 2012: 83).

This is all he says about the potential significance of climate change for humankind. And, unfortunately, it is probably mostly wrong. According to current expert opinion, the Maya collapse – if that is the right word for it – consisted of a decentralization and de-urbanization that took place over several decades in the ninth and tenth centuries. It was bad for ruler, but ordinary Maya might well have regarded it as a liberation. (They left no texts so we cannot know for sure). Royal demands for tax and conscripts disappeared. With respect to climate, a severe and prolonged drought, one of three Central American “megadroughts” of the past 2,000 years – not “small reductions in rainfall” – deflated the rural economy. Many

things contributed to the “collapse”; specialists point to soil erosion, increased warfare, and half a dozen other unfavorable trends. And so to say it occurred “thanks to relatively minor climate changes” is doubly wrong – the climate changes were major and they provide only part of any explanation. Lastly, resilience to fluctuations in rainfall was probably among the strengths of the Maya, who had sophisticated water management (Stahle et al. 2011; Beach et al. 2015).

The *Scientific American* article (Caldeira 2012) illustrates some of the hazards of interdisciplinary work mentioned above. The author accepted blithe assertions about the Maya, without probing the specialist literature. Perhaps he felt he was too busy to research an issue tangential to his article; perhaps he trusted a careless research assistant too much.

My point is not to castigate a distinguished earth scientist for writing a few sentences of ill-informed history, or not merely to do so. It is, also, to argue that historians must bring their skills and sensibilities to the issue of climate change. Otherwise oversimplified histories penned by earth scientists, journalists, environmental activists, and climate deniers will prevail unchallenged.

What have historians done with climate so far?

The great majority of professional historians for the last 200 years have completely ignored any possible significance of climate. In many cases, this neglect is justified: climate had nothing to do with King Henry VIII’s unhappy marriages or Marx’s debts to Hegel or any of countless other matters important to historians. But on bigger scales, when one considers the trajectories of regions and societies, this persistent neglect is surely unjustified.

When thinking about historians and climate, and about how climate affected human history, it is important to draw a fundamental distinction, one that historians, and others, have from time to time ignored. That distinction is between climate regime and climate change. By and large, until quite recently, among those who attributed any significance in human affairs to climate, it was climate regime, not climate change, they pointed to.

Once upon a time, most thoughtful and educated people believed

the global climate was fixed. Some thinkers, from the time of Theophrastus – a student of Aristotle’s – if not before, thought that *local* climates might change. Aristotle himself in one passage implied climate had changed over the centuries in the Argive plain around Mycenae, and, further, suggested that pattern might be more general.

In the time of the Trojan wars the Argive land was marshy and could only support a small population, whereas the land of Mycenae was in good condition (and for this reason Mycenae was the superior). But now the opposite is the case, for the reason we have mentioned: the land of Mycenae has become completely dry and barren, while the Argive land that was formerly barren owing to the water has now become fruitful. Now the same process that has taken place in this small district must be supposed to be going on over whole countries and on a large scale.

This passage, so far as I know, is a rarity among ancient thinkers, who preferred to believe that climate was fixed except perhaps locally in response to loss of forest cover. (The indispensable guide to ancient environmental thought is Glacken 1967). And there is some difficulty of interpretation here: Aristotle did not mention climate specifically, even though the passage quoted above appears in Book I of his *Meteorologica*. Perhaps he had something else in mind, such as drainage. If Aristotle did have climate change in mind, as seems most likely to me, he was out of step with his age. The fact remains that (as far as the textual evidence can tell us) thinkers of the ancient world typically regarded climate as fixed rather than changeable. Modern historians, on the rare occasion when they gave the matter any thought, normally agreed.

Many deep thinkers, however, supposed that climate *regimes* shaped the essence of societies or the characteristics of peoples. Herodotus, Hippocrates, Aristotle, Ibn Khaldun, Montesquieu and thousands of shallower thinkers shared this view. The various bands of latitude, they maintained, each had their own climates, and each climate had its impact on people’s abilities or society’s characteristics. The fourteenth-century polymath from today’s Tunisia, Ibn Khaldun (1967: 58) for example, following the ancient Greeks, claimed that:

The human inhabitants [of the 3rd and 4th zones, bands of latitude in his scheme] are more temperate in their bodies, colour, character ... such are the inhabitants of the Maghrib, Syria, the two Iraqs, Western India, and China, as well as Spain; also the European Christians who live near by, the Galicians ... Iraq and Syria are the most temperate of all these countries.

Ancient and medieval writers typically thought, in short, that climate regimes were very important in human affairs because climate shaped, or even determined, temperament and intelligence. More recent commentators on these issues, such as the Baron Montesquieu, thought in a similar vein. His influential book, *L'Esprit des Loix* (1856) is substantially devoted to climate's supposed impact on temperament. Montesquieu, like his predecessors, attributed great importance to climate regime, but did not normally think in terms of climate change. That viewpoint was logical enough: no one lived long enough to see climate change in operation.

In the course of the nineteenth century, the idea that global climate might change acquired currency. Geologists convincingly demonstrated the ebb and flow of glaciers, for example, a phenomenon which seemed to require changes in climate. Some deep thinkers began to modify their views, and admit changing climate, at least on regional scales, as a possible motor in human affairs.

One of the first prominent historians to do so was the Englishman Arnold J. Toynbee. He held a marvelously contradictory set of positions about the role of climate in human history. His significance, perhaps, is that he was a transitional figure, who attributed importance both to climate regime and to climate change.

Toynbee was born in London in 1889, in the same week as Adolf Hitler and Charlie Chaplin. He was a scholarship boy at a famous public school, where he learned Latin, Greek, and German very well. He had a humanistic education with minimal exposure to natural science. After distinguishing himself at Oxford and logging a stint in the Foreign Office during World War I, he settled into his work as a historian – of everything.

Few historians outworked him: by the time he was 28, he had written 7 books. Then he hit his stride, and from 1921 to 1974 he

published, on average, upwards of 200,000 words a year. Between 1947 and his death in 1975 he was the most famous historian alive. Perhaps no other historian has achieved such celebrity – before or since.

His most famous work was his 10-volume *Study of History*, which he began to write in the mid-1920s. The cartoon version of it is that human history over the past 6,000 years featured 21 civilizations (he eventually admitted a few more), each of which followed roughly the same trajectory.

He believed that the proper unit of historical analysis was not the state or nation, but the civilization. All civilizations arose, he believed, as creative and original human responses to specific challenges. Their eventual demise he attributed to failure to respond to subsequent challenges, usually political and economic. The whole scheme had a mystical quality to it. Its greatest merit was its scope: true global history.

In the first volume of *A Study of History*, published in 1934, he included a section on the insignificance of environment as a factor in the rise of civilizations. Favorable climates did not necessarily promote achievement, greatness, or anything in particular. Nor did harsher climates necessarily make achievement harder. The opposing view, which he sometimes called “The Hellenic Theory” and attributed to Hippocrates and Herodotus, he disparaged.

However, by the middle of the first volume, Toynbee had modified his position somewhat: *changes* to climates could be important. In discussing what he called Egyptaic civilization, he found that it arose as a human response to a challenge, specifically the desiccation that he – following the archeologist Gordon Childe – believed affected all of North Africa and southwestern Asia around 5–6,000 years ago. To cope with growing aridity, peoples of the Sahara poured into the Nile valley and built dikes, dams, berms and canals, and more broadly, built a civilization.

Moreover, he decided that Sumeric (to use his preferred term for what others call Sumerian) and Minoan civilizations arose as responses to the same challenge of climatic deterioration. Several other civilizations arose from the challenges of “untamed” environments, whether tropical rainforests (the Maya) or arid plateaux

(in the Andes). In every case, changing or difficult environments formed part or all of the challenge that provoked the response of creating a civilization. But, as he pointed out frequently, the environmental challenge alone was insufficient explanation: not every case of desiccation gave rise to a civilization. Indeed, most did not.

While Toynbee's giant book is full of contradiction and inconsistency on this point, most of the time, despite his initial protestations against it, he granted environmental factors a significant role in provoking the origins of civilizations. Climate changes fit nicely into his overarching scheme of challenge and response.

He put his faith in climate regimes as well as climate change. He not only adapted desiccation theory to help explain the origins of three Eastern Mediterranean civilisations, but he posited what he sometimes called "the Golden Mean". Some climates were too easy and presented no challenge. Some were too harsh, and presented challenges that could not be overcome. Others offered a challenge that was "just right".

He used this concept to explain, for example, the success of New Englanders in dominating – as he saw it – the history of North America. In a most confusing argument, at least to an American, he finds the New England environment more stimulating than that of French Quebec, Dutch New York, English Virginia, or Spanish Mexico. In an equally confusing passage, he attributes the economic vitality of the North of England to the quality of its environment, and contrasts that to the softer challenges of the Thames valley and the Home Counties. He draws a line between the estuaries of the Severn and the Humber, to the northwest of which the environment provides a bracing challenge, and to the southeast of which it does not (Toynbee 1934–61, 2: 64–73).

Toynbee (1934–61, 2: 65) concludes his discussion of climate and environment in Britain by claiming that the contrast between the "legendary Scotchman – solemn, parsimonious, precise, persistent, cautious, conscientious and thoroughly well educated – and the legendary Englishman – frivolous, extravagant, vague, spasmodic, careless, free-and-easy, and ill-grounded in book learning – follows the same lines, and corresponds to the same contrast in the local physical environment ...".

Since Toynbee wrote these words in the 1930s, the economic history of Britain has not been kind to his interpretations. The south-east of England has flourished, and the industrial north declined in relative terms. The contrast between the legendary Scotchman and the legendary Englishman, which must have struck some readers as strange even then, now seems ludicrous.

In these passages he does not specify what he means by “environment”. Almost everywhere else he brings it up, he mainly means climate, and here descriptors such as “near-Arctic” for Quebec suggest he had it in mind here too.

When it came to the decline of civilizations, a matter of equal concern to Toynbee as their rise, he left out climatic variables altogether. Declines were a matter of weakening moral fiber among cultural and political elites. On this, at least, he was consistent.

Toynbee’s ideas show that it is not only distinguished earth scientists who may entertain simplistic ideas about the relationships between societies and climates. Great historians with Oxford educations can make a hash of it too. Historians of today will have to do better to deserve a voice in today’s climate debates and to earn the attention of those seeking wisdom in climate history. Fortunately, we can do better, and some have already done so.

More recent historians, to whom I will now turn, if interested in climate at all, saw matters almost precisely the other way around from Toynbee: climate change mattered more than climate regime, and it mattered in the fall of societies, states, and civilizations more often than in their rise. Where Toynbee sometimes saw climate crisis as the spur to creative moments, more recent historians usually saw only crisis.

Among more recent historians the most prominent to inquire deeply into climate’s role in human affairs was Emmanuel Le Roy Ladurie, born in 1929 to a farming family in Normandy. In the 1950s Le Roy Ladurie began to research climate change, mainly in Europe, for the period after 1000 CE. He began publishing on climate, harvests, subsistence crises and so forth in 1956, and eventually took on board the evidence of glaciers and tree rings – proto-superhistory – in a book (Le Roy Ladurie 1967) called *Histoire du climat depuis l’an mil*.³ While amassing evidence for changing climate, especially

of the Little Ice Age, Le Roy Ladurie made minimal claims for the significance of climate change outside of areas marginal for human occupation such as Iceland and Greenland. It was an unusual history book, both for its methodological innovations, and for its conclusion which was, in essence: my subject, on which I have labored mightily for ten years, is unimportant for human history.

In 2010, I had the opportunity to ask Le Roy Ladurie about his climate history work. He told me, and he has told others the same thing, that in the 1950s and 1960s he was afraid to claim significance for climate variables. He disguised his true views. He was then making his way upward in French academia, and feared (probably correctly) that being labeled a climate determinist or environmental determinist would derail his career. His friends and those whose good opinion he needed in order to flourish, were Marxists or at least marxisant, as he was himself. (He was a member of Parti Communiste Français from 1945 to 1963, but inactive after the Soviet suppression of the 1956 Hungarian uprising). The *eminences grises* of French academia would surely have reacted with scorn if he were to suggest, for example, that the French Revolution happened in part because of adverse climate shifts of the 1780s. Such conformism did not hurt Le Roy Ladurie's career: he became a member of the Collège de France in 1973 and was later director of the Bibliothèque Nationale. After reaching the pinnacles of French intellectual life, Le Roy Ladurie in effect recanted, publishing a three-volume work in which he claims a much larger role for climate change in shaping human events (Le Roy Ladurie 2004–2009).

Le Roy Ladurie's prominence – he was for a while among the most famous historians in the world and is still widely, and justly, admired – helped open the subject of climate change for other text-based historians to explore. So too did the example of archeologists who increasingly took climate change seriously – an important story I will not try to sketch.

Toynbee and Le Roy Ladurie stood as giants in the historical profession. Their books reached a wide public. Their specialized work, although not always their broader efforts, enjoyed the admiration of their peers. No one since has achieved such stature, certainly among those taking positions on the significance of climate in

history. Nonetheless, an adventurous minority of historians, when looking for more than rises and falls, found climate change almost everywhere. In the 1980s, Joe Miller (1988) used new information on the history of drought to help explain the waxing and waning of the slave trade in Angola. In drought years, vulnerable people had to sell their children or surrender themselves to the more fortunate, who in turn sold some of them to transatlantic slavers. In the 1990s, Wolfgang Behringer (1999) offered a bold new interpretation of the witch craze in western European history: the bad years of the Little Ice Age, especially 1560–1660, sharpened the persecution of women held to be witches in early modern Europe. They were accused of, among other crimes, arranging bad weather through their pacts with Satan. Climate fluctuations, thus, pertain to social history as well as to harvests and the various collapses of this or that dynasty.

As one might expect, the history of arid and semi-arid regions more often suggests a strong role for climate change. Where rainfall just barely allows agriculture, and people survive with little margin for misfortune as a result, small droughts could have big consequences – rather like modest reductions in average temperatures in Scandinavia. The Middle East offers a fine example. Recently, historians such as Richard Bulliet (2009) and Ronnie Ellenblum (2012) have offered climate-driven analyses of the economic and political history of the region in medieval times. The same centuries that brought warmer and moister weather to Europe, brought cold and drought to the Middle East. From 950–1200 or so, conditions frequently proved unfavorable for farming. The Nile more frequently carried too little water for Egyptian agriculture. Dry farming in Iran often failed altogether. States that depended on revenues from agriculture collapsed and opportunities for invaders, such as the Seljuk Turks from Central Asia, improved markedly. These historians, both essentially text-based scholars, but influenced by the findings of natural scientists, have opened exciting new vistas on the medieval Middle East.

For a later period of Middle East history, Sam White has done something similar (White 2011). Using a large amount of proxy evidence from historical climatology, as well as the familiar texts of historians, White has made a strong case for the relevance of drought

and cold – the Little Ice Age – to a series of revolts in the seventeenth-century Ottoman Empire. White was not the first to suggest a connection between the Little Ice age and the Celali rebellions, but he argues the case much more carefully and convincingly than his predecessors. None of these authors, Bulliet, Ellenblum, White, may be fairly accused of reducing Middle East history to climate. But all of them make climate changes one of the driving forces behind deep political and cultural changes in the region.

Chinese history has become an especially welcoming environment for arguments based on climate change. A telling indication is the recent work of Timothy Brook. Brook is a text-based historian of imperial China with formidable abilities in East Asian languages, well aware of rival explanations for dynastic cycles. Brook (2010) argues for secular climate shifts as a major factor in the decline and fall of several dynasties in the last millennium.

China historians may be more easily converted to the gospel of climate because Chinese texts often have detailed information about it, more so than the available texts from India and the Islamic world, for example. Gazetteers compiled more or less systematically since the Yuan dynasty (1271–1368) include weather observations, especially of strange anomalies. Indeed China historians and Chinese texts may be more predisposed than most to take climate seriously, because the concept of the Mandate of Heaven, important for two millennia in China, invests anomalous weather with political meaning. When floods, droughts or any meteorological mishaps seemed to come thick and fast, it was taken to mean the emperor and his lineage had lost the Mandate of Heaven, and thus the right to rule. Locust plagues, which apparently came more often on the heels of cold snaps, invited similar conclusions.

China's economy may also have proved more sensitive to climate shifts than economies elsewhere. Like other parts of Asia where irrigated rice was important, the quantity and timing of monsoon rains mattered deeply to Chinese harvests. Monsoons varied, partly in response to the giant Pacific climate oscillation known as El Niño or ENSO. Beyond this shared feature, the Chinese transportation system depended to an extraordinary degree on boat traffic on canals. The unique degree of marketization in China since the Song

Dynasty (960–1279), matched nowhere in the world until perhaps the Netherlands in the seventeenth century, rested on networks of canals. When these froze, transport was hobbled. When they froze for more months than usual, cold year after cold year, the ability of the market (or the state) to move grain to areas of shortage was correspondingly diminished. China was, in effect doubly sensitive to climate shocks: both in the production of food and in its delivery.

The same cold and dry spells that were apparently devastating in China probably helped their chief enemies, the steppe peoples and Mongols in particular. (Brook does not make these arguments: they are my extrapolations from discussions of Mongol warfare).

For at least a century, some scholars have supposed that irruptions of pastoral nomads of the East Asian steppe were driven by episodes of drought. I expect that, at least some of the time, this was true in part. But perhaps the extraordinary cold of the thirteenth to fourteenth centuries – now known in detail thanks to proxy evidence – made Mongol warfare easier? Frozen rivers and canals paralyzed transportation for China and inhibited it for most of the other peoples of Asia. But for Mongol war parties, frozen rivers and canals were highways. Given ice a few inches thick, they could move quickly and reliably through vegetated landscapes that in other conditions they would have had to try to burn to the ground in order to pass through.

Large patches of East Asia and Iran, and even larger ones of Russia and Eastern Europe, still carried forest cover in the thirteenth century. That forest provided barriers against equestrian forces – except when rivers or canals were frozen to sufficient depth for thousands of horses to ride along safely. The longer and colder the winter, the more mobile the Mongols, and the less mobile their enemies. Rivers usually led to cities, the prize targets. So to reduce this argument to its essence: the onset of the Little Ice Age cold raised the odds somewhat of Mongol military success, not on the steppe itself, but in China and especially in Eastern Europe. (Notice I do not say the Little Ice Age caused the Mongol success or permitted the Mongol success. It might have happened anyway – it was merely made more likely by the extraordinary cold.) This Mongol example is the only original one – at least I think it is original – in this paper, and

should be distrusted as a result. It is only a hypothesis, yet to be tested against the evidence, such as the seasons of the year in which the Mongols did most of their campaigning and conquering.⁴

The most ambitious examples of text-based historians taking climate evidence and putting it at the center of an analysis are now Geoffrey Parker (2013) and John Brooke (2014). In a sprawling work, Parker catalogues the rebellions, revolutions, and wars of the seventeenth century, from southern Africa to Japan and from Southeast Asia to the Andes, and in almost every case finds a strong dose of adverse climate change – typically drought or cold associated with the Little Ice Age – prominent among the causes. Parker is a major figure in the community of historians of early modern Europe, as Bulliet is among Middle East historians and Brook among China historians. His work commands attention. Almost every historian working in the early modern period will need to confront Parker's analyses, and wrestle with the significance of the Little Ice Age.

Brooke (2014), meanwhile, has attempted to put climate shifts and extreme climate events at the center of historical causation. His *magnum opus* takes the entire human career as its subject, and finds climate variables involved in almost every major historical episode until the end of the Little Ice Age. From about 1800 onwards, he argues, humankind has changed climate more than climate has affected us.

Dozens of ingenious and properly researched arguments about the significance of climate now exist in the historical literature. The situation is now far removed – and far better – from the facile formulae of Toynbee or the timid position of the early Le Roy Ladurie. Openness to the data coming from ice cores, pollen, tree rings, glaciers and so forth has made a gigantic difference in the strength and precision of arguments historians can now make.

Not only are the climate data better than ever, but the nuance and subtlety with which historians link climate to historical events has moved far beyond Toynbee. Besides the obvious and direct relevance of climate shifts to harvests, grain prices, famines, and social and political unrest – itself quite enough – historians have found climate shifts important in other ways, through other linkages. One is the changing populations of insect disease vectors, whether the

fleas of bubonic plague or the tsetse flies of trypanosomiasis – or my beloved mosquitoes. Vector abundance is crucial in determining the prevalence of diseases such as malaria, dengue fever, or yellow fever. Another linkage is the impact of climate upon natural fire regimes, consequential in Australia, Indonesia, and western North America among other places. Yet another is the effect of climate shifts upon the movements of animals such as deer and fish, from which some peoples have at times drawn goodly portions of their livelihoods. Almost all these linkages concern climate's impact on basic human concerns, such as food supply and health.

Some of the arguments, no doubt, are overdone. I confess skepticism especially about some of the claims made for the deeper past for which we now have some climate proxy evidence but very little of other sorts, a situation that leads us into temptation. While historians of recent centuries are as a rule probably too skeptical about climate's significance, because they have too much other evidence, historians (and archeologists) of the deeper past are perhaps too credulous because they have too little.

Some historians, upon discovering the new proxy evidence on climate, write with the zeal of the convert. This is all the more reason for *more* historians to wade into the discussion, to bring their awareness of context and rival explanations to bear. But to do it responsibly and well, they must come to grips with the new evidence pouring forth from the natural sciences, and to practice, in effect, the new superhistory.

Soon historians will also need to take proper account of the human impact upon climate change rather than merely changing climate's impact upon humans. Le Roy Ladurie (2004–2009, 3) has begun to do this. Other scholars, not historians but geoscientists, have posited strong human impacts upon climate from the early millennia of agriculture, and claimed the depopulation of the Americas after 1492 deepened the Little Ice Age (Ruddiman 2005). The idea here is that resurgent vegetation in the Americas took carbon out of the atmosphere, weakening the greenhouse effect, and cooling the planet. Historians have yet to grapple seriously with Ruddiman's contentions. It remains to be seen whether historians will see their craft differently in an age of anthropogenic

warming, as a prominent cultural historian, Dipesh Chakrabarty (2009), following in the path of Richard Foltz (2003) has recently urged that we should.

Conclusion

My conclusion takes the form of a parting question. Let us suppose historians do take climate seriously and continue to find more and more occasions, and more and more pathways, by which its oscillations affected human affairs. And suppose historians also find more occasions on which, and pathways by which, our affairs affected climate. At the beginning of this chapter I considered how the new kinds of evidence from the natural sciences might affect the choices historians make about geographical scale. But what about temporal scale? How might that evidence, and in particular, attention to climate change, affect the schemes of periodization that historians use and need?

We historians live with a cacophony of incompatible periodizations. In the Americas we typically have pre-Columbian, colonial, and national. Each of these is subdivided, and rather differently from place to place. Europeanists typically begin with ancient, medieval and modern, but subdivide those categories differently from place to place. Historians of India sometimes take these terms, but use them differently so that medieval India can last until 1857. China historians use dynasties. African history features the pre-colonial, colonial, and independence periods, which has the curious consequence that almost all African history falls in the first of these periods, which ends about 1885. George Brooks (1994) tried to create a periodization for West Africa based on the rhythms of wet and dry periods between the eleventh and seventeenth centuries, but his approach did not catch on.

The old habits are comfortable, and have with a bit of buffeting stood the tests of time. Feminist historians have raised questions – e.g. did women have a renaissance? (No, said Kelly (1977)). But they seem nonetheless to have accepted the basic frameworks. Other historians have asked whether India had an early modern period (Yes, said Richards (1997)). Global historians have struggled to find

a single scheme into which to wedge all the twists and turns of world history, but without much luck to date (see the effort in Bentley 1996). Will climate change ever seem powerful enough in human history to suggest its own scheme of periodization?

If not, will climate change rearrange our sense of continuities and discontinuities enough to make us question some, maybe most, of our periods? Will the end of antiquity come with the climate disasters of the 540s, felt throughout the world, rather than the sack of Rome in 476, which specialists now regard as merely a *coup de grâce*? Will the end of the Little Ice Age come to provide the distinction between early modern and modern? Will the onset of rapid warming create for historians a new period after 1950, which we now know variously as postwar, post-1945, postmodern, and after 1960, postcolonial?

Unlike geologists, we historians are at liberty to adjust our periodization as we please. We can label any period anything we like, without consulting anyone. Geologists must propose any new vocabulary of periodization to a series of committees and ruling bodies, a process that is now in train for the term “Anthropocene”, suggested to refer to the present period of the Earth’s history in which humankind has played a preponderant role in shaping environmental processes. Geologists will formally decide, by vote, in 2016 as to whether or not the Anthropocene exists. In the meantime, historians, thanks to our institutional anarchy, can steal a march and begin to write the history of the Anthropocene, a history of a new age, for a new age, an age of – among other things – rapid climate change.

We will do so with new evidence, evidence from ice, from tree rings, from our own DNA, from that of camels and viruses, and no doubt from sources as yet unimagined. That evidence at times will corroborate the information coming from textual sources, and sometimes will challenge it. This presents a familiar quandary to historians, who have always had to reconcile divergent textual accounts. But we will need to learn new skills to parse the validity not only of one text versus another, but of texts versus isotopes and alleles. This is a revolution in historical method which, like most revolutions, will involve mistakes, confusion, and wasted effort. But, like most revolutions, it should be exciting and revealing nonetheless.

Notes

- 1 In addition to early practitioners such as Rashid al-Din, Ibn Khaldun, or Sir Walter Raleigh, see the twentieth-century tradition of world and global history, excerpted and analyzed in works such as Manning 2003; Dunn 1999; Costello 1995; Bentley 2012.
- 2 Not Bernard Bailyn (1982), who wrote about “manifest history” and “latent history”.
- 3 Le Roy Ladurie was substantially influenced by the Swedish scholar Gustaf Utterström, especially Utterström 1955.
- 4 My remarks here were composed before the appearance of a fascinating and provocative paper by an interdisciplinary team that seeks to explain early Mongol success by reference to a few years of above-average rainfall in the Mongols’ homeland. This fifteen-year period, 1211–1225, is the only one over the past 1,112 years that shows above average rainfall in every year, so it amounts to a unique moment in the history of the Mongolian steppe. According to this argument more rainfall meant more grass, more sheep and ponies, and more Mongols, giving them an advantage in numbers (both of people and ponies) over their neighbors who did not enjoy the same good meteorological fortune. This argument, based on tree ring evidence from Mongolia, refers to the early Mongol expansion, not their subsequent conquests in China, Iran, and Russia for which (I claim) cold climate was helpful. See Pederson et al. 2014.

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On source criticism in world history

Jankeŋ Myrdal

As noted by several authors in this volume, the lack of discussion about source criticism in world history is a serious problem. It is, in fact, a threat to the credibility of this branch of history. When world history forms the basis for grand theory, with implications for how we deal with current problems in the world, source criticism will be of particular importance. We must develop methods of evaluating overarching surveys and syntheses. Preferably, they should be testable in a Popperian way: it should be possible to refute them (Popper 1965: 220, 232).

One way to write world history is as a general synthesis valid for a large part of the world and involving key elements of historical change: macrohistory. Such surveys are often constructed as narratives and combine condensed descriptions of processes with selected facts and extensively described examples. Indicators are combined to sustain a grand theory.

In testing such a general synthesis I will focus on a specific kind of indicator: the measurable. I include not only numbers in tables, but also presentations in maps and graphs. The reason I focus on this kind of indicator is that the results can be corroborated or, if necessary, rejected. Thus they form a kind of touchstone for grand theory in world history. If a grand theory is contradicted by important measurable indicators, this will affect the plausibility of the theory.

This may seem like a plea for quantitative methods, and to a certain extent, this is correct, but any such quantification requires a detailed qualitative analysis and description. Measurable indicators

also form only a minor part of all empirical evidence that is combined to corroborate grand theory, and critical methods must be developed to evaluate how other indicators are handled and combined.

Another way to construct world history is to combine examples, typically regional studies, into comparative history, which also can form the basis for grand theory. There is some, but not sufficient, methodological discussion about this kind of world history. Issues related to context, depth, and consistency have been raised (Adas 2012). Since including elaborate studies is a goal, multidisciplinary area studies become a base for such comparisons (Manning 2003: 86–91, 146–162, 221; Manning 2011: 112). A problem treated by Eva Myrdal in this volume is the different quality and intensity in research regarding the areas compared. Another important problem is that the examples often are presented in ways that are not comparable. This is treated by Arne Jarrick and Maria Wallenberg Bondeson in this volume.

A further major problem in comparative history, seldom discussed, is representativeness. If a comparison between cases forms the basis for a synthesis, a bias in the choice of examples may weaken the conclusions. Often it seems that cases are chosen from well-explored regions, or from regions the participants in the volume, or the project, themselves know well. The most common method of handling this problem is to provide a context. If, for instance, a region in the Philippines is chosen, then we also get an overview of such things as early colonial history, contacts with China, and so on. This implies that macrohistory is important also for comparative history – and certainly both these ways of writing world history are integrated and sometimes merging into each other.

Below I will treat some problems with measurable indicators for world history. I start with surveys based on subjective measurements, arguing that reporting the basis for measurement is always preferable. Then I move on to surveys based on out-dated measures. A third step is to accept that these indicators only show one aspect, and alternative measurements will make different aspects appear. Next I discuss how strict selection can create a better basis for interpretations, which also relates to the question of the categories utilized. The last part is devoted to a test of a grand theory, where I use the proposed methods. I focus on a part of the theory, or more specifically on a prerequisite for the theory.

My examples are from different investigations I have made, most of them instigated by a frustration I felt facing the lack of a methodological discussion in this branch of history. All the indicators I discuss are related to statehood and ideological-political processes, as it seemed to be an advantage if they were roughly in the same field and in some way related to each other. In a parallel project I, together with Mats Widgren and others, am doing a survey of the world history of agriculture, which forms a background to my work with source-critical problems. I do not touch upon that project in this text, but see Widgren's contribution to this volume. My purpose is only to discuss methodological questions, and if results concerning political world history are presented, they are a side effect. Before I start to discuss how source criticism can be developed, something must be said about the sources.

Sources

Literature is the main source that world historians work with, a fact that is sometimes mentioned in passing but seldom discussed at length (e.g. Conrad 2013: 89). To put it a bit drastically: other scholars should be treated as sources to be scrutinized in a source-critical way. I will refer to three layers: besides primary and secondary sources I also discuss tertiary literature for works mainly relying on secondary literature.

Primary sources. A researcher who works with large syntheses may obviously have been using primary source material to substantiate the discussion, but this research is usually published separately. Sometimes world historians work with editions of primary sources and then are dependent on translations and secondary literature – see Jarrick and Wallenberg Bondeson in this volume – and also the example about agricultural treatises below.

It can be an advantage if the author of a synthesis has researched primary source material in depth. The historian who has devoted years to archival research or the archaeologist who is experienced in fieldwork has an advantage in realizing how fragile factual evidence can be. Important syntheses can certainly be written by others than historians and archaeologists, such as sociologists, but those who have

not worked with primary source material sometimes have difficulty in understanding the need for source criticism.

Secondary sources are interpretations of primary sources, such as archaeological excavation reports or monographs based on a specific series in archives. Any scholar working with inclusive world history can only cover a small fraction of this literature, and strict selection must be made in using secondary literature.

In a discussion about methods for comparative history as a basis for synthesis, Theda Skocpol argues that it is imperative to make occasional “targeted primary investigations” referring back to the primary sources (Skocpol 1984: 382–383). An extensive reading of original research on specific topics in secondary literature is equivalent in a general synthesis to Skocpol’s principle of targeted investigations. In-depth studies can be made as a general control, but there are also specific reasons to make such controls. A scholar may find it necessary to read secondary literature when tertiary literature is not available for that region or period. Other situations forcing the scholar to be acquainted with secondary literature are when the tertiary literature presents results that are difficult to interpret or when alternatives rendered by different scholars are contradictory.

When scholarly literature is regarded as source material, the researcher is faced with a major concern, namely evaluating secondary source material. When evaluating one can be content with just identifying researchers to be trusted, as Chris Wickham once frankly stated in his grand synthesis about the early Middle Ages in Europe (Wickham 2007: 7). Sometimes one has to resort to this method, but it has risks. Skocpol has pointed out that there is danger in relying mainly on well-known scholars. Important texts may sometimes be found in the forgotten corners of research (Skocpol 1984: 382–384). In this volume Mats Widgren discusses source-critical problems in secondary literature.

Tertiary sources, then, are based on secondary sources and form the foundation for much of current world history. A fundamental issue to be addressed is that older texts are less useful because of the rapidly growing amount of research on primary sources. This problem does not apply to the same extent to secondary sources, as

a study of primary source materials may be relevant even if it was published many years ago.

Besides world history proper, several other types of tertiary sources can be used, such as national surveys, which tend to focus on political history but can also include economic history, cultural history, or more specific areas; standard works for larger regions about specific topics (e.g. Needham's series on science in China, Needham 1954–); specialized encyclopaedias (e.g. *Der Neue Pauly* for antiquity, Cancik & Schneider 1996–2003); historical atlases, and statistics.

To evaluate tertiary sources, scholars use a number of simple checks as a matter of course. One is to run through the dates of publication of works included in the references. If the latest publication in the list is old then the text as a whole may be less useful. Another control, which can be done mainly by those who have worked with primary sources, is to assess sections on specific topics that one is particularly familiar with. If there are too many errors the entire text may be unreliable.

It must also be recognized that the limits of critical world history are determined by the quality of the existing literature. When working with measurements, descriptions in historical sources or in scholarly literature often have to be transformed into something measurable, be it a map or a graph. In the discussion below I will try to reveal such weaknesses also in my own investigations.

Synchronoptic graphs

The first source-critical problem I will discuss concerns how to avoid subjective measurements.

Eurocentrism is often in focus when bias and source-critical problems are discussed (see several of the chapters in Bentley 2011 and in Northrop 2012). A popular kind of synthesis is *synchronoptic graphs*, which try to describe historical change at one glance, but they are seldom discussed or analyzed in scholarly world history.

Visualizing is an instrument used in world history. In texts about world system analysis, graphs or maps are often used to describe core and periphery or the economic systems covering large regions.

In the graphs I will discuss Eurocentrism is obvious, and therefore

they form a good example of what subjective measurements may entail. But the problem lies not only in this bias, but also in that these graphs cannot be tested because the basis for the measurements is not indicated. The same criticism affects many of the sketches made in world systems research. I do not suggest that such sketches should not be made, but they could be supplemented by graphs or maps where the underlying dataset is reported in detail. In this volume Rikard Warlenius discusses measurements that could be developed to sustain theories about world systems.

A synchronoptical graph typically describes the rise and demise of nations and empires. Each nation's history is converted into a flow, like a stream through history. The width depends on the influence awarded to this nation. The flow runs from top to bottom, sometimes from left to right. For example, the Roman Empire begins as a small stream, grows into a mighty river and then disappears completely. The aim is to give an immediate picture of world history as a whole. Such graphs have been mass-produced, and have influenced the popular view of world history. In all these graphs, what is measured is a subjective estimate of importance, and they show a clear Eurocentric bias.

The graphs are often quite beautiful, like works of art, and are also collectibles. Daniel Rosenberg and Anthony Grafton (2010) have provided a thorough description of their historiography. In the mid-eighteenth century the first real flowchart was made by the cartographer Thomas Jeffreys in 1753; among later examples the most famous were by Joseph Priestly in 1769 and Fredrik Strass in 1804. Flowcharts became popular in the United States. Sebastian Adams published a pictorial variant in 1871, still sold today as a curiosity; it starts with God creating the world.

The most common flowchart during the twentieth century was the *Histomap* from 1931. It was made by an amateur, John Sparks, and was on sale until the late twentieth century. According to relatives he began constructing the chart for his own amusement, to pass the time. The success when it was published came as a surprise to him (Rosenberg & Grafton 2010: 217–219).

Europe is dominant in the charts from the eighteenth century. Jeffrey in 1753 allots 50 per cent of the space to Europe, and Priestly in 1769 allots 57 per cent, with Asia awarded a surprisingly high

ON SOURCE CRITICISM IN WORLD HISTORY

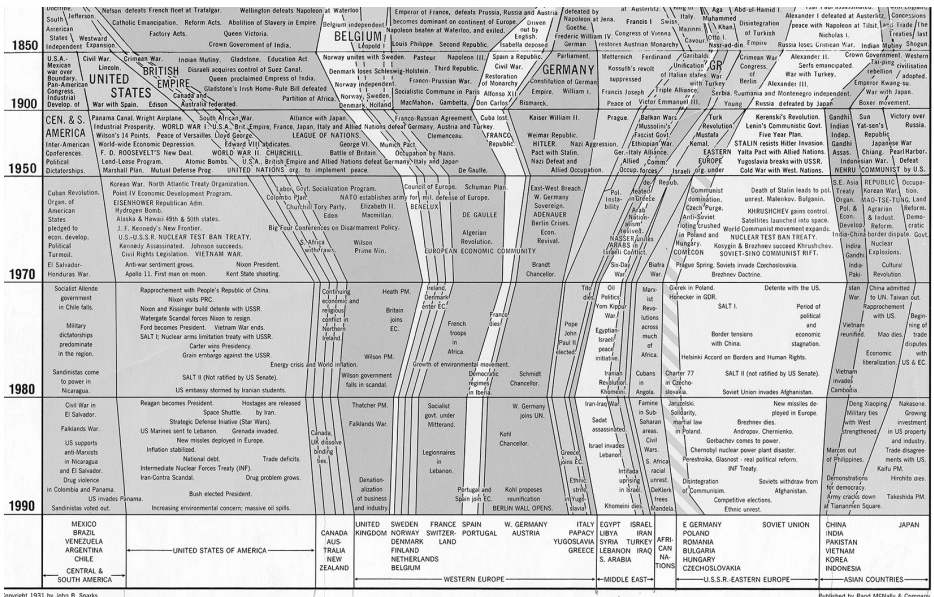


Figure 1. The final part of Spark's *Histomap* in the 1990 version, from c.1600 until today. The lower half of this section covers the twentieth century. The triangle to the left is the US. Immediately to the right of the US we find the British Empire, and two other large "streams" in the middle are France and Germany.

proportion, 37 per cent in Jeffrey and 27 per cent in Priestly. The German Stream from 1804 is slightly harder to estimate as the regions in the flowchart are of different sizes in different periods, but around 1800 Europe took up 75 per cent of the space.

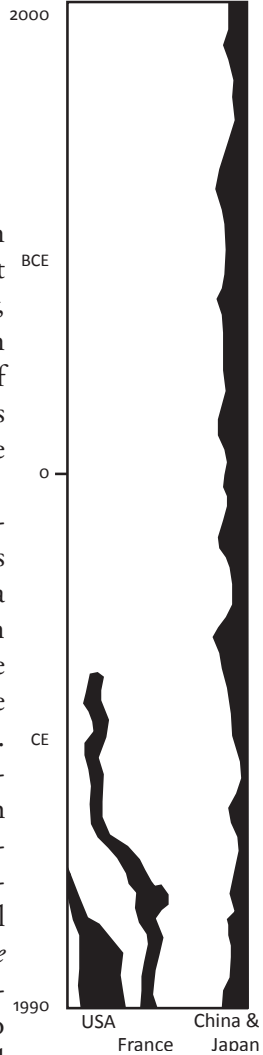
This distortion remains today. To exemplify this Figure 2a and 2b show how two modern graphs describe four countries: the United States, France, China and Japan. One is based on the very widespread American *Histomap* from 1931 (here used in its 1990 edition), the other is a French graph from 1991 (Fournet 1991). As expected, the American graph awards great significance to the US in the twentieth century, and both emphasize the importance of the West. Europe is awarded 41 per cent around 1900 in the American graph (in the 1931 version Europe was awarded as much as 48 per cent). In the French graph Europe covers 34 per cent. A graph that has replaced the *Histomap* as the most popular, the *World History Timeline* (2014, produced by

Figure 2a. A simplified sketch of the complete *Histomap* 1990. Countries marked from left to right: USA, France, China, Japan. This graph illustrates a relative proportion (i.e. where the whole is 100 per cent). Source: Sparks 1990.

Oxford Cartographers), has similarities with the French graph and gives Europe 32 per cent of the space and Asia 31 per cent. Interestingly, we find the same pattern in a Japanese graph from the 1990s (Yamasaki 1999). All parts of the world are placed within strict rectangles and Europe gets about 40 per cent of the space, East Asia about 25 per cent.

These graphs reveal a marked Eurocentrism: one reason they remain influential is that scholars seldom discuss them. In fact a challenge came from Arno Peters, best known for his world map, presented in 1973, where he used a projection of the world giving a more accurate and less Eurocentric representation. The map caused intense discussion (Kuchenbuch 2011; Oswalt 2015: 185–202). Though the map as such was not accepted by geographers, it contributed to a change in standard projections. The map actually was a sequel to Peters' earlier project, the *Synchronoptische Weltgeschichte* (Synchronoptic World History), first published in 1952. This was also based on his opinion that all civilizations had the same value, and his choice of presentation was guided by his interest in propaganda and outreach. Instead of a graph of states he had a timeline with people, political events and cultural achievements. His book was criticized by conservative historians (see Kuchenbuch 2011: 834–839). Today such timelines are rather common and Eurocentrism is less pronounced (e.g. Teeple 2002).

The synchronoptic graph, like all other means of presentation,



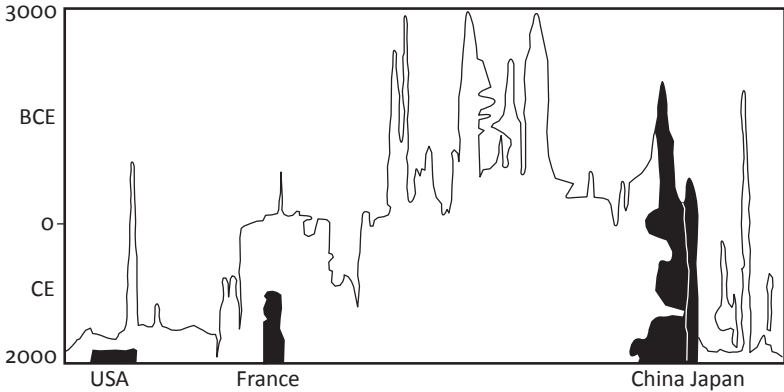


Figure 2b. A sketch of a French synchronoptical graph from around 1900. This graph illustrates how states occupy an absolute proportion of the world. The French graph starts in 3000 BCE, and every century is assigned the same space. Source: Fournet 1991.

can be used as an analytical tool. To leave this kind of presentation to popular and subjective graphs is a scholarly mistake and underestimation of how powerful they are in visualizing a conception of history. Did the US really have such an influence in the world in the twentieth century as the *Histomap* indicates? Such a claim has to be sustained in a way that makes it possible to reject. These graphs, with their question about the influence of nations, could have been made verifiable by relating them to measurable factors such as population or military strength.

I have chosen to show how such a graph can be used to analyze a particular phenomenon. When the first larger states in Eurasia were established nearly all of them grew along a line across Eurasia from the Mediterranean to northern China. In the synchronoptical graph the measurements establish that these larger states were situated approximately along a line at a latitude of 40 degrees N. Unlike more subjective measurements this can be tested and questioned.

The dataset is a survey of the historical atlases published in the decade around 2000 (see below and Figure 3). This is a tertiary source, and my graph cannot compensate for the weaknesses in this source. One such weakness could be that the earliest states, such as the Shang dynasty in China, should be marked differently to emphasize their different character.

The graph does not go beyond 500 CE. The Islamic caliphate and subsequent empires can no longer be portrayed as an approximate band across Eurasia. (Even in my graph states with an extension along the north–south axis, such as the Maurya Empire, will be underestimated.) The graph includes the larger early states, gradually concentrating on the empires, defined as entities united under one authority and eventually transformed into a mega-state.

Thus in the later part of the diagram states of similar size as Egypt in the earlier part are not included. Had all such states been included there would have been hardly any white space in the lower part of the diagram. It would have been difficult to read and even more difficult to construct. The main reason I have simplified the lower part of the graph so that only the largest states have been included is that a complete reporting would have added nothing essential to the interpretation about a rapid increase of larger states around 500–200 BCE.

The dotted line includes the area under the influence of these larger states from the middle of the first millennium BCE (concerning influence, see below). What is depicted is a partially irreversible process, and even if larger states were dissolved, such as in China where three states replaced the Han dynasty, the structure of the society had fundamentally changed. The graph illustrates how larger states and their influence became of major importance over most of Eurasia from the middle of the first millennium. This process started in the core area in West Asia. In the following centuries it spread, at first eastwards and then to the west. Around the beginning of our era Eurasia, in a broad band across the mega-continent, was organized under large states.

The interpretation of this graph is here mainly left aside, with questions remaining, such as how long-distance trade across Eurasia contributed to the growth of states in relation to intrinsic state-forming processes. The example is intended, rather, to show how this form of presentation would be used in a falsifiable way as a contribution to the analysis of a phenomenon in world history.

The graph is based on the newer generation of atlases discussed in the next section, and maps are of course approximations. What the graph gives is a basic outline and its exactness should not be overestimated.

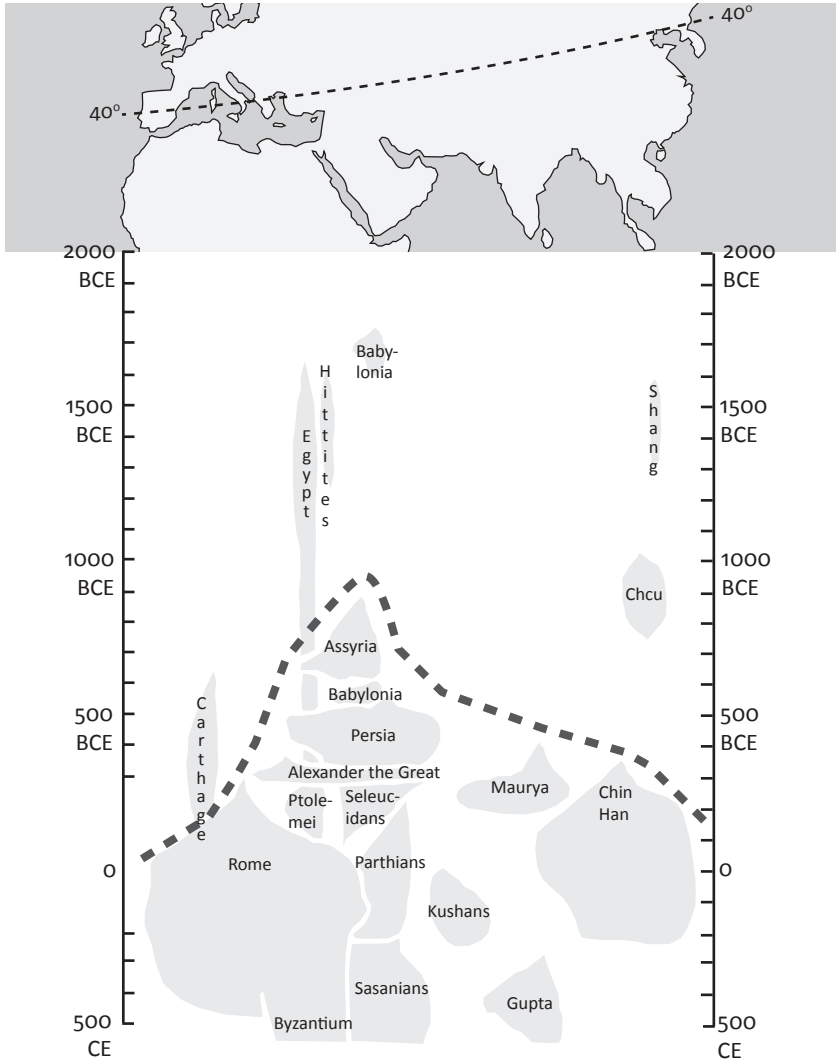


Figure 3. The major empires approximately along a line at a latitude of 40 degrees N across Eurasia until 500 CE.

Note: The X axis is the map above the diagram. Narrow protruding land areas have not been included, such as the Chinese corridor covering the trade routes into central Asia over the Taklimakan Desert. The dotted line marks the area of influence for larger states.

Source: Myrdal 2003.

Measuring empires and states

I now turn to the source-critical problem that appears when scholars use outdated datasets. It came as a surprise to me when I realized that many world historians used a dataset based on atlases from the 1960s and 1970s, although a shift in cartographical description of world history had occurred around 2000. Thus I started to measure empires and nations in the new atlases (Myrdal 2012).

Research on large states and empires is a classical field of world history, and it is most often written descriptively. A number of well-known empires are described and then conclusions are drawn. In addition, there is a long tradition of measuring the strength of empires with quantitative data. The possibility of finding a quantitative indicator is based on two related factors. First, rulers have been interested in marking the extent of their power with monuments or in documents. Thus the approximate area of states can be followed far back in history, for some early states back to around 3000 BCE. Second, historians have traditionally been interested in political history. When historical atlases were first put together, the extent of states was the wholly predominant phenomenon to be mapped. (A very simple definition was used: a state is a political entity that controls a territory.)

These factors seemed to make it possible to measure the area of states, and this has been done for more than a hundred years. In the late 1970s an Estonian scholar who had emigrated to the US, Rein Taagepera, decided to measure all the states in available atlases. His articles were, for the time, an impressive attempt. The most often-quoted graph was his diagram of the extent of the three largest states through history, that is to say, those that at a given time were the largest. He was able to prove their almost constant expansion, with some peaks on the way: 1) the Roman Empire/Han dynasty, 2) the Mongol Empire, and 3) the British Empire (Taagepera 1978, with the most-used graph and the summarizing article in Taagepera 1997). Taagepera bases his measurements on a number of historical atlases from the 1960s and around 1970.

First it must be said that Taagepera's graphs marked a major advance, and he also meets the Popperian requirement. These graphs can be tested and – partially – rejected, as there is a problem with them. The dataset has changed.

In the late 1970s a totally new generation of historical maps appeared. This shift occurred first in the United States, but was also accepted in other Western countries. *The Times atlas of world history* (Barraclough 1978) was the first in this new generation, with the first edition published in 1978. The idea was to give a fair description of other countries in the world apart from Europe and North America. Then other atlases followed, and around 2000 a number of new historical atlases were produced.

The historiography of historical atlases is described in two important books by Jeremy Black (Black 1997a; Black 1997b). In particular he highlights the shift in the 1960s and 1970s and onwards from Eurocentrism. He also notes examples of historical maps used for political purposes or to enhance the author's own nation. Two historical atlases that provide an exemplary scientific apparatus and detailed references are Schwartzberg (1978) and *Grosser historischer Weltatlas. Erläuterungen* 1–4 (1976–1996). In the latter there are essays about each map. In the former, besides long texts accompanying each map, there is also a thought-provoking review of ten different accounts of the Kush (Kushan) Empire's limits, and of nine accounts of the Mughal Empire. This is one of the best source-critical reviews I have read about measuring empires (Schwartzberg 1978: xxix–xxxv).

These new generations of atlases were not only less Eurocentric, they were also based on new research that tended to problematize the concept of state and state control (for a discussion of archaeology, see Yoffee 2005, for history Manning 2003: 190).

A different picture emerged for the first 3,000 years. Taagepera's measurements did not even come close to those found in the new atlases. The area under the firm control of early states was much smaller in the modern cartographer's view. Another important feature is that modern cartographers sometimes work with two units of measurement: direct control and influence. This is the result of a deeper understanding of the different nature of earlier states compared with later states. In a world with large tribal areas, states often did not have a border against other states, but against tribal areas where state control was not a strict border but a gradient. If the area under direct control had been vastly overestimated in earlier atlases, the area under

state influence often stretched beyond the area earlier considered to be under state control. We then get two curves, one much lower than the Taagepera curve and the other much higher. They approach each other in the last centuries before our current era (see Figure 4a–b).

In contemporary atlases there is growing awareness of this. In the atlas to *Der Neue Pauly* from 2007 (Wittke, Olshausen and Szydlak 2007) this principle of separating areas of control and influence is used consistently. Guy Halsall has made an interesting attempt with maps of the fall of the Western Roman Empire. With shades of white and black he shows the empire's gradual loss of control over the provinces during the fifth century (Halsall 2007: 235, 246, 275).

We also need to problematize later periods. One example is that for some large empires such as the Mongol Empire, we have to work with the two categories just mentioned. The same is valid for the British Empire in many of its colonies: firm control versus influence.

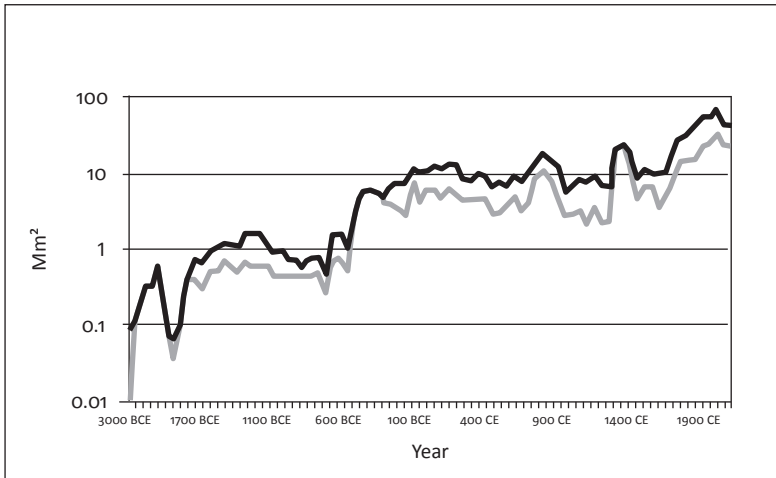


Figure 4a. Area of the largest empires—three largest empires. Taagepera's curve from 1978, complemented with values for the last decades and adjusted.

Note: the largest state (lower curve) and the three largest together (upper curve). Time is compressed for earlier periods on x axis; y axis logarithmic.

Source: Myrdal 2012.

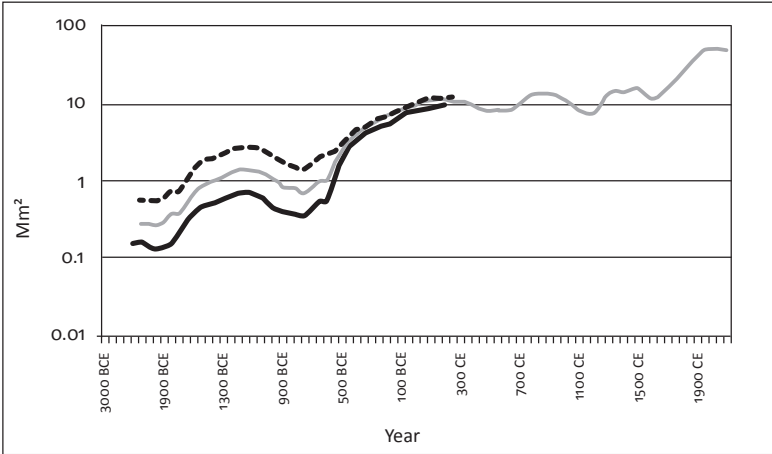
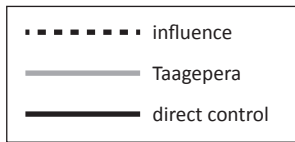


Figure 4b. Areas of control and under direct influence, curve based on atlases from around 2000.

Note: Lower curve under direct control, upper curve area of influence, middle curve Taagepera's estimate. Later part of the curve similar to Taagepera's curve, but must also be problematized (see text). Time is compressed for earlier periods on x axis; y axis logarithmic.



Source: Myrdal 2012.

The atlases today do not give any basis for such estimates. Perhaps future generations of historical atlases will incorporate such intricate measurements. Still, the two different patterns in Figure 4b will remain a result that one must relate to. In an earlier period we had a more gradual, floating structure with regard to states and state control.

An interesting conclusion might be that if we have different patterns in older periods a new pattern may emerge. If the whole world is divided up into states, as it is today, a number of alliances will be formed. Such alliances will develop into very firm structures and if in the future historians want to understand late twentieth-century politics, a map showing NATO and the countries of the Warsaw pact would probably be more instructive than a map showing different nations. One might describe this as a return, on a new and

higher level, to the phenomenon of core and influence zones (e.g. with the United States as the core and NATO as the influence zone).

Another conclusion based on these findings is that earlier graphs tended to underestimate the changes in the last centuries BCE. State power grew more than was previously thought, and a politically new landscape of increasing state control over nearly all of Eurasia followed.

Other measurements of states and empires

The next source-critical problem to be treated is that a single indicator yields a simplification. Thus one should strive to present several indicators of the same phenomenon. To phrase it differently: one can produce differing results by changing the conditions for the calculations. Two such different methods of computing are first, to look at the largest states' share of the population, and second, to look at all states rather than just the top three (Myrdal 2012).

Population is an important unit of measurement because it indicates a state's ability to organize a social structure. I calculated the two largest states' share of world population (see Figure 5). For an analysis of the development of human societies this is a more important measurement than the ability to control territory. Taagepera (1997) made such an attempt, but it is difficult to identify individual states. His calculations, which show the same tendencies I observe, are seldom referenced.

I have to make a small digression about population estimates. Like other historians, I use McEvedy and Jones as the basis for calculations, though their book was published nearly four decades ago (McEvedy & Jones 1978). The advantage of McEvedy and Jones is that they present calculations for every country in the world, with at least one page of discussion and often more about what the figures are based on. The reader also learns about the available sources.

Recent discussions about the history of world population often compare alternative estimates (see the website of the United States Census Bureau), but mainly refer to publications from the 1970s (except for the last 200 years where newer surveys exist). In an attempt to construct data for economic development over long

periods Angus Maddison made estimates, but they were based on a rather small number of older references to demographic literature (Maddison 2007: 230–240). I will try to identify the weakness in the data presented by McEvedy and Jones, which is seldom done by those who utilize these data.

In comparison with other estimates, McEvedy and Jones often give low figures, especially for older periods. To understand why, something must be said about McEvedy, who was the leading author. He produced a number of historical atlases, also widely used, as he had a talent for summarizing knowledge and presenting it in a condensed and easily understandable manner. McEvedy was a psychiatrist by profession, but his passion was history (Oles 2011). Late in his life he worked on a book about towns in the Roman Empire, using the same method as in his population book. For 120 cities he discusses a possible size, mentioning estimates by other scholars. His own estimates tend to be cautious. An example is Rome itself, where the figure he gives is far below other estimates (McEvedy 2011: 319–320).

We are in great need of a new compilation of the kind made by McEvedy and Jones. It would affect many branches of world history, for instance GNP, which Rodney Edvinsson discusses in one of the chapters in this volume.

The relative proportion, which I have used, would be a more reliable measurement than absolute numbers if McEvedy and Jones were consistent in their low estimates during a certain period. However, it seems that they underestimated Africa and America more than Europe and Asia (following their precautionary principle – less was known at that time about these parts of the world). Thus the relative proportion given below would tend to be higher than the actual proportion, as none of the states included were situated in Africa or America. This overestimation is not dramatic, as Asia always has had by far the largest share of the world population and was home to the most important empires. The uncertainty margins are nevertheless so large that I only included the largest empires (more than *c.* 15 per cent of the world population).

One cannot get further than the best available tertiary literature allows (without starting a project to replace it). At the same time, this calculation, with its weaknesses, allows us to reach a more rele-

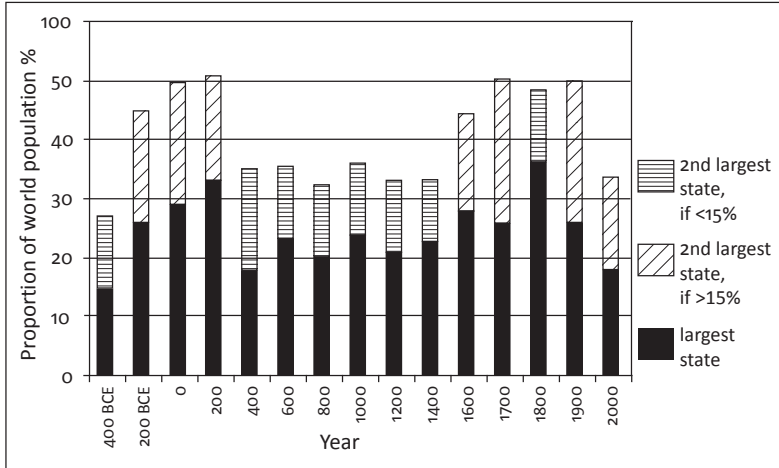


Figure 5. Relative populations of the two largest empires or states. The two largest states' approximate proportion of world population at the times indicated. Note: The second largest state is represented by a pattern of horizontal lines if it is less (<) than c.15 per cent; if more (>) it is marked with diagonal lines. The bars with horizontal lines, less (<) than 15%, are all represented as 15%, but actually most of them ought to be shorter as they reach lower percentages. The reason for not giving precise numbers below 15% is that the uncertainty of these figures increases for smaller empires. Source: Myrdal 2012.

vant conclusion than one based only on area. There is no tendency that a few empires increasingly dominate the world.

The result of the calculation was instead a long wave, with two crests: one during the period 200 BCE–200 CE and the other in the period c.1500–1900. The two largest states' share of the total world population was about 45–50 per cent during these peaks. In the intervening periods, it was about a third of the global population, which is also true today.

The first conclusion from this is the surprisingly large share of the world population that was organized under empires in the centuries around the beginning of our current era. In the Eurasian corridor along a latitude of 40 degrees N, not being subordinate to an empire was actually atypical. Then this pattern broke down, and the empire became just one form of state organization among others. There is no historical trend towards a greater concentration of population in major empires.

The other alternative estimate was the entire area under state control, and for this I have used two newer atlases. It turns out that there is a substantial increase in outright state control of the Earth's surface over the last 200 years, from half of the surface to the whole world. Colonial powers increased their share, which gave rise to subjugated nations' desire for liberation. What happened was that social organization increased, which was certainly not accomplished by the colonial powers alone, but was a result of social and economic change in general.

Table 1. Area controlled by states as a per centage of the total landmass, based on two atlases from the late 1990s.

	Haywood	Black
2500 BCE	1	1
2000 BCE	1	1
500 BCE	8	9
250 BCE	12	15
0	13	15
250		14
500	13	21
800	19	31
1000	20	30
1200		29
1300	32	40
1400		37
1500	23	38
1600	29	38
1700	49	55
1800	69	71
1850	74	82
1900	96	98

Source: Haywood 1997; Black 1999; cf. Myrdal 2012: 41.

To conclude this critical assessment of measuring states, three accompanying patterns appear. First, the three largest states controlled an increasingly large part of the landmass – today, however, the curve has turned downwards. Second, the two largest states' share of the total world population has remained fairly constant over the last 2,000 years, with fluctuations between one third and one half. Third, over the past two centuries the entire earth has come under state control. The concomitant increase of medium-sized and small states is the reason that the area belonging to the three largest states has decreased during the last decades.

Different pictures emerge, and all of them contribute to an overall interpretation of the long cycles in state-forming processes. The leap forward in the centuries between 500 and 100 BCE was dramatic, but just as dramatic was the inclusion of the whole world under state control in the period *c.* 700–1900.

These measurements of states and empires are based on what I have labelled tertiary sources, mainly atlases. In the future, when population estimates have been compiled and when atlases consistently work with core areas and areas of influence for earlier states, better data will become available. Another improvement would be atlases that also include such estimates for later periods, taking alliances and dominance into account. My investigations are thus only a part of the necessary process of methodological development in measuring states and empires.

A further question relates to the definition of a state. Most of these maps and calculations work with a simple definition: a state is a political entity (a “government”) which controls a territory. Indeed it could be claimed that a state in 2000 BCE is quite different than a state in 1500 CE. A refinement of the definition would probably result in several overlapping graphs for different periods. Here we are faced with limitations in the source material, historical atlases that are tertiary sources. The question about categories will be crucial in the next section about popular rebellions, where I have created the dataset by also utilizing secondary sources.

Categories and rebellions

I now turn to the essential question about working with strictly defined categories, which is a core issue of source criticism relating to measurable indicators.

Categories being compared must be similar. Lack of consistent and comparable units makes comparison futile. In comparative world history geographical units should preferably be of equal size. We cannot compare England with all of China. Instead, for example, the Yangtze delta and a European nation such as England give a more balanced comparison (Pomeranz 2000: 7). In general synthesis this is not a problem as all regions are included. Instead the definition of what to compare is crucial. Here I want to highlight that the goal is not to acquire as much evidence as possible, but to obtain comparable units. Sometimes a smaller number, carefully analyzed, is more revealing than a larger number of cases. My example will be popular rebellions in Europe from the High Middle Ages to the early modern period.

A large number of cases allow more complex calculations, and there is a lower limit to what can be considered as providing a basis for measurability. The advantage of a smaller number of cases is that a stricter selection can provide greater similarity between the cases. It is also a means to avoid assigning better explored regions greater weight in comparisons. Moreover, one can discuss the individual cases in detail.

That the cases may be discussed in detail establishes similarities to comparative world history. The crucial difference is that here the cases are selected through an all-inclusive survey. A common pattern in earlier scholarship has been to present some well-known popular rebellions, overestimating the significance of those in France and England, and then perhaps supplement the discussion with German cases from the end of the Middle Ages. To survey Europe in its entirety means that important cases in Eastern Europe and Scandinavia are included and discussed. Another part of such a strict selection of cases is that numerous rebellions that do not meet the criteria are excluded. In well researched areas too, some cases often mentioned in the scholarly literature have to be excluded.

States cannot be understood solely by investigating forces that held them together. It is obviously equally important to understand

the forces that tended to break them down and divide them. One of the occurrences that scholars have tried to count is the number of wars. Since major wars are mentioned in chronicles, the source material allows such calculations as soon as we have written sources.

Pitirim Sorokin was one of the first to attempt this, and since then there have been several compilations. William Eckhardt's calculation is one of the most ambitious (Eckhardt 1992). Similar calculations have formed the basis for other conclusions such as Steven Pinker's reasoning about the decreasing use of violence (Pinker 2012). I will argue that a deeper understanding of large-scale conflicts has been hampered by methodological shortcomings.

Eckhardt's figures are not based on a selection of cases with clear boundaries between different types of conflict. He does not provide descriptions of the individual conflicts. There are simply too many cases in the statistics, and the conclusions are doomed to be sweeping. With clear demarcation of a manageable number of cases, one can cross the boundary between the quantitative and the qualitative and achieve a descriptive catalogue. Sorokin is in fact easier to work with, as he has a catalogue with a description of all conflicts. He also separates wars from civil wars, though in the latter category he mixes strife within the upper class with popular rebellions, which makes his results blurred (Sorokin 1937–1941, 3: Appendix to part three, 579–620).

I will focus on one type of conflict: popular uprisings. There are several compilations of revolts, all suffering from the problem of mixing large and small. Hugues Neveux (1997) published an ambitious catalogue of more than 150 revolts in Europe during the period 1300–1675. In his mixture, pillaging a monastery is one of the smallest conflicts and nationwide rebellions like the German Peasants' War are among the largest. His catalogue provides information, but it is difficult to use as an analytical instrument.

Another example comes from Geoffrey Parker's book from 2013 about the crisis in the seventeenth century. He argues that political unrest around 1635–1666 has to be understood in a worldwide perspective, partly as a result of economic problems caused by climate change. As a starting point, he enumerates 49 "major revolts and revolutions" of which 22 led to regime change (Parker 2013: xviii–xix, map and list). The list contains rather disparate political revolts. The

Danish Revolution in 1660 was a coup carried out by the King to gain more power. The Pequot War was an armed conflict between American Indians and English settlers. The rebellion in China did not last a single year (as one might believe from the table), but went on from c.1636–1646 and was one of bloodiest popular rebellions ever, with millions of casualties. The Goa rebellion against Portugal was a religious movement, when the Christian community decided that they would not submit to Portuguese dominance. And so on.

Of course Parker is aware of this mixture. In another and much earlier publication he has made a distinction between areas affected by war, areas affected by popular revolt and areas affected by political rebellion (Parker 1978: 5, map). One must take into account that his intention in the 2013 book is to paint a picture of general unrest, and he describes many of the conflicts in detail. Nevertheless, we cannot be sure that he is correct without a delimitation of cases and comparison with other periods using the same criteria.

A set of criteria must be established when conflicts of a similar kind are identified. To separate popular rebellions from other civil wars, one criterion must be that broad strata of the population take part and that they have some influence over the rebellion. A key indicator to estimate the influence of the common people is to look at the demands made during the uprising. Another indicator is to examine the composition of the army, but this indicator is weaker because peasants could be involved in conflicts between leading groups in the society, being recruited as soldiers. Another criterion to identify the major revolts is to examine certain kinds of violence. This is a relevant criterion for the period I have chosen, since armed rebellion was the option available for common people to put pressure behind their demands. In a time series stretching into later periods, and especially into contemporary times, that would not be a relevant criterion as it would exclude, for example, large strikes.

Visibility in the secondary and tertiary sources is essential and thus smaller conflicts have to be excluded. The assumption is that large rebellions are always mentioned in national surveys of political history. Smaller conflicts can be enumerated for particular regions if a scholar has endeavoured to go through all the documents and look for them. (I have done that for medieval Sweden, and know that it

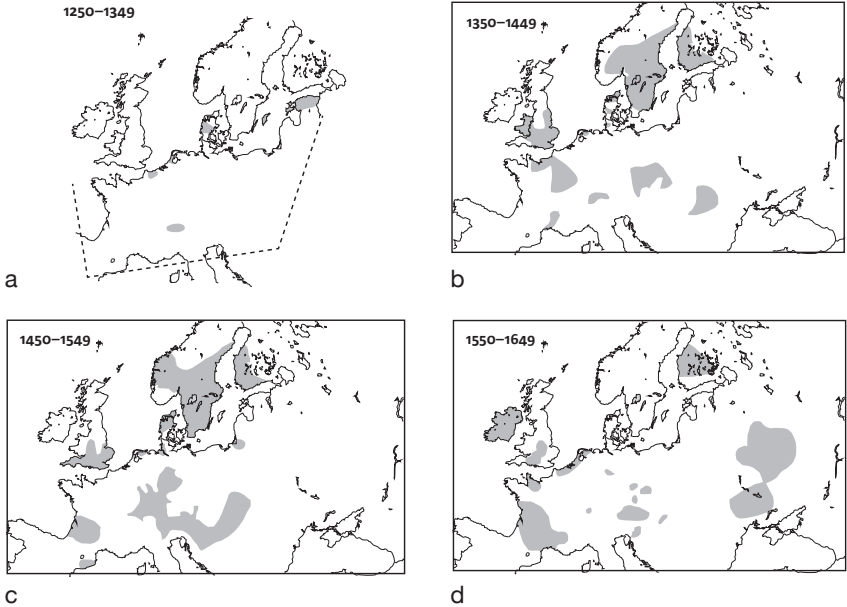


Figure 6a–d. Large popular rural rebellions 1250–1650 in Europe.
Source: Myrdal 1995; Myrdal 1999.

is extremely cumbersome work). Such surveys cannot be included in an interregional comparison, since this would result in an overrepresentation of regions where scholars have carried out such surveys.

To present the goal for the rebellions is important for an analysis. A religious movement is not the same as a nationalistic movement or a rebellion among peasants against oppression by the king or local lords. Certainly goals are mixed, but identifying them facilitates discussion of long-term trends. This also implies a descriptive text about every case, which works against including too many cases.

For Europe I have put together such a catalogue of large popular rebellions from the eleventh to the seventeenth centuries, using the aforementioned criteria (Myrdal 1995). I also added the restriction that the rebellions should be rurally based, thus excluding some urban revolutions. The source materials, besides a number of books specifically about rebellions, consisted of national histories published in languages that I could read. In the catalogue, every conflict was described, which is the same method McEvedy and Jones used for population.

Conflicts, as I defined them, did not exist until a state apparatus was established. Between 1100 and 1250 a number of conflicts were related to the spread of feudalism to the European periphery. In Scotland, Scandinavia and the Baltic states the conflicts had elements of popular rebellion but also resembled conflicts among the upper classes and even conflicts between regions. It was not until around 1250 that a kind of popular rebellion comparable to later equivalents appeared over large areas of Europe.

The catalogue detailed about 80 large popular and rural rebellions that took place during the period 1250–1650. After an increase from *c.* 1250–1350, the number of large rural rebellions was fairly constant, about 20–30 per century until the early seventeenth century. When they were mapped, patterns emerged more clearly. The increase from the mid-fourteenth century is clearly evident on the series of maps.

Another interesting fact is that large rebellions in the late medieval period occurred in the economic and political core areas of Europe, whereas at the beginning of the early modern period there were instead more large rebellions in the periphery. (Figure 6 a–d. For the earliest period, Russia on the eastern periphery is excluded due to lack of sources.) The core areas were also those where a strong state was established, partly as a reaction to large-scale popular rebellions.

Feudal rent was in focus directly after the Black Death, but then state taxation and control over the administration of the nation became more important (Myrdal 1995; Myrdal 1997). A sequence of rebellions with increasing demands from below for influence during the late Medieval period came to a halt with the establishment of strong states in the core of Europe during the sixteenth century. The strong state was the main deterrent to large popular rebellions in these parts of Europe.

If this study were to be extended in time and space to include the whole world, criteria would obviously have to change to cover greater diversity. Another, more feasible method might be to establish a complementary time series, such as before and after the establishment of the state. This prospect offers very interesting work on methodology. Instead of one long time series of rebellions (or wars, or conflict of

other kinds), we have to identify several series, overlapping each other and illuminating different aspects. Such a methodological approach is probably also valid for several other phenomena where we want to create a basis for long and worldwide time series.

Critical test of a theory – the axial age

In the following two sections I apply the source-critical requirements I have introduced in the preceding sections. I focus on an easily measurable aspect of the theory, starting with indicators that include a number of cases, and then turn to a more strict selection of cases.

Theories are a blessing. Without them we could not reflect on history. They do, however, have to be tested. A provocative theory will often instigate research, but sometimes theories are not tested using stringent methods and verified data. One argument might be that such grand theories are impossible to test. That may be correct if we look at the theoretical structure as a whole, but elements of it can always be tested, and if it is proved that several elements are incorrect, the rest of the theory is in jeopardy.

The example will be the theory concerning the axial age. A real test would require a much longer text, and I will only hint at possible ways to test parts of the theory. The relationship to previous examples is that states are not held together solely by military power; even more important is legitimacy. A loyal bureaucracy and an ideology that persuades individuals and groups to accept and even support the state are necessities. Large-scale changes of socio-political structures normally go hand in hand with ideological shifts. The theory of an axial age, however, goes far beyond ideologies for governing a state.

Before I delve into the theory itself it must be admitted that it does not belong to mainstream world history. In two extensive overviews covering more than 700 pages each, the axial age is mentioned just once (Northrop 2012, in connection with Eurocentrism) and twice (Bentley 2011). On the other hand, its adherents are a confident group, and lately their ideas have reached a wider audience through the eloquent writer Karen Armstrong (who is not a member of this scholarly coterie).

To summarize the theory: nearly 70 years ago the philosopher

Karl Jaspers noticed that between the eighth and second centuries BCE, with a focus around 500 BCE, a number of large-scale intellectual systems were created, both religious and philosophical. In this intellectual leap forward, people in different parts of Eurasia started to consider intellectual phenomena such as contradictions and especially transcendence. The term “axial” refers partly to the axis through Eurasia where these ideologies took form, from China to Europe, but the core of the metaphor is that these new ideas constitute an axis for all intellectual history (for an interesting visual presentation of the theory see Holenstein 2004: 50–51.)

This has been developed by several generations of scholars. Among the leading ones is S.N. Eisenstadt, who emphasized the tension between the transcendental and the mundane as a result of the emergence of an intellectual elite. With the formation of a more complicated society, social protest, solidarity and the social division of labour became important issues (Eisenstadt 1986: 11). Utopian ideas attracted followers imagining another world order.

Björn Wittrock has proposed a definition. He sees the axial age as the emergence of an institutionalized critical reflexivity together with historical awareness about change and the possibility of change. Arenas for intellectual discussion were established. Wittrock also emphasizes that different but parallel paths were followed, which gave the new way of conceptualizing the world a partly different content in various large regions such as Europe, West Asia, South Asia and China. This follows from what he labels “cultural crystallization”, when structures for creating order and teaching knowledge were established (Wittrock 2005).

A partly different approach has been suggested by Karen Armstrong. Her interpretations can fairly easily be combined with the group of ideas discussed above. She takes her starting point in the “golden rule” (one should treat others as one would like others to treat oneself), and aims to describe how this basic moral approach developed during the axial age. Jesus was only one of the later prophets who advocated this, building on a long tradition (Armstrong 2007).

There are also opponents. Jan Assmann, for one, has argued against the idea of one crucial moment with a common outcome, asserting that we must identify changes before and after this period (Assmann 2012).

Scholars working with the axial age today do not, in fact, focus solely on the first great leap forward, but also on other fundamental and structural intellectual leaps. The period around 1000–1300 in Eurasia and the European Enlightenment around 1600–1800 have been regarded as such epochs of intellectual change (for examples, see chapters in Arnason & Wittrock 2011).

Neither supporters nor opponents have exposed the theory to stringent tests with quantitative data. Here one aspect is chosen that tests the underlying prerequisites for the theory rather than the theory as such. My question is: are there periods – long or short – when the development of ideas and ideologies is more rapid than during other periods? I will avoid the question of how rapid a change must be to justify the term “revolution”, a discussion that seems pointless to me. If such periods can be identified it makes the idea of an axial age more probable. The next step would be to analyze their significance, which would demand a selection of texts to be closely examined.

Attempts to identify periods of intellectual change with quantitative measurements have been made, though not directly in connection with this debate. An example is Jan Luiten van Zanden. His question was how a society based on knowledge developed. A main indicator is the quantity of manuscripts and published books in Europe for the last 1,500 years. A decline occurred around the middle of the first millennium an increase began in the eighth and ninth centuries. After a short stagnation this continued to a peak from the eleventh to the thirteenth century. Then a new rapid increase started with mass-production of printed books from the end of the Middle Ages, and the increase continued into the sixteenth century. The next increase, which was a leap, came from the mid-eighteenth century and onwards (van Zanden 2009).

Another grandiose attempt was made by Pitirim Sorokin. One of his goals was to identify a large intellectual cycle where the focus went from more materialistic to idealistic and the reverse. He registered thousands of philosophers, whom he labelled “thinkers”, according to certain main schools of thought and ideas. Here I am only interested in the quantity of “thinkers” he registered. He graded them according to influence and presented them in appendices, using the ninth edition of the *Encyclopædia Britannica*, although he was aware that this

overestimates Anglo-Saxon thinkers (Sorokin 1937–41, 2: 143, 152), and also two American, one British and two German philosophical lexicons published in the 1920s (Sorokin 1937–41, 2: 635). His accounts apply only to the West. In his tables and diagrams we see a long period of high numbers from the fifth century BCE to the fifth century CE, then a shorter peak in the eleventh and twelfth centuries, and from the seventeenth century an ongoing upward trend (Sorokin 1937–41, 2: 29–31, 185–189 and 4: 353; Sorokin 1957: 288–289).

An interesting fact is that Sorokin and Jaspers were contemporaries (though without referring to each other). Jaspers was less Eurocentric, but did not substantiate his theory, as Sorokin attempted to do.

A recent and corresponding publication gives another basis for calculations. Randall Collins has registered nearly 3,000 philosophers during the last 3,000 years and the intellectual connections among them (Collins 1998). His goal is to identify the flow of ideas, and he gives all his data in diagrams and appendices. The sources are modern books about the history of philosophy, published according to different regions. He emphasizes the value of measuring philosophers' influence long after they were active (Collins 1998: 58–61).

For China he uses nine different publications, all of them published from the 1950s to the 1980s, seven in the US and Britain, two in Singapore and China. As a source of additional information, he refers to eighteen other books published during the same period. For Greece in antiquity he uses a mixture of texts from that period and books from the twentieth century. Besides three historical sources he draws on eleven books from 1915–1990 as the main source, and twelve additional scholarly books from the same period as supplementary sources (Collins 1998: 950). He also points out that the numbers for China and Greece cannot be directly compared. He has similar sources for other regions and periods such as India and early modern Europe.

Collins is a typical example of how a tertiary source is limited by the quality of secondary sources (which cannot be examined here). Though he strives to give the same weight to every part of the world that produced philosophers, he is limited by having to rely on texts written in the languages he has mastered, which in practice means texts from Anglo-Saxon countries.

Collins establishes a hierarchy of philosophers, distinguishing

between major, secondary and minor figures. His categorizations are based on the space they are given in the books used as sources. Collins focuses on the major figures.

If crucial periods exist, the number of philosophers remembered today should be larger during those periods, assuming the thinkers made valuable contributions. Collins argues that minor philosophers often lack “originality and depth”: they are followers, though in their own time they may have been considered major figures (Collins 1998: 62–63). To him only major and secondary philosophers produced real change in the world of ideas.

Collins discusses certain periods as hotspots. One such is Greece between 500 and 265 BCE with fourteen major and thirty-one secondary philosophers. Another is China from 365–235 BCE with five major and nine secondary philosophers (Collins 1998: 57–59). These periods stand out distinctly from other periods: the figures seem to be in line with the notion of an axial age, though it occurs later than Jaspers proposed.

Collins does not expend much effort on discussing all the hundreds of “minor” philosophers. As a further test I have counted all the names in his tables and diagrams for China between 500 BCE and 700 CE. From this it seems that the number of philosophers mentioned in modern works is fairly constant, but with a dip around the beginning of our current era (see Table 2). A calculation for Greek and Roman philosophers shows a similar pattern, but with an expected dip in late antiquity.

The explanation for a constant number of minor philosophers could be the institutionalization of an earlier breakthrough. The minor philosophers did not formulate new ideas, but they transmitted accepted ideas in an established intellectual structure.

The decrease in Europe in late antiquity is easy to explain. The decrease in China around the beginning of our era would require a longer discussion. Here I only point out that during this period we see one of the most dramatic intellectual upheavals in China. Under emperor Wang Mang (9–23), an usurper who did not belong to the royal family, a number of reforms were tested. Needham has mentioned that historians of science have a weakness for Wang Mang because he was interested in science and called together the

first assembly of experts. After he was overthrown all his reforms were rescinded (Needham 1954: 109–110). This could be significant for the later recognition of minor philosophers from this period.

The methodological conclusion is that a qualitative analysis is necessary. The numbers in the table give a rough indicator of a long trend of a rather constant number of minor philosophers remembered in much later surveys. A further refinement of these data would be to make a more strict selection, perhaps using the methods suggested by Sorokin: selecting them according to the main content in their preserved publications.

Table 2. All philosophers, mainly “minor”, in China mentioned in Collins 1998.

500–400 BCE	20
400–300 BCE	17
300–200 BCE	27
200–100 BCE	28
100–1 BCE	9
1–100	5
100–200	13
200–300	25
300–400	27
400–500	24
500–600	28
600–700	25
700–800	15

Note: As some philosophers were active over the turn of a century, I have assessed which century they belong to.

Source: Collins 1998. In his catalogues for China he lists 25 major philosophers, 61 secondary and 356 minor from 535 BCE to 1565 CE. For Greece he has 28 major philosophers, 68 secondary and 237 minor from 600 BCE to 600 CE. He has corresponding figures for other parts of Eurasia, such as India. In all, more than 2600 persons are mentioned (Collins 1998: 77).

The number of philosophers only provides a framework for an interpretation. Data from Sorokin and Collins indicate that during a long period, mainly after what is normally considered the peak

period of axial change, a relatively large number of thinkers/philosophers were active.

An in-depth analysis would require working with a restricted number of texts to see if reflexivity and historical awareness become more prevalent, preferably also using quantitative methods. A very important factor is then the selection of texts. In the next section I will look closely at one category of texts.

The axial age exemplified by agricultural treatises

The emergence of natural science, which started to replace natural philosophy and religious thought, is a part of the Axial breakthrough (Wittrock 2005: 53). Texts on agriculture are the first extant genre about science. I have made a survey of all texts before 1500 (Myrdal 2014). Here I will address their number.

In my investigation of these texts I also discuss their content, but then in terms of a question not directly related to the theory of an axial age. As they are a main source for agricultural history, my question was how closely they relate to actual agricultural praxis. The answer was that to a large extent they describe a reality, though with some bias towards larger estates.

To find comparable units by strict delimitation is essential. Only original works that had been preserved were included, since they were the only ones that could be checked. Early translations are certainly an indicator of interest in these issues, but are often harder to date. Only longer treatises were included (with a lower limit of 10,000 words/Chinese characters). This was based on the assumption that longer texts were mentioned in secondary and tertiary sources, which had to be the basis for my compilation.

If shorter texts had been included there would have been a bias favouring regions that have elicited more extensive research about agricultural history. One of the best-known medieval agricultural treatises was written by Walter Henley in England in the thirteenth century. It contains fewer than 10,000 words, and its influence was restricted to England, compared with, for instance, the Italian Crescentiis, who in 1315 wrote a long text that continued to be copied and referred to until the end of the Middle Ages; it was even

translated into French. One reason Henley's text is mentioned so frequently is that in modern agrarian history regarding the Middle Ages England is predominant and often serves as a role model for research on other countries. In Table 3 Henley is not included.

After 1500 the number of agricultural treatises increased worldwide. In Europe, explosive growth followed, especially of printed texts, and a further leap came in the eighteenth century.

Table 3. Number of extant agricultural treatises by century.

	Rome- Byzantium	China (and East Asia)	The Islamic World	Europe	India
200–100 BCE	1				
100–1 BCE	2				
1–100	2				
100–200					
200–300					
300–400	1				
400–500	1*				
500–600	2*	1			
600–700	1	1			
700–800		2			
800–900		1			1*
900–1000	1*				
1000–1100		7	3		1*
1100–1200		5	2		
1200–1300		6	2		
1300–1400		4	5+	2	
1400–1500		2#	1	2	

Note: During antiquity, Italy is included in “Rome-Byzantium” and during the Middle Ages in “Europe”. Post-eleventh century, Byzantium essentially does not exist as a separate cultural sphere, although it did exist as a country for a few more centuries.

* Datings are uncertain for the late classical-Byzantine period and for the Indian texts. + Including one Persian work. # Both from Korea.

Source: Myrdal 2014.

In the case of China, it seems that texts from the Han dynasty (c.200 BCE–200 CE) may be as many and as long as those from the Roman Empire, although the Chinese texts have not been preserved. Some of the oldest ones preserved were of an impressive length: e.g. Columella 210,000 words (60–65 CE) and Palladius 80,000 words (c.400–425). A second increase came during the period 1000–1300. The longest European text, with 220,000 words, was by Crescentiis in 1315, and the longest Chinese text, with 110,000 characters, was by Wang Chen, c.1315 (in older Chinese a character normally corresponds to a word). Both were surpassed by the longest agricultural treatise in the medieval world, by Ibn al-Awwam, with 500,000 words, written in Spain in the twelfth century and based on the scholarship of the Islamic countries.

These texts represent an accumulation of knowledge and an interest in such topics among intellectual groups, essentially the upper class. Several of the texts, but not all, are related to periods of agricultural expansion. In periods of threats to agriculture, texts were written to preserve knowledge built up under preceding periods, such as the text by Wang Chen written under the reign of the Mongols (Yuan dynasty).

Agricultural treatises are just one category of texts with detailed descriptions of natural phenomena, but they indicate that the breakthrough for this intellectual endeavour came after the axial age proper. They belong to the empire-building periods in Rome and China, when a large upper class and a substantial group of bureaucrats could form the basis for such an interest. A second crucial period came around 1000–1300, and a third leap in the sixteenth century. Without a strict limitation the importance of Europe would have been overestimated.

Summarizing my attempt to test the hypothesis about crucial periods of intellectual change, the evidence indicates that they exist. It has yet to be established, however, that there was a specific Axial period around 500. Rather, the data presented here point to the period after the formation of the first larger states in Eurasia as more crucial: this was a period of rapid change in economies and political structures. A second Eurasia-wide intellectual change seems to have occurred in the centuries around and after 1000 CE, again a period of economic and social restructuring. An alternative theory

would then be that large intellectual leaps tend to be coordinated with periods of general changes in the society.

A basic idea in the Axial theory is that certain ideas are launched during one period of time. An alternative hypothesis is that such ideas are formed more stepwise during several periods. One also has to consider the possibility of setbacks and then a reformulation of the core ideas.

I have not tried to quantify core ideas such as reflexivity – but I claim that it would be possible to make such quantification in a strictly selected number of texts spread out over a long period.

A major objection is that preserved written sources form a restriction for testing the theory. Other indicators have to be utilized. For instance, art and buildings could be such sources. The erection of large buildings for religious purposes in all of Eurasia during the centuries around 1000 CE is a further indicator of a crucial change of belief systems.

My findings challenge the idea of a changed mentality around 500, pointing to a later period and different context for the major change. Indeed, an empirical investigation with measurable estimations of the core ideas in a restricted number of texts still needs to be done.

Conclusions

I have mainly considered inclusive syntheses for world history and have focused on graphs and tables that can support general syntheses and grand theories. Below are my suggestions for improving the standard of these.

- 1) The first step is to define a measurement. It is certainly justifiable to illustrate a theory or hypothesis with a sketch. The synchronoptical graphs widely distributed today show a Eurocentric bias. Future synchronoptical graphs may have a less pronounced Eurocentric bias, but if the measurement remains subjective they cannot be used as analytical tools.

This critique also applies to several of the maps and graphs used in world system theory. They are presumably not as biased as the graphs I analyzed – but it would be preferable to find indicators that could be a basis for showing contacts between regions.

- 2) The next step is to try to use the latest research findings. Tertiary sources will always be lagging behind the secondary literature – and both change. Data based on tertiary sources, such as historical atlases, must therefore be renewed constantly.

The arc of the largest states is just one example, and one of the most desirable new databases would be population statistics. Data compiled and discussed at length, country by country, based on the newest demographical research, would produce a new reference work for world history.

- 3) It is inherent in quantitative synthesis, maps and graphs that they only show one aspect, and thus scholars must seek several indicators that will show different aspects of the course of events.

In such a pluralistic approach different disciplines could work together. For instance, archaeology and history could together reconstruct a history of world trade over millennia by using several indicators. This would also reveal different aspects of how long-distance trade has developed.

- 4) To collect as much evidence as possible is tempting and has advantages, but it could lead to blurred results, which are difficult to interpret. A strict limitation leading to a smaller number of cases will often allow a closer analysis.

Such a restriction can be combined with detailed descriptions of every case, and in this respect it has similarities with comparative world history. However, the difference is that the limited number of cases in a global survey still requires a complete and inclusive search for such cases.

- 5) Even grand theories can be tested by selecting phenomena related to the predictions of the theory. My attempt to test the theory of the axial age seems to indicate that the process was more complex and diverse than what was anticipated – but also that elements of the theory, about periods of more rapid change, probably are correct.

Accumulating data to support or refute a theory is not the only way to test it. Instead it is better to have fewer criteria that are better controlled and discussed. Two doubtful graphs or diagrams do not give more support than one that is carefully selected and scrutinized.

Finally, I want to return to what will always be a main method in world history, narrative description. I have not argued for replacing this, only for developing methods so that this kind of world history could be better controlled, refuted or substantiated. To end with a quote from Popper, just as I began this chapter, I concur with his claim that if a theory “passes certain tests it will be better than some other theory” (Popper 1965: 217).

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Four myths in global agrarian history

Mats Widgren

It is in the nature of global history to have to rely on secondary sources. An important role among those secondary sources is played by existing syntheses addressing individual countries or regions (cf. the discussion in Myrdal 2009 and in this volume). The author of global syntheses must therefore navigate through mountains of secondary literature and be able to separate the wheat from the chaff, or in this case, substantial, empirically based conclusions from shortcuts and sweeping generalizations lacking substance.

This chapter is based on observations made while reading secondary literature for the project *Mapping global agricultural history* (Widgren 2010b). The project aims to summarize the existing evidence for global agrarian systems in the last millennium. A central point has been to present current knowledge of global agrarian history using maps, in a format that can be easily compared with other datasets. In my reading of different types of secondary material it has struck me how often authors rely on commonly accepted assumptions rather than on empirically based generalizations. In this chapter, I will focus on four such assumptions that I claim take the form of myths. They especially creep into scholarly literature in regional and global syntheses, where scholars, for lack of empirical research, need them to help paint a broad picture. Often they are myths that once served particular interests. Although they may have been refuted a long time ago in the specialist literature, they are exceptionally resilient. They tend to resurface in new syntheses and in new guises, time and time again.

Previous maps of global agricultural history

Previous work on mapping global agriculture can be found in scholarly works in two very different fields of research: climate modelling and global history. In our project we aim to present a synthesis in map form that communicates with these two different fields of research. Both these fields often suffer by replacing actual empirical evidence with assumptions that may seem to be based on common sense, but are in fact deeply problematic.

In climate research, it is a well-established fact that there is a close causal relationship between changes in land use and greenhouse gases. Forest vegetation sequesters carbon during its growth, while the clearing of forests leads to the emission of CO₂ into the atmosphere. It is thus increasingly recognized that early developments in land use had implications for the global climate system. Historical aspects of land use have come more and more into focus in climate research, especially following the seminal article by Ruddiman addressing the effect on climate of early human influences on land cover. Ruddiman proposed that the Neolithic revolution, as well as continued agricultural expansion well before the industrial revolution, impacted on the emissions of greenhouse gases and the global climate system (Ruddiman 2003; Ruddiman et al. 2011).

In the modelling of the relations between long-term carbon cycles and historical land use, different data aimed at reconstructing historical cropland change are used. The SAGE dataset was initially published in 1999, and covers the period 1700 to 1999 (Ramankutty & Foley 1999). At the same time the HYDE database was built up and initially also covered the last 300 years. Pongratz and co-authors in 2008 published a dataset covering 800 CE–2000 CE (Pongratz et al. 2008). All these datasets are based on recent global distribution of croplands, from which the cropland distribution in previous centuries is calculated, based on historical population estimates.

The most commonly used and quoted dataset now is probably HYDE and the most recent version of HYDE covers the last 12,000 years (Goldewijk et al. 2011). The basic assumption behind the HYDE dataset is that previous croplands only existed within the boundaries of the recent distribution of croplands and that these areas of cropland were successively filled up. Allocation of historical

cropland between recent state territories (and in some cases between regions within states) is made in relation to historical population estimates (mainly based on McEvedy & Jones 1978, with some recent updates) and an assumption on cropland per capita. The distribution within these geographical territories is based on algorithms and weighting maps which take into consideration such things as soil suitability for crops according to recent FAO maps of global agro-ecological zones. In addition, coastal areas and river plains are weighted positively and steep terrain is weighted negatively.

Historical data on agricultural history and cropland distribution cannot be entered into this weighting model. Only a few evident and well-known facts from agrarian history are taken into account in the underlying datasets and weighting procedures, and this is seldom stated explicitly. In sum: in the absence of an empirically-based, spatially-explicit history of agricultural land, a dataset based on hind-casting and environmentally deterministic algorithms is used here.

One would perhaps expect that in the fast-growing field of global history one would find a more evidence-based view of agricultural history. But that has so far not been the case. When agricultural systems are treated in works on global history, the overviews are often based on relatively dated and Eurocentric overviews. An alarming example is Malanima's recent work on the premodern European economy in which a global map of agriculture in the year 1500 shows only sparse occurrences of agriculture in America, and no agricultural lands in Sub-Saharan Africa (Malanima 2009: 99).

The French historical geographer and geohistorian Christian Grataloup (2007) discusses why Western Europe and the United States became the main axis of control. In that argument, he bases his understanding of the material background, sixteenth-century global agricultural systems, on a map originally published in 1954. Grataloup quotes Braudel, but Braudel in his turn based his map on the works of the American ethnologist Gordon Hewes, who in 1954 published a map of world cultures in 1500 CE (Hewes 1954). In a critical article on Braudel's work, Samuel Kinser pointed out how Braudel's interpretation of Hewes' work emphasized a hierarchical interpretation of the relations between different agricultural systems and thus gave support to a Eurocentric view. This was, according

to Kinser, an aspect that was much less pronounced in Hewes' own categories and Hewes in fact argued against such simplifications and against a Eurocentric world-view (Kinser 1981: note 20). But Grataloup chose to use Braudel's interpretation rather than Hewes' and thus supports the idea of modernity emerging in Western Europe and later spreading throughout the world. With its foundation in a timeless ethnographic perspective it replaces time with space, and assumes that the agricultural systems recorded by ethnographers can be ordered along an evolutionistic timeline.

As can be seen from the above, both quantitative modellers and global historians thus easily fall into the trap of replacing empirical knowledge with assumptions. The quantitative modellers use environmentally deterministic assumptions that are clearly at odds with what we actually know about where and when different agricultural systems developed and decayed historically. In global history on the other hand, there is a tendency for assumptions about a hierarchically ordered historical sequence of agrarian systems to replace the empirical evidence.

Mapping global agricultural history

It is thus clear that there is a need to develop methods for reconstructing land use, methods that accord the established empirical facts of agrarian development greater importance than hindcasting models or evolutionistic assumptions can ever do. For many regions of the world, historians, archaeologists and historical geographers have, after all, fairly good documentation on the timing and location of expansions, the abandonments of agricultural settlements and lands, and also of technological changes in farming. A project has therefore been set up with the aim to produce a series of maps covering the last millennium, in which the known agricultural history is made spatially explicit. Three cross-sections in time have been chosen:

- 1800 – before the second wave of globalization that drew large parts of the global south into commercial agriculture
- 1500 – or more precisely 1491, on the eve of European oceanic expansion and before the Columbian exchange

- 1000 – a period when African and American polities and landscapes were distinctly different from those of the late fifteenth century

The design of the project was inspired by the work done by historical geographers in the United States on pre-Columbian agriculture in North and South America. The perspectives outlined in the works by Bill Turner and Karl Butzer (Turner & Butzer 1992; Butzer 1992; Turner et al. 1995) and in three syntheses on the cultivated landscapes of different regions of the Americas (Doolittle 2000; Denevan 2001; Whitmore & Turner 2001) formed a model for our work. In the dissemination of that work to a broader audience, Charles Mann also later showed that it was possible to summarize such knowledge in map form (Mann 2005).¹

Syntheses like these, expressed in map form or readily converted to map form, are not yet available when it comes to the rest of the world. In a few cases, archaeological and historical research has provided overviews that are spatially and chronologically explicit and can be directly used in the mapping. This is the case, for example, with regard to the archaeologically documented irrigation structures in the Angkor Wat settlement in Cambodia. Here recent analyses based on a combination of ground surveys and remote sensing make possible a detailed mapping of the distribution of irrigated fields in that period (Evans et al. 2007). In a number of other instances, the spatial detail of current and abandoned irrigation and terracing can be accurately mapped based on later maps and aerial surveys, but the chronological issues remain vague. This is the case for many of the instances of terraced agriculture in Western Africa and the Sahel. There are strong arguments that many of these areas were in existence in 1800 CE, but their exact distribution at that time cannot be accurately mapped (see the map in Widgren 2010a).

Scholars in China have taken a leading role globally in spatially explicit reconstructions of croplands, based on historical sources. This reflects a strong tradition of Chinese historical geography based on the rich material of early written sources. Also, comparatively strong state funding has been decisive for climate-related research, including a specific focus on the role of land cover history. This

research is also fuelled by geopolitics concerning climate change. Conflicting results have been presented by American and Chinese scholars on the role of earlier land conversions regarding the historical debt of emissions. Ge and co-authors (Ge et al. 2008) claimed that a detailed study based on Chinese historical records for the past 300 years showed significantly lower emissions caused by land use conversion than that previously established by Houghton and Hackler (2003). According to Chinese researchers, these discrepancies are due to an underestimation of early agriculture and deforestation in China. By assuming that present croplands were cleared during the last 300 years and not earlier, a larger part of the burden of historical emissions is transferred to recent times. A larger share will thus be attributed to the period after the mid-1800s, which is usually the starting point for calculating historical emissions.

In this research, tax records and other information are transformed into gridded data in the format used by climate modellers. This has been done for the nineteenth and twentieth century expansions of croplands in the Northeast of China (Ye et al. 2011) and for central China in 1820 (Lin et al. 2009), but more remarkably also for the Song Dynasty (1004–1085 CE) (He et al. 2012). Of course, the usual source-critical caveats commonly applied to agricultural statistics based on tax records also raise questions in this case. Nevertheless, the chronological and spatial detail is far greater than what is usually possible for such early periods.

For most areas of the world, such detailed data on historical agricultural systems and land use is not available. For some regions of Eurasia, however, general works on economic and agrarian history are sometimes precise enough in their verbal characterization of agricultural systems in different periods and in different regions to permit a mapping of changes in agriculture over time. Examples of such works are the grand synthesis of China by Joseph Needham, with a contribution by Francesca Bray on agriculture (Bray 1984). Similarly, Irfan Habib's atlas work mapping surplus products from different parts of the Moghul Empire permits conclusions to be drawn about the types of agriculture in different parts of present-day India and Pakistan in about 1500 CE (Habib 1982). Although not as detailed in its evidence, James Scott's history of upland Southeast

Asia makes possible a mapping of the contrasts between intensive rice-producing and stratified areas and more extensively used areas with shifting cultivation (Scott 2009). These are just a few examples of scholarly work that can fairly easily be transferred to regions with different agricultural systems with a reasonable chronological specification.

Only a few regions of the world, however, can boast such syntheses of agrarian history. We are thus dependent on general works of history. This is the case for most regions of Africa and for many regions in Asia, apart from India and China. Many regional histories include an introductory chapter on land, people or subsistence, and often take the early nineteenth century situation as their starting point. The qualities of such overviews vary considerably. A few are based on actual agricultural history, while others substitute assumptions of different kinds for knowledge. The reflections in this chapter are based on a substantial trawling through such works for facts on agricultural development. Then, a sharp eye has to be used to weed out the shortcuts, so common in syntheses, from the genuine facts. It is in such situations that the checklist of four myths discussed in the remaining part of this chapter is important to bear in mind.

Similar myths or assumptions on agriculture of the past also flourish in much work on rural and agricultural development. As Daryl Stump has shown, these references to past agriculture may go in one of two directions. They either emphasize the inefficiency and inertia of traditional agriculture or they hail indigenous knowledge and the sustainability of previous farming systems (Stump 2010).

The four myths

Myth 1: Empty or under-utilized land

Narratives of empty or under-utilized land are common in descriptions of areas that were in fact populated by foragers, pastoralists or shifting cultivators. This is the myth that has most clearly been connected to political and economic interests, especially in the power relations of colonization. Historically, arguments about empty land have served to legitimate colonial interests in many parts of the world. The notion has also often gone hand in hand with

the notion of under-utilized land, and with the idea that itinerant hunter-gatherers and pastoralists have weaker rights to the land than permanent farmers.

One of the clearest cases of this myth is the contested settlement history of South Africa. The idea that European and African farmers entered South Africa at about the same time was disseminated as part of apartheid history writing, but the notion was not at all confined to the direct political interest of the apartheid regime. Much scholarly work outside South Africa was also based on this understanding. One does not have to search for long to find it in historical works and atlases, both in South Africa and in the rest of the world. In his history of Africa south of the Sahara, Donald Wiedner gives a vivid illustration of this idea in a map showing the "Occupation of South Africa 1652–1775" (Wiedner 1964: 123). Black and white arrows indicate "Bantu migration" and "European migration" respectively. While Europeans advanced eastwards and northwards from Cape Town, black African settlement was assumed to have advanced southwards at the same time. According to this map, these two migrations arrived at the Fish River in the Eastern Cape in 1775. "Conflict inevitably ensued when the Caucasian Boers and the Negro Xhosa met on the Fish River in 1775" writes Wiedner (1964: 125). A similar myth of empty land was also put forward for a later period in South African history, when it was claimed that most parts of the Highveld in the eastern parts of present-day South Africa were "depopulated by the Zulu wars before 1834" (see map from Theal 1891 reproduced in Davenport & Hunt 1974).

In the 1970s, archaeologists in South Africa moved to a position from which they were able to challenge this view with empirical facts. With the help of radiocarbon dating they had gathered enough evidence to revise some of the previously accepted chronologies based on pottery only. In the early 1980s, Tim Maggs presented the detailed evidence that once and for all refuted the idea of parallel black and white colonization. He compiled detailed maps of the spread of archeologically known African farming settlements north and east of the Fish River. In the era of apartheid, the need for meticulous work on indisputable empirical evidence of this process was important. From these maps it is clear that the expan-

sion of African farming communities along the coast had already reached the Fish River more than 500 years before Europeans even set foot in the Cape. Two grains of truth contained in the myth were first, that African farming expansion towards the south was indeed a continuing process that had not yet ceased when the first European explorers arrived in the area. Second, the Fish River did coincide with an important climatic boundary, and African farmers were not able to expand into the winter rainfall region in the southwestern area beyond the Fish River, where African grains (sorghum and millet) would not grow. It was only with the introduction of European grains that the Western Cape could become a farming region (Maggs 1980; Maggs 1984).

The South African case might seem to be an extreme version of politicized settlement history, and one might perhaps expect the idea of empty land to be an obsolete myth, but the idea of a sparsely populated Southern Africa survives in recently published historical land cover reconstructions (Goldewijk 2011). Such reconstructions use as their main input what is known today about historical populations. Unfortunately, the work on historical population reconstructions for early periods in Africa has advanced very slowly. It has not taken into account to any great degree the last thirty to forty years of archaeological work in Southern Africa. The atlas of world population history from 1978 still forms the basis of much work on African historical populations (McEvedy & Jones 1978). Nobody has yet seriously used the new archaeological evidence to reconstruct populations in Sub-Saharan Africa.

As noted above, the notion of empty land is also closely connected to the idea of under-utilized land. The idea that land is used below its full capacity has in history and in the present often been formulated as a motivation for state-led colonizations or for large investments in agribusiness to the detriment of the existing users of the land. In the context of recent large land acquisitions, Olivier De Schutter has pointed out the problematic use of the term under-utilized for occupied lands whose existing use is not perceived by governments as productive (De Schutter 2011: 260).

Myth 2: Historical sequences of agrarian systems

The notion of a historical sequence of land use intensity (foraging – pastoralism – shifting cultivation – permanent cultivation) is part and parcel of much work on regional and global syntheses in a long-term perspective. In a long-term perspective, human use of the environment has indeed progressed towards more intensive forms. The sequence from foraging to cultivation and from there to more intensive forms of cultivation is unquestionable. However, in the light of the most recent research, there are two question marks in the sequence presented above. One concerns the role of specialized pastoralism in such a sequence, and the other concerns the role of shifting cultivation. I will show how the idea of a historical sequence must be used with caution. Its predictive value across space and time is weak. In two broader fields, where an assumed historical sequence is often used, it can be considered a myth.

The first field of dubious usage is the idea that the resource utilization systems and the social organisation of ethnographically documented foragers, pastoralists and shifting cultivators, truly reflect previous stages in the evolution of technology and social organization. As has been shown in many recent studies, this is indeed a problematic assumption. It does not take into account the history of these people and their changing use of the land. At the same time, this interpretation most often ignores the wider political economy under which these extensive systems of land use exist today. With regard to the San-speaking peoples (bushmen) of Southern Africa, Wilmsen has argued that their present situation as foragers in the peripheral drylands of Southern Africa is a recent development that must be seen in relation to their subordination to incoming Tswana farming communities. It is not their isolation from the outer world that explains their lifestyle, but rather their contacts and interactions with politically and economically stronger groups (Wilmsen 1989). Similarly, it has been shown in Eastern Africa that specialized pastoralism developed during the last 200–400 years among groups who previously had much more diverse economic strategies based on small-scale dryland agriculture and foraging. This is, among other things, based on meticulous research in oral history to assess the time depth of age-sets in these pastoral groups

(Bollig et al. 2013). Specialized pastoralism reflects developments in exchange and political economies rather than being a stage in a historical sequence (Håkansson 2012).

In a parallel argument concerning inland South East Asia, Scott shows that crops and agricultural systems do not reflect stages in a historical sequence. Shifting cultivation was instead a political choice to stay away from rice-based hierarchical societies. Scott classifies farming systems and crops, not along an evolutionary ladder, but according to their political significance as means to escape the hierarchical state: escape agriculture and escape crops. The post-Columbian introduction of American crops into Asia strengthened shifting cultivation and escape agriculture. Cassava and sweet potatoes in particular were not only perfect escape crops but also made shifting cultivation more productive than before (Scott 2009). To summarize, we are now in a position to avoid seeing today's foragers, pastoralists and swidden cultivators as "Stone-Age survivors" in the midst of modernity, and rather to see their present subsistence strategies as defined by a wider political economy.

The second field where ideas about a historical sequence of agricultural systems erroneously influence broader synthetic work in history is the assumption that the early stages of farming must always have involved some form of shifting cultivation. If farming is proved in the archaeological record (e.g. from macrofossil finds of grain), or in the historical record as areas with a surplus of agricultural produce, it is often assumed that it must have been based on shifting cultivation whenever there is no definite evidence of permanent agriculture. In many of these cases it is only the assumption of a historical sequence that supports the idea that shifting cultivation must have been the first stage of cultivation in a specific region. Strangely enough, for all other forms of agriculture (permanent, irrigated etc.) the burden of proof seems to be heavier than for shifting cultivation. A source-critical view of shifting cultivation would say that we need proof to show that slash-and-burn was actually practised as a rotation system, not just that burning was used to clear fields. There is also much evidence to support the idea that intensive permanent agriculture in many contexts may predate shifting cultivation (see for example Davies 2015). It might also be useful to be reminded of

one of the earlier classifications of stages in agricultural development that emphasize horticulture versus agriculture rather than shifting versus permanent (Lenski 1966: 91ff.).

Concerning the Eastern Woodlands in North America, there was, for example, for a long time a consensus that Native Americans practised slash-and-burn or a swidden-type of shifting agriculture before Europeans arrived. This idea is still represented in many general works on the history of Native Americans (see e.g. Stoltman 2000: 571). However, a detailed investigation of the evidence by William Doolittle leads to the opposite conclusion. Doolittle scrutinizes all available sources by early European writers in the area, many of which had previously been taken as evidence for slash-and-burn systems. He convincingly argues that Native Americans practised a form of permanent agriculture on large fields where stumps were removed. As has previously been argued by Denevan concerning the Amazon, there is thus strong evidence that shifting cultivation was also a post-European phenomenon in the Eastern Woodlands of North America, to a large degree made possible by the arrival of iron tools (Doolittle 2004).

Myth 3: Agrarian inertia

Assumptions about agrarian inertia have often substituted for real historical studies of agrarian change in periods and regions where sources are deficient. Such ideas have also played a political role, and have gone hand-in-hand with pleas for agrarian modernization. This was true for eighteenth-century agrarian reformers in Europe as well as in the mid twentieth-century colonial sphere, but such assumptions also form a part of many uninformed development agendas of the twenty-first century. They are also present in the debate concerning European landscapes, where the idea of “traditional landscapes” conveys a vision of unchanging landscapes (see Antrop 2005 and for a critique Widgren 2012). Many scholarly overviews of African history take their point of departure from a timeless description of precolonial agriculture, as if it had not changed since its beginning. Agriculture is then often described on the basis of late nineteenth and early twentieth-century ethnographic observations and it is com-

mon to assume an almost essentialist connection between ethnical groups and their types of agriculture. As was pointed out by Paul Richards in 1983, many Marxist scholars have also fallen into this trap in their discussion of a “natural economy” as a starting point when discussing capitalist penetration in African agriculture. He argued that

the demographic and ecological processes subsumed under the category “natural economy” (or alternatively “precapitalist subsistence production”) are more plausibly viewed as products of capitalism (Richards 1983: 1).

Many such assumptions emphasize the primitive nature of agriculture and the lack of technological change over time. There is however often no basis for such assumptions. For example, overviews of African agricultural technology are sparse but the recent very informative overview by Blench of the diversity of agrarian implements gives an indication of a long period of innovation and change (Blench 2013).

The idea that agriculture has not changed over time is, paradoxically, not only applied to what are seen as backward and unsustainable forms such as shifting cultivation, but also to more intensive and seemingly sustainable forms involving, for example, terracing and irrigation. Such forms of agriculture are often assumed to be much older than they actually are.

The Balanta in present-day Guinea-Bissau were known from early records for being good paddy rice producers. The connection between the Balanta and paddy rice production is manifested in local proverbs and in creation myths. This would indicate a long history of paddy rice cultivation. However, in a detailed history of the relation between agricultural systems and the political economy of the area, Walter Hawthorne has shown that the expansion of rice production among the Balanta was a gradual process that occurred as a result of colonial penetration. Before the Portuguese arrived in the area, the Balanta cultivated other crops than rice, and in more extensive systems. Paddy rice production at that time in the sixteenth century, was restricted to Mandinge speakers in the Senegambia. Through their control of the trade in iron, they had also developed

a technologically advanced rice cultivation in which large iron tools played an important role. With the arrival of the Portuguese traders, other groups along the coast like the Balanta were able to get hold of iron. The Portuguese demand for rice made it possible for the politically decentralized Balanta to evade slave raiding by producing rice. Paddy rice cultivation spread southwards during the seventeenth and eighteenth centuries and came to form an integral part of the slave-trading network. The story of the labour-intensive paddy rice cultivation among the Balanta in Western Africa from the eleventh to the nineteenth centuries thus reflects an intricate interplay between the political economy of the slave trade, ethnic affiliations, and an advanced iron-using technology (Hawthorne 2003).

Another case of intensive agriculture thought to have had a long history is the area of rice terraces in the Philippine Cordilleras. Unesco, on their website, claim an early dating of these World Heritage areas:

For 2,000 years, the high rice fields of the Ifugao have followed the contours of the mountains. The fruit of knowledge handed down from one generation to the next, and the expression of sacred traditions and a delicate social balance, they have helped to create a landscape of great beauty that expresses the harmony between humankind and the environment (Unesco 2014).

However, recent archaeological excavations and radiocarbon dating in the area have shown that the expansion of these rice terraces occurred, not 2,000 years ago, but during the seventeenth and eighteenth centuries, and was related to movements of people in response to the Spanish colonization (Acabado 2012). Prior to that, taro would probably have been the most important crop in these areas. Moreover, Håkansson has shown how this expansion of investments in rice terraces was closely connected to regional economic networks, and thus is an expression of changes in world systems rather than a purely local development (Håkansson 2014).

Myth 4: Environmental determinism

The problem of environmental determinism would, like that of historical sequences, really need a longer treatment. Most geographers are strongly opposed to anything that has the slightest smell of environmental determinism. We are sometimes misunderstood by other scholars, who think that we oppose the idea that physical factors such as climate and soils have a profound influence on the type of agriculture developed in a particular environment. That is not the point. What the historical geography of agriculture tells us is that the major regional types of agriculture in the world cannot be explained on the basis of the environment alone. Areas with similar environments exhibit very different types of agriculture. Geographers have been aware of this for a long time (some examples: Whittlesey 1936: 209; Morgan 1988: 69).

Ideas based on environmental determinism have, however, continued to thrive in many other disciplines. For example, it is evident that the idea of Oriental despotism, as well as the notion of the Asian mode of production, were to large degree based on a misunderstanding by Marx and Engels of the determining factor that arid Asian environments supposedly had their own modes of production (Blaut 1993: 82–4).

In 1954, on the basis of her experience of archaeological work in South America, the American anthropologist Betty Meggers formulated what she called “the law of environmental limitation on culture”. She argued that

[d]ifferences in soil fertility, climate and other elements determine the productivity of agriculture, which, in turn, regulates population size and concentration and through this influences the sociopolitical and even the technological development of culture (Meggers 1954: 802).

and formulated her law as

the level to which a culture can develop is dependent on the agricultural potential of the environment it occupies (Meggers 1954: 815).

Her argument was to a large extent based on contemporary knowledge of archaeology in the South American Amazon rainforest, which she claimed could only support hunting and gathering or slash-and-burn agriculture. However, Hirschberg and Hirschberg criticized her arguments very early on, on theoretical grounds. They reformulated the law as follows:

The level to which a culture develops is dependent on the amount of food the people know how to raise (Hirschberg & Hirschberg 1957: 891).

However, for a relatively long period, the position that Meggers took on the agricultural potential of the Amazonian rainforest became part of a general assumption about what kind of prehistory one would expect there. Towards the end of the last millennium, archaeological research came to reverse that story fundamentally. Complex societies based on dense settlements and advanced agriculture from pre-Columbian times were discovered and this completely overthrew Meggers' ideas about the "the level to which a culture can develop" in a tropical rainforest environment (Heckenberger et al. 2003). It has also been shown that the type of agriculture that was fundamental for these settlements was to a large extent based on permanent agriculture rather than on the slash-and-burn agriculture known in the area from later times. Permanent agriculture was based on a form of soil improvement that had been unknown until these archaeological discoveries were made. Woody vegetation was charred and the charcoal incorporated into the soil, forming what is now known as a new type of anthropogenic soil – Amazonian Dark Earths (Glaser & Woods 2004). The discovery that the Amazonia rainforest was not virgin has also raised the issue of a new research agenda addressing the cultural history of other rainforest areas in the world (Willis et al. 2004).

The interpretative mistake that Meggers made was to assume that the type of agriculture that was practised in the area in later times was a good indication of the type of agriculture that was possible given the environmental conditions. This led her to conclude that similar adaptations might have existed in the past.

Recent findings from the Central African rainforests illustrate a similar case, where assumptions about what was possible in the past were erroneously based on recent agriculture. In the Central African rainforest, bananas and tubers play a decisive role in agriculture. It has been assumed that the earliest agriculture in this area must have been based on a similar repertoire of crops. It should, however, be noted that many of the crops important in the area in the present and in recent history, which form the basis of present forest vegetation systems are of American origin. Based on archaeobotanical evidence, Kahlheber and co-authors show that pearl millet and Bambara groundnuts were cultivated in the rainforests of South Cameroon in the period 400 BCE–400 CE. These crops have conventionally been seen as part of a savannah type of agriculture usually found much further north, and the environment of the African rainforest is generally considered to be too wet for them (Kahlheber et al. 2013). The new finds thus suggest that the more recent forest vegetation systems should be considered as a secondary phase of agricultural development in the area, and not the earliest and only possible adaptation to that environment (Fuller et al. 2013: 22).

Conclusions

Common to the four myths discussed above is that they deny agriculture and farming communities a history, and replace empirical facts with assumptions. In broader syntheses there is always the risk that such assumptions will creep in. The global historian must therefore always read secondary literature with a critical eye, having these resilient myths in mind. When land is described as virgin, is it only because there is no data available on the inhabitants and their land use? When agriculture is proved to have been present in an area, but is not known in detail, the burden of proof for arguing that it was based on shifting cultivation should be as heavy as it would be for, for example, irrigation. Ideas about historical sequences cannot be substitutes for empirical data. When descriptions of early land use are based on the empirical documentation of more recent forms of agriculture, there is always the risk that the author has fallen into the trap of environmental determinism, believing that recent “traditional”

use of the land is the only possible option given environmental constraints, and that agriculture did not change in the past.

It is interesting to see how such myths have become part of a general discourse both in academic history and in applied fields such as development work. In many cases, the persistence of such myths must be understood within the relevant political (often colonial) contexts. They do indeed represent discourses, in the sense of ways of talking and writing that reflect existing power relations. However, in the cases I have referred to, when researchers have been able to debunk such myths, they have not based their critique on discourse analysis but on painstaking and detailed empirical work, often of an interdisciplinary nature, in the field and in the archives.

The results of recent research have also motivated us to be very humble when imagining past worlds. Most of the results disclosed by new research on agrarian systems of the past could not have been predicted from the environment, from population density, or from historical sequences of agricultural systems. When writing global history, one must be wary if syntheses and secondary literature use such assumptions about agriculture rather than referring to empirical investigations. The new results also illustrate, most clearly perhaps in the case of the Amazonian Dark Earths, that human ingenuity is immense, and that the variations of systems and techniques for producing food are many and indeed difficult to predict – let alone hindcast.

Notes

- 1 For a cartographically clearer version of Mann's map see <http://www.utexas.edu/courses/wd/MannMap%202013.pdf>.

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Archaeological investigations, interpretations, and theories

The cases of Sri Lanka and Sweden compared

Eva Myrdal

The focus of this chapter is how field studies of the history of settlements and land use, including the history of agriculture and metallurgy, are more advanced for Europe than for Asia. This is not merely a question of a difference in the number of documented sites of a specific type. There is also a discernible difference in the types of former activities that has been searched for.

One example of this phenomenon is premodern iron production (using direct and indirect production techniques), an undertaking which consumes charcoal (trees). Historical sources and field observations have given us every reason to believe that South Asia was an important area for the production of iron as early as the mid-first millennium BCE, and for export of iron and steel at least from the mid-first millennium CE (Bronson 1986: 19–21; Forenius & Solangaarachchi 1994: 135–142; Juleff 1998: 2). This research potential has however not been followed up, in contrast to the survey coverage and close reading of the iron production landscape in Northern Europe. This has resulted in a mapping of the landscape of iron production with a low resolution in South Asia, and a high resolution in Northern Europe.

A wide range of interesting questions arises from a detailed coverage of iron production sites if there is also good survey coverage of settlements and other production sites. These include the environ-

mental impact, control of raw material resources, how production and trade were organized, and the possible presence of groups of people controlling the labour of others.

Such studies could contribute to a global perspective on, for example, the long-term environmental impact of human activities. On the other hand lack of field data constitutes a problem when comparative or global perspectives are sought for, and if inequivalence between sets of data is not acknowledged, the end result will naturally be slanted and biased.

This is the more disturbing because as far as we can infer from historical sources, intensively surveyed case-study areas, and casual remarks regarding the field situation, large parts of Western, Southern and Eastern Asia should have a richer source material of ancient remains pertaining to settlement patterns and land use through time than most of Europe. In the case of what is now Sweden on the northwestern periphery of the Eurasian continent, this is definitely the case.

When certain types of data and perspectives of research pass unnoticed by mainstream actors in the field, they will remain almost invisible to the broader public of research consumers (including those who write global history) – in spite of the fact that they may represent lived experience for millions outside the academy.

A node for a close and comparative reading of field data, which has no equivalent on the Asian continent, is constituted by the bi-annual conferences of RURALIA (held since 1995). Here the focus is on the archaeology of settlement and rural life in Europe, 500–1700 CE. An introductory comment by Eva Svensson in the proceedings of RURALIA VI, “Arts and Crafts in Medieval Rural Environment”, can serve as a sounding board for the South Asian studies discussed below:

Archaeology has the potential to challenge the stereotyped image of the medieval peasant. But it demands that we open our minds to the possibilities of the archaeological material, and not restrict ourselves to “prove” statements made by historians on the basis of written documents (Svensson 2007: 189).

What is suggested below is that quite apart from Europe having a better coverage of archaeological surveys than Asia, the questions differ in focus. Tentatively, I refer to this difference as a problem of a social bias and I will discuss the implications of this below. I will also discuss the impact the concrete colonial experience may have had on the retrieval and documentation of ground truth and of archaeological field data in South Asia as compared to Europe. The view taken here is that this has had a bearing on the development of fieldwork practices such as surveying for maps, and for the development of antiquarian practices. This refers mainly to how governmental practices in these fields have developed. The concrete examples are from Sweden and Sri Lanka respectively.¹

A few basic remarks pertaining to “Sweden” and “Sri Lanka” introduce the comparative discussion, in “Setting the stage”. The section “Documentation of ground truth – maps in Sweden and Sri Lanka” introduces a discussion of the collection of field data in the nation state as compared to the former colony. This discussion is followed up in “Documentation of ancient remains in the field: Sweden and Sri Lanka”, indicating that what was created as archaeological ground truth under these different regimes has had a bearing on how mainstream research perspectives have developed.

In the section “A comparative perspective”, a more detailed account is given of the state of the art regarding studies of past land-use such as agriculture and iron production.

The inequivalence of the sets of data will be returned to in the concluding discussion, as this has a direct bearing on the possibilities for discussion on a global level.

Setting the stage

The lower time limits for this discussion are when mankind first migrated to Sri Lanka and Sweden and the upper time limit is the present.

The oldest dated remains of human occupation in Sri Lanka found to date (2016) come from the 28,500 BP (before present) habitation site of Batadomba-lena (Deraniyagala 1992: 118). Stray finds of Palaeolithic implements have been documented, but no sites have been found and dated.

The oldest dated remains of human occupation in Sweden found thus far (2015) are from the Aareavaara site, a Mesolithic campsite of reindeer hunters dated to *c.*8700 BCE (10,700 cal. yr BP), situated in the most northeastern area of Sweden, in the Pajala municipal region (Möller et al. 2012: 110). This site is representative of people coming from the east or northeast, following the retreating ice sheet. An almost contemporaneous site of a late Palaeolithic reindeer-hunting camp of the Bromme culture in the far south has been dated to 8500 BCE (*National Atlas of Sweden* 1994: 7). This is representative of people tracking the Ice Age fauna migrating northwards as the ice sheet retreats.

Size in terms of square kilometres may be another factor to bear in mind when aiming at a comparative perspective. Sri Lanka covers an area of 65,610 km². The area of Sweden is 450,295 km² (CIA). However, since the end of the last Ice Age, the size and shape of the Sweden's land area has undergone dramatic change. The main trend over time (decreasing in speed) is an increase in the land area available for human habitation.

Depending on the particular situation, human habitation and land use may be identified as either an asset or a problem in relation to the planning and realization of fieldwork. Population censuses have been carried out for the whole of Sri Lanka almost every tenth year since 1871 (*National Atlas of Sri Lanka* 1988: 62). In 2013, Sri Lanka had a population of 21,675,648. The average population density was 323 per km². In 1950 the numbers were 123 per km². The population of Sweden in 2013 was 9,647,386. The average population density was 20.6 per km². In 1950 the population density was 16 per km².

Today's figures give a rough understanding of Sweden as a country which is comparatively large in area, but with low population density, while Sri Lanka is the opposite, a comparatively small country with high population density. The figures for 60 years ago imply a similar situation, though the increase in population, and population density, is more dramatic in the case of Sri Lanka (United Nations; CIA).

These average figures have to be complemented by more area-specific data in order to reflect different conditions in a fieldwork situation. A general observation is that intensified and mechanized use of

the landscape for agriculture or forestry threatens the possibility to learn the history of the area later, if it is not combined with relevant antiquarian practices of documentation in parallel. Likewise, local knowledge of the cultural landscape is an asset for field research, whereas a situation where people with this local knowledge been forced to move constitutes a loss of knowledge, and interviewing people who have had to move in adds a source-critical aspect to interpretation of the information retrieved.

The amount of arable land will vary over time. Which factors were of importance for the increase or decrease of arable land in Sri Lanka and Sweden, and to what degree the amounts changed over time, is a field of study in its own right. However, the amount of arable land partly reflects what climate and geology permit. There are areas in both countries which have never been suitable for agriculture, and which remain so today.

In Sri Lanka, the Dry Zone has witnessed agrarian colonization based on artificial irrigation from *c.* 300 BCE until the thirteenth century CE: irrigation facilities were abandoned between the thirteenth and the late nineteenth centuries, before re-utilization and construction of facilities for artificial irrigation took place during the twentieth century (see for example Myrdal-Runebjer 1996). The thirteenth century decline of irrigated agriculture and the demographic shift towards the western coastal areas has been a focus of academic and politically motivated discussion (Indrapala 1971 and further references). The census indications of an unstable demographic situation and the decrease of inhabitants during the nineteenth century, after British colonial power established itself in the entire island, have been very little discussed (see for example Farmer 1976: 11–12). Neither of these presumed demographic shifts has been approached from a detailed reading of field data, which is why these issues have a direct bearing on the questions raised in this chapter, and will be returned to in the discussion of the state of the art.

Today, arable land in Sri Lanka constitutes 18.3 per cent of the total area (12,000 km²) and 980 km² are covered by water. In Sweden, arable land constitutes 5.8 per cent (26,117 km²) of the total area, with water covering 39,960 km². However, it is important to

bear in mind that with artificial irrigation, the same field for staple crops such as rice may be harvested two to three times per calendar year in Sri Lanka, a possibility created by climate and labour input. This gives a support potential (and potential of appropriation) that should also be considered in a historical perspective. The same goes for the seed/yield proportions. The wet-rice seed/yield proportion before modern high-yielding varieties of paddy rice were introduced has been given as 1:100 (Bray 1989: 15). For Sweden, the seed/yield rate for the staple crops wheat and barley during the fifteenth century has been estimated at 1:3–4 (Myrdal J. 1999: 235).

Documentation of ground truth: Maps

As is stated in the introduction to this volume, the task of writing global history implies using mainly secondary sources. The following examples raise the question of who authored what, and why.

Fieldwork is necessary at some stage in most data-gathering undertakings aimed at advancing knowledge of past physical processes and events. Maps are crucial to any fieldwork, and maps are most often drawn to be used in economic, military or legitimizing contexts.

The oldest maps known today which show Sri Lanka are those of Ptolemy (127–151 CE) and Al-Idrisi (1100–1166), produced by foreigners who had not visited the island themselves (*National Atlas of Sri Lanka* 1988: 8). Al-Idrisi's map of "The Island Serendib" (*gezira sarandib*) depicts and names the central highlands, as well as eleven coastal and four inland settlements, of which one inland settlement lacks a name (*National Atlas of Sri Lanka* 1988: 8; Idrisi). These early maps reflect Sri Lanka's central geographical position for map-producing societies active on the ancient trade routes across the Indian Ocean.

The oldest maps depicting the Scandinavian peninsula have a character similar to the oldest maps of Sri Lanka, being small-scale maps initiated by people operating outside the area itself and thus not drawn from ground truth. However, the Scandinavian maps are 1,300 and 300 years younger than the Sri Lankan ones (Nicolaus Germanus' *Cosmographia* 1482; Olaus Magnus' *Carta Marina* 1539, published in Venice). The later date reflects Scandinavia's peripheral geographical position for early map-producing societies.²

As maps always have an author, and were often made to order, it may be useful to consider on whose behalf, or under what circumstances, the small-scale maps of Sri Lanka and Sweden made during the following seventeenth to nineteenth centuries were drawn.

In the case of Sweden, we may note that from the state authorities' point of view, the country had never been occupied and ruled by foreign forces. Before 1658, border areas of what now constitutes Sweden belonged, in fact, to a neighbouring king, and additional neighbouring territory was subject to the Swedish crown. The present borders were established in 1809 when Russia conquered Finland.

Historical experience in Sri Lanka before 1948 was different. Increasing areas of territory had been lost to colonial powers since the first arrival of the armed Portuguese in 1517. The process continued through the period of Dutch colonization and the subsequent British colonial warfare from 1796, with British victory over the last king of Kandy coming in 1815. The process finally culminated in the defeat of the all-island uprising in 1817–1818 (Emerson Tennet II 1977: 547–622; Davy 1983: 246–247).

After the various colonial powers had established themselves on the island, they initiated the drawing up of maps of Sri Lanka, based on some physical knowledge. Early examples of small-scale maps produced in a colonial context are those of Cypriano Sanchez (in 1560–1566), Knox (in 1681) and John Davy (in 1821).

In Sanchez' map, produced during the early Portuguese rule of part of the island, the focus is on demography, with several remarks referring to "deserted land", such as, for example, in the southeastern part which is said to have been deserted for 300 years because of disease.³ Roads and many additional named inland settlements have been added to this map as compared to Idrisi's. Information regarding economically important resources is also to be found. South of the central highlands a note refers to "Serra de Ferro", Iron Hill (*National Atlas of Sri Lanka* 1988: 8). This relates to an area where iron production was carried out from the Early Historic period to colonial times (Coomaraswamy 1956: 190–193; Juleff 1998, see further below). Just east of the note "Reino de Candea", the Kingdom of Kandy, reference is made to "Cardamome" which was an export item. (*National Atlas of Sri Lanka* 1988: 8). Geographical

information on “iron” and “cardamom” has not been a key factor when historians have told the “grand narrative” of Sri Lanka.

The geographically more correct outer contour of the island is easily recognized in Robert Knox’ (1641–1720) late seventeenth-century map. The map is an illustration in his book about his almost twenty years of captivity in the Kingdom of Kandy, and his flight to the coast and back home to England. He was not a subject of the then colonial power (the Dutch) and he named the map “A new map of the Candy Uda in the Island of Ceylon”. A more detailed depiction of named inland and coastal settlements, roads and rivers can be noted compared to previous maps. The focus is on political boundaries between the Kingdom of Kandy, the Dutch and the Vanniars in the North (Knox 1981).

The map by John Davy (1790–1868) from 1821 approaches ground truth in relation to settlements, roads and rivers to such a degree that it is possible to use it for at least an approximate orientation in the field. John Davy (M.D., F.R.S.) was an army surgeon and physician and in Sri Lanka he was in attendance on the British governor, Sir Robert Brownrigg. In 1817–18 Davy accompanied him and British troops in the field during the all-island uprising against the colonial power. The map he produced has the title “Map of the Island of Ceylon. Showing military routes, Out posts and Forts of Early British Times” (*National Atlas of Sri Lanka* 1988: 10).

A brief overview of map production in Sweden can help us to reflect over how the colonial situation exerted an influence on the production of maps and their subsequent use in Sri Lanka.

Andreas Bure (1571–1646) was commissioned by the king to produce a map over Sweden, and it was published in 1636. Two years later, he was received a royal commission to organize the Survey Department. The maps were produced in the interest of the State authority, the small-scale maps should be seen in the context of the crown’s interest in expansion to the north and east, while the large-scale maps were made with effective taxation in mind.

From the time of Gustaf III (1771–1792), the armed forces took over the responsibility for geographical mapping. A field corps of surveyors was organized at the beginning of the nineteenth century and it was not until 1937 that the work came under civil

administration. The maps were made available to the general public from 1857.

One important difference between Sweden and Sri Lanka is the production of large-scale cadastral maps, in Sweden from the seventeenth century and in Sri Lanka from 1800, and in whose interest they were made.

During the late eighteenth and early nineteenth centuries, the focus in Sweden was on large-scale cadastral maps as an aid to the state-initiated land reforms re-parcelling the agricultural land of the villages. The fieldwork was organized by the Survey Department. In parallel with this, personnel from the Swedish Navy triangulated the coastal areas in their entirety.

As a basis for better statistics on agricultural produce, economic mapping started in the mid-nineteenth century. A mapping project which has a direct bearing on the organization of the antiquities survey (see below) was the "Economic map" started in 1935 and completed in 1978. Revisions and a change of scale have followed. This is a mapping project involving the whole of Sweden except the high mountains and their adjoining forest land in the far north and northwest of the country. The map was produced on the scale of 1:10,000 for the southern part of the country and on the scale 1:20,000 for forested areas of inland northern and mid-Sweden. Buildings, homesteads, roads, footpaths, agricultural land, ancient remains and the names of farms and villages are depicted. The work was undertaken by "Rikets allmänna kartverk", the national mapping office.

Four years after the British colonial power took the colonized part of Sri Lanka from the Dutch in 1796, land alienation and land settlement made it necessary to conduct cadastral surveys. The first cadastral maps were produced at the inception of the Survey Department in 1800. The objective was to separate state lands from private lands. Private land was not individually surveyed (*National Atlas of Sri Lanka* 1988: 12).

In 1897 the colonial power decided to conduct a topographical survey of the entire country, and to produce a map on the scale one inch to one mile. In 1908, an adequate field methodology had been worked out for producing a contour map, necessary for the

planning of railways and roads. The one inch to one mile contour map of Sri Lanka was completed in 1924.

A map publication project in a different political set-up was the *National Atlas of Sri Lanka*, published in 1988. A quotation from the introduction by the chief editor T. Somasekaram, Deputy Surveyor General of the Survey Department, may help to highlight the difference as compared to the colonial period. He formulates the national agenda of the project and concludes the introduction with a statement on how to put the Atlas to use:

The Atlas depicts graphically, as no words or tables can, the uneven distribution of people, resources and services in the country. How could a balanced development be achieved? ... If the National Atlas enables people to ask such questions and seek answers, this labour of love of four years by a dedicated team would have been worth the effort (Somasekaram 1988: VII).

The Agricultural Base Mapping Project (ABMP) was started in 1977 with the aim of producing a map series on the metric scale 1:50,000 with 5 meter contours, using the old maps and aerial photography, and revised by surveyors in the field. The maps were to be used in planning for self-sufficiency in foodstuffs (*National Atlas of Sri Lanka* 1988: 12).

Documentation of ancient remains in the field in Sweden and Sri Lanka

Sweden

The short overview of the history of documentation of ancient remains in Sweden given below focuses on the initiators and the results in terms of spatial and social coverage.

The first antiquities law in Sweden was passed in November 1666 and the registration of antiquities (in Swedish “Rannsakingar efter antikviteter”) started in 1667 after an initiative by the Director General of the National Heritage, *Riksantikvarie* Johan Hadorph (1630–1693). He argued that the memorials to the nation’s ancestors were under threat of destruction. A request was sent from the

state authorities to the Archbishop and the Bishops in which they were urged to contact all the parish clergymen in the country. The priests were requested to involve the churchwardens and the six laymen trustees in each parish in helping them to establish lists over all antiquities to be found in the parish. The lists were to be sent to the Committee for Antiquities (*Antikvitetskollegiet*). The Rannsakingar continued until 1693 (Rannsakingar).

The types of remains to be registered, and thus protected, were castles, hill forts and other types of fortification, runestones, tumuli, the graves of Kings or other aristocratic persons, all graves in churches and in churchyards, and anything else that might serve as a memorial to historic achievements, persons, towns or families. Later, Hadorph added holy wells, old books, coins and folk songs.

The archive contains material from 1,200 parishes, none of them situated in the northernmost provinces of Norrbotten, Västerbotten and Lappland. Burial grounds, tumuli and hill forts from the Iron Age are among the registered remains. Very few remains older than the Iron Age were documented. Documentation, of runestones and sometimes of legends and traditions connected with them, dominate the material. Further documentation of the immaterial cultural heritage exists in the form of legends regarding the building of parish churches, of holy wells and of various folk customs and traditions.

It is obvious that the survey was socially biased, not only in terms of results but also in terms of intentions. Nevertheless, the actual people working in the field were priests and laymen from parishes lying mainly in rural areas. The priests obviously prioritized written records (runestones) but to some extent what the locals related in connection to these and other objects has also been recorded.

The geographical bias, with a total lack of material from the northern half of what is now Sweden, might partly be the result of the types of remains specified.

The antiquities law of 1867 established that physical encroachment on ancient remains was punishable by law. It should be noted that ancient monuments are not private property in Sweden.

During the eighteenth and nineteenth centuries, antiquarian activity was conducted through regional and local initiatives. Apart from priests with an interest in local history, regionally-based civil

organizations were important initiative-takers in compiling antiquarian documentation during this period, until the state organized the antiquities survey of 1938. Among these civil organizations were the Rural Economy and Agricultural Societies (started in 1791 on Gotland and established in all counties in 1850), the Swedish Antiquarian Society (established in 1869) and the Swedish *Hembygdsrörelse* (home district movement, started in the early twentieth century).

This is not the place for a thorough discussion of this development⁴. What is important, given the questions at issue here, is to note the presence of the Royal Swedish Academy of Letters, History and Antiquities (RSALHA, founded 1786) on the one hand – the Secretary of which was also the *Riksantikvarie*. On the other hand there were also a rich variety of regional and local practices which were organized but not tied to specific religious, political or economic interests, which however, is not to say that they were ideologically neutral.

State-organized antiquarian practices developed through the RSALHA from 1826, when the Academy was established as the highest state authority for ancient remains in the country. The Swedish National Heritage Board was established in 1938 as an independent government agency. When the national antiquities survey started as a collaboration with the national mapping office for the production of the Economic Atlas the same year, it had the previous local and regional inventories as a basis for the field survey. The field personnel were also in communication with the local civil community during the surveys. With ancient monuments marked on the map, the landowners had an enhanced possibility of following the Antiquities law.

The field surveys and production of the first edition of Economic maps ran from 1938 up until 1978. Over time there was development in relation to the types of remains that were registered. This was partly linked to the revised Antiquities law (1942) that allowed for registering new types of remains such as production and settlement sites from historical times (Selinge 1989: 17–18). However, the dominating categories of registered ancient remains during this period were hill forts, runestones and prehistoric graves. The latter represent a spatial and chronological overview of settlements

for some historic periods and regions, though the focus on visible burials involves a social bias (Jensen 1997: 115).

As the survey covered new areas, additional types of remains were added: trapping pits for moose in the northern part of the country were noted as from the late 1950s, receiving the status of protected monuments in 1965. Stone Age settlement sites were incorporated into the register when the survey covered the rivers and lakes in Norrland in the 1960s, these having been separately registered from 1942⁵ in relation to the building of hydro-electric power plants. The remains of agricultural land use had been noted on the field maps from the start, and some remains such as the prehistoric stonewalls were given the status of protected monuments in the 1940s. Finally, in the county of Jämtland the remains of farmsteads abandoned not later than the sixteenth century, including adjoining infield areas, were registered as protected monuments from the late 1960s (Selling 1989: 20). An antiquarian interest in remains related to low-technology ironworks (bloomery furnaces, slag heaps etc.) is documented from at least 1874 when Hans Hildebrand, the *Riksantikvarie* at the time, documented slag along the river Dalälven and in the province of Västergötland (Magnusson 1986: 28). These types of remains were noted on field maps from the start of the antiquities survey, but they gained the status of protected monuments in the 1960s during the survey of Dalarna and Norrland (Selling 1989: 18). Since the early twentieth century, low-technology ironworks formed part of the archaeologists' research agenda, more or less intensively, and parallel studies of the medieval and later historical ironworks were undertaken (Magnusson 1986: 28–29).

When the survey accompanying the revision of the Economic map started in 1974,⁶ a new and broader programme for the antiquities survey had been formulated (Jensen 1997: 116). The broadened view of what constitutes ancient remains worth preserving was a result of communication between the public, after many local and regional studies had been undertaken since the eighteenth century, and the government agencies in the field of cultural heritage management, as observed by K-G Selling (Selling 1989: 21).

The revised survey thus had the ambition to register as many types of cultural remains as possible, not only those with the status

of protected ancient monuments. Today there are 155 types of remains mentioned in the list used by the National Heritage Board.

When actually working in the cultural landscape, it is apparent that the register gives a biased picture. There is still better coverage of the southern half of the country than of the northern half. There is better coverage of open agrarian landscapes than of forested areas. Also, the remains related to abandoned agricultural land, to preindustrial production, to the dispossessed section of the rural population in historic times, and to the Saami population, are less well covered. Many of the latter remains are to be found in present-day forested areas. The need for a complementary survey was felt since most forests in Sweden are actively used for highly mechanized forestry production, which, according to reports, is destructive towards protected ancient monuments as well as other cultural remains (protected by the Forestry Act of 1994, though this is not as strongly formulated). For details of the impact of forestry on the cultural landscape, see, for example, Myrdal-Runebjer 1998; Aronsson 1998; Hällström et al. 2001.

The Forest and History survey of cultural heritage sites was initiated by the County Forestry Board in Värmland-Örebro in 1995, in close co-operation with the National Heritage Board and the County Antiquarian Agency. In the beginning of 2000, Forest and History surveys were under way in 14 counties (Myrdal-Runebjer 1999; Myrdal-Runebjer 2002). Today 115,000 cultural heritage sites are digitalized in the Swedish Forestry Agency's register on a national level, and relevant remains are being included in the National Heritage Board's digitalized register of ancient remains at the pace allowed by available resources.

To conclude this overview, one may observe that the initiative to conduct the antiquities survey was taken within a national, ruling-class context, but with a strong focus on oral tradition and "Griffter och Ättebacker" (burials and burial-grounds), the specific and local came to form part of the documentation from the beginning. The various local initiatives that were subsequently undertaken and the continuous dialogue between laymen and the antiquarian professionals and later the Ministry of Culture and Parliament, have enabled field studies of land use history with both "castle" and "cottage" perspectives. For research purposes it should be possible

to develop syntheses of the local and the specific when it comes to analyses of power relations and the control of resources and the labour of others.

The development from the 1930s of one centrally-managed register for ancient remains should be noted, as it forms a common ground for discussions of perspectives and evaluation.

Sri Lanka

The documentation of ancient remains in the field, before the Archaeological Department was formed in 1890, was carried out by representatives of the colonial power whose main duties were other than antiquarian documentation and cultural heritage management. The pioneers during the early nineteenth century were British military personnel.

In 1818, Lieutenant Fagan, a British officer, was the first European to visit the ruined city of Polonnaruva from the Middle Historic Period. He arrived there in pursuit of some “rebels” during the military campaign of 1817–1818. He published an account of his observations in the *Ceylon Gazette* in 1820 (Ievers 1899: 213).

The Collector of Mannar, T. Ralph Backhouse, had taken part in Fagan’s military expedition the previous year, 1817. During this field campaign he had also visited Anuradhapura, the site of the ancient capital, and Mihintale and Kavuduluväva (“väva” means irrigation tank), measuring and describing ruins and tank bunds (Ievers 1899: 213; Silva 1969: 1162).

The ancient capital had been abandoned as the main administrative centre as early as the eleventh century. However, as with most once-settled areas in the world, the area itself continued to be used by people in various ways. In 1679, six hundred years after it had ceased to be the seat of kings, Robert Knox came to visit the site during his flight from the Kingdom of Kandy. He had been advised to go to Anuradhapura to procure meat. He describes the area as follows:

It is a vast great Plain, the like I never saw in all that Island: in the midst whereof is a Lake, which may be a mile over, not natural but made by art, as other Ponds in the Country, to serve them to water their

Corn Grounds. This Plain is encompassed round with Woods, and small Towns [villages] among them on every side (Knox 1981: 353).

He further describes how people outside the area perceived of the site as a former centre “where they say Ninety Kings have Reigned” (Knox 1981: 100).

The army surgeon Dr. John Davy never visited the site of Anuradhapura during the military campaign of 1817–18. However, he gathered some information from “natives” and an officer who were there during the rebellion, and he mentions that Anuradhapura 140 years after the visit by Knox was “a small mean village in the midst of a desert”. He also writes that the site with its ruined dagobas was considered a sacred spot and a place of pilgrimage (Davy 1983: 225).

The Assistant Agency of Nuwarakalawiya was located at Anuradhapura in 1833. It initiated some clearing of jungle, bringing to light previously undocumented ancient remains such as stone inscriptions (Silva 1969: 1162). Lieutenant (later Major) Skinner was the first to give more reliable accounts of what was to be found above ground in Anuradhapura, based on a field survey in 1832–33. During the work he was able to gather information on the “traditional names” of specific buildings (Ievers 1899: 213). This is another indication that the site at the time was part of a cultural landscape in use.

In their proceedings of March 1831, the Board of Kandyan Commissioners notes the reuse of ancient constructions and warns against using local information on names:

From this period onwards many of the caves and abandoned vihares were re-occupied by priests – generally natives of the Seven Korales. These priests frequently pretended to know the ancient names of the places they occupied, and gave names which their reading of the chronicles suggested. Thus it is not safe to rely on the names without further evidence (Ievers 1899: 213–214).

The various operators in the Anuradhapura cultural landscape had of course different objectives. The “priests – natives of the Seven Korales” were part of the local social network, as Buddhist priests having to be fed by others. They had taken advantage of

the overthrow of the King of Kandy by the British troops, and they now claimed the right to live in and name the structures. The representatives of the colonial administration, on the other hand, were in the process of finding ways of putting the colony to use, and were gathering various types of information for later practical and legitimizing ends.

Using the Buddhist chronicles to identify structures in the field was a practice continued by the colonial rulers. In 1842, Major Skinner gave the plan of Anuradhapura to the British civil servant George Turnour (1799–1843) (Ievers 1899: 214). Turnour was the first European to study the Pali Buddhist chronicles of Sri Lanka, and his translation of the Mahavamsa was published in 1837.

Major Forbes was able to give an improved description of the site of Polonnaruwa (the capital in the eleventh to mid-thirteenth centuries) during a visit in 1831. He was also the first European to document a visit to Sigiriya, which for a short period of time in the fifth century was the site of the capital (Paranavitana 1983: ii).

From the mid-nineteenth century onwards, representatives of the colonial power other than military personnel become more visible in the documentation work. In 1848, Sir James Emerson Tennent, the Colonial Secretary of the British colony in Ceylon at the time, was obliged to tour the colony because of an uprising. The news that the monarchy had been overthrown in France and a republic established had been used by some people in the former Kandyan territories to raise awareness among the local people of the new taxes imposed by the colonial power on for example shops, firearms and dogs. They were aiming, in Emerson Tennent's words, "for the restoration of their national independence" (Emerson Tennent 1977: 1009).

During his tour, Tennent was able to visit various ancient sites, among them Sigiriya. He and his group were hindered from penetrating the ruined galleries at the foot of the rock towards the Mirror wall by "insufferable heat" and "the oppressive smell caused by the bats that inhabit them in thousands". Numbers of snakes and a bear added to the uncomfortable survey situation (Emerson Tennent 1977: 1018).

T.H. Blakesley from the Public Works Department was the first to conduct a more detailed survey of the site of Sigiriya, including

part of the remains of the reservoir south of the moat and the city rampart (Blakesley 1876: 53–62). Not being a trained historian, his dating and historical interpretation of the site were incorrect, but the plan is useful.

Surveys of ancient abandoned reservoirs and canals, once used for artificial irrigation, were undertaken from the latter half of the nineteenth century. The surveyors were either engaged in ordinary surveys for the one-inch map or later specific surveys of particular irrigation systems with a view to renovation. This was not a field of study for antiquarians. The Superintendent of Surveys, R.L. Brohier, wrote the first comprehensive report on the ancient irrigation systems in Sri Lanka in 1934 (Brohier 1979).

Was it, however, the representatives of the colonial power who were first to claim an interest in the ancient remains of the country? In Anduradhapura we note that pilgrims had been coming to the site long before the surveying and clearing work began, and that religious remains were reclaimed by Buddhist priests in parallel with the surveying and building of an Assistant Agency. The site of Sigiriya likewise formed part of a cultural landscape through time. The capital city was abandoned in 495 CE, but archaeology indicates various activities at the site until the thirteenth century. From the sixteenth century to the early nineteenth century (that is, until the British colonization) Sigiriya village and the surrounding settlements formed an outpost of the kingdom of Kandy according to the historical records. Surface finds of material from the seventeenth and eighteenth centuries have also been documented (Bandaranayake 1984: 6). The site was not only used in a physical sense. For a period of 700 years after the city was abandoned as a capital, it was reflected upon and formed a site of fame and glory in a secular sense in the minds of at least the literate elite from various parts of the island. This is evidenced by poems scribbled by visitors to the site between the fifth and thirteenth centuries.

The epigraphist, Archaeological Commissioner and first Research Professor of Archaeology in Sri Lanka Senarat Paranavitana copied, deciphered and interpreted 685 of the verses dating from the eighth to the tenth centuries. They were published in 1956 (Paranavitana 1983).

The overwhelming majority of the edited poems, 666 of them,

refer to the paintings on the rock above. Twelve of the published poems were written by women, and 556 certainly by men. Sixty-five different villages in four different provinces are mentioned as the places of origin of the visitors, and fifty family or clan names are given. More than thirty of these early tourists were monks and nuns. The others were clerks, physicians, superintendents of slaves, military officers, merchants, keepers of elephants, ladies and wives (Gooneratne et al. 1984: 219–228). But in the thirteenth century a visitor states:

I am Jalaka a drummer by profession, who visited this place (Priyanka 1990: 215).

So we can conclude that before the Archaeological Department was inaugurated in 1890 there were two types of remains documented by two different types of colonial representatives. The remains relating to religion and the elite were documented by military personnel and later on by high colonial civil servants; and the large-scale abandoned constructions for artificial irrigation from 300 BCE to 1200 CE were documented by the Survey and Public Works Departments. Some of the irrigation structures were left in an abandoned state, but some were renovated and especially the minor tanks and irrigation schemes in particular were often renovated without any, or any further, documentation (Myrdal-Runebjer 1996: 21).

The religious and elite-related buildings were later considered protected ancient monuments. To interpret the constructions with regard to dating and usage, antiquarians were in communication with representatives of the Buddhist sangha, and later with Orientalist researchers who had read and translated the Buddhist chronicles. The compilation of the chronicles has been dated from the fifth century CE (the Mahavamsa), up to and including 1815, when the British ousted the King of Kandy.

An Archaeological Commission was appointed in 1868 with the aim of investigating the possibilities of preserving the architectural structures of ancient monuments and of collecting copies of inscriptions. A programme for clearing ancient monuments and their surroundings in Anuradhapura was set up. Monuments with

stone carvings and sculptures came to light and personnel from the Public Works Department drew them to scale. In the following years, some government initiatives were made for the collection and study of ancient inscriptions, and the Ceylon Civil Service conducted excavations of buildings in the ancient cities of Anuradhapura and Polonnaruva, followed by conservation.

The former District Judge of Kegalle, H.C.P. Bell, was appointed Archaeological Commissioner in 1890, and his first task was to conduct a complete survey and excavation of Anuradhapura and to survey the remains on and around the Sigiriya rock.

No legislation governed the protection of ancient monuments until The Antiquities Ordinance was passed in 1900. It was considered ineffective since there were, for example, limited measures to prevent the unscientific restoration of monuments on private land. Privately owned monuments gained extended protection through the Antiquities Ordinance of 1940 and the Ordinance passed in 1955 facilitated the procedure for declaring an area an Archaeological Reserve (Silva 1969: 1162–1175).

In the *National Atlas of Sri Lanka* there are two maps presenting the acknowledged state of the art regarding the ancient cultural landscape: “Archaeological Sites and Monuments” and “Ancient Cities and Settlements” (*National Atlas of Sri Lanka* 1988: 56–59). These maps are discussed below.

A comparative perspective

To follow up on the suggested high and low resolution of archaeological field data and the social bias in data collection, an overview of the types of remains registered and mapped in Sweden and Sri Lanka through time is given below. Two tables are used to frame the question: “Archaeological sites” and “Architecture and cultural environment”. The designations of types of cultural remains are taken from the national atlases of Sweden (1994) and Sri Lanka (1988) respectively, and all designations from the respective works have been included.

Table 1. Archaeological sites.

Sweden: <i>National Atlas</i>	Sri Lanka: <i>National Atlas</i>
The History of the Settlement of Sweden	Prehistoric sites (67 sites)
Early Hunting Cultures	
Stone Age Peasants	
Bronze Age Settlement	
Rock Art (prehistoric)	
Graves and Iron Age settlement	Proto- and Early-Historic period (49 sites)
Agriculture during the Iron Age and Middle Ages	Settlements
Strongholds and Power	Cities Other urban cities
Runic inscriptions	Early Brahmi Inscriptions Early and Middle Historical Period Inscriptions
Routes and Fairways	Ports and important landing centres
The Cultural Landscape of Iron	
Traps and Trapping	
The Saami Cultural Environment	
Places of Sacrifices and Popular Belief	

What is of interest here are examples of social bias, in that there are remains appearing in the Swedish atlas which are absent from the Sri Lankan atlas. This is not because these types of remains are not to be found in Sri Lanka, but because they have not been included in what is considered to be “the heritage”. The tables further give examples of the low resolution of field data in the Sri Lankan case.

Table 1 shows archaeological sites that have been grouped together in the atlases under these specific designations. “The Cultural Landscape of Iron” is discussed as the first comparative example. In the Swedish atlas this refers to remains of direct and indirect production processes such as Iron Age furnaces, charcoal pits, slag heaps, and so on. Medieval mining, ironworks and tilt hammers, and nineteenth-century ironworks are included. As discussed in the introduction, there are such sites in the field in Sri Lanka, very

Table 2. Architecture and cultural environment.

Sweden: *National Atlas*

Our Churches

Buildings and Farms in Rural Areas

Castles, Palaces and Manor Houses

Public Buildings

Towns, The Cultural Environments of
Townships

Industrial Monuments

Place names

Sri Lanka: *National Atlas*

Dagoba (Buddhist)

Image-houses (Buddhist and Hindu)

Halls and dwellings of monks

Bathing places for royalty and monks

Hospitals

Sculpture: Buddha Image

Sculpture: Boddhisattva image

Other important Sculptures

Moonstone (first ornate step of a flight of
steps at the entrance of religious
buildings)Guardstones (decorated stones placed on
either side of the
Moonstone)Paintings (wall- and rockpaintings
of the historic period in elite or
religious contexts)

few of which have been registered, and even fewer excavated and dated.

During the British colonial period in South Asia, the then prevalent low-technology, direct process iron and steel production were commented upon, and documentation was undertaken, for example, in the course of mapping raw material resources. Af Geijerstam observes that in pre-1900 observations, approximately 150 locations of traditional iron production within the boundaries of today's India are mentioned (Geijerstam 2004: 71).

From the seventeenth century to the early twentieth century there

are many published accounts of low-technology iron and steel production in Sri Lanka (Knox 1981; Davy 1983; Baker 1983; Emerson Tennent 1977; Coomaraswamy 1956).

Until the 1990s, the dating of ancient iron or steel production was not related to the production sites (Ghosh 1990: 328). It was in settlement layers or burials where iron objects or indications of iron production were found and given dates. From the 1990s onwards, a growing number of iron production sites have been excavated in South Asia, including furnaces.

The generally agreed-upon beginning of iron production in India is now set in the fifteenth century BCE (Eran) and the thirteenth century BCE (Hallur), in the southern part of the country. By the sixth century BCE, iron was regularly used (Juleff 1998: 9). In Sri Lanka, tenth-century BCE slag contexts from the inner city of Anuradhapura constitute the earliest dating of inferred iron production, followed by the ninth-century BCE dating of slag contexts at the Aligala protohistoric site in the Sigiriya region (Juleff 1998: 14; Karunaratne and Adikari 1994: 58–60).

At the fifth-century BCE to fourth-century CE Proto-Early historic settlement site Ibbankatuva in Sri Lanka, furnaces were located within the settlement itself (Karunaratne 1994: 107–108). During the SARCP archaeological survey (1988–1994) of the Sigiriya hinterland (45×35 km), thirty-three iron production sites were registered. The late second-century BCE to fourth-century CE Dehigaha-āla-kanda iron production site south of Sigiriya represents a specialization, with its huge quantities of slag from the production of tens of thousands of tons of iron, and its unusually large furnaces of which only five have been excavated (Forenius & Solangaarachchi 1994: 140).

In Gillian Juleff's archaeological survey of a 78 km² area in the southern foothills of the Central Highlands in 1988–1996 two hundred and fifty sites were registered in an area that had been described as having little historical importance. Of these, one hundred and twenty-three were sites for the production of iron or crucible steel (wootz). The dates obtained represent ironworking in the area from the fourth century BCE to modern times (Juleff 1998: 53, 98). She remarks that, as there is a lack of field data, few models

have been developed to explain the nature and chronology of iron production in South Asia (Juleff 1998: 21).

During the latter half of the twentieth century, proto-historical and historical archaeology in Europe gathered round a close and comparative reading of particular spheres of production, such as the production of iron. From the 1960s to about 2005 the Comité pour la Sidérurgie Ancienne helped archaeologists to keep in contact and gather at conferences, building a “research environment”.

This type of research has helped to develop a more holistic view of premodern times in Europe. In Norway, for example, several large projects relating to outland use during the Iron Age and early medieval period have changed perceptions of the inhabitants, at that time on the periphery of what is now a part of Norway (Narmo 1997; Rundberget 2007).

The archaeology of iron production is now well established in Europe, but the Comité is no longer active. An Asian-based conference focusing on the technological development of metallurgy, the “Beginnings of the Use of Metals and Alloys” (BUMA), was founded in 1981 by a Chinese and an American archaeometallurgist. The conference meets every fourth to sixth year in Eastern Asia with a focus on the production and use of metals. It also takes an interest in cultural interactions and evolutions over time and space, especially between the West and Asia. Most certainly, this will enhance the possibilities of a future “global perspective” on history. Archaeologists from both India and the West have presented research on material from India.

“Traps and Trapping” in the Swedish atlas refers to remains of trapping pits for moose, reindeer and wolves. The spectrum of trapping devices includes permanent fishing devices. Documentation of hunting, trapping and fishing show that similar types of traps are found in Sri Lanka even today, for other kinds of animals of course. Trapping pits for deer and wild boar are examples of trapping devices which must have had a history and which are potentially visible after abandonment, though dating would constitute a problem (Myrdal-Runebjer & Yasapala 1994: 267–268).

About 3,000 registered inscriptions have been systematically documented in the field in Sri Lanka, which is clearly seen in the

wide distribution of registered sites for “Early Brahmi Inscriptions” and “Early and Middle Historical Period Inscriptions”. Similar types of remains (“Runic inscriptions” sixth to eleventh century, c. 3,400 registered) have been the focus of field documentation in Sweden since the seventeenth century.

To follow up on the suggested social bias, we may note “Routes and Fairways” in the Swedish atlas. This refers to ancient routes and roads still visible in the terrain, constructed roads with milestones, road maintenance stones marking the stretch of the road which were the responsibility of specific landowners, stone-arched bridges, transport canals, harbours, beacons, remains of fishing huts, light-houses and shipwreck sites. The Sri Lankan map lists “Ports and important landing centres”.

The entry “The Saami Cultural Environment” represents a start for acknowledging the remains of a traditionally non-agrarian and partly nomadic Fennoscandic population in the cultural heritage of Sweden. This practice started in earnest as late as the 1980s, and much fieldwork still remains to be done before coverage similar to other areas of Sweden will be reached. The remains listed are rectangular hearths, cot foundations, early Saami graves, Saami metal hoards and (forest Saami) fenced enclosures. For Sri Lanka there is no similar entry, but it would be possible to discuss whether or not the remains pertaining to the activities of semi-sedentary swidden, hunting and trapping communities could form the basis for such an entry.

“Places of Sacrifices and Popular Belief” lists votive offerings, prehistoric and protohistoric cult sites with traces of cult buildings, traditional names indicating cult activities, sacrificial and holy wells that are in use even now, in modern times. Apart from these, indications of cult practices or popular beliefs not related to the mainstream practices of the Christian church include guardian trees and heaps of votive stones or branches that are still being added to and thus are not exactly abandoned remains in the strict sense. Folklore and popular religion have been documented by ethnographers and social anthropologists in Sri Lanka, but although such practices will have had a history of development and change, the physical remains of the activities and place names indicative of specific locations for

practices are not included in the heritage on a par with, for example, “Image houses” (Table 2).

Field data is being gathered, though. By way of introduction, the project researching the Anuradhapura hinterland recognized the social bias of previous research which relied on the Buddhist chronicles (Coningham & Gunawardhana 2013: 10, see below). This awareness allowed analysis of new types of sites – for example dating the (end first millennium CE) non-Buddhist cult practices related to the terracotta objects of what has been termed the “Tabbova-Maradanmaduva-Culture” (Coningham & Gunawardhana 2013: 144–149, 464).

Turning to entries where similar types of remains found in both the Swedish and Sri Lankan lists, we start with settlements. “The History of the Settlement of Sweden”, “Early Hunting Cultures” and “Stone Age Peasants” have their equivalent in “Prehistoric sites” on the Sri Lankan map. The low resolution of field data is shown in the number of sites compiled for the map in the atlas, but “Stone-Age archaeology” in South Asia already has a global focus. Remains of human activities technologically related to the “Stone Age” are acknowledged and researched in Sri Lanka and in the rest of South Asia in cooperation with natural and environmental scientists (Deraniyagala 1992). Since the 1980s, a number of archaeological field studies in South Asia have approached the question of the domestication of cultivars and the dating and spatial development of the introduction of agriculture by using methods developed by the natural sciences (Kajale 1988; Premathilake 2003; Saxena et al. 2006; Pokharia 2008; Fuller 2006). This links up with a growing interest in the environmental history of Asia (see e.g. Grove et al. 1998). To date, no remains of “Stone Age Peasants” have been identified in Sri Lanka.

Turning to settlements dating from after the introduction of agriculture in Sri Lanka, a few words must be said on how to identify rural settlement sites in Sweden and Sri Lanka. In both countries, for most locations and periods, constructions such as houses on the settlement site itself will not be visible above ground. Thus other constructions which are visible above ground have been used to identify a settled area in both countries.

Constructions for burials have been used in both countries to identify areas settled during the iron-using, prehistoric period. “Graves and Iron Age settlement” on the Swedish map has its counterpart in “Proto- and Early-Historic period” on the Sri Lankan map. Again it is evident that the Sri Lankan data has a low resolution, and field research related to the period has only just begun.

The suggested social bias is even more visible in Table 2 relating to the architectural and environmental cultural heritage. The map and text of “Archaeological sites and Monuments” in Sri Lanka refers exclusively to the environment of the lay and religious elite, and to material culture related to institutionalized religious practices.⁷ In the *Swedish National Atlas*, “Buildings and Farms in Rural Areas”, “Public buildings”, “Industrial Monuments” and “Place names” are acknowledged parts of the cultural heritage.⁸

In Sweden, the number and variety of sites documented during the antiquities survey helps in planning for land use studies. Regarding the equivalent of the Historic Period (Late Iron Age and Medieval times in Sweden), several large projects over recent decades have approached the history from the village and rural household point of view (Svensson 1998; Andersson & Svensson 2002; Emanuelsson et al. 2003; Lagerstedt 2007; Svensson et al. 2013).

“Agriculture during the Iron Age and Middle Ages” in Sweden refers mainly to constructions on the ground related to the field systems or the movement of cattle from the village sites to pasture land, but also to the sites of the farmhouses. Most of the pre-medieval sites of buildings are not visible above ground, but are inferred from other contemporary remains or stray finds and documented through excavation. Though invisible above ground, they are protected monuments under the legislation.

Palaeobotanical remains from excavations in the central settlement of the citadel of Anuradhapura indicate that agriculture in the form of rice cultivation was practiced in the ninth to sixth centuries BCE (Deraniyagala 1992).

The entry “Settlements” in the Sri Lankan atlas is based on the location of irrigation works such as reservoirs and the c. 25,000 minor village tanks, and not on registered settlements sites. The time frame for this map is the Early and Middle Historic Period (300 BCE to

1300 CE). The source-critical aspects of the distribution thus shown are discussed by the author Senake Bandaranayake (Bandaranayake 1988: 58). They are: the lack of chronological control (1,000 years projected on one map); the “white spots” in the central mountains and in the southwest where rain-fed agriculture was undertaken; and in the north where well irrigation dominated.

An entry specifically highlighting the exceptional remains, nowadays often restored, of the huge pre-thirteenth century irrigation constructions would have added to their visibility in a global comparative perspective, especially if their chronology could be demonstrated. Included here are bunds of reservoirs with a storage capacity of up to 98.6 million m³ of water and transbasin canals up to 80 km long traversing an almost level plain. An entry describing the known *c.*25,000 village tanks, chronologically ordered, would put the large structures into context. As has been observed above, field documentation of such remains has almost exclusively been the work of surveyors and irrigation engineers. Historians, not archaeologists, have tried to interpret the constructions from the perspectives of technological development and social context (Gunawardana 1971, 1978, 1979, and 1982; Siriweera 1978, 1986, 1989).

As shown in a few case-studies, mapping and sampling the tank and canal bunds and their bottom sediments gives opportunities for dating the construction and utilization phase (-es) of the irrigation works (Abeyratne 1990: 19–29; Deraniyagala 1992: 732–733; Myrdal-Runebjerg 1996: 128–133; Risberg, Myrdal-Runebjerg & Miller 2002: 41; Gilliland et al. 2013: 1024). It would thus be possible to develop a field practice for identifying, registering and gaining a spatial overview of irrigated agriculture through time.

Apart from the “low resolution of field data” there is also the problem of the “social bias”. Questions of different types of agrarian land use have been raised in South Asia, also based on natural scientific methods, but a close reading is lacking when it comes to the relations between the human beings who once created what is now the archaeologists’ material (Petralgia & Allchin 2007; Kingwell-Banham & Fuller 2011). One of the few examples of palaeo-ecological research combined with the latter type of study in India, is the close reading of the agricultural landscape surrounding the city

of Vijayanagara in Karnataka, India, c.1330–1650 CE (Morrison 2000; Morrison 2009).

The author of the Ancient settlement map was one of the leading archaeologists in Sri Lanka in post-colonial times, Professor Senake Bandaranayake. Regarding previous priorities in historical and archaeological research relating to the Early and Middle Historical Period (300 BCE to 1300 CE) he remarked in 1990 that it had been “largely confined to the study of structural remains, royal and official inscriptions and political and religious history – almost all of it relating to the apex or superstructure of the historical society.” (Bandaranayake 1990: 15).

On Bandaranayake’s initiative a multidisciplinary archaeological project (SARCP) was run from 1988 to 1994 “to investigate the settlements and settlement network of a small but representative part of the archaeological landscape of the northern Dry Zone, the principal area of settlement during the Anuradhapura and Polonnaruwa periods”.⁹ He defined the project as an experiment of the archaeology of the village, focusing “the vast rural base” (Bandaranayake 1990: 15). A number of research projects with similar broader perspectives have been undertaken since then, not only in Sri Lanka but in other parts of South Asia.

The field results furthermore made it clear that the cultural landscape had been in a state of flux. Settlements moved, and tank bunds built by cultural layers from previous settlements were abandoned. There were also the remains of paddy-fields, some in the beds of dry village tanks now in scrub jungle, and some on land in other succession stages of the swidden cultivation landscape (Myrdal-Runebjer 1996: 72). The presumed depopulation and/or migration during the thirteenth century, parallel to the abandonment of the large-scale irrigation structures, has been mentioned above. Together with these earlier indications of abandonment, there are some texts from later centuries that could help to formulate questions for field research pertaining to the abandonment of settlements.

The seventeenth-century remark by Robert Knox regarding the Kingdom of Kandy may be noted. He writes that “[t]owns ... lie desolate, occasioned by their voluntary forsaken them.” Adding that “some will sometime come back and re-assume their Lands again”

(Knox 1981: 102). Here he mentions sickness and superstition as reasons for moving away, but further on in the text he states: “But if any find the Duty to be heavy, or too much for them, they may leaving their House and Land, be free from the King’s Service, as there is a Multitude do.” (Knox 1981: 167).

Alternative food procurement strategies as indicated in this and later texts from the nineteenth century, including the Sigiriya settlement study itself, have been swidden, hunting and trapping, and harvesting other forest produce such as honey (Myrdal-Runebjer 1994: 246–260). The un-stratified village life as envisioned by twentieth century historians in the “tank-stupa-village synchronism” seems questionable, considering the seventeenth-century observation that people who did not have good paddy-land “are fain to sow on dry Land, and Till other mens Fields for a subsistence” (Knox 1981: 167).

John Davy described the methods used by the British army in the crushing of the rebellion of 1817–1818. These included the burning of houses, chopping down of fruit trees, and putting to death of “all who made opposition” (Davy 1983: 246f). In 1819 he travelled through the country noting deserted fields, houses in ruin, depopulated villages and famine (Davy 1983: 298–302, 319).

We might further reflect over the remark made in 1833 by Thomas Skinner, the Commissioner of Public Works that the population of the Anuradhapura region (Nuwarakalawiya) was rapidly decreasing due to disease and drought (Farmer 1976: 11). Thus to obtain a chronology of settlements in this landscape in a state of flux would be one important step towards understanding its history from a village point of view.

The research developed in the Sigiriya hinterland from 1990 was followed by hinterland studies around the ancient city of Anuradhapura (Coningham et al. 2007; Coningham & Gunawardhana 2013). Beginning in 2005, hundreds of sites were documented and a selected number excavated and dated. An overall dating of the usage phase of irrigation facilities was obtained, approximately following the dating of the development of the central settlement, Anuradhapura (Gilliand et al. 2013). The approach further made it possible to introduce the concept of “change” in the analyses of the Anuradhapura hinterland (Coningham & Gunawardhana 2013: 464–468).

So what is standing in the way of a mainstream reorientation in the field and, regarding societal interpretation, putting “the apex or superstructure of the historical society” into context?

Another senior Sri Lankan archaeologist, Professor Sudarshan Seneviratne, has commented on how available data has been interpreted. He identifies as a problem the fact that the narrative of the “peopling of the island” and “the emergence of civilization” related in the Buddhist chronicles were accepted at face value “by the Orientalists, the Antiquarians, the colonial administrators, and the twentieth-century Nationalists.” This he argues has resulted in “an ethno-religious history with a sectarian bias.” (Seneviratne 1996: 266).

Without a critical reading of ground truth, settlement studies could fit well in to this view of the Sri Lankan past. Seneviratne notes that “*the tank-stupa-village synchronism*” has been identified as the roots of a “national culture” which requires preservation and perpetuation and is thus not to be questioned (Seneviratne 1996: 273).

One hypothesis might be that this perspective fits well into the view of the world inherited from the Orientalist tradition by many researchers in Europe and North America. Thus the emerging picture does not “ask for” further questions among the dominant researchers of either research community.

Concluding discussion

It is not surprising that there will be different types of remains in Sweden and Sri Lanka. Sri Lanka has been inhabited for almost three times as long as Sweden. Sweden has been thinly populated through time, rural, and, up until the nineteenth century, very poor, whereas Sri Lanka shows clear indications of early high population density areas, early central settlements and an affluent lay and religious elite, consuming monumental architecture, but not constructing it themselves. Sweden is located in the boreal forest, stretching up to and beyond the Polar circle, on the northwestern periphery of earlier major areas of high population density, far from the ancient trade-route highways. Sri Lanka, on the other hand, is located just north of the equator, directly on the early marine highway across the Indian Ocean. One may also note that in Sweden, Christian

churches belong to the architectural heritage, and in Sri Lanka, Buddhist stupas and Hindu temples. But this is not the focus of the comparative perspective discussed in this chapter.

What has been discussed above is how a historically determined difference in data capture creates a problem for research with a world-history-perspective. Examples were taken from Europe (Sweden) and South Asia (Sri Lanka).

Two problems were identified in this chapter: high and low resolution of field data and a social bias in selection of sites for documentation. The expression high and low resolution of field data relates to a difference in degree of coverage in the respective areas of similar types of activities relevant for world history researchers. The “social bias” relates to a disregard of the primary producers in historical archaeological research in South Asia.

One disturbing result of this social bias is how the creative, reflective, active local population come forward in recent interpretations of the archaeological record in Europe, in contrast to the anonymous, silent contributors to the affluence and glory of kings and religious elites (and colonial powers) in South Asia.

Archaeological and palaeo-ecological field data has increased during the past four decades in Northern Europe. It includes data with a bearing on social organization and, hence, differential access to various resources, including the labour of others. It also includes a greater emphasis on research regarding rural settlements and production sites. Thus the possibility has increased to also understand the social dynamics behind changing land use patterns.

When these types of field data have been collected in South Asia, the same questions are seldom asked. When scholars do ask such additional questions, and even succeed in showing a picture differing from the one inherited from the Orientalist research tradition, the work is not taken up as part of the grand narrative or the mainstream, commonly agreed upon, story.

This being systematically so, what has been attempted here is not primarily to suggest types of fieldwork that could help fill the lacunae seen in comparisons to the rich archaeological database for settlement and land use history in Western Eurasia. It is not a lack of

awareness among the archaeological community in South Asia that constitutes the problem, as the Sri Lankan examples showed. The aim is instead to underline the necessity of being on source-critical red alert when attempting to write global history. This necessity exists irrespective of whether the focus is on the environmental impact of land use or on human society as such. The social context of data collection in colonized countries during the nineteenth and twentieth centuries constitutes a heavy legacy. The incorporation of preconceived colonial ideas of class and gender in national research agendas today does not make the task any easier.

If secondhand source consumers such as the writers of global history were to take into account the reports from the scattered but more holistic projects and the published suggestions of alternative research agendas, an account of the inequivalent sets of data could at least be presented openly and explicitly.

Notes

- 1 Throughout the text “Sweden” signifies the area within this state’s present-day borders. Sri Lanka is now an island, and its area has been constant within the time limits and resolution of interest in the present discussion.
- 2 Even if the interpretation that “Sweden” is marked in the VIIth climatic zone on Al-Idrisi’s map (which has not survived as an original) is accepted, the distorted geography represented on this part of the map and lack of place-names still makes the point valid. This part of the world was not a focus of the Mediterranean world in the twelfth century.
- 3 Later maps produced during the Portuguese colonial rule such as those by Plancius (1592) and General Constantine de Saa Noronha (early seventeenth century) also mentioned that the southern part of the country had been depopulated by “sickness” 300 years earlier. These maps have been cited to in the discussions of the causes of “the downfall of the Rajarata civilization”.
- 4 See for example Baudou 2001; Bertilsson & Winberg 1978; Selinge 1989.
- 5 And in the southwest of Sweden by archaeologists during the so-called “Göteborgsinventeringen” 1880–1923. The argument for the initial decision not to include them on the Economic map was the lack of visible boundaries for Stone Age settlement sites (Selinge 1989: 17).
- 6 The survey ended in 1995.
- 7 The entry “Hospitals” in Sri Lanka should be noted in a comparative perspective. It represents the much earlier development of institutions for medical treatment in Sri Lanka as compared to Sweden.
- 8 A Swede with a critical mind might reflect over the entry “Our churches” in the

National atlas. Who are “we” and what sets the churches apart from rural farms in rural areas, public buildings and industrial monuments?

9 c. 500 BCE–1300 CE.

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What can be understood, compared, and counted as context?

Studying lawmaking in world history

Arne Jarrick & Maria Wallenberg Bondesson

Why laws?

The comparative study of law and its history has been on the research agenda for quite some centuries, or even millennia. Confucius, Grotius, Pufendorf, Locke, Montesquieu, Bentham, Maine, Marx, Spencer, Weber – all made important contributions to our understanding of law-making and the societal context in which it has always been immersed (Glenn 2010; Pospisil 1971; Maine 2012; Montesquieu 1989). Or consider the work of less well-known modern or contemporary scholars, such as Anners, Hart, Berman, Glenn, Pospisil, Luhman, Menski, Benton and many others – and one cannot but be impressed (Anners 1975; Anners 1980; Hart 1997; Berman 1983; Berman 2003; Pospisil 1971; Benton 2002; Luhmann 2004; Menski 2006).

However, bold as they were in generalizing and theorizing their findings, only rarely did scholars of the past take the pains to carry out the systematic empirical groundwork necessary to establish the validity of their ingenious thoughts. Some did, of course, but many did not (Maine 2012).

Max Weber, who indeed made heroic comparative efforts to come to grips with the history of lawmaking around the world, did not do so with sufficient rigor to allow his successors to actually assess the credibility of his findings. In his studies of laws in ancient China, for example, he was so imprecise that it is all too often impossible

to really tell which of the codes he was referring to (Weber [Rhein-stein] 1954: 54, 184–186, 236–237, 242, 264). Perhaps today’s legal scholars are more meticulous in carrying out their empirical work than Weber and others were. On the other hand, only a few seem to be engaging in the sort of large-scale global comparative work that some of their predecessors could not resist attempting. Consequently, to date, systematic, large-scale, empirically grounded, historical generalizations about lawmaking are still as needed as they are wanting (Duve 2013: 21).

Our ambition is to redress these shortcomings. To this end we are presently about to complete an empirically grounded, worldwide, long-term comparative history of lawmaking. By law we mean a set of officially authorized regulations of the social interaction among a certain agglomeration of humans who are subjected to the authorized rule of those in charge of setting the regulations up. Essentially, this empirical, long-term approach is nothing new to us. What is new is the global comparative approach.

On many occasions, when we have presented our ambition and our preliminary results, quite a few researchers have questioned whether it is really possible to trace trajectories of general changes in lawmaking – if not any aspect of long-term societal change (Jarrick 2003; Jarrick 2007). Methodologically, they base this criticism on questioning if we are in a position to properly understand the meaning and content, as well as the form, of laws from a distant past and from distant cultures. Accordingly, they have also expressed strong doubts as to whether such “entities” can be compared with one another in any meaningful way, a skepticism extended to our basic idea of how to carry out contextualizing work. Since doubts about intelligibility, comparability and contextualization are so often voiced among fellow scholars, such concerns have to be addressed, especially since we consider them highly unwarranted. This is precisely what we do in this chapter, which is a detailed advocacy of the possibility of understanding manifestations from seemingly alien cultures.

However, before presenting our argument in detail, we need to introduce the framework: our overarching aim, as well as the sources used and considered to be comprehensible. Having presented our

defense of the high potential of comparing laws from different cultures, we proceed to a concise presentation of our methodological design and of some of our major substantial results.

Our overarching aim is to improve our understanding of the dynamic forces behind the uniquely human evolution of culture. Human culture is the incessantly ongoing interplay between creativity, innovation and transmission, constituting itself as a seemingly never-ending transformation of society, despite the relatively stable genetic set-up of its human agents. The general process of cultural evolution is nothing but a series of mutually intertwined concrete processes, of which different layers and types of lawmaking in different periods and corners of the world in particular constitute a profound aspect. This is precisely why lawmaking is our specific scientific target. We could have picked other processes for scrutiny, but for various reasons laws serve us incredibly well. Why?

In order to follow processes as prolonged as possible in human history, we need extended time series covering as many different and dissimilar parts of the world as possible. Therefore, written laws are especially useful: they have existed and been partly or entirely preserved for a period of more than 4,000 years. Furthermore, the way they are composed enables us to compare them reasonably well. Laws are optimal also because they signify an *explicit* regulation of human interaction, and testify to a conscious attempt at a *lasting* regulation of this interaction. It is also of relevance that the legislators displayed an ability to handle the non-present, which is at the core of what makes humans unique in the world of living species.

What we do

Thus, what is set out here is a comparative, essentially global and long-term study, still in progress. Also, differences in the amount of legal material preserved from different areas have made certain prioritizations necessary. We are also mostly confined to the use of translations of the original languages. Taking all this into account, we have selected certain geographical core areas, whose legal development can be charted in detail and for long periods of time. Thus

far, these are West Asia, China, France and the Nordic countries of Denmark, Norway and Sweden.

Primarily we have sought to accomplish a global selection of legal cultures of different types, from societies with different political, social and religious features and organization. To achieve this, we have prioritized mutually independent legal cultures: that is, if a number of legal cultures were available for study in a specific geographic area, we have selected those which were most ancient or least dependent on previous neighboring legal cultures.

From the first of these core areas, the earliest legal codes and collections were produced in Sumerian city-states in the period around the millennium 2000 BCE. These codes – which are quite poorly preserved – were followed by codes produced in the dawning empires of the Old Babylonian period (from the eighteenth century BCE). Among these codes are the famous *Laws of Hammurabi*, the first code of this area to be preserved almost completely. Later, codes were also produced, for example, by the rulers of the Hittite Empire (1650–1180 BCE), and the Kingdom of Israel. To later periods in the history of this area belong the further development of Israelite law (in the form of, for example, the *Mishnah*, the *Talmud* and the *Mishneh Torah*) and the advent and development of Islamic law (among others Roth 1997; Westbrook & Beckman 2003; Neusner 1988; Maimonides 1949; Glenn 2010: 99–132, 181–236 and Twersky 1980).

In China, legislative activity was continuous and extensive from at least the third century BCE, although the first Chinese law codes are known either only by name (e.g. Head & Wang 2005), or preserved only in part (the codes of the Qin and Han dynasties). The first Chinese code of law that has come down to us in full is the *Tang Code* (of either 653 or 737). Promulgation of codes was often associated with the assumption of power by new dynasties and the earlier stages of their rule, although many revisions sometimes followed before a final version was established. Thus codes of law were promulgated early in the Song (960–1279), Yüan/Mongol (1279–1368), Ming (1368–1644) and Qing dynasties, with the final version of the *Qing Code* (of 1740) remaining in force until the collapse of the Empire in 1912. However, there were also other

types of legislation in ancient China than the law represented by these “national”, general codes, such as administrative rules (e.g. Head & Wang 2005; Jones 1994; Jiang 2005).

In the regions of present-day France, legal history can be said to begin with Roman law, which over time and in various ways, was built on the distinction between what was common and not common to people subjected to Roman rule. For the pre-imperial period this has been described either as a distinction between the principle of specific *rights* attributed to local peoples (*jus civile*), whether Roman or non-Roman, and different types of *legal actions* (*jus honorarium*), or as *jus civile* working alongside *jus gentium*, i.e. laws common to all – at least to all Italian communities, again, whether recognized as citizens of Rome or not (Ando 2011: 2–3).

In the imperial period the actions system was pushed aside, and by the Dominate the Emperors had definitively taken over both legislation and the execution of justice. In the later Empire, Roman law consisted primarily of the legal codes of the Emperors Theodosius (r. 379–395) and Justinian (r. 527–565), containing the laws of the Roman Emperors from the year 312 in revised form, and more informal Roman provincial law. Laws appointed for humankind as a whole were called *jus naturale* (Ankarloo 1994: 15–18, 54–61; Maine 2012: 44–53; Watson 1985; Fisher Drew 1991; Tellegen-Couperus 2012).

The rulers of the Germanic confederations that assumed power after the fall of Roman Gaul quickly took up the legislative activity of the Roman Emperors. Under the Merovingian and Carolingian rulers a large number of nations and provinces in the former Roman West received their own legal codes. These codes were supplemented by so-called *capitularies*, short collections of legal provisions divided into *capitula*: headings or chapters (Wormald 1999: 1–43; Wormald 2003; Wood 1994: 102–119; Wood 1993; Fisher Drew 1991; Rivers 1986). However, with the decline of the Carolingian dynasty in the ninth century, the production of legislation in the form of both codes and capitularies ceased. Under the Capetian dynasty (987–1328), France was ruled through unwritten provincial law, more or less influenced by “rediscovered” Roman law. The next spurt of written legislation in France came when these

legal traditions were given written form in the so-called *coutumiers* (recordings of customary law) from the thirteenth century and into early modern times (Akehurst 1992: xiii–xxxii; Akehurst 1996: xxi–xliv; Friedland 2012: 46–52; Cohen 1993; Berman 1983). From early modern times, national legislation emerged and grew in scope, for example in the form of a substantial ordinance governing civil and criminal procedure in 1670. Finally, in the late eighteenth and early nineteenth centuries, extensive legal codes were created, first by the revolutionary government and, only a little over a decade later, under Napoleon (Anners 1980; Friedland 2012: 47).

In the Nordic countries of Denmark, Norway and Sweden, written legislation first appears in the Middle Ages. The oldest texts of this kind are probably the remnants of the early twelfth century Norwegian laws of the Eidsivating and Borgarting (Sigurdsson, Pedersen & Berge 2008; Tamm 2011: 15; Hoff 1997). In Denmark and Sweden, codifications of the same kind are preserved from the decades following the year 1200 (Brink 1996; Hoff 1997; Ekholst 2009).

About twenty provincial codes were compiled in total in these three countries. However, from the latter part of the thirteenth century in Norway and the middle of the fourteenth century in Sweden, national codes were introduced. The four Danish provincial codes, however, continued to apply until the 1680s. In Sweden, the first national code was promulgated by King Magnus Eriksson, sometime in the period 1347–1352. About a hundred years later, the code of King Magnus was revised and reissued, although the earlier version continued to be used well into the sixteenth century. In Norway and Denmark, new national codes were issued during the 1680s, while it took until 1736 until the Medieval codes were finally replaced in Sweden by a new code in 1743: *1734 års lag* (Tamm 2011: 15–18; Gelting 2011: 92–94; Andersen 2011: 121; Collin & Schlyter 1869: LXXXIV; Hoff 1997; Ekholst 2009).

The study of the legal development of these “core areas” is supplemented with analyses of material from other areas, primarily the British Isles, from which there exists a comprehensive series of legal codes, from, about 600 (Attenborough 1922; Robertson 1925);

Russia (from *Russkaia Pravda* to the *Ulozhenie of 1649*, see Kaiser 1992; Hellie 1988); and India, primarily the *Dharmasutras* and the *Laws of Manu* (Olivelle 2000; Olivelle 2004).

Why some say that we cannot do it and we insist that we can

Below we will demonstrate that it is indeed practicable to make intelligible, and thereby to compare and contextualize, law codes from cultures separated from each other by huge temporal and geographical distances. For the sake of analytical clarity, we will address the intelligibility, comparability and contextualization as three separate problems, despite being fully aware of the fact that they are intimately intertwined.

Intelligibility

First, can we truly claim that we are in a position to discern what is really meant in thousands of different regulations from such dissimilar laws as the ones that we have studied?

Here the importance of the translations of critical editions of the codes, with their often very comprehensive commentaries, must be emphasized. In fact, it is the appearance of a great number of legal codes in modern translations during the last thirty years or so that has made truly global comparisons of this type possible at all. To return to the issue of comparability proper, a theoretical approach to the issue is not as helpful as a discussion of concrete cases. Below we will share our reflections on some illustrative cases – from the most easily decoded, through those that need some decoding, to the very few that really puzzle us.

Almost all law codes, wherever and whenever they have been laid down, include regulations on non-lethal violence (as well as on lethal violence, of course). For example, in *Pactus legis Salicae*, a Frankish law from about 500 CE, it is stated that if

anyone mutilates another's hand or foot, or knocks out an eye, or cuts off an ear or cuts off a nose ... let him be held liable for 4,000 denarii (*Pactus Legis Salicae*: 29.1, Rivers 1986).

It is crystal clear that the above regulation deals with injuries caused by someone's physical violence against someone else; that the body parts affected cannot be anything other than precisely foot, eye, ear and nose; and that these are body parts on human beings and no other species. The sanction is also explicitly described, so as to leave no doubt either about the type of punishment or the amount of money prescribed as compensation for the injury suffered. Furthermore, the regulation also makes sense also from the vantage point of our contemporary world, where similar regulations are commonplace, except that in our time of volatile prices the amount of money would not be cast in stone.

One might consider it too easy to show that we are capable of decoding a law that is hardly in need of interpretation at all, a law, what is more, from our own cultural sphere, albeit at some temporal distance from the present. However, many regulations from cultures more distant in time and space are as easily understood as the above example. Let us illustrate this with the 2,000-year-old *Code of Manu* from India, laid down in a culture profoundly different from the Western World (Maisels 1999: ch. 4). This example is about stealing and other deeds, which the lawmaker in a typically casuistic way associates with other kinds of misbehavior:

A man who steals a rope or a bucket from a well or tears down a place for distributing water should pay a fine of 1 Masa and restore that article (*Law Code of Manu*: 8.319, Olivelle 2004).

What might be concealed here we cannot tell. We cannot even see a cultural gap to bridge in this example, and, indeed, the *Code of Manu* abounds in such regulations, although quite a few are also far less intelligible than this (see below). Such cases are by far the most common ones. Our point is that in such cases we can dispense with knowledge about the local context and still reach an understanding of what was intended with a certain regulation.

Of course, this is not always the case. Frequently, we come across less easily decoded laws. A first illustration of this can be taken from Salian law. As with the previous quotation from the *Pactus*, the following rule is quite clear in regard to *what* is meant, i.e. the mean-

ing of what is outlawed and the sanction that follows transgression, whereas the *why* of it is far from obvious without knowledge of the local cultural context:

if anyone shears the long hair of a free-born boy without his parents' consent ... and it can be proven that he did this, let him be held liable for 1800 denarii (*Pactus legis Salicae*: 24.2, Rivers 1986).

Still, whatever might have been the normative rationale behind this restriction, there is no ambiguity in the message as such. The same applies to the following example, from *Leviticus* of the Biblical Law:

If any of the Israelites slaughters a bull, a lamb or a goat within or outside the camp, without in advance bringing the animal to the entrance of the tent of revelation, in order to give it as a sacrifice to the Lord at his abiding place, this he will be held accounted for as a blood sin. He has shed blood; that man shall be expelled from his people (*Leviticus*: 17.3–4, *Bibeln* 2000, our transl.).

However, being rather familiar with the local Biblical context, we find it even less strange than the previous example. On the other hand, since the sacralization of places is common in almost all confessional systems, it is perhaps not that local after all, and therefore likely to be properly understood by almost anyone coming across it whether familiar with the Old Testament tradition or not.

The next extract from *Leviticus* may, however, seem somewhat more puzzling, although even here one would have no trouble understanding how properly to comply with the law, if one imagines being placed in a cultural setting where one is supposed to do so:

You must not let two animals of different species mate. You must not let two kinds of grain grow in your field. You must not wear clothes that are woven of two different kinds of yarn (*Leviticus*: 19.19, *Bibeln* 2000, our transl.).

However, even though it might be beyond our reach to truly grasp the specific linkages between these three pairs, with mutually excluding

species (animals, grains) and artefacts (yarn), never to intermix, on the meta-level this way of thinking resembles mindsets that we are already familiar with. It is not too far from the exogamy law and the incest taboo in mating that we recognize in most cultures from different periods of history, although here it has been extended in a rather unintelligible way.

We close this series of cases along the gradient of intelligibility with one example that from our vantage point seems even more difficult to grasp than the cases above, from the Indian *Code of Manu*. It is about rules of conduct for what is here called the Bath-Graduate (religious graduate), and particularly about the relation between telling the truth and saying what is pleasant:

He [the Bath-Graduate] should say what is true, and he should say what is pleasant; he should not say what is true but unpleasant, and he should not say what is pleasant but untrue—that is the eternal Law (*Law Code of Manu*: 4.138, Olivelle 2004).

The logical implication of this rule of conduct seems to be that the Bath-Graduate should only tell pleasant truths and otherwise stay silent. However, what mystifies us greatly in this message is what is said in the following sentence, namely that he “should call a lucky thing ‘Lucky’; or rather he should call everything ‘Lucky’” (*Law Code of Manu*: 4:139, Olivelle 2004). Everything – whether true or not? Pleasantness overriding the truth, being the basic norm? How should the poor individual sort this out: stay silent about unpleasant truths or convert them into something pleasant? Or should we rather interpret the request for silence as actually conveying a meaning: silence as a way to communicate disapproval? This we cannot tell.

The following passage from the same code – still concerning rules of conduct for the Bath-Graduate – is also obscure, but in another way: we have not managed to grasp what they have in common:

He must never blow on a fire with his mouth; look at a woman when she is naked; throw anything filthy into a fire; warm his feet over it; place it under his bed; step over it; place it by his feet; hurt

living creatures; eat, travel, or sleep during the time of twilight; scribble on the ground; take off his own garland; deposit urine, excrement, sputum, blood, poison, or anything smeared with filth in water; sleep alone in an abandoned house; awaken a sleeping superior; speak with a menstruating woman; or go to a sacrifice uninvited (*Law Code of Manu*: 4:53–57, Olivelle 2004).

Obviously, much in this passage shows an urge to avoid everything filthy, but not all of it. Why all these things have been linked is not apparent to us. Certainly, some of this law might make sense if we became familiar with the local context. In this case we do feel somewhat lost.

The last example is exceptional. It is unusually unintelligible – although not completely so. So in settling the issue of the intelligibility of laws, aided by the translators' commentaries, we maintain that we are able to understand the intended meaning of the bulk of the regulations in the laws at issue here.

Given that this conclusion drawn here could be generalized, why should this be the case? Why does it seem that the basic meaning of legal regulations can be properly communicated over huge cultural and temporal distances? In general, it is so because humankind is a communicative species whose members normally want to make themselves understood, and on the whole are not waging Machiavellian tug-of-wars of mystification against each other (Gärdenfors 2000: 102 ff.; Laland & Brown 2002: 166). This is particularly the case with lawmakers, who have had very good reasons to make their laws as clearly understandable as possible in order to instill obedience among their subjects.

Comparability

Here, our intention is to argue that ours is a worthwhile undertaking. Therefore, let us briefly discuss two pairs of examples where it is possible to interrelate otherwise unrelated laws.

The first example concerns a case where the same type of action is treated in radically different ways. It is about marriage between a male slave and a free woman. According to the 3,500-year-old *Hittite*

Laws, such a relationship was considered legal, although obviously in need of regulation:

If a male slave [takes] a [free] woman in marriage, [and they make a home and children, when they divide their house], they shall divide their possessions [equally, and the free woman shall take] most of [the children,] with [the male slave taking] one child (*The Hittite Laws*: 32, Roth 1997).

In a Medieval Frankish capitulary, the case was quite the contrary, and a misalliance of this sort was harshly punished:

If a woman unites in marriage with her slave, let the public treasury acquire all her property and let her be outlawed ... Let that slave endure the worst death by torture, that is, let him be broken on the wheel (Merovingian capitulary, *Capitulary III*: 98, Rivers 1986).

In our view, the above example would demonstrate the comparability of the two cases, provided that slaves were socially distinguished in roughly the same way in the Hittite culture as in Frankish culture 2,000 years later. If so, they would commonly have been treated as property, i.e. been subject to purchasing and selling. This was certainly the case in both these cultures, though in passing we would add that this far from exhausts their role. To the degree that they could be held responsible for their misdeeds, put to trial, convicted, and suffer punishment, they were also treated as humans, unlike other items sold at the market (Darnton 1984: ch. 1; Friedman 1985: 218–229).

As with the examples above, in terms of type of action and of sanction, the examples below concern two rather similar cases, also 2,000 years apart. They concern theft, in Old Babylonia of a plough and in Medieval France of vegetables:

If a man steals a plow from the common irrigated area, he shall give 5 shekels of silver to the owner of the plow (*Laws of Hammurabi*: 259, Roth 1997).

If anyone thievishly enters another's garden, or turnip-, bean-, pea-, or lentil-patch, or steals [something there], and it can be proven that he did this, let him be held liable for 600 denarii ... in addition to its value and a fine for the loss of its use (*Pactus legis Salicae*: 27.7, Rivers 1986).

These cases are not as perfectly analogous as the marriage cases discussed above. However, both can be considered economic crimes, both concern stealing, and in both cases the sanction stipulated is compensation to be paid to the owner. Thus, it is valid to subsume both of them under rather general categories without becoming conceptually distorted.

In conclusion, we would say that the comparative part of our work is as possible to carry out as the decoding part, although we know perfectly well that in some instances neither really works.

Contextualization

It might be claimed that the intertextual comparison of clauses, i.e. disconnecting them from the intratextual environment of other clauses, is an assault on their intended meaning. Is it not indispensable to see them as integrative parts of a whole? Yes and no. As we will see, it is necessary to apply different arts of reading – both the holistic and the analytical. Among other things, a holistic approach requires validation of the intratextual representativity of a certain passage, meaning that one tries to make sure that the interpretation of this passage is not violated by the meanings of other parts – unless intended so by the (assumed) author. Once validated, it is equally legitimate to try to establish the meaning and significance of the passage in question by also applying intertextual contextualization (Jarrick 2002: 133–146). Moreover, having studied a series of law codes from the very first to the very last ordinance, we have found that it is not always true that a text is a genuinely integrative whole. Actually, the history of lawmaking may even be perceived as a secular striving towards textual coherence and integration.

Some scholars have been concerned about the absence of considerations of the local cultural context in the societies where the laws

under scrutiny were laid down. These are completely reasonable concerns. In addition, it should be pointed out that variation is a profound feature of all phenomena in the world. Without diversity, neither cultural nor natural evolution would take place. So this is simply something that has to be taken into account, in order to serve as a critical test of the explanatory power of causal reasoning.

The *Tang Code* could serve as case in point. In order to determine the degree of harshness in the many seemingly draconic sentences in this code, it is essential to know that if conviction was not obtained, false accusation rendered the complainant as harsh a punishment as was meant for the originally accused (Johnson 1997: 6). Thus draconian sentences were not only intended to deter potential criminals from transgressing the law, but also to discourage accusation abuse among law-abiding people, thus implying a subtext of a lower degree of harshness than one might be inclined to conclude at first sight. It seems to have been only slightly less dangerous to frivolously voice suspicions of other people's transgressions, than to be a perpetrator oneself. Applying our terminology, one could say that this is part of the intratextual context.

This should, however, not be interpreted as if the lawmaking authorities in ancient China did not consider certain transgressions serious misdeeds deserving tough sanctions. Taking the extratextual local (= Chinese) context into account, one would be inclined to explain this by pointing to the fact that "the *Code* was regarded as the last means by which to protect society when all other attempts ... had failed" (Johnson 1997: 5). The basic philosophical pillar upon which laws were erected was the profound need to preserve the harmony between man and nature, which could be disrupted by overly heavy punishments or by sentencing people at the wrong season (Johnson 1979: 10, 15). Knowledge of such peculiarly Chinese circumstances is crucial for understanding and explaining other, similarly unique features of Chinese law. And yet, the explanatory value of this rests with a comparative contextualization making it plausible that similar conditions in other societies did have similar effects (or that dissimilar conditions did not).

Yet, in this project, although they are analyzed, local cultures are not at the core of our contextualizing efforts. Rather, the major

context of each of the laws studied *is the other laws* studied. The justification for this is that our overarching aim has been to identify and explain general trajectories of lawmaking, making it indispensable to compare laws from clearly distinct cultural and historical contexts. This has enabled us to establish the overall presence or evolution of certain general processes or attitudes, but it has also forced us to rule out some hypothetical generalizations. And this is also contextualization! Again, by this measure we can test, modify, or even falsify, explanations built only on local context – explanations that too often have ignored the fact that the same phenomena emerged in very different cultural settings (for example Berman 1983).

Below, we will illustrate this point with two pairs of simple examples. The first case concerns a husband's responsibilities towards his wife and what she would be entitled to if he were away on business. Here the *The Code of Manu* states that a

man should provide for his wife before he goes away on business, for even a steadfast woman will go astray when starved for a livelihood. If he provides for her before going away, she should live a life of restraint; but if he leaves without providing for her, she may maintain herself by engaging in respectable crafts (*Law Code of Manu*: 9:74–75, Olivelle 2004).

In the *Laws of Hammurabi*, compiled about 1,500 years before the *Code of Manu* appeared, in a culture quite distinct from it, the same topic is treated in the following way:

If a man should be captured and there are sufficient provisions in his house, his wife [she will not] enter [another's house] ... If a man should be captured and there are not sufficient provisions in his house, his wife may enter another's house (*Laws of Hammurabi*: 133a, 134, Roth 1997).

In both these cultural settings, a man is above all supposed to provide for his wife. If he fails to do so while being away for some reason, his wife has to restrain herself from taking any steps towards independence, but given that she is not provided for, as a last resort she

may maintain herself either by respectable work or by entering into another marriage. This applied equally in India and in Babylonia, and, we can tell, in many other cultures as well, although they may have been culturally alien to each other in many other respects.

The second case is about the illegal cutting down of trees in someone's field or orchard. "If a man cuts down a tree in another man's orchard, he shall weigh and deliver 20 shekels of silver", states the 4,000-year-old Sumerian *Laws of Lipit Ishtar* (§ 10, Roth 1997). 2,500 years later, the Salian law almost identically stipulates that if "anyone cuts down a planted tree in another's field ... let him be held liable for 1,200 denarii" (*Pactus legis Salicae*: 27:15, Rivers 1986). This mirrors the fact that property and the violation of property rights are among the most frequently regulated aspects of human action in most legal systems.

Of course, no conclusions about the universal characteristics of laws can be drawn from the examples given here. They have been chosen simply to illustrate the potential of global intertextual contextualization.

This is how we do it

In order to address the issues raised above, we have developed a number of specific methods for long-term analysis of legal development, which are presented below. On an overall level, the study comprises two main subsurveys: first, analysis of the long-term development of the content of legal codes, and second, analysis of the long-term development of their form, although the latter is not treated in this article. The former subsurvey is comprised of two parts: quantitative analyses of the entire contents of codes, and analyses of specific aspects of the codes.

The contents of the codes: quantitative analyses and selected themes¹

Here, the aim is to provide an overall picture of the contents of the codes, so as to form a basis for comparisons over time and between cultures, and to analyze a number of aspects of the codes in depth.

The former is done above all through collection of two types of basic information: the *types of human (inter)action* regulated in the provisions of the various codes, and the *consequences prescribed* in those same provisions.

A systematic comparison of this kind clearly requires quantification and classification. How can we create categories that will work in all the historical contexts of the survey? How can we make sure that our analyses are intersubjectively verifiable? And how can we create a system that will work from the outset, but still can be revised as the work proceeds? In other words, how can a system be established, which is at the same time stable and flexible, and readily comprehensible and transparent?

To achieve this, we have developed a classification system with three main features: the use of explicit definitions of categories, a concentration on basic human interaction, and classification at different levels of abstraction.

The insistence on an explicit definition of the categories used is intended to stabilize the system, and to cater to the need for transparency and clarity. We want to avoid the pitfall of a too assumptive use of certain terms and categories – for example, by only defining key terms, and not creating a system of categories.

The second feature is based on the awareness that global comparisons of this kind call for the primary focus to be on a more general level (see also Reynolds 2013: 16). Understandably also, we cannot go into the smallest of details. However, this in no way precludes in-depth analyses or attention to detail per se. In fact, such analyses are necessary in order not to risk only seeing what is similar in the societies and legal systems we are studying and thus lose out on variation. Indeed, in order not to lose sight of the comparative context, it is essential for such analyses to always be connected to basic traits and processes found in most, or all, human cultures. In other words, there is a general level on which a connection can be made.

We have solved this problem of the need to be both general and attentive to variation by the creation of an analytical system where the legal material is classified at different levels of abstraction. In practical terms, this involves each rule in the codes in question being 1) summarized, 2) classified in a specific way, for example,

as concerning theft, and 3) classified in a more general mode, for example, as concerning property crime. The same principle is used to classify the consequences prescribed in the codes. This solution is also intended to ensure that the system is not too rigid and that it can be revised as and when new insights are gained. This is so because when changes are required it is always possible to go back to a more concrete level. It allows us to move a certain category at one level of abstraction without thereby (necessarily) ruining the classificatory order at another. For example, theft committed by a “slave” could be classified as a transgression either by a piece of “property” or by a human being, neither enforcing a change of the division between property and people at one level, nor the division between “theft” and other crimes at a more concrete level.

This broad analysis of the content of the codes is connected to a number of in-depth analyses concerning matters closely related to our overall objectives: the issues of equality or inequality before the law, legitimization of law, obligations versus rights, and of penal principles. In a forthcoming study our analysis of content will be paralleled by a detailed analysis of the form of law codes and of the intricate interplay between form and content.

These are our major results

We have spoken in defense of the possibility to understand, compare and contextualize laws from very different times and cultures, and we have also gone into some detail about the methods applied to make this possibility come true. Below, we will expand somewhat on the knowledge advanced by applying the methods presented above.

General themes and trends

Above, we claimed that one of the bases of intelligibility is that lawmakers to a substantial degree have tackled similar problems of human conduct and misconduct, despite huge cultural and temporal distances between them. The likelihood that this applies to the overall themes of laws in general is indicated in the figure below. Here we have collated evidence of the themes in four laws spanning more

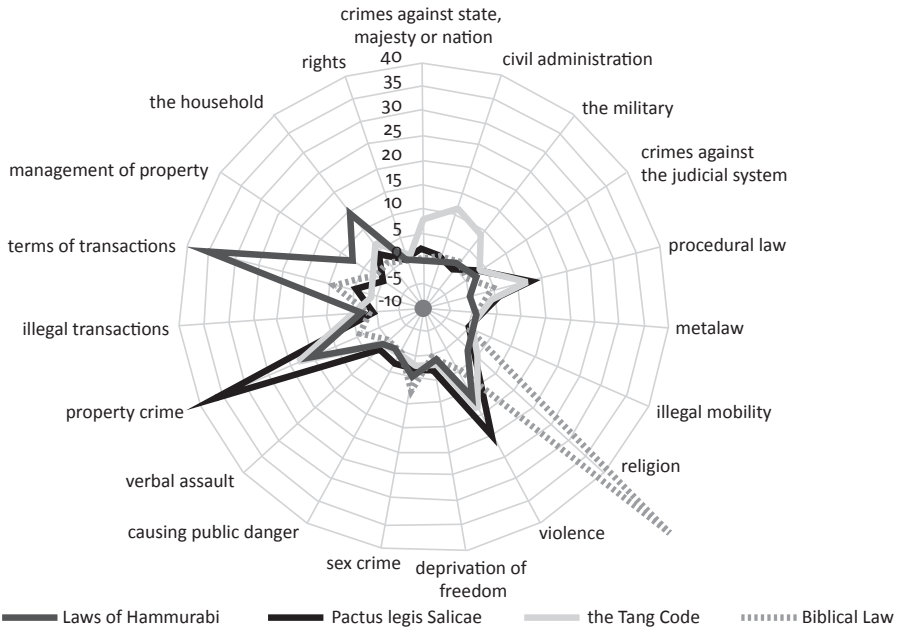


Figure 1. Themes in different laws. Per cent.
 Sources: *Laws of Hammurabi* (Roth 1997); *Pactus legis Salicae* (Rivers 1986);
Tang Code (Johnson 1979; Johnson 1997); *Biblical Law* (*Bibeln* 2000).

than 1,500 years. They represent substantially different societal and cultural contexts, from Babylonia in the eighteenth century BCE, through the Middle East and Early Medieval Europe (Frankish law) to the Chinese Tang dynasty of the sixth century CE.

It is obvious that these very diverse laws are all extensively pre-occupied with property crime and illegal violence, as are almost all other laws that we have examined. And it is highly probable that the presence of these themes in the laws reflects major concerns in all the sedentary societies or civilizations in which the legislators lived. This means that the similarities are not primarily due to some internal logic of legislation, even if this might play a role, since a simple statistical analysis reveals that the more comprehensive the law studied, the more even the distribution of subjects addressed in it, and vice versa.

It is equally clear that important resemblances between different

laws can be discerned also on lower levels of abstraction, although this is invisible in the figure above because of its low level of resolution. One such topic, recurrently targeted for regulation, is the matter of slaves, and another one prominent in its ubiquity is the handling of stolen property (e.g. *Laws of Hammurabi*: 9–13; *Pactus legis Salicae*: 10.1–7, 37.1–3, 47.2, Rivers 1986; *Tang Code*: 296, Johnson 1997; *Russkaia Pravda* (Short Version): 11, 13, 16, Kaiser 1992).

All these similarities can serve as a case in point for the fruitfulness of using comparisons between laws from profoundly different societies as an essential basis of contextualization.

It is just as clear that the laws presented in the figure above display great variation in their main focus. In Biblical Law it is religion (of course); in the *Laws of Hammurabi* what might be called “proto-civil” terms of transaction; in Frankish Salian law it is property crime; and, as expected, in the *Tang Code* it is civil administration. This variation reflects the profound cultural differences between the societies where the laws were promulgated. Variation is, of course, no less an intrinsic aspect of culture than of nature, and is as much a precondition for evolutionary change in the former as in the latter area of life, the one also often being indistinguishable from the other. And yet, through the course of time, certain general trends seem to evolve whatever the particular cultural point of departure, and wherever the multi-generational learning process of lawmaking took off. By taking a bird’s eye view on this process, we will here point to important long-term trends, while for the time being to a large extent disregarding variation.

Some of the trends have previously been identified by other legal historians. However, this has been done without being based on a broad and firm systematic analysis. Through our comparative enquiry, we are able to corroborate what others have suggested, qualifying and relating it to long-term structural or formal changes in lawmaking. Furthermore, our findings will serve as a basis for a set of hypotheses about how these trends might be causally connected to other major historical processes. This will be touched upon at the end of this section.

From inequality to equality before the law

One trend that we have identified is a general and gradual change from outright inequality to equality before the law. Some legal historians claim that laws generally originated in order to codify inequality and social differentiation, but eventually were recoded to emphasize equality (Glenn 2010). Be this as it may, and whatever the driving forces behind the process of codification might be, inequality was undoubtedly the rule in the beginning, and most often deliberately and explicitly so. The example below, from the *Laws of Hammurabi*, may serve as a case in point. It is about physical violence within and across the social classes. In one clause, concerning socially horizontal violence between people of the highest class of Old Babylonia, it states the following:

If a member of the *awilu*-class should strike the cheek of another member of the *awilu*-class who is his equal, he shall weigh and deliver 60 shekels of silver (*Laws of Hammurabi*: 203, Roth 1997).

Quite the opposite applies if the same transgression is committed by a slave against a socially superior victim:

If an *awilu*'s slave should strike the cheek of a member of the *awilu*-class, they shall cut off his ear (*Laws of Hammurabi*: 205, Roth 1997).

The same ideology is repeated over and over again in most laws in most of the history of human legislation. One example of that is the provincial law of Gotland (*Gutalagen*), which was launched 3,000 years later in a remote part of the gradually evolving Swedish state, in a cultural context very different from that of Old Babylonia. Since inequality is one of the constitutive principles of the *Gutalagen*, it is explicated in many of its regulations. For instance, concerning rape, it states that if a man commits rape,

then he shall pay twelve marker of silver to a Gotlandic woman, but to a non-Gotlandic woman five marker of silver and to an un-

free woman six örar. If rape is committed together with a legally married woman, whether Gotlandic or not, then he has forfeited his life (*Gutalagen*: 224, Holmbäck & Wessén 1979, our transl.).

The principle of inequality applies fairly often to procedural law too, as is illustrated by two curious passages in the *Code of Manu*, concerning the validity of testimonies in court. The first clause exhorts people to tell the truth, and for the individual witness to base his or her testimony on “what [he or she] has seen or heard” otherwise [he or she] will end up in hell in the afterlife (*Law Code of Manu*: 8.74–75, Olivelle 2004).

Except for the consequence of trespassing not being a stipulated sentence but a predicted otherworldly fate, so far this makes sense also from the vantage point of modern legislation. But then follows a strange regulation that encourages false testimony, given that this might rescue criminals of certain classes from execution:

when a man, even though he knows the truth, gives evidence in lawsuits contrary to the facts for a reason relating to the Law, he does not fall from the heavenly world; that, they say, is divine speech. When telling the truth will result in the execution of a Śūdra, Vaiśya, Ksatriya, or a Brahmin, a man may tell a lie; for that is far better than the truth (*Law Code of Manu*: 8.103–104, Olivelle 2004).

Yet, even in the midst of an outright ideology of inequality, rudiments of equality can be found. For example, in early times it was already often stipulated that the sovereign was supposed to obey the law in the same way as his subjects (e.g. Canning 1996: 23. See also Hart 1997: 58). This even applies to the *Code of Manu*, despite its incessantly repeated emphasis on the legal significance of differences in social status. For example, it states concerning theft that the “king must restore to individuals of all classes any property of theirs stolen by thieves; if the king retains it for himself, he incurs the sin of its thief” (*Law Code of Manu*: 8.40, Olivelle 2004); and in “a case where an ordinary person is fined 1 Kārsāpana, the king should be fined 1,000” (*Law Code of Manu*: 8.336, Olivelle 2004). Correspondingly, even in old codes such as that from legalist China,

there are traces of the principle of *equal treatment* regarding certain *crimes*, although for a long time this occurs simultaneously with the principle of *unequal treatment of people* of different status.

In the very long run, however, the idea gained ground that all humans should be treated as equals before the law, by which they also were transformed into citizens instead of mere subjects of the sovereign. In many codes, age-old specific references to the differing status of people were now replaced with references to “any person” (or just “the person”), as in the Ottoman civil law of the 1870s (*Mejelle*: f.i. 8. 902, or almost anywhere in the law); or “every individual” as for instance in the French criminal code of 1810, *Code Pénal*:

Every individual, who shall have given any wounds or blows, shall be punished with solitary imprisonment, if there shall have resulted from such acts of violence, a sickness or inability to work, for more than twenty days (*Penal Code*: 309).

In modern law this is not just visible in the specific regulations, but also anchored in statements of principle, such as in the following passage in the *Swedish Constitution* of 1809:

Public power shall be exercised with respect for the equal worth of all and the liberty and dignity of the private person (*The Constitution of Sweden: The Fundamental Laws and the Riksdag Act* 2003: 63).

Once formulated however, there were no guarantees that this celebrated idea would really become the guiding principle of the entire legal system. It took time before it genuinely permeated the minds of the lawmakers. This is obvious in many instances. For example, it is clearly stated in the introduction to the famous Napoleonic *Code Civil* that “[e]very Frenchman shall enjoy civil rights” (*Code Napoleon*: 8). However, this does not prevent it from containing gender-biased regulations, among other things stipulating that the “husband owes protection to his wife, the wife obedience to her husband” (*Code Napoleon*: 213); that the “wife cannot plead in her own name” (*Code Napoleon*: 215); and that the “husband may demand a divorce on the ground of his wife’s adultery,” whereas the “wife

may demand divorce on the ground of adultery in her husband”, only “when he shall have brought his concubine into their common residence” (*Code Napoleon*: 229–230).

So, after all, everyone was still not as equal as everyone else. But in due course everyone was declared to be equal, whatever the degree to which it was implemented in judicial practice. Yet, alongside this development a kind of re-differentiation has evolved where the social conditions of suspected criminals have gained increasing significance in law as well as in court practice. Certainly, rudiments of such considerations can be seen very early on in the history of lawmaking, but they did not proliferate until the equality principle began to become established, rather as its corollary than as its precondition, and rather as an integrative part of its spirit than its negation. This is so since such considerations serve as an equalizing corrective to inequalities between the suspects in terms of social and psychological conditions.

Towards secularized legitimization of law

Intrinsically linked to this development, the legitimization of law has been subject to substantial change over the millennia. It is not that legitimization as such emerged at some point in the history of lawmaking: as far as we have been able to survey the trajectories of written legislation, it is an ever-present ingredient in law.

It is of course possible to think of power simply as brute force, implemented by power-holders having no incentive to justify themselves, and thus in no need of making laws. However, in practice this is almost never the case. Quite the contrary, history abounds in rulers, brutal or not, who have attempted to make their rule reasonably agreeable to their subjects through various legitimizing measures. Although this does not necessarily mean law initially, it does in due course (f.i. Glenn 2010; Newman 1983). And simply by its existence, law signifies a quest for legitimization, regardless of whether this aim is explicitly stated or not. Yet, in most cases laws do include metastatements of this kind.

In most of these cases, reference is made either to impersonal forces, such as magic, tradition and heaven, if not nature itself; or

to heavenly personae (Friedman 1985: 236; Weber [Rheinstein] 1954: 8 ff., 106); or to “innerworldly” agents, such as the rulers and legal scholars responsible for putting the rules together; or to the consent of larger groups, most commonly elite collectivities such as councils of elders or nobles (e.g. Bjarne Larsson 1994: 18, 37, 216–218; Canning 1996: 59–64; *Laws of Wihfred*: Prologue, Attenborough 1922); but in more recent times also to the people as a whole. References of the former types are inserted in order to anchor what may seem like regulations too contingently human or temporal in something more solidly extra-human or eternal.

Simplifying somewhat, we would say that the legitimization of law started as a mixture of references to magic, tradition and the worldly authorities themselves (Newman 1983: 10).² Subsequently, it turned to heavenly powers (Glenn 2010: 93; Newman 1983: 10), although some of the earlier figures of legitimization lingered on. Eventually, it gravitated all the way back to the mortal beings generally referred to as “the people”, let alone that the people in their turn have often been resacralized into the common will and human law has been elevated to natural law (Friedman 1985: 204; Weber [Rheinstein] 1954; Newman 1983: 28; Guchet 1993: 48).

How this is reflected in early law in different cultural areas depends very much on the particular context or stage of socio-political development where written law was first introduced. More specifically, there seems to be a connection between state building or state expansion and increasing resort to “otherworldly” legitimization, such as reference to personal gods. Thus, in the earliest laws preserved, the codes of the early state societies of Sumer and Babylonia, religious references loom large. In order to appease his subjects and potential challengers to his position, King Hammurabi of Babylon opened his code by asserting that he was “selected by the god Enlil”, that the god Marduk had “commanded [him] to provide just ways for the people of the land (in order to attain) appropriate behaviour”, and that the god Shamash had “granted [him] (insight into) the truth” (*Laws of Hammurabi*: Prologue, Epilogue, Roth 1997). By this Hammurabi copied and elaborated the legitimizing rhetoric already present in older Mesopotamian codes, such as the *Laws of Ur-Namma* and the *Laws of Lipit-Ishtar*

(from the twenty-second and twentieth centuries BCE respectively) (Roth 1997).

As important as religious reference was to these rulers in their efforts to legitimize their laws, it should also be noted that this was supplemented with more “innerworldly” references to their own suitability for the task: their might, wisdom and benevolence towards the people (Jarrick 2008: 205–207; *Laws of Ur-Namma*: Prologue; *Laws of Hammurabi*: Prologue, Roth 1997).

Also in the laws of the European Middle Ages and the early modern period, we find a mixture of frequent references to God and innerworldly concerns (*Burgundian Code*: Preface, Fisher Drew 1976; *Pactus legis Salicae*: 1–2: Rivers 1986; Bjarne Larsson 1994). However, God is mostly no longer presented as the one who had explicitly appointed the king to his task as a legitimate lawmaker. No doubt, the Medieval “code wrights” spoke “in the name of God” (*Burgundian Code*: Preface: Fisher Drew 1991; *Pactus pro tenore pacis*: 92, Rivers 1986; *I Aethelstan*: Prologue), and pretended to act in “accordance with God’s intent” (*Pactus pro tenore pacis*: 92: Rivers 1986). One may also speak of a certain distance between the kings and God here, since Medieval kings sometimes refer to the representatives of God rather than to God himself. Thus, several of the English kings who from the seventh to the eleventh centuries promulgated laws carefully noted that they had done so with the counsel and consent of their bishops and ecclesiastical advisors (e.g. *Laws of Ine*: Prologue; *I Aethelstan*: Prologue, Attenborough 1922; *I Edmund*: Prologue; *V Aethelred*: Prologue, Robertson 1925). In this sense, the grand He was out of fashion as the ultimate taskmaster as early as the European Middle Ages, at least when it came to lawmaking. Whether this change of ways of referring to God could be seen as a rudimentary step towards the secularization of lawmaking cannot be settled, although this is exactly what happened in the very long run. It might also be added that Ashutosh Dayal Mathur has claimed a similar development for Medieval Indian law. He refers to a “secularization of dharma sastra” in Medieval Hindu law (Mathur 2007: 5).

However, occasionally references to God are more direct, which to a large part seems to coincide with state expansion and the cen-

tralizing ambitions of rulers (Bjarne Larsson 1994: 7–8; Canning 1996: 47–64). Thus Charlemagne describes many of his specific legal rules as being in accordance with the “Lord’s law”, situating his own judicial duties in a truly otherworldly context (e.g. *General admonition*, 789 (Boretius 22); *Programmatic capitulary*, 802 (Boretius 33), King 1987), and the English King Alfred the Great included Biblical provisions more or less unchanged in state law (*Laws of Alfred*: 1–48, Griffiths 1995).

The emphasis on otherworldly legitimization of law and the judicial activities of rulers, and the inclusion of religious law in secular codes, appear also in early modern times (Konung Karl IX: s stad-fästelsebrev till KrLL, Collin & Schlyter 1869: 4–6; *The Muscovite Law Code (Ulozhenie)* of 1649: Prologue, 1–9, Hellie 1988). However, a profoundly new element did appear in this field in the late eighteenth century. In the preamble of the *American Constitution* from 1789, it was declared that “[w]e the People of the United States ... do ordain and establish this Constitution for the United States of America” (*American Constitution*, preamble). As is well known, what happened in America was synchronous with the great turmoil in France, which led to a secularization of lawmaking there too. As already stated, in 1788 a “nation means the community formed by the association of individuals who decide to live freely under a common law, forged by their representatives” (Furet 1992: 50). Within this mental framework, reaching its apex after the downfall of the monarchy, the only authority or “sovereign” recognized was the people. This was repeated in the many constitutions that appeared during the different phases of the French Revolution (Furet 1992: 87; Guchet 1993: 54, 61–62, 86).

Above, we claimed that the long-term trend towards equality before the law was intrinsically linked to the secularization of lawmaking. This is probable in the sense that the waning of legitimizing references to powers beyond emerged alongside the evolving general idea that no one should be considered above anyone else anyway. Thus the dismantling of extraterrestrial powers was just one aspect of the disapproval of inequality altogether.

However, beautiful as it might seem, reference to the people as the sole legitimizing basis of law was sometimes little more than lip

service paid by an elite that more and more often tended to speak in the name of the people and less and less through the people. This is at least what some scholars have claimed was the fate of the French Revolution, where gradually ordinary women and men were being marginalized by the very process that also “sacralized” them as the “the People” or even “the General Will” (Furet 1992; Guchet 1993).

*From emphasis on obligations
and particularistic rights to general individual rights*

However, in due course the two long-term processes discussed here were accompanied by a third process: the change from a general emphasis on obligations and group privileges to general individual rights, eventually dissolving the lofty references to the General Will.

All the way up to the eighteenth century CE, there was a general emphasis on obligations, whether we look at laws from Mesopotamia, Europe or China. Obligations are ubiquitous, like the collectivistic distinguishing and lumping together of people of differing social status, as illustrated by the following example from the *Tang Code*:

All cases in which officials of the seventh rank and above, [and relatives] of those officials and nobles permitted petition, commit a crime punishable by life exile or less shall follow this principle allowing reduction of punishment by one degree (*The Tang Code*: 10; Johnson 1979).

This legal culture lasted for a very long time, and we will not go into any detail here about the process that eventually caused it to fade away. Suffice it to say that this fading away finally occurred in the eighteenth century. Implied in the secularized and democratic perspective on the legitimization of law was the notion of general individual rights, which now for the first time accompanied obligations as an essential ingredient of law codes (Guchet 1993: 59). Thus, in Article 4–5 of the famous *French Declaration of Rights* from 1789 the following is stated:

Article 4. Liberty consists in being able to do anything that does not harm others: thus, the exercise of the natural rights of every man has no bounds other than those that ensure to the other members of society the enjoyment of these same rights. These bounds may be determined only by Law.

Article 5. The Law has the right to forbid only those actions that are injurious to society. Nothing that is not forbidden by Law may be hindered, and no one may be compelled to do what the Law does not ordain.

Although Napoleon's civil code was far more despotic than the proud declaration of rights cited above, civil rights also appear here, in fact already on the first page of Book One, where it is stated that every Frenchman "shall enjoy civil rights" (*Code Napoleon*: I.I.I.7–8).

Certainly, to the degree that older laws contain such obligations where people's responsibilities towards each other were stipulated (and not only towards the authorities), what one person owed another could namely be understood as what the other had the right to claim from that individual. One may also be inclined to label as rights certain privileges frequently given by the sovereigns of premodern societies. In that sense rights were not altogether absent in ancient law.

However, when present, as, for example, was frequently the case in the *Institutes of the Justinian Code*, rights were most often stated in positive terms for certain activities, such as the wife's rights in marriage (*Institutes of the Justinian Code*, 535 CE), or as group privileges, instead of being indiscriminately offered the "generic" citizen at her or his discretion (e.g. Berman 1983: 395–396; Lev: 2–5, *Bibeln* 2000; *The Etablissements de St Louis*: 76, 113, Akehurst 1996; Guchet 1993: 113; Friedman 1985: 195). And it never happened that people were offered freedom of expression, although, admittedly, the Justinian recognition of everyone's right to use public resources belonging to no one (such as rivers and ports) borders on a modern perspective on rights (*Institutes of the Justinian Code*: Book II.I.2), while outright repression of heresy certainly does not (*Annotated Justinian Code*: Book I:I, 5–11). Furthermore, "rights"

implied through someone's obligations towards someone else are not the same as the explicit recognition of the individual rights offered each and every individual considered a citizen. Whereas obligations are specific, normally individual rights are deliberately unspecific. And where collective privileges are granted to one group at the expense of all the unprivileged, the opposite is the case with individual civil rights, being collective utilities in the sense that their use by one citizen does not to any degree reduce their accessibility to any other. Thus, not only did secularized law originate with the people, from now on it also guaranteed certain rights to the people, who by this reform were differentiated into individuals (Guchet 1993: 47; Glenn 2010: 142).

However, it must be added that the idea of rights has rather often been violated in modern law despite being formulated as a basic principle there. For example, not all adult inhabitants were included in eighteenth- and nineteenth-century French legislation on civil rights. Women were not considered citizens in the full sense of the word (*Code Napoleon*: 7, 19, 214–226).

Summary and discussion

Focusing on methodological issues, we have tried to demonstrate the scientific fruitfulness of a global and systematic comparative approach to historical studies of law. The motivation is that we want to improve our understanding of the cultural dynamics of human society, an objective compelling us to also improve our methods of enquiry. For such an objective, laws and lawmaking suit us extraordinarily well, partly because they testify to profound aspects of human interaction, and partly because they can be followed over a considerable time span of at least 4,000 years. Although most parts of the world will soon be covered by our study, only a few out of all the innumerable laws will be analyzed. They have however not been chosen at random. Rather, certain core areas have been picked, particularly those where the content and form of lawmaking can be followed over long periods of time.

As a response to concerns as to whether it is at all possible to make large-scale comparisons, we started out by showing that laws

even from distant and dissimilar times and cultures are more easily decoded and compared than many researchers believe. Furthermore, we also showed that the corpus of laws studied can serve as a means by which each of them could be contextualized. What is present or absent in a law code from a certain culture throws light on corresponding absences and presences in law codes from other cultures and thereby on the significance of local conditions for recurring or unique traits in laws. However, to truly make the possibility of comparisons of legal regulations come true, we have equipped ourselves with clear and stable definitions, and we have applied a three-level system of categorization in order to bring flexibility as well as stability to our analyses.

Thematically common to most laws is their preoccupation with property crime and illegal violence, and this applies to laws as dissimilar as the *Tang Code* and the *Laws of Hammurabi*, despite huge cultural differences between the societies in which they were promulgated. While laws in their earlier phases were biased as to their thematic orientation, later on the themes became more evenly distributed, the more so the more extensive and the more encompassing the laws became.

Reducing the development of the substance/content of lawmaking to the major long-term trends, we have identified the following changes:

- From deliberate inequality to equality before the law
- Towards secularized legitimization of the law
- From an emphasis on obligations, particularistic rights and group privileges to general individual rights

Another trend that we have identified concerns the long-term course of development of the death penalty. Over a very long timespan we have identified a curvilinear track from leniency, through harshness and back to leniency. Since we will deal with this subject elsewhere, here we will make do with just pointing it out.

We are fully aware of the fact that these changes have evolved neither in a unidirectional nor in a uniform way, and we cannot claim that the tide can never turn and undo what has happened to

date. In some respects it is even now about to happen. Nevertheless, a certain direction in the gradual development of lawmaking can be still be discerned.

We assume that the dynamics of a cultural system have a certain direction, simply meaning that a system never really returns to its “point of departure”, despite all kinds of feedback mechanisms at work in the course of history. Yet this should neither be understood as if the system has to be unilinear, nor that if it in itself shows anything like an intention, although it is peculiar to such a system that its basic components, the human agents, do have intentions. Implied in our assumption is that culture is a process, where societal change unfolds according to specific causes. In other words, the history of culture is deterministic in the sense that certain processes seem to be the necessary precondition for other processes to occur.

The general cumulative directionality of history, be it unilinear or not, can be established as nothing but the unfolding of a number of specific processes. Without the concrete flesh of history, there can be no history at all – naturally. However, history luckily abounds in examples of this simple but surprisingly oft challenged fact (Jarrick 2013). For example, once in the distant past sedentary life grew out of nomadic life, in its turn being the precondition for the emergence of urban clusters. Furthermore, it is unlikely that regular wars could be fought unless people had settled down, let alone that homicide has been immanent in human culture throughout history. The opposite trajectory is hardly thinkable: a history departing from sedentary societies in a sparsely populated prehistoric world gradually giving way to an overcrowded world of wandering people. Moreover, the exchange of utilities necessarily developed before money was introduced in order to facilitate exchange, little by little being transformed into commodity trade. A parallel development is how religious ideas had to appear before religious associations did, before churches and other devotional buildings were erected, a long time before some people in the modern age began to distance themselves from the whole otherworldly package altogether.

Being a crucial example in our specific context, the history of laws also testifies to the directionality of human affairs as much as other fields of human interaction do. This has been shown above.

In addition, two other aspects of the directionality of the history of lawmaking processes are worth mentioning.

First, as has frequently been shown, in most if not all societies, unwritten behavioral rules anteceded written law. Since human interaction sooner or later becomes regulated in one way or another, and humans have lived together long before they acquired literacy, this is almost self-evident. Furthermore, this order of institutional change should be understood not only in the chronological, but also in the causal-intentional sense, meaning that once they learned the art of reading and writing, people could intentionally draw on experiences from the former when developing written law. It has been claimed that this development could be linked to the emergence of the concept of transcendent powers, and gods, stipulating laws for humankind (Elkana 1986: 47). Traces of unwritten regulations everywhere in the oldest written law codes clearly testify to this.

Secondly, not only rules of behavior but also how to handle the violation of the rules – the administration of justice – was developed long before official courts or the use of written records and other such instruments were introduced. Correspondingly, it took some time until conflict settlement with the aid of third parties emerged, and still more time before this procedural component was made mandatory (Newman 1983: 51–52). As expressed by Catherine Newman: “self-redress systems relying upon retaliation [which] entail clearly defined notions of right and wrong”, were developed “despite the absence of third-party authority figures” (Newman 1983: 61).

More could be added, but the above examples suffice to make it obvious that the history of legislation and jurisdiction fits well into the picture of history as a cumulative process with a certain discernible direction. Furthermore, an inverted process could hardly be imagined, dawning with written law that over centuries gradually dissolves into the oral regulation of human behavior.

We have demonstrated that the cultural dynamics of the human society, as studied through the history of lawmaking, have a certain direction, and that they constitute a process of cumulative change. We are inclined to add that our “story” also indicates that the significance of culture increases with the development of culture, in the sense that laws neutralize or diminish the effects of natural

selection, among other things through the protection of the weak. In the long run this is amplified to the principle of equality before the law (supplemented, among other factors, by consideration of social background).

It is likely that the long-term evolutionary implications unearthed through our law study apply to other processes of cultural change too, though this is still to be demonstrated. Also, we believe that the methods used here could open up for studies of other aspects of the cultural dynamics of the human society. They could for example, serve as a model for quantification of other material so far not considered in such a context. We will ourselves actually be using this study as a point of departure for further studies of the connection between law and other profound aspects of long-term cultural change.

Notes

- 1 We have described this method in close detail in a previous article (see Jarrick & Wallenberg Bondesson 2011).
- 2 According to Yehuda Elkana 1986: 47, in the very beginning the authority of laws rested with worldly authorities before they were made transcendental through the emergence of the old, axial world religions. This may be partly true, but in the *Laws of Hammurabi*, written before the breakthrough of axial religions, Hammurabi referred to God as the ultimate authority.

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Core and periphery in the early modern world system

A time-space appropriation assessment

Rikard Warlenius

In this chapter, methods usually associated with ecological economics are applied to world history studies, in an example of what Martínez-Alier and Schandl (2002) have called “ecological-economic history”. It is understood as going beyond a general ecological concern in environmental history to the application of measures and methods originating from ecological economics. Ecological economics is a heterodox school of economic thought which acknowledges the planetary limits of (a sustainable) society and in which biophysical measures such as ecological footprints and material flow analysis are frequently applied to economic analysis.

This chapter uses a biophysical method grounded in world system analysis and ecological economics – Alf Hornborg’s (2007) *time-space appropriation* – to investigate one of the main questions posed on the “battlefields” of global history: was the early modern world economy centered on Europe or China? As a case study, it focuses on the exchange of Swedish bar iron and Chinese Bohea tea in the eighteenth century via the Swedish East India Company. In time-space appropriation, the amount of productive land and human labor embodied in the commodities exchanged is assessed, as well as the quantities exchanged at the prevailing price relation, in order to establish a net flow of biophysical quantities – time and space – between the parties of exchange. The underlying assumption is that monetary prices are masking uneven relations of power, and that an equal exchange of

money often hides an unequal exchange of biophysical resources. In this particular case, the method is used to discuss the structural position of Western Europe and China respectively within the early modern world system, assuming that the net-receiver of resources in the exchange is the more central area. The empirical findings and assessments are collected in a separate section (p. 206).

The “ReOrientation” of world history

With its strong focus on methodology, the rich theoretical and historical debate about Eurocentrism in (world) historiography is largely left aside in this chapter. I only wish to note that a strong movement for dismantling Eurocentric historiography has existed for some decades, which is most closely associated with the “California school”, a fairly heterogeneous historical school that borrows ideas and methods from neoclassical economics and institutionalism, as well as structuralism. Its most prominent work is arguably Kenneth Pomeranz’ *The Great Divergence* (2000). It was, however, to an even greater extent structuralist scholars, including Janet Abu-Lughod (1989), Samir Amin (1989), James Blaut (1993, 2000), and Andre Gunder Frank (1993, 1998), who pioneered this “ReOrientation” of world history.

Frank offered the most radical revision and the importance of his *ReOrient* (1998) is equal to the work of Pomeranz (2000). Although the theoretical perspectives differ, their conclusions on Eurocentric historiography are quite similar. Frank’s main aim, stated in the first sentence of his book, is to “turn received Eurocentric and social theory upside down” (Frank 1998: xv). He asserts that more recent historical research, mainly from the peripheries, synthesized holistically into a new world history, shows that other parts of Afro-Eurasia were at the same economic level as Europe, or were even more advanced, up until circa 1800. In a posthumous book (Frank 2014) he argues that for China, this was the case even up to the last three decades of the nineteenth century.

According to Frank (1998: 111), in the early modern world system, China was the core for two main reasons: its “preeminence in production and export” and its “function as the final ‘sink’ for the world’s production of silver”. The two factors were connected, since

China's exports of silk, porcelain and tea were unrivaled, based on its superior productivity, and led to a trade surplus "with everybody else" – other parts of Asia, Europe, Africa, and America (Frank 1998: 116) – which was balanced through silver payments. The silver enabled the Chinese market economy to expand on a silver basis. In fact, the whole world economy was on a silver standard, and China was the center of this silver-based world economy.¹ However, silver also played the role of a "Trojan horse" that would eventually create a shift in global dominance. The extremely rich silver discoveries in Mexico and Bolivia not only fueled China's further economic expansion, but became Europe's ticket to the much greater Asian economic train (Frank 1998: 277). Still, it was not until after the Western Industrial Revolution that Westerners were able to "displace the Asians from the locomotive" of the world economy (Frank 1998: 37).

Flynn and Giráldez, the most prominent historians on the early modern silver trade, also share Frank's Sinocentric view and the importance of China's silver imports. They disagree with Frank's contention that China was enriched by its silver imports and emphasize the high social costs of substituting a practically free paper money system with silver paid for with exports. However, China's capacity to do so for centuries only "underscores the centrality of the Chinese economy as global juggernaut" (Flynn & Giráldez 2002, cf. Flynn & Giráldez 2000).

Ecological-economic history

Environmental history has now become an established subdivision of history, and while for a long time it was mainly national in scope, environmental explanations for world historical events are now both advanced and acknowledged. However, Jason W. Moore (2003) notes that environmental history is surprisingly void of social theory, and mentions James O'Connor's "second contradiction", John Bellamy Foster's theory of the metabolic rift, Wallerstein's world system analysis, actor-network theory, and political ecology, as potential inspirations for a more theoretically-concerned environmental history. From a methodological perspective I would like to add ecological economics as another potential inspiration.

Ecological economics is a growing academic field that started as a “break-away” from neoclassical economics, merging economics with natural science to investigate the biophysical foundation of economic activities.² Several such biophysical measures have been developed in ecological economics. These include Odum’s emergy concept (see Brodin 2006; Foster & Holleman 2014); Material Flow Analysis elaborated by the Vienna Social Metabolism school (e.g. Fischer-Kowalski & Hüttler 1999); Borgström’s (1965) concept of ghost acreages; and the more elaborated concept of ecological footprints (Wackernagel & Rees 1996; WWF 2010).

Ecological economics shares with world system analysis a critique of neoclassical theories of trade and prices. At the base lies a conviction that prices on the world market do not neutrally reflect value solely as a response to supply and demand, but in some way reflect power, social relations and cultural constructions. As the human ecologist Alf Hornborg puts it:

[T]he cultural bubble of neo-liberal economics excludes all those other possible measures of exchange [besides money] – such as energy, materials, hectares, labor time – with which it is fairly easy to show that world trade is indeed highly unequal (Hornborg 2009: 262; cf. Hornborg 2001: 39).

Hornborg is critical of any value theories, including those of Marx, Odum (1983) and Bunker (1985), since he argues that any “objective” valuation of a commodity denies that valuation is highly cultural. Prices are not a neutral reflection of supply and demand, as economists would have it, but related to power relations, social relations and cultural valuations. Even if prices mask power relations, and there is no “objective” value theory with which prices can be compared and unmasked, they can be measured against real, physical inputs such as labor time – what the structuralist economist Arghiri Emmanuel (1972) called *unequal exchange*.

The theory of unequal exchange is rooted in Ricardo’s (1953, orig. in 1817) classical trade theory of comparative advantages, according to which a country gains by specializing in the production of commodities in which it has a relative advantage, and by trading with

other countries for the commodities in which they have a relative advantage. Given immobility of labor and capital, both countries will gain from the exchange – although Ricardo fully understood that this would lead to an unequal exchange of labor time. Marx picked up on Ricardo’s theory and observed in *Capital* that “the privileged country receives more labor in exchange for less” (Marx 1981: 345). However, this agreement, in substance, leads to very different conclusions. Ricardo’s theory laid the foundation for the Heckscher-Ohlin model of factor endowments and all current mainstream trade theory in which low wages are regarded as a comparative advantage or the result of an abundant factor of production to be used in international trade. In Marxist and structuralist theory the concept of unequal exchange – in essence “more labor in exchange for less” (Marx 1981: 345) – has been regarded as a core explanation of uneven development and the maintenance of core-periphery relations in an unjust world system.

Later on, this concept was situated at the centre of attempts to ‘green’ world system analysis by analyzing uneven or unequal exchange, not only of labor but also flows of material and/or energy. An ecological version of unequal exchange was first developed by the sociologist Stephen Bunker (1985). To be able to actually test the hypothesis that the world system is characterized by uneven flows of energy and matter, the biophysical measures developed within ecological economics have proven useful. For instance, ecological footprints have been used to measure uneven flows of trade in several studies (for recent overviews, see Hornborg 2009: 249; Foster & Holleman 2014: 11).

Ecological economists have also occasionally applied their methods to historical cases³ – what Martinez-Alier and Schandl (2002) call “an *ecological-economic history* concerned with the physical assessment of the impacts of the human economy ... a history of ‘social metabolism’”⁴ – but historians are still mostly uninterested in quantifying the entropic flows, ecological footprints, energetics regimes and so on of past societies and economies. One exception is Andre Gunder Frank, who late in life began to see entropic flows as an important analytical tool for global history (Frank 2006, 2014; Bergesen 2011; Denmark 2011). The English ascendance

of the nineteenth century, he argued, was characterized by the displacement of ecological and social entropy from the center to the periphery, permitting greater order and democracy in core regions while simultaneously imposing disorder, ecological devastation and conflict/violence onto the periphery forced to absorb the entropy. This displacement of entropy, as well as the converse transfer of exergy to the core, is distributed through the world market where price differences favoring the core serve as the engine (Frank 2006: 304–307). In his posthumous book *ReOrienting the 19th century* (2014), Frank set the task of explaining the “the great divergence” partly through entropic flows, but unfortunately he was not able to finish those sections before he was lost to cancer in 2005 (Denemark 2011).

One obvious problem in measuring asymmetric entropic flows in general, but especially in history, has been the difficulties in putting it into operation in empirical research based on existing data. Hornborg suggests one feasible way:

[D]etect such structural asymmetries in trade by converting statistics on commodity flows into quantities of “embodied land” and “embodied labor”. Both these factors of production can be sources of exergy for the accumulation of capital, but have the advantage of being quantifiable, for example in annual hectare yields and in hours of human labor (Hornborg 2007: 261).

Thus, the method Hornborg labels time-space appropriation is an attempt to measure ecologically unequal exchange. He uses the trade between England and its former American colonies in the mid-nineteenth century as a case study. To actually assess the land and labor embodied in the traded commodities turned out to be much harder than anticipated, however.⁵ However, by studying a broad spectrum of literature and estimated figures often used for other purposes, an assessment was possible. Accordingly, in 1850, 1,000 sterling pounds worth of raw cotton from the former colonies embodied circa 21,000 hours of mostly slave labor and 59 hectares of land, whereas 1,000 sterling pounds worth of cotton textiles from England embodied circa 14,000 hours of labor and less than one

hectare of land (Hornborg 2013: 91). This unequal exchange was made possible through technological superiority emanating from capital accumulation that, over time, became self-enforcing when the profits derived from the unequal exchange were invested in further technology (Hornborg 2007: 267). In conclusion, Hornborg's calculations are used to reinforce an analysis of British industrialization, not so much as the rise of growth and productivity, as of successful appropriation of other people's land and labor in what is essentially a zero-sum game: "to save time and space by the application of increasingly 'efficient' technologies may often tend to imply that someone else in the world system is losing time or space in the process" (Hornborg 2007: 270).⁶

By including both land and labor, Hornborg bridges the Marxist-oriented world system analysis, within which the concept of unequal exchange focusing on labor emerged, and ecological economics and global history, which focus on flows of embodied land.

A time-space appropriation assessment of an early modern Swedish–Chinese trade exchange

Time-space appropriation

To summarize the introductory part of this chapter, a non-reductionist, structuralist, non-Eurocentric, ecologically concerned global history is favored, and special attention is paid to biophysical methodologies associated with ecological economics. One such method, time-space appropriation (TSA), is used to build a case that aims to test, or at least shedding some light on, a world history controversy, namely Andre Gunder Frank's hypothesis that the early modern world system was essentially Sinocentric and not Europe-dominated. This approach is novel in at least one sense. It seems self-evident which parts of today's world system are cores and which parts are peripheries, but going back in history, this is not as obvious – hence the controversy. While the methodologies related to (ecologically) unequal exchange have been developed to explain how existing cores exploit peripheries, and how an unjust world system is thereby maintained, here the aim is to determine which part of a historical world system was core, and which periphery. Presupposing that the

same tendencies that apply now were also in function some hundred years ago – basically, that the peripheries exchange more labor and more nature for less – measuring energetic net flows in world trade might indicate which parts of the world system were more central, and which parts were more peripheral.

TSA has been chosen for a number of reasons. It has a solid and straightforward theoretical underpinning that is not based on controversial and, arguably, metaphysical value theories. While most biophysical measures, such as ghost acreages, or ecological footprints, only take ecological resources such as land into account, and while Emmanuel's unequal exchange only focuses on labor and wages, TSA defines energetic flows more broadly by including both land and labor. Like unequal exchange, it is reciprocal, i.e. it compares both import and export to assess net flows, which gives a fuller picture than a one-way assessment. Finally, its variables rely on data that should be assessable for many historical cases. There are, of course, also some disadvantages and complications with the method that will be dealt with as they arise and summarized towards the end.

First though, it is not self-evident that TSA should be used as a general measurement of unequal exchange – of core/periphery status – within any world system. Even though Wallerstein is not ignorant about ecological factors (cf. Moore 2003), he nonetheless defines economic status in the world in exclusively economic and political terms; in *The Modern World System I* (Wallerstein 1974), variables such as “the complexity of economic activities, strength of the state machinery, cultural integrity, etc” (Wallerstein 1974: 349) are described as the differences between cores and peripheries. The world economy is distinguished by its axial or geographical division of labor, and the core countries dominate through tasks “requiring higher levels of skill and greater capitalization” (Wallerstein 1974: 350). These are factors that are not easily operationalized. In 1974, Wallerstein made no reference to Arghiri Emmanuel or unequal exchange. Over time however, Emmanuel's analysis was incorporated into world system analysis (see Wallerstein 1980: 50; 2004: 28, 98), and was even proposed as a measure of core/periphery status.⁷ In his theoretical introduction, *World System Analysis* (2004), Wallerstein states that cores and peripheries are defined by their production

processes, and that their status can be decided by examining the degree to which they are monopolized, or submitted to the rules of the free market (core production processes are more monopolized and peripheral ones are more exposed to competition, Wallerstein 2004: 28). Core/periphery status is here defined by the rate of profit of the production processes, but profitability is seen as directly related to the degree of monopolization. In an exchange, the competitive commodities are in a weaker position than the (quasi-) monopolized ones, which results in a permanent flow of surplus value from the peripheral producers to the core producers. “This has been called unequal exchange” (Wallerstein 2004: 28).⁸

Following Wallerstein, either unequal exchange or factors relating to the level of profitability or market dominance should be operationalized in order to measure core/periphery status within a world system. But if we agree with Bunker, Hornborg and other proponents of a “green” structuralism – that the world system is not only characterized by an unequal exchange of labor time, but also of natural resources, which is also in line with the above excursion on the relevance of ecology in historical explanations – then it becomes logical, and true to the original world system analysis, to use *ecologically* unequal exchange as an indicator of core/periphery status. As has already been shown, time-space appropriation is one usable method for measuring ecologically unequal exchange.⁹

Constructing a case of time-space appropriation

To use measurement of time-space appropriation to shed light on “divergence” before the Industrial Revolution, we need, first and foremost, to single out one, or a few, representative cases. Hornborg’s example, the exchange of American raw cotton and finished British textiles in the mid-nineteenth century, is key to understanding the Industrial Revolution. Does such an emblematic case of exchange between Europe and Asia in the early modern period exist? Having identified such a case, we shall need to create a theoretical model, and find dependent variables and relate them to each other, before getting into the empirical details and calculating the results.

Europe’s main export commodity to Asia in early modern times

was silver, and the main import product was tea (silk in the earlier period), so it would be natural to use silver and tea (or silk) in our case study. There are, however, four problems with using silver, of which one seems insurmountable. First, silver is mysterious because of its use partly as money, partly as a commodity. The non-economic use value of silver is limited and it is hardly an important part of any economy's direct social metabolism. If we were to treat silver merely as money – as a means of payment and a residual store of exchange value – then it might be unsuitable for a time-space assessment, since its value might be largely symbolic and backed by states and might therefore say very little about its biophysical content. The relation between its embodied exergy and its price would then be distorted and hard to compare with other commodities. There are, however, good reasons to treat silver as a commodity in the early modern world market (cf. Flynn & Giráldez 1995 and 2002; Pomeranz 2000: 160). Even if it was mainly used to mint coin, its advantage as money, compared with, e.g., paper money (which in China had failed bitterly during the Ming era), was that it also had a market value as a metal commodity, which made it less risky to accept as money. Thus, even when used as money it seems that its value was market-based rather than state-backed. Therefore, silver may be compared to other commodities and could be suitable for a time-space assessment.

Second, we might still suspect that silver's market value was a result not only of the land and labor it embodied, but also of its scarcity. This could also distort the correlation between exergy and price. The counterargument would be that scarce metals are also related to exergy input, since much labor time is usually spent on finding the ore and refining the metal. Third, that silver is an *abiotic* – nonliving – resource is not ideal for a time-space assessment. No attention is given to the fact that it is harvested only once, from a natural process that is extended in geological time rather than in space (as opposed to biotic, i.e. living resources), which might distort the land factor used in TSA. On the other hand, this concern is probably of little economic importance since what nature “gives” – either photosynthetically or geologically – is usually considered in economics as *gratis*. In any case, as we will see below, the extraction of minerals often had a large land component anyway because of the

wood or charcoal needed for refining, or at least this was the case in the “old” organic regime when energy was mostly dependent on land.

There is, however, a fourth problem with using silver, which has nothing to do with its particular material characteristics, but is rooted in the unique role this money-commodity played in the early modern world economy. One important assumption in how time-space appropriation is used here is that the net receiver of land and labor in a particular exchange – such as England in Hornborg’s example – is more central in the world system, higher up in the hierarchy, with the power to influence pricing. In this case, a confirmation of a Frankian, Sinocentric hypothesis would mean that the land and labor embodied in the silver exported to China is higher than in the commodities for which it was exchanged (e.g. tea), making China more central. But even if this were the general pattern, there might be exceptions to the rule. Not every single commodity exported by China would have to be less land- and labor-intensive than silver for the general pattern to be true. Perhaps, not even some very important commodities have to follow the general pattern for the pattern to be true. The problem here is that several informed observers claim that Europe’s silver export to Asia *was* this exception to the rule. According to Pomeranz (2000: 160), silver was one of very few commodities with which Europe could beat its global competitors, and for Frank it was precisely the profits from silver that in the long run made Europe strong enough to overthrow the Asian economic hegemony, even though it took several centuries (Frank 1998: 37, 277).

Because of silver’s supposed deviation from the general rule, a result pointing to a net transfer of land and labor to Europe would not be easily translatable into a more general claim of European world dominance. At the same time, it would not be reasonable to make the opposite claim: to regard a net flow of land and labor towards Europe as a vindication of Frank’s and Pomeranz’ claims, because what would a result supporting a Eurocentric world economy then look like? Since both results could be interpreted in favor of the hypothesis, constructing a falsifiable theory on silver’s role in the time-space appropriation of the early modern world system seems impossible. Another possibility is to look for other European export commodities to compare with those imported from China.

A major problem is that there were hardly any: silver was essentially the only thing the Chinese wanted from the Europeans, until the British success in balancing its China trade with Indian opium, but by then we are already on the verge of the industrial era.

If silver was almost the only thing the Chinese wanted in exchange for their tea and other commodities, and this exchange is likely to have been characterized by a net time-space flow to Europe, do we have to give up on using TSA as a measure of structural position within the world system? I think it is a better idea to circumvent the particularities of silver by constructing another case study where silver is included but treated as money instead of as a commodity, and see if such a case study implies a net transfer to China or to Europe. Several such case studies can be constructed. For the trade carried out by the Swedish East India Company that I have studied, the most relevant case, for reasons explained below, is the exchange of Swedish iron for silver, which in turn was exchanged for Chinese tea. A focus on other commodities traded by the British, Dutch or French East India Companies might have been as relevant, or even more relevant, for understanding whether Europe or China was more central in the early modern era, but they have not been investigated so far. Hopefully, this particular example can still provide some methodological insights for anyone tempted to construct further case studies.

Iron for tea

Founded in 1731, the Swedish East India Company (“Swenska Ostindiska Compagniet”, SOIC) conducted 132 expeditions to Asia. These expeditions were almost exclusively to Guangzhou (Canton) in China, although a few voyages were also made to India before the SOIC was dissolved in 1813. Tea was the most important Chinese import to Europe in the eighteenth century, for the SOIC as well as for the other East India Companies, and black Bohea tea was the most preferred sort. According to Robert Constant, a French merchant in Guangzhou in the mid-eighteenth century, “it is tea which draws European vessels to China; the other articles that comprise their cargoes are only taken for the sake of variety” (quoted in Gardella 1994: 33). Between 1739 and 1767, a total of 16,533 tons were imported by

the SOIC (Koninckx 1980: 211). Bohea tea constituted 13,851 tons or 84 per cent of the total known imports of tea (Koninckx 1980: 207). According to the statistical compilation of the value of imported goods by the SOIC in Nyström (1883), tea comprised 71.4 per cent of total value of imports between 1769 and 1777.¹⁰

If we assume some stability over time, it seems that Bohea tea alone made up more than half of the cargo arriving at Gothenburg on the Swedish East Indiamen. This makes Bohea tea the most relevant Chinese commodity for further research in this study. I have not investigated whether the ratio between land and labor in tea is representative for the other important commodities imported to Sweden, such as raw silk, cotton clothing or porcelain. Obviously, the data for Bohea tea cannot automatically be applied to Chinese export commodities in general since it might theoretically be another exception, just as with silver, discussed above. However, it has been chosen because it was by far the most important commodity.

What then did the SOIC export? Mainly metals and timber. According to Koninckx (1980: 184), whose study on the SOIC covers the years between 1731 and 1766, “[m]etals in the form of semi-finished or finished products constituted the bulk of the Swedish Company’s exports. The most interesting items relate to iron”; “[i]n general, bar iron always dominated Swedish exports, at least at the beginning of the eighteenth century” (Koninckx 1980: 185). Thus, bar iron seems to be the most important export commodity and is chosen for the same reasons as Bohea tea. However, the problem of the dominance of silver in the Sino-European trade returns. It turns out that most, if not all, of the bar iron and the other Swedish export commodities were not carried to China, but only to Cádiz in Spain, where they were sold to pay for chests of silver (Koninckx 1980: 193). Up to 1766, there is data on silver cargoes from a dozen of the SOIC’s expeditions and Koninckx confirms that silver was practically the only thing the Swedes could sell in China: “the cargo of Spanish piasters was the *sine qua non* of the Company’s trade” (Koninckx 1980: 190). By treating the silver as money, as a residual store of the value of the bar iron for which it was exchanged, a comparison between the iron and the tea bought with the silver coins is still possible.

Relations of the variables, and hypothesis

TSA is the difference in land and labor input into commodities from two areas exchanged at a certain price rate. If commodities from the areas are called I and II, TSA exists if there is an exchange of I and II and if $I \neq II$. TSA is the difference $II-I$. It is assumed that the part with the highest value is peripheral to the part with the lowest value. If $I > II$, Area A is periphery and Area B is core. It is crucial to establish the price relation of the exchange: otherwise it will be impossible to assess how much of I was exchanged for II.

The ratio II/I expresses the magnitude of the inequality in the exchange. In order to assess the magnitude of the TSA, we also need data on what quantities of the commodities were exchanged. By multiplying the sum with the quantities of the exchange, the total TSA is assessed. In this case, however, the total quantities of the exchange are not the focal point. Both Pomeranz' use of ghost acreages and Hornborg's use of TSA aim to measure European ecological relief by former colonies, and therefore emphasize the total sums of the land and labor saved, but I am interested in using TSA here as a measure of the relative exchange of land and labor in the trade between Sweden and China, in order to settle their positions in the hierarchy of the world system.¹¹

Each factor (I and II) has two dependent variables: annual hectare yields (i) and the human labor (ii) embodied in production (cf. Hornborg 2007: 261). That there are two variables measured in different units is a complicating factor since no clear independent variable can be established in the equation. If the measure is formalized as

$$TSA = (I \neq II)$$

and the dependent variables of I and II are I.i, I.ii, II.i and II.ii but no common unit exists for the categories i and ii, the equation is unsolvable unless divided into two equations:

$$\begin{aligned} \text{space appropriation (SA)} &= (I.i \neq II.i) \\ \text{time appropriation (TA)} &= (I.ii \neq II.ii) \end{aligned}$$

where, of course,

$$TSA = TA + SA$$

If there is a net transfer of land and labor in the same direction, such as

$$(I.i < II.i) \wedge (I.ii < II.ii)$$

it is possible to qualitatively conclude that there is time-space appropriation to either Area A or Area B, and it is also possible to calculate quantitatively the SA and the TA of the commodities separately, but it is not possible to quantify the extent of total TSA. If there is a mixed result, such as

$$(I.i < II.i) \wedge (I.ii > II.ii)$$

it is not possible to conclude whether there is any time-space appropriation at all; even if, for instance, the net TA appears to be much larger in one direction than the net SA in the other direction, trying to forge them together without a common unit is like comparing apples with pears.

For this case we have four dependent variables:

- I. i: Annual hectare yields embodied in the cultivation and production of Chinese Bohea tea per price unit.
- ii: Days of human labor embodied in the cultivation, production and transportation of Chinese Bohea tea per price unit.
- II. i: Annual hectare yields embodied in the extraction and production of Swedish bar iron per price unit.
- ii: Days of human labor embodied in the extraction, production and transportation of Swedish bar iron per price unit.

A clear-cut Sinocentric relation of these variables must state that China was the net receiver of both embodied land and labor. The quantitative part of this essay's hypothesis can thus be stated as:

$$(I.i < II.i) \wedge (I.ii < II.ii)$$

Frank and Pomeranz had divergent views on the importance of land and labor in determining “the Great Divergence”, and the basis for this was their differing views on the relative wages and land pressure in China and Europe. Pomeranz asserts that both wages and pressure on land were roughly equal, while Frank claims that wages were lower in China, mainly as a consequence of higher land pressure. Straining these assertions somewhat, it could be claimed that Pomeranz would expect that the labor and land input per price unit in the Sino-European exchange would be roughly the same, while Frank would expect that the Chinese input of labor per price unit would exceed the European input, but the opposite would be true for inputs of land, which would be higher in Europe than in China. The logic behind these assertions would be that the relative costs for land use¹² and labor would decide how much of these factors could be put in commodities that are equally exchanged price-wise.

If the logic is accepted, three scenarios could be set up, corresponding to the hypothesis of this essay (RW) and to my interpretations of the implications of Pomeranz’ (KP) and Frank’s (AGF) hypotheses:

RW: $(I.i < II.i) \wedge (I.ii < II.ii)$	Embodied land and labor of Swedish export commodities exceed those of the Chinese
KP: $(I.i \approx II.i) \wedge (I.ii \approx II.ii)$	Embodied land and labor of Swedish export commodities roughly equal those of the Chinese
AGF: $(I.i < II.i) \wedge (I.ii > II.ii)$	Embodied land of Swedish export commodities exceeds those of the Chinese, while the opposite is true for embodied labor

Approximate values

To give approximate values to the dependent variables and relate them quantitatively through prices and currency exchange rates, all data collected would ideally be from a limited time period. To

obtain that requires hard empirical work, in my experience, since the labor time and land requirements for commodity production are usually not readily available in the standard works of economic history. Through reading of a lot of sources, and with some puzzling, estimating and adding, I think it has been possible to get reasonable estimates for the data needed. Of course, the values could have been based on even more detailed and comprehensive studies, but one has to draw the line somewhere. I decided to include the direct labor used in the production of the commodities as well as in the most important inputs – raw material and fuel, and the labor used to transport the commodity to the domestic staple port. For land, it is also essentially the land required for raw materials and fuels that is included in the assessment. Thus, the land and labor needed to feed workers and animals are not included, neither is an assessment of the land and labor invested in capital (industrial or landesque), or the labor put into, for example, the trans-oceanic voyages and their ships.¹³

One interesting methodological question concerns how to account for the additional labor and increased price due to long-distance transport. What I call the staple port method may be able to give an accurate answer. In this method, the prices of bar iron and Bohea tea are compared in the staple ports of Gothenburg and Guangzhou with respect to the currency rate between Swedish *silver daler* and Chinese *taels*. The method implies that the role of the SOIC was limited to transporting the items involved, adding transport labor costs and profits to the selling prices, but not really altering the price relation between the commodities. Instead of dividing the extra labor of the journey and the resulting price increase equally between the two commodities, transport and higher prices are cut out of the operation. Hence, the currency rate is very important in determining the price relation between the commodities. Besides data on the embodied land and labor of the commodities involved, the staple port method requires data on the prices of the commodities in the staple ports – bar iron in Gothenburg and Bohea tea in Guangzhou – as well as the currency rate between Swedish silver dalers and Chinese taels.

Results: exchange of time and space

A compilation of all the data is reported in the last section (p. 206), and the results of the quantitative estimations are summarized in Table 1.

Table 1. Embodied land and labor in eighteenth century Chinese Bohea tea and Swedish bar iron.

I.i:	Embodied land in tea: 3.1 (2.9–3.3) hectares/ton.
I.ii:	Embodied labor in the production and transportation to Guangzhou of tea: 1,432 (1,377–1,487) working days per ton.
II.i:	Embodied land in bar iron: 41 (37.4–46.9) hectares (mid-century), 38 (34.3–42.7) hectares (late century) per ton.
II.ii:	Embodied labor in the production and transportation to Gothenburg of bar iron: 163 (141–184) working days per ton.

Time-space appropriation, as defined previously, requires a comparison of land and labor content per price unit. The annual hectare yield and days of labor embodied in 1,000 silver dalers' worth of Swedish bar iron are calculated in Table 2, and for Chinese Bohea tea in Table 3. They indicate that there was an extraordinary net transfer of land from Sweden to China, while the net transfer of labor went in the same direction, but not at all to the same extent. According to Table 4, the ratio for land exchange is between 130:1 and 161:1, i.e. the land transfer per price unit was more than 100 times greater from Sweden to China than in the opposite direction. The ratio for labor exchange is between 1.23:1 and 1.40:1. Thus, the transfer of labor per price unit is measured to be circa 23–40 per cent greater eastwards than westwards.

Table 2. Embodied land and labor of Swedish bar iron.

Year	Price per ton (silver daler:öre)	Amount for 1,000 silver daler (ton)	Embodied land of 1,000 silver daler (hectares)	Embodied labor of 1,000 silver daler (working days)
1743	100:13	9.96	408 (373–467)	1,623 (1,404–1,833)
1748	113:25	8.79	360 (329–412)	1,433 (1,239–1,617)
1770	158:9	6.31	240 (216–269)	1,030 (890–1,161)
1772	185:–	5.41	206 (186–231)	881 (763–995)

Table 3. Embodied land and labor of Chinese Bohea tea.

Year	Price per ton (taels)	Amount for 1,000 taels (ton)	Amount for 1,000 silver daler (ton)	Embodied land of 1,000 silver daler (hectares)	Embodied labor of 1,000 silver daler (working days)
1743	234.6	4.26	0.8875	2.75 (2.57–2.93)	1,271 (1,222–1,320)
1748	247.2	4.05	0.723	2.24 (2.10–2.39)	1,036 (996–1,075)
1770	234.6	4.26	0.513	1.59 (1.49–1.69)	735 (707–763)
1772	226.2	4.42	0.502	1.58 (1.46–1.66)	719 (692–747)

Table 4. Exchange ratio of land and labor per price unit for Swedish bar iron and Chinese tea.

Year	Embodied land bar iron:tea	Embodied labor bar iron:tea
1743	148:1	1.28:1
1748	161:1	1.38:1
1770	151:1	1.40:1
1772	130:1	1.23:1

According to Table 4, the amount of both time and space appropriation is greatest in 1748 and 1770. In the first case, this is because of high Chinese prices and a currency exchange rate that favored China. In the 1770s, it is the combined effect of inflation in Sweden and deflation in China that causes the considerable differences between 1770 and 1772. In 1772, tea prices were among the lowest in Glamann’s (1960) table covering 1732 to 1772.

These results clearly suggest that the eighteenth-century trade between Sweden and China was characterized by ecologically unequal exchange, as measured by TSA. The quantitative part of the hypothesis, defined as

$$RW = (I.i < I.ii) \wedge (I.ii < II.ii)$$

with a price unit equal to 1,000 silver dalers, can be expressed as

$$RW = (2.75 < 408) \wedge (1,271 < 1,623)$$

for the year 1743 and is thus verified with an immense margin for land and a slight margin for labor. The results for 1748, 1770 and

1772 reveal a congruent pattern. The measures for labor in Sweden and China could be defined as roughly the same, thus confirming the Pomeranz-inspired KP scenario for labor but not for land, where the difference is vast and robust. The RW hypothesis can be regarded as further strengthened by the reasonable assumption that Swedish bar iron embodied more “dead” labor in the form of greater capital stocks than Chinese tea. Had the resulting higher labor productivity been included in the calculation, the calculated net transfer of labor from Sweden to China would have been greater.

These results do not fit the Frankian AGF hypothesis for labor, since it predicted higher input per price unit for the Chinese export commodities than for the Swedish. For land, it predicted higher input in European export commodities, similar to the RW-thesis. This was also very much the case. If the assumed higher Chinese land productivity had been quantified and included in the study, the difference in land input would, however, have been less striking.

Even though no far-reaching inferences can be drawn from this study on the divergent opinions of Pomeranz and Frank regarding wages and land pressure in Europe and China, I can at least conclude that the results do not support Frank’s claim that wages were lower in China, and they do fit into Pomeranz’ view of roughly similar wages. On the other hand, the results do not support Pomeranz’ assertion that land pressure was roughly equal in Europe and China. They indicate greater land pressure in China since vastly more land could be invested in the export commodities from Europe.

Regarding the qualitative part of the hypothesis of this essay – “In the early modern world system, China was in a more central structural position than western Europe” – the results point to a confirmation since the quantitative part – “and thus net receiver of embodied land and labor in the SOIC’s eighteenth century trade between Sweden and China” – is verified. However, whether or not the net transfer of land and labor is indeed an expression of structural position within the world system cannot be settled by quantitative methods: it remains a point of analytical persuasion. The arguments for such an understanding were thoroughly discussed above.

Taking into account the (semi-)peripherality of both Sweden and tea-producing Fujian to the interregional cores, plus Sweden's access to subsidized Spanish-American silver *and* the clear results in this case study; taken all together it at least suggests that the early modern Western European nations were peripheral to the world system core of China. The results thus support Frank's view of a Sinocentric world system rather than Pomeranz' view of a polycentric one.

Developing time-space appropriation

Time-space appropriation is quite a novel and undeveloped method, but with the potential to be a sophisticated indicator of ecologically unequal exchange in the past. I have briefly discussed some methodological concerns, such as how to account for the past ("dead") labor put into the capital used in the production of the selected commodities; where to draw the line for which land- and labor-requiring activities to include in the production of the selected commodities, including the reproduction of the labor force; how to value abiotic resources harvested only once; and the lack of a common unit for land and labor which is potentially obstructive to unequivocal results.¹⁴ The discussions of TSA as a relevant measure of core/periphery status in the world system and the methodological choice of the staple port method were more detailed. Without any doubt, all these concerns or problems could be developed further, as could the theoretical foundations discussed in the first part of the chapter. My conclusion is that it would be worthwhile to do so, since the advantages of the method, on balance, seem greater than the drawbacks.

Empirical accounting in detail: Approximate values and their relation

Points in time

The initial ambition was to be able to calculate the time-space appropriation for this case of Swedish–Chinese exchange as early as possible after the foundation of the SOIC in 1732. The reason is that the early modern period is the focus of this chapter; it ended about 1800 but many Asian economies started to show signs of decline as early as in the mid-eighteenth century (Frank 1998: 264). However the determining factor, it turned out, was access to the necessary data. To calculate the time-space appropriation in this case, we need data about the embodied land and labor of Swedish bar iron and Bohea tea, their prices in Gothenburg and Guangzhou, and the exchange rate between the Swedish silver daler and the Chinese tael. Access to this data is patchy and insecure, as will be shown below. The final approximations are rough, but I still deem them solid enough to provide an interesting result.

The point in time when the most data are simultaneously available is the early 1770s. We have access to market price scales in Sweden for all of the 1770s, and Guangzhou tea prices for 1770 and 1772. I was also able to conduct calculations for two years in the 1740s. There was a fast depreciation of the Swedish currency during this decade. In 1743, the silver daler was at a peak in relation to the tael, with a ratio of 4.8:1. Five years later the tael was almost one silver daler more expensive. We have access to Guangzhou tea prices for the 1740s as well as market price scales for bar iron in Värmland. However, there are no observations permitting an adjusted estimation of the transportation costs of bar iron from Värmland to Gothenburg. I will therefore also use an estimation of transportation cost from the 1770s for the 1740s. It biases the study only marginally since the transportation cost is only a small part of the total cost.

Swedish bar iron: embodied land

The land embodied in iron is not derived from the area used for the mine, which would be minimal, but from the forest area needed to grow the timber used for the charcoal and the “mine timber” necessary for production. The only estimate obtained of mine timber consumption – 15 cubic meters per ton of iron – concerns the mid-seventeenth century (Sundberg et al. 1995), but I assume it also to be valid also for the eighteenth century. Estimates of the charcoal needed in the foundry and at the trip hammer for the production of one ton of bar iron vary between 50 and 52.5 cubic meters early in the eighteenth century, and 40 to 44.5 at the end of the century (Arpi 1951: 92–93; Hildebrand 1987: 77).¹⁵ According to Arpi (1951: 110), to produce one volume unit of charcoal required 1.2 units of fresh wood, while Nordström (1952: 33), uses a ratio of 1.57 cubic meters of wood per cubic meter of charcoal, to “maximize the safety margin”, and Sundberg et al. use a 1:1 volume ratio. I will stick to Arpi’s better-founded ratio, which is close to the mean value of the other two estimates and implies that the amount of fresh wood needed to produce the required amount of charcoal is between 60 and 63 cubic meters early in the century, and 48 to 53.4 at the end of the century.

The sustainable yield of Swedish forests in the eighteenth century varied with climate and forestry practices, and approximations in the literature are few. After comparing sources, Arpi (1951: 214) arrives at the estimation that the average forest growth in the iron-producing area of Sweden in 1830 was between 1.5 and 2.0 cubic meters per hectare of forest land. He uses the median figure, 1.75, for his calculations. Since productivity was probably somewhat lower in the eighteenth century, a marginal 5 per cent reduction of Arpi’s figures leads to a productivity between 1.42 and 1.9 cubic meters. This is also close to Nordström’s (1952: 33) assumption of a growth rate of 1.5 cubic meters per hectare in the eighteenth century.¹⁶ This uncertainty, however, widens the span of probabilities.

Table 5. Estimates of fresh wood and land requirements for production of Swedish bar iron.

	Mine timber (m ³ /ton)	For charcoal (m ³ /ton)	Total (m ³ /ton)	Forest land (hectares/ton)
Early 18 th (≈1700–1732)	15	60–63	75–78	47.2 (39.5–54.9)
Mid 18 th (≈1733–1765)				43.9 (36.3–51.5) ^a
Late 18 th (≈1766–1799)	15	48–53.4	63–68.4	40.6 (33.1–48.2)

^a Mean values of Early and Late eighteenth century estimates.

Sources: Arpi 1951; Hildebrand 1987; Sundberg et al. 1995.

Thus, all in all, some 44 hectares of forest land were needed to produce one ton of bar iron in the mid-eighteenth century, decreasing to 41 hectares at the end of the century.

Swedish bar iron: embodied labor

To produce iron, labor was put into logging, charcoal-making, and the transport of wood and coal to the trip hammer; into the often long transport of iron from the trip hammer to the staple port; into manual work at the production sites (smiths, melters, fire-watchers etc.), as well as into administration (scribes, clerks etc.). None of the three studies referred to below reports all of these kinds of labor input, and therefore I will have to use different sections from different studies to complete the puzzle.

In 1762, 332 persons were on the payroll for the iron works *Horn-dals bruk* in Dalarna, but this figure corresponded to only about 100 full-time employees (Hildebrand 1987: 89). The consensual opinion (Boëthius 1951: 414; Essemyr 1989: 73, Montelius 1962: 245) is that a year in Sweden at the time consisted of 300 working days, which for Horndal gives us 30,000 working days to produce between 200 and 250 tons of bar iron per year in the second half of the eighteenth century (Hildebrand 1987: 89). Using the mean figure, 225 tons, every ton of bar iron from Horndal in 1762 would thus have embodied 133 working days. However, parts of the production process are missing, such as administration and management, as well as the mining of the ore. In a similar study on the Säfsnäs agglomerate of iron works in Dalarna, mining is included and assessed at 5–6 per cent of the total labor input in bar iron (Montelius 1962:

288). If we suppose that the same relation was true in Horndals bruk in 1762, the total number of working days embodied in one ton of bar iron rises to 140 (126–159) days.¹⁷

Mats Essemyr (1989) lists the people working at the iron works at Forsmark in northern Uppland in the year 1765. His thorough presentation also includes administration and management – even the priest and the parish clerk. If we exclude them,¹⁸ 114 persons were employed in the industry (Essemyr 1989: 46), which, if they worked 300 days a year, corresponds to 34,200 working days. The production of this industry in 1765 was 3,042 ship pounds “mine weight”, or 455 tons (Essemyr 1989: 73, 195). This would mean that the embodied labor of each ton of bar iron at Forsmark amounted to 75.2 working days. This excludes the labor required for the mining of the ore and only includes a minor part of the production of the charcoal needed. It probably also excludes a large part of the transport work needed. Adding the estimated 6–9 working days needed for mining the ore used produce one ton of bar iron in Säfsnäs, the number rises to 82.5 (81.2–84.2) working days per ton. Most of the charcoal needed was produced by the surrounding farmers (Essemyr 1989: 45). By estimating the total input of labor needed to produce the charcoal and fresh wood, and subtracting the working days put in by Forsmark employees, the labor input of farmers, including transportation to the mine and iron works, adds over 30,000 working days a year or 67.3 working days per ton of bar iron.

Commonly, peasants from the surrounding area also carried out most of the transportation work. According to Essemyr’s list, nine drivers and three boatswains were employed by the industry, capable of 3,600 working days or about 5.3 per cent of the labor time calculated so far. In the other studies, transport (excluding transport of charcoal) amounted to 19 per cent in Horndal 1762, and 32 per cent in the more remotely situated Säfsnäs in 1768 (Montelius 1962: 288). Even though the location of Forsmark, about 30 kilometers from the Dannemora mine and close to the Baltic sea, certainly lowered the need of transport work compared to the inland iron works, it seems very doubtful that only 5.3 per cent of the total labor input would have been used for transport. A conservative guess is to at least triple the amount of total transport

work to 10,800 working days. This adds another 15.8 working days per ton of bar iron bought from farmers and puts transport at about 14 per cent of the total labor input at Forsmark. All in all, one ton of its bar iron thus embodied 166 working days.

From Forsmark we can now derive the factors missing for Horndal; i.e. administration and mine timber. In Forsmark, the administration consisted of 6 employees, equalling 4.0 working days per ton of bar iron, and labor input in mine timber was 6.8 working days per ton. When added to the earlier calculation based on Horndal's iron works in 1762, the total number of working days embodied in one ton of its bar iron rises to 151 days. We now have at least two fairly detailed estimations of embodied labor in Swedish eighteenth century iron: 151 (137–170) working days per ton of bar iron in Horndal in 1762, and 166 in Forsmark in 1765. As a conservative estimate, in further calculations I will use the mean of the two earlier estimates, which is 158.5 (137–180¹⁹).

For this study, it is, however, justifiable to further increase the input of transport labor. The reason is that the Swedish East India Company sailed from Gothenburg, which is further away from the iron-producing sites than, for example, Stockholm. The iron traded in Gothenburg regularly came from industries in Värmland, and was first transported to Karlstad or Kristinehamn on Lake Vänern, and thereafter shipped to Vänersborg on the southern shore of the lake. Until the Trollhättan Canal was opened in 1800, further land transport was necessary from Korseberg near Vänersborg to Åkerström south of Trollhättan, from whence the river Göta älv was navigable to Gothenburg. The cost of getting the bar iron from Kristinehamn to Gothenburg “was widely greater than a simple trip on Mälaren” to Stockholm, according to Hildebrand. One calculation from the 1770s reports that it cost almost 3 copper dalers to freight one ship pound of iron from Korseberg to Åkerström, but only 21 öre (about one fifth of the cost) to send it by boat from Åkerström to Gothenburg (Hildebrand 1957: 334–337). Additionally, the iron works in Värmland had a longer than average journey to the staple ports. The bar iron from the large-scale industry in Uddeholm, for example, had to be trans-shipped from rowing boats to horsedrawn carts and back to rowing boats eight times before even reaching Karlstad (Hildebrand 1957: 339).

When adding an estimate of the transport labor input for bar iron in Gothenburg, it therefore seems most reasonable to use the estimation of transport used for the remote Säfsnäs (which also de facto delivered iron bars to Gothenburg). At Säfsnäs, the transport of bar iron constituted 9 per cent of the labor input of the iron ore in 1768, according to Montelius (1962: 288), while mining constituted 6 per cent. Above, we estimated mining to contribute 7.3 working days per ton of bar iron, which would mean that transport of bar iron amounted to 11.0 working days per ton. From Horndal's iron works in the middle of the iron-producing region of Sweden, still inland but not as remote as Säfsnäs, the transport of bar iron in 1762 required 6.8 working days per ton of bar iron²⁰ (Montelius, Utterström & Söderlund 1959: 218), i.e. 4.2 working days fewer than for Säfsnäs. An additional 4.2 working days per ton of bar iron therefore seems reasonable, if conservative, in this particular case. To sum up, one ton of eighteenth-century Swedish bar iron transported to Gothenburg would have embodied about 41 hectares of land and 163 (141–184) working days. This labor assessment is based on studies of the 1760s, and I will assume that the figure is valid both for the middle and the late eighteenth century.

Chinese Bohea tea: embodied land

The most preferred tea from the Swedish East India Company – as well as from the other European Companies – was Bohea black tea, originating from the Bohea, or Wuyi, mountains in the north-west of the province of Fujian. This tea undergoes several stages of production. After being picked, the leaves are spread in thin layers that allow a current of warm air to circulate around them for roughly twenty-four hours. The withered leaves are rolled in order to break down the cell walls and release their oils, and are then sorted according to size and condition into various classes of tea. Next comes fermentation, in which the leaves are spread out and exposed to very humid and temperate air for one to three hours. The leaves are finally dried in hot air (Stella 1992: 39–40). The assessment of the embodied land of eighteenth-century Bohea tea will be divided into three parts; first, the actual land needed for

cultivation of the tea bushes; second, the land needed to provide the firewood used for processing the leaves into dried tea; and third, the land needed to provide the manure or other types of fertilizers used in cultivation.

I have not been able to find any literature referring to original sources on the hectare yield of Bohea tea in the eighteenth century. There are, however, sources on and estimates of the yield in the late nineteenth and early twentieth centuries.²¹ Robert Gardella, in *Harvesting Mountains*, refers to conservative estimates, based on both English and Chinese sources, of an average tea yield per acre in Fujian in 1887 and 1941 of around 400 pounds in both cases (Gardella 1994: 118). Even though not explicitly stated by Gardella, the land productivity of Fujian tea-farming does not seem to differ substantially between the eighteenth and the late nineteenth or early twentieth centuries. I will therefore assume that an average yield of 400 pounds, or 181.4 kilograms, of dried tea per acre is also valid for the eighteenth century. One ton of tea thus required 5.5 acres – 2.24 hectares – of tea plantation.

The production of tea also required fuel for heating, for which mainly firewood was used. I have come across no historical data or assessment on the amount of fuel needed to produce any specific amount of tea. The closest I came is an article reviewing the energy consumption of Chinese tea production in the 1980s, which also gives a figure for 1949 (Ni & Zhou 1992). Accordingly, the 1949 Chinese output of 46,000 tons of tea required 93,700 tons of CE (Coal Equivalent: 2.93 GJ/ton), or 2.04 tons of CE per ton of tea. According to Ni and Zhou, the processing of black tea of the Bohea type requires less energy than green teas. While the processing of one kilogram of green tea consumes on average 2.04 kg of CE, black tea only requires, on average, 1.27 kg. If we assume that energy efficiency improvements between 1949 and 1980 were the same for black tea as for the total tea production, then the energy consumption for one ton of black tea in 1949 was 1.40 tons of CE. That corresponds to circa 2.45 tons of firewood. This can be compared to the claim that “in some mountain areas in Zhejiang Province, the villagers still cut 4 tons of firewood for making one ton of tea” (Ni & Zhou 1992). In the same paragraph, it is stated that “in Yingshan County,

Hubei Province, 9,000 tons of firewood are needed every year for tea baking, equalling an annual productivity of 36,000 mu (5,929 acres) of forest”, which implies that one acre of Hubei forest annually produces 1.52 tons of firewood. That would imply that the 2.45 to 4 tons of firewood estimated for the production of one ton of black tea required the annual firewood yield from 1.61 to 2.63 acres of forest.

Turning to fertilizers, Rawski (1972) covers, among other things, the peasant economy during late Ming and early Qing China in the Chien-ning prefecture in northwest Fujian, of which the Wuyi mountains form a part. In the early seventeenth century, gazetteers in Chien-ning recommended the use of ash from firewood and other plants to enrich the soil (Rawski 1972: 82). There were no significant changes in the use of fertilizers for a very long time; even in the twentieth century, one third of the fields in northwest Fujian were not fertilized at all. Rawski's conclusion is that Chien-ning's agriculture remained backward (Rawski 1972: 96). In the light of this general description, it seems that the manure from humans and animals at the farm and the ash from the quite large amounts of firewood used in tea baking would have been the bulk of the fertilizers used in eighteenth-century Wuyi tea production. If so, no further embodied land needs to be added to the production of Bohea tea.

To summarize, I have estimated that in order to produce one ton of black tea in eighteenth-century Fujian, 5.5 acres of tea plantation were needed. In order to process the tea leaves into dried tea, the firewood yielded by 1.61–2.63 acres of forest land was needed. Using the mean figure, the embodied land of one ton of tea was 7.62 (7.11–8.13) acres, equal to 3.08 (2.88–3.29) hectares.

Chinese Bohea tea: embodied labor

The most precise and solid figure that I have come across for labor in Chinese tea production is mentioned in the classic work of J.E. Buck, *Land Utilization in China*, and refers to a massive study of thousands of Chinese farms between 1929 and 1932. In his Table 14. “Man labor requirements (number of days per crop acre) for growing various crops” the figure mentioned for tea is 126 (Buck 1964: 302). I consider this data to be applicable also to eighteenth

century conditions, since no major changes in productivity are reported to have occurred in tea farming between the eighteenth and the early twentieth centuries. However, it is not clear exactly what “tea” means in Buck’s table. My assumption is that he is referring to crude tea output including all manual work at the farm – planting, fertilizing, picking, baking etc. – while it seems reasonable to exclude the logging of the firewood needed. Since this is quite a marginal part of the embodied labor of tea, I have allowed myself to use a figure from Sweden. Previously, it was concluded that the logging and transportation of wood in seventeenth-century Sweden required circa 0.5 working days per cubic meter of wood, and I will apply the same figure for China.²² Above, I have claimed that 4 tons of firewood were consumed per ton of tea, which adds 2 working days per ton of tea. For the 400 pounds of tea produced on each acre, the addition of embodied labor for the logging and transportation of firewood amounts to only 0.36 working days.

Additional labor was of course required to produce dried tea from crude tea. I have seen no data, but some valuable clues about labor requirements for the processing of tea do exist. In Gardella (1994: 154), the total employment in China’s tea industry in 1935 is estimated. Tea farmers and tea pickers/crude processors outnumbered fine processors, tea manufactories’ employees and merchants by 9 to 1. This implies that the bulk of the labor embodied in tea is put in on the farms. However, these figures can hardly be seen as an exact proportion of labor input, since many of the farmers and farm workers were not occupied full-time with tea, while processors and merchants probably were to a higher degree. Another indication is found in Table 21 in Gardella (1994), which deconstructs production costs of Qinmen black tea in 1935. Here, crude tea represents almost two-thirds of the production cost of tea, while processing and packing represent a little more than 20 per cent of the cost. Transportation is 5 per cent and tax, profit and interest on capital constitutes 9 per cent (Gardella 1994: 159). The distribution of costs between crude tea and packed, dried tea is thus 75 to 25 per cent. If we assume that the figures for Qinmen black tea in 1935 are valid for eighteenth-century Fujian tea, and if we assume that the salaries of farm workers and processing workers were the

same, we have to add 44.4 working days to the 126 working days, to reach a total of 168.4 working days for the yield of one acre, i.e. 400 pounds of tea. Measured per ton of tea, processing and packing took 245 working days.

The British botanist and East India Company advisor on tea Robert Fortune travelled through the tea districts of China in the 1840s, and tried to estimate the cost of transporting the tea from the Wuyi mountains to Guangzhou and Shanghai.²³ According to his report, crude tea was bought by merchants at the mountain farms and most of it was brought to the city of Tsong-gan-hien for final processing. There, finished tea was bought from the merchants of Guangzhou and Shanghai connected to the interregional and international trade. The tea route from Tsong-gan-hien started with coolies carrying two chests of tea on their shoulders for five to six days northwards to a riverside at Hokow. There, the tea chests were loaded into boats. If intended for the Guangzhou market, they proceeded down the river westwards to the lake Poyang. When describing the rest of the route, Fortune refers to another nineteenth-century representative of the British East India Company, Samuel Ball. According to Ball, the tea chests were

conducted to the towns of Nan-chang-foo and Kanchew-foo, and they suffer many transshipments on their way to the pass of Ta-moey-ling. ... At this pass the teas are again carried by porters; the journey occupies one day, when they are re-shipped in large vessels, which convey them to Canton. The time occupied in the entire transport from the Bohea country to Canton is about six weeks or two months (quoted in Fortune 1853: 224).

Ball stated that the overland route “accounted for more than one third of the total transport cost, which was itself equal to one third of the initial cost of the tea at the point of origin” (quoted in Rawski 1972: 60). Ball’s figure of six weeks to two months is a vague estimate but the only one I have come across. I will proceed by assuming that seven weeks was the normal time for the journey.

The labor input in the distance travelled on land – six to seven days according to Fortune and Ball – is the easiest part to calculate.

According to Gardella (1994: 158), one chest of tea contained 0.54–0.62 piculs.²⁴ One picul equals circa 133 pounds (Gardella 1994, 6) which means that the contents of one chest are 72–82 pounds of tea. Two chests carried by one coolie implies that it would have taken about five (4.9–5.6) coolies to carry the annual crop of one acre (400 pounds). If this was done in six to seven days, the labor input amounts to 29–39 working days per 400 pounds.

After being carried for 6–7 days, and assuming a total transport time of seven weeks, 42–43 days of water transport remained until the tea reached Guangzhou. How much work did this add? In Fortune's study of the cost of taking the tea from Tsong-gan-hien to Shanghai, a journey he estimates at 28 days (of which 4 are spent waiting without any cost attributed), he reported that the cost per chest for one day of land transport is 133 cash, while transport on water cost 33–38 cash ("cash" here is a monetary unit, and the cost varied slightly on the different parts of the route). This cost difference hardly mirrors the difference of labor input exactly, since shipping is more capital intensive and perhaps was also better paid than carrying and therefore might have cost more per hour of labor. However, I will use the cost difference as a rough estimate of the difference in the labor input of land and water transport. I assume that the labor input on water is one fourth of the labor input on land ($133/4=33.25$), and that the transportation of 400 pounds of tea on water requires 1.2–1.4 working days per day ($4.9-5.6/4$). For the normal 42–43 days on water, the sum would be 50–60 working days. Adding the land transport, the total labor input for the transport of 400 pounds of tea from Tsong-gan-hien to Guangzhou would be between 79 and 99 working days.

To conclude this section, the production of 400 pounds of crude tea – the average yield of one acre of tea plantation – required 126 days of labor. The processing and packing required an extra 44.4 working days. The embodied labor of the firewood needed was marginal, assessed at 0.36 working days. The transport of the tea from Tsong-gan-hien to Guangzhou adds up to between 79–99 working days. Converted, the estimated labor input per ton of Bohea black tea transported to Guangzhou is shown in Table 6.

Table 6. Embodied labor in Bohea tea in Guangzhou.

Activity	Working days/ton
Crude tea	695
Firewood	2
Processing and packing	245
Transport to Guangzhou	435–545
Total	1,377–1,487 (mean: 1,432)

Prices of bar iron in Gothenburg

There are several sources for the prices of Swedish bar iron during the eighteenth century, but no continuous source on prices specifically in Gothenburg (cf. Boëthius & Kromnow 1947: 37). Therefore, I use the market scale prices collected for bar iron from Värmland, the major iron-producing region closest to Gothenburg, and add a reasonable transport cost. According to Jörberg (1972: 572), the bar iron prices per ship pound (149.6 kg) in Värmland were 13 silver dalers in 1743 and 15 silver dalers in 1748. In 1770, the price had risen to 21:21 in 1770, in 1772 to 25:21,²⁵ a more inflationary trend than expected.²⁶

A calculation from the Björneborg iron works in the 1780s stated the freight cost of one ship pound over lake Vänern from Kristinehamn in Värmland to Vänersborg at $3\frac{1}{3}$ or $6\frac{2}{3}$ shillings depending on season.²⁷ In the early 1770s, another calculation stated that the total cost of freighting one ship pound from Vänersborg to Gothenburg was 4 copper dalers and 26 öre (Hildebrand 1957: 336). Since the prices were quite stable in the 1770s and 1780s, without major inflationary tendencies, I will add both these costs in order to get a rough total for the transportation costs from Värmland to Gothenburg.

The $3\frac{1}{3}$ shillings for the trip over Lake Vänern amounts to 13 öre according to the pre-1777 monetary system. Adding the cost from Vänersborg to Gothenburg means that the total freight from Värmland to Gothenburg adds up to 2 silver dalers $\frac{2}{3}$ öre per ship pound. This figure should be reasonably valid at least for the time between the instabilities of the 1760s and the inflation in the late 1790s. Adding the transport cost gives us the estimated Gothenburg prices of bar iron as stated in Table 7 below:

Table 7. Estimated prices of bar iron in Gothenburg (silver daler:öre).

Year	Per ship pound ^a	Per ton
1743	15: ² / ₃	100:13
1748	17: ² / ₃	113:25
1770	23:21 ² / ₃	158:9
1772	27:21 ² / ₃	185:–

a of 149.6 kg.

Sources: Jörberg 1972: 571–572; calculations by Warlenius.

Prices of Bohea tea in Guangzhou

No exact information has been found on how much the Swedish East India Company paid for Bohea tea in Guangzhou. There is, however, some sparse data. Colin Campbell, the supercargo of the first Swedish vessel sailing to China, writes in his diary that he bought Bohea tea for 13 taels per picul and green tea for 10 taels (Johansson 1992: 59). Kjellberg (1974: 217) has found one contract drawn up between the supercargo Jean Abraham Grill and Cantonese merchants in 1767. The price was set at 15.5 taels per picul. There are, however, better sources for the prices paid by the contemporary Danish Asiatic Company, and there is no reason to assume that the Swedes would not have paid about the same. Glamann (1960: 131–133) has compiled a table of median prices of Chinese Bohea tea in Guangzhou for the years 1734–72. It reveals that after fluctuating prices during the first years of trade, there was a continuous increase in tea prices until 1754, when a sharp fall occurred and low prices prevailed until an increase in 1759, followed by high prices in the first part of the 1760s. The last years of the period show a fall in prices. Glamann's table has one column for median prices (taels per picul) for each ship loaded, which could be more than one per year, and another for the range of prices (taels per picul) paid for the loadings – one shipload could consist of tea delivered under slightly different business conditions. I use only the median price in Glamann's Table 6.2, and only data for the years where iron prices in Gothenburg could be assessed. For 1743 there is no data, but the median price in 1742 was 15 taels per picul, and in 1744, 13 taels per picul, so I use the mean value of 14 taels per picul. For

1748, there are figures for two shiploads at 14.7 and 14.8 taels per picul. Again, I use the mean value.

Table 8. Median prices of Chinese Bohea tea in Guangzhou (taels).

Year	Per picul ^a	Per ton
1743	14.0	234.6
1748	14.75	247.2
1770	14.0	234.6
1772	13.5	226.2

a of 59.68 kg.

Source: Glamann 1960: 132–133.

Currency exchange rates

The eighteenth-century exchange rates between Swedish silver dalers and Chinese taels was assessed by Edvinsson (2010) through the silver content of the respective coins. The exchange rate is listed in Table 9 for the years we are concerned with here:

Table 9. Silver dalers per tael.

Year	Silver dalers
1743	4.8
1748	5.6
1770	8.3
1772	8.8

Source: Edvinsson 2010.

Edvinsson's calculations are congruent with other historians' observations, although these are patchy. According to Kjellberg, the value of the tael was 4.5 silver dalers in 1743. Four years later it had risen to 5.4 silver dalers, and in 1777 to 8.8 silver dalers (Kjellberg 1974: 296). According to Nyström (1883: 151), one tael was worth 1½ riksdalers specie equal to 9 silver dalers, which is close to Kjellberg's data from 1777. In Koninckx (1980: 442), the exchange rate is set at "1 tael = minimum 4½ daler smt. [silver dalers]". No date is mentioned, but the period analyzed in the book is 1732–1766.

Notes

- 1 Pomeranz (2000: 4, 107) and Parthasarathi (2011: 22) reject both the received Eurocentric and Frank's Sinocentric views of the early modern period, regarding it as a polycentric world with several cores that besides China and Western Europe also included Japan, India, Persia, Russia, and the Ottoman Empire.
- 2 For introductions to ecological economics, see Martínez-Alier 1987 or Daly & Farley 2004.
- 3 See the special section in *Ecological Economics* 41(2) (Martínez-Alier & Schandl 2002) as well as later, occasional articles (see Barca 2011 for a fairly recent overview), and the chapters in the second part of Hornborg, McNeill and Martínez-Alier 2007.
- 4 My emphasis.
- 5 Hornborg's comment that "my impression is that the conventional economic discourse on industrialization conspires to keep such questions – and their answers – out of view" (Hornborg 2007: 362) is something that I can also endorse regarding early modern history.
- 6 Similar conclusions have been made earlier, by e.g. Borgström (1965) and Pomeranz (2000).
- 7 Another reason for redefining core/periphery status after incorporating Emmanuel might be his pertinent critique of the common view of peripheral production as "primary" and core production as more complex, requiring more skill or technology: "[S]ugar is about as much 'manufactured' as soap or margarine and certainly more 'manufactured' than Scotch whisky or the great wines of France" ... "Are there really certain products that are under a curse, so to speak; or is there, for certain reasons that the dogma of immobility of factors prevents us from seeing, a certain category of countries that, whatever they undertake and whatever they produce, always exchange a larger amount of their national labor for a smaller amount of foreign labor?" (Emmanuel 1972: xxx–xxxi).
- 8 He continues by stating that unequal exchange is not the only way to transfer accumulated capital from peripheries to cores; plundering, in the form of underpriced "privatizations" of state properties etc., was and still is another important method (Wallerstein 2004: 28), but accumulation by plunder is not considered in what follows.
- 9 Though we should keep in mind that when we include an ecological variable in the measurement of core/periphery status, it is not under the assumption that peripheral production is necessarily more "primary", but that the resources of the peripheries, like their labor, is underpriced for structural reasons and therefore constitutes an important loss that should be included in the equation.
- 10 Nyström does not state more closely what "value" refers to. It could, for instance, be the purchase price in Guangzhou or the sales price in Gothenburg, but it could also be the customs value set by Swedish customs for the purpose of taxation.
- 11 To a certain extent, this research design is a consequence of patchy access to data, making total figures of the exchange impossible to obtain. But the most important reason is that China's early modern exchange with Europe was not about ecological relief. It bought almost exclusively silver from the Europeans, and even though

silver production actually did require vast amounts of land (Moore 2007: 125–128, 133), land pressure was not the reason for China to import silver. There simply did not exist enough silver ore in China to satisfy the market, so imports were the only alternative. Another indication that the import of silver was not about ecological relief is that another precious metal could have been used as a monetary base for the re-monetization, if deemed necessary. China's silver imports were not a necessity, but a possibility.

- 12 It is presupposed that higher land pressure increases the cost of land use.
- 13 Arguments for these methodological choices are given in Warlenius 2011.
- 14 The possibility of converting land and labor into a common unit to get a clearer result in TSA analyses is discussed in Warlenius 2011. See also Hornborg 2009: 250–251.
- 15 Sundberg et al. 1995 arrive at 60 m³ of charcoal per ton of iron for the mid-eighteenth century, which, assuming the same efficiency rate as Arpi and Hildebrand for the nineteenth century, would be within the limits mentioned.
- 16 Sundberg et al assume a yearly “growth potential” of 4 cubic meters per hectare for the seventeenth century, but this figure does not seem to be reached through research in historical sources and is left unconsidered.
- 17 There is no presentation of total labor time at Säfsnäs in Montelius 1962.
- 18 Since no personnel conducting religious services are included in the assessment of Chinese tea in the next section.
- 19 The upward end of the span is assumed to be as far from the mean value used as the lower end, although I have not found any indications of such a high figure.
- 20 Transport of bar iron is said to be 9 per cent of the working days of the production of bar iron, and the total number of working days is 133.
- 21 It is of course a problem that this and also other data from China is from the nineteenth or twentieth century, and it is only partly comforting that several authors claim that no major changes in tea production occurred until the second half of the twentieth century. I am aware that referring this data back to the eighteenth century is congruent with the Eurocentric view of a “stagnant Asia”, but when earlier data is lacking, I do not see any alternative.
- 22 Transportation must be much longer in the more sparsely populated Sweden than in China, where trees were generally cultivated near the farms. According to Pomeranz (2000: 231) “transport costs [for fuel-wood] were minimal”. However, transport in Sweden was also fuel efficient since it was mainly carried out in wintertime when snow reduced friction.
- 23 His reason for this was to calculate the profit made by the Chinese merchants who arranged this transport, and to figure out whether cost cuts could be made if this trade was overtaken by the EIC. His conclusion, however, was that “it would appear that the profit upon common teas is very small, so small indeed as to make it a matter of doubt whether they will ever be produced at a reduced rate” (Fortune 1853: 228).
- 24 He equates 130,000 chests to 70,000–80,000 piculs.
- 25 Even though not clearly stated by Jörberg, the pre-1776 prices seem to be expressed in the tables as silver dalers and öre with the ratio 1:32.

- 26 The trend continued the following years, rising to 30 silver dalers per ship pound in 1775.
- 27 It is not stated which season cost more, but I assume it to be the winter season.

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National accounts in world history

Methodological problems and possible solutions

Rodney Edvinsson

Did the European economy overtake that of China as early as the fifteenth century, or was China more advanced economically than most of Europe up until the early nineteenth century? To answer these questions, historical national accounts of all the countries and regions in the world must be constructed leading back to the Middle Ages. In the last ten years, major progress in this direction has been made (Broadberry & Gupta 2009; Broadberry, Campbell, Klein, Overton & van Leeuwen 2010; Bassino, Broadberry, Fukao, Gupta & Takashima 2011; Broadberry, Guan & Li 2012). The main pioneering work in this field was carried out by Angus Maddison (2010). In his database he has extended the GDP series for all countries back to the year 1 CE.

Despite the progress made, there are a number of problems with various estimates concerning several countries. Co-ordination among researchers and comparisons between countries are still not fully developed, and there is a lack of international standards for historical national accounts. Gregory Clark (2009) remarks that:

All the numbers Maddison estimates for the years before 1820 are fictions, as real as the relics peddled around Europe in the Middle Ages. Many of the numbers for the years 1820, 1870, and 1913 are equally fictive.

Despite this, Maddison's database has gained wide currency among economists analyzing economic growth. His books are among the most quoted among social scientists, historians and economists in Google Scholar. Clark continues:

Just as in the Middle Ages, there was a ready market for holy relics to lend prestige to the cathedrals and shrines of Europe – Charlemagne secured for the cathedral in Aachen, his capital, the cloak of the Blessed Virgin, and the swaddling cloths of the infant Jesus – so among modern economists there is a hunger by the credulous for numbers, any numbers however dubious their provenance, to lend support to the model of the moment. Maddison supplies that market.

Since Maddison's death in 2010, a project has come into existence to update his database, but it also reproduces the problematic methods applied by Maddison, for example, his method to use the purchasing power parities of 1990 as a benchmark for all other periods.

The main concern of national accounts is how to measure different aspects of the production and distribution process. In the general debate, the impression is often given that the value of aggregate production, often taken as synonymous with GDP, is unequivocal once you have reliable sources. Sometimes the data is taken for granted even if it is not based on reliable sources at all. Aggregate production can, however, be calculated using different methods and definitions, which can lead to quite divergent interpretations of economic development. GDP is a controversial measure from many points of view. There are also different methods to calculate GDP that can give quite different results (Inter-Secretariat Working Group on National Accounts 1993: 14).

The constant price fallacy

Volume or real value is a kind of constant price estimate. The effect of different price levels must be eliminated when volume growth or volume values are measured. In national accounts, this is achieved by making a comparison in constant prices of the production at two different points in time or in two countries. The nominal series is deflated by a price index to arrive at volume values. The difficulty does not end there, since the question still remaining is which constant prices and index formulas to use. Various techniques to eliminate the inflation component result in different conclusions concerning economic growth and the relative standing of various

countries. This drawback is often glossed over by economists. Part of the problem is theoretical in origin. Many theories of economic growth, mostly of neoclassical origin, use a one-commodity model. In international comparisons the focus is on aggregate GDP, often disregarding comparison of the various components of GDP. Real economies, however, consist of many goods and services.

A simple example can illustrate the deflation procedure. Take an economy that in year 1 produces one billion tons of apples and in year 2 one billion barrels of oil. Has the economy experienced a positive, negative or zero growth rate in constant prices? This depends on how a barrel of oil is valued in comparison to a ton of apples, i.e. on the relative prices of oil versus apples. If a barrel of oil is valued at more than a ton of apples, the economy has experienced positive growth. If a barrel of oil is valued at less than a ton of apples, the economy has experienced negative growth. Finally, if a barrel of oil and a ton of apples are valued equally, the economy has experienced zero growth.

A further issue is which year's relative prices should be used. In the above example, assume that in year 1 a barrel of oil is worth more than a ton of apples, and in year 2 a barrel of oil is worth less than a ton of apples. In the prices of year 1 there has then been positive economic growth, while in the prices of year 2 there has been negative economic growth. The first is called a Laspeyres volume index, while the second is called a Paasche volume index (for both indices, the first year is here considered the base year, and the second the compared year). The difference between the two indices is often small, but can accumulate to large differences over time. Over longer periods, a Laspeyres volume index tends to display a higher growth rate than a Paasche volume index if the base year is earlier in time than the compared year, the so-called Gerschenkron effect (Jonas & Sardy 1970: 83). The Gerschenkron effect arises when activities whose relative prices are falling tend to increase their volume shares of total production and vice versa. This is what happened during the industrial revolution. Manufacturing expanded its volume share of GDP at the same time as the relative prices of manufactured goods decreased. This was due to faster increases in productivity than for other sectors.

If the Laspeyres volume index displays just a 0.3 per cent higher growth per year than the Paasche volume index, over 200 years this amounts to 82 per cent. Comparing the relative development of two countries that use different index formulas over a longer period of time can have huge consequences. Maddison's database consists of such series, and the difficulty has been ignored or glossed over by many economists and social scientists who use his data. Some researchers have argued for the use of the geometric average of the two indices – a so-called Fisher price index – or equivalent formulas, but this has seldom been adopted in historical national accounts.

In poor countries, the prices of goods and services for domestic consumption are, in general, lower than in rich countries. In addition, exchange rates tend to fluctuate considerably. Therefore, when the GDP or GDP per capita of various countries are compared, Purchasing Power Parities are constructed to eliminate differences in price levels, which is equivalent to the comparison of a country over two time periods. These parities are expressed in national currency units per United States dollar (similar to the exchange rate). The relative price level in one country is the ratio of the purchasing power parity to the exchange rate. Even if the prices of many goods did not converge internationally until the nineteenth century (O'Rourke & Williamson 2002), the price differences between countries today for many services may be as large or even larger. The problem of different prices is as much a problem today as it is for the reconstruction of historical national accounts of the preindustrial period.

A difficulty with purchasing power-adjusted GDP is that it can be computed in different ways. PPP-adjustment removes the difference in the absolute price level, but not the relative price differences. In addition, there are two ways to compare countries' GDP per capita over time: the use of current and constant PPPs.

Current purchasing power parity entails two countries' GDP for one year being compared in that year's PPPs. This means that the new PPPs have to be calculated each year, and this method is very time-consuming.

The other method entails PPPs only being calculated for one year, the benchmark year. When comparisons are made for earlier or later years, they are based on the estimated economic growth

rates of the various countries. For example, assume that the GDP per capita of country *A* grows by 100 per cent between 1950 and 1990 and of country *B* by 200 per cent during the same period. If the two countries' GDP per capita is valued at 20,000 dollars in 1990's prices and PPPs (the benchmark year), country *A* had a GDP per capita of 10,000 dollars in 1950 and country *B* a GDP per capita of 5,000 dollars in 1990's constant PPPs. No current PPPs are needed for 1950 to make this calculation. This is basically the method applied by Angus Maddison in his comprehensive database. All the data back to the year 1 CE is expressed in 1990 so-called Geary-Khamis dollars.

There are drawbacks with both methods. Let us continue our example with oil and apples. Suppose that Norway's GDP consists of only one billion barrels of oil, and Sweden's of only one billion tons of apples. Which country has the highest purchasing power-adjusted GDP? It depends on the relative price of oil and apples. If a barrel of oil is valued at half as much as one ton of apples, Norway's real GDP is half that of Sweden. However, if world oil prices increase significantly in relation to apples, so that a barrel of oil is valued at twice as much as one ton of apples, Norway has twice the real GDP of Sweden (here we assume one world price, but if the relative prices are different in Sweden and Norway, using Sweden's or Norway's prices results in different PPPs as well, as in the example of growth from one year to the next). This would illustrate why, for example, Norway's purchasing power-adjusted GDP per capita fluctuates sharply from one year to another, despite the fact that Norway's real GDP per capita does not display any equivalent fluctuation. The explanation is the high share of oil production in Norway's GDP and the high volatility of the relative price of oil on the world markets.

Using constant PPPs has the advantage that annual fluctuations are reduced. However, we then have another dilemma pertaining to long-term growth and terms of trade. Assume that between 2010 and 2020 Norway increases its oil production by 100 per cent, from one billion to two billion barrels of oil, while Sweden's production remains stagnant at one billion tons of apples. In constant PPPs, Norway's GDP has doubled compared to Sweden's. However, assume that the price of apples remains the same, while the price

of oil declines by 75 per cent. The growth in Norway would then be counteracted by a decline in its terms of trade. In fact, while in constant PPPs Norway's GDP would double in comparison to Sweden, in current PPPs it would be halved compared to Sweden's, between 2010 and 2020. In the example, Norway became relatively poorer since it could buy fewer apples and less oil on the world market than Sweden, despite experiencing faster economic growth. Again, the differences are usually quite small for shorter periods. For longer periods, which are what matters for economic historians, the accumulated effects could be huge.

The method of constant PPPs is primarily used in international economic-historical research, such as in Maddison's database. Most historical reconstructions of GDP transform their figures into 1990 Geary-Khamis dollars, to make the new series comparable with Maddison's database. This entails comparisons of countries' volume GDP for earlier centuries being distorted and not being the same as if current PPPs had been used. If a country produces a large share of goods and services whose relative prices fall faster than for other goods and services, this country shows a higher per capita growth than other countries if the position compared to other countries does not change in current purchasing power parities. In this case, current PPPs provide a more accurate picture of a country's long-term economic development in relation to other countries, although the constant PPPs provide a more accurate picture of annual changes. The only way that Maddison's method could work is if the terms of trade of all countries were unchanged for longer periods of time, an assumption that cannot be supported theoretically or empirically.

Against this background, Prados de la Escosura (2000) argues that current purchasing power parities are preferable when examining how countries' relative positions change over time. Ward and Devereux (2003) show that estimates of current purchasing power parity entails the United States having had a per capita GDP that was well above that of the UK as early as the 1870s, while Maddison's method of constant purchasing power parity implies that the United States did not overtake Britain until around 1900. Broadberry (2003) argues that there are problems with Ward's and Devereux's calculations, and maintains that both methods should

be used. Recently, Broadberry and Klein (2012) presented the PPPs for the years 1905 and 1927. Their result is that Sweden's purchasing power-adjusted GDP per capita for the year 1905 is 10 per cent higher if 1905 year's purchasing power parity is used instead of 1990 international Geary-Khamis dollars.

The definitional dilemma

A great difficulty for international comparisons is that the historical national accounts of different countries are not based on the same methods and definitions. Alternative measures of production and volume growth are mostly ignored. Generally, historical national accounts attempt to follow modern national accounts. However, due to the constant revisions implemented by modern statistical offices, historical national accounts tend to use older methods and definitions. Some definitions of the production boundary are also specific to individual studies.

Certain assumptions and definitions are necessary for the reconstruction of historical national accounts and trends for economic growth. One problem is taking for granted that assumptions and definitions are based on a fully developed market economy. For example, assumptions that various assets, such as land and equipment, were used competitively even during the Middle Ages, can lead us to the wrong conclusion concerning the level of income these assets generate. Even today, much of production is not profit-maximizing.

One issue concerns which activities to include in aggregate production – i.e. where to put the “production boundaries”. According to modern international guidelines for national accounts, a distinction is made between activities that are “productive in an economic sense” and those that are not. There are many inconsistencies in this respect when it comes to official national accounts. Many economic activities, mostly outside the market economy, are not included in the GDP – most importantly, unpaid domestic or voluntary work – while all goods produced for own use are to be included since a revision of international guidelines implemented in 1993, which is of particular importance for developing countries (United Nations et al. 2009: 99).

One issue in historical national accounts is whether manufacturing in homes is to be included in the measured GDP or not. For some countries this has significant repercussions on the estimate of industrial production. Modern guidelines today entail the inclusion of manufacturing in homes. Internationally, historical national accounts often calculate the approximate scope of manufacturing in the 19th century by following the intermediate consumption of raw materials. Indirectly, that entails the inclusion of manufacturing other than factory production and handicrafts (Bourguignon & Lévy-Leboyer 1990: 266; Grytten 2004: 249; Horlings et al. 2000: 37–45). However, the production boundary of SNA 2008 is not used consistently for all countries. For example, the historical national accounts of Finland include manufacturing in homes for sale on the market, but not for own use (Hjerpe 1996: 33). In Swedish national accounts two different definitions have been used, one including manufacturing in homes (Edvinsson 2013) and one excluding it (Edvinsson 2005; Schön & Krantz 2012). A problem with Maddison's database is that he mixes GDP data in which different definitions of the production boundary are applied.

One of the largest drawbacks in constructing national accounts is the reliance on price. This is especially problematic when valuing non-market production. In the agrarian societies production was mostly for self-use. At least during the early modern period a large part of it was traded. Prices exist for most goods and services. One important aspect with indices is that even if they have low validity, if they are measured consistently they may still be quite good indicators of economic growth and fluctuations. That is the main, although quite shaky, argument for using modern definitions of GDP for the preindustrial period as well. It is also reasonable to argue that it should be possible to compare production in one period to the level in a later period.

A major weakness is how to deal with products and services that are not priced, or where the pricing mechanism is distorted. These products and services can either be excluded or assigned a fictitious price tag. Historical national accounts deal with economies where market relations only affected a small part of production; but modern economies also consist of large sectors that are not priced. Although

the international guidelines recognize that unpaid household services are “productive in an economic sense”, it is argued that the “inclusion of large non-monetary flows of this kind in the accounts together with monetary flows can obscure what is happening on markets and reduce the analytic usefulness of the data” (Inter-Secretariat Working Group on National Accounts 1993: 5), and that “there are typically no suitable market prices that can be used to value such services” (Inter-Secretariat Working Group on National Accounts 1993: 124). For historical national accounts this statement is particularly awkward, since the purpose of reconstructing a GDP series is to measure production rather than to provide data suitable for economic policy. Some researchers therefore argue that there are good reasons for historical national accounts to include unpaid household services (Jonsson 1997: 49). Especially for earlier times it is difficult to apply the distinction between “paid” and “unpaid” labor, since most of the production was for final self-use.

Different methodologies have been developed to measure the value of unpaid household work by putting a price tag on it (Edvinsson 2009). One method is to equal the value of these services to the labor input, utilizing the wage of paid domestic labor as an indicator. This method has been put into operation in *The National Income of Sweden* (Lindahl, Dahlgren & Kock 1937: 213–215) and later in Swedish historical national accounts (Krantz 1987: 17). However, doing this runs the risk of putting the wrong value on the actual work performed. A more appropriate method is to estimate the market output of these services (Nyberg 1995: 22–28). This also gives different results, depending on the indicators used. One solution may be to equal the value added per unpaid household working hour to the average value added per working hour within the market sector (Folbre & Wagman 1993: 285). Nevertheless, such a measure has little to do with how such services would actually be valued on the market, and it does not add any new information apart from that already provided by the estimates of unpaid household work in terms of actual working hours. The question of the labor productivity of unpaid household work in relation to market activities needs to be empirically investigated and not taken as given.

According to Anita Nyberg (1995: 25–27), the monetary estimates

of the value of unpaid household work in different industrialized countries vary between 30 and 60 per cent of GDP, which is quite sizable. It is likely that the proportion is even greater for earlier periods.

How do we measure changes in the level of production if we are dealing with an economy that does not know of any prices, such as a self-subsistence economy? Surely, it should be possible to construct volume indices for economic growth for such an economy as well. One solution is to use the relative prices of a modern economy, but such relative prices might be completely different from the relative valuations of the economy under study. Not all economies or activities are priced, but labor is the foundation of all human production.

An alternative to relative price may be to compare how much labor time it takes to produce two goods, i.e. to use relative labor times instead of relative prices to construct a volume index, accounting for the labor time embodied in intermediate consumption as well. Rickard Warlenius discusses in this book how embodied labor can be used to analyze flows at the international level. In studies of hunter-gatherer societies, where there are neither money nor prices, anthropologists often use the number of hours spent on different activities of the total worked per week to describe the economic structure of these societies (Cashdan 1989: 23). The labor time is a cost, in terms of foregone free time. Such a volume index is, in contrast to the usual one applied in national accounts, completely independent of price relations. If prices are proportional to labor values, then this type of volume index gives exactly the same result as the volume index based on relative prices. But when prices and labor values diverge, the labor value volume index favors activities that have a low value, reckoned per working hour, if it is assumed that labor productivity is the same as in other types of activities such as, for example, government and household services. It is of course desirable that the productivity differences between laborers should be considered. However, without information on price, such comparisons can only be made if similar types of product are considered. Table 1 presents an example of using labor productivities as an alternative to prices when weighting quantities produced. Imagine an economy *A*, consisting of one household (one woman and one

man), with a strict, gendered division of labor. The woman and the man each work 8 hours a day. In total there would, therefore, be 16 working hours per day. The woman produces 8 kilograms of grain, while the man produces 8 kilograms of meat. This means that the productivity of economy *A* is 1 kilogram of grain per working hour and 1 kilogram of meat per working hour. However, because of gender discrimination and due to the strict, gendered division of labor, women's work is valued less than men's. In our example, the price of grain is set to 1 dollar per kilogram and for meat to 2 dollars per kilogram. This means that the total daily work of the woman is valued at 8 dollars, while that of the man is valued at as much as 16 dollars, in total 24 dollars. It is also possible to make this calculation if no prices are known for economy *A* (i.e. if there is complete self-sufficiency), if we apply the prices of a market economy at about the same level of development.

Imagine now that we observe another economy, *B*, which produces 16 kilograms of grain and 4 kilograms of meat. We may not know the productivity or the prices of this economy. Economy *B* may, for example, be an adjacent household or the same household at another period in time. To estimate the volume of production of economy *B* compared to economy *A* we can use the usual method based on constant prices. In the prices of economy *A*, economy *B* is valued at 20 dollars, i.e. one sixth less than the value of economy *A*. However, calculating how many hours it would take for the economy *A* to achieve the production of economy *B*, we arrive at the figure 20 hours, which is 25 per cent longer labor time than in economy *A*. The difference arises because in the prices of economy *A* men's work is valued more than women's. Both indices of production are Laspeyres volume indices, if the economy *A* is considered the base period. The Paasche volume indices, which use the prices and labor productivities of economy *B*, produce other results that are, however, not presented in the table.

Under perfect market conditions, if men and women were to produce the same amount of grain and meat per hour, then there would be no price difference between grain and meat. In reality, gender discrimination would probably come into play, preventing women from doing men's work and vice versa. Furthermore, we may

not know the prices of economy *A* and *B*, if both were self-sufficient. In that case only a volume index based on labor productivity should be calculated, since using the prices of a third economy could be misleading (the possible discrimination of the third economy may not apply to economies *A* and *B*).

Table 1. Illustration of how to compute a volume index to compare two economies using prices and labor productivity respectively.

	Production	Productivity	Price	Volume index, prices of economy A	Volume index, productivity of economy A
Economy A	8 kg grain	1 kg grain	1 dollar	24 dollars	16 hours
	(women)	per hour	per kg		
	8 kg meat	1 kg meat	grain		
	(men)	per hour	2 dollars		
			per kg		
			meat		
Economy B	16 kg grain			20 dollars	20 hours
	4 kg meat				

Errors and lack of transparence

The empirical material of historical national accounts consists of both primary and secondary sources. These sources do not use the same classifications and definitions. Breaks often occur when different time series for the same variable but different periods are compared with each other. Modern standards for national accounts change constantly, which requires retrospective revisions (“Utredningen om översyn av den ekonomiska statistiken” 2002: 18–23). Such revisions are made regularly by the statistical offices, but unfortunately, often only for an insufficient number of years, which creates new breaks in the series. When time series are linked with each other, the figures of the original time series are changed, and there is a risk that the linked time series will provide an inadequate picture of the actual values or levels.

Some of the difficulties when constructing historical national accounts stem from the status of doing this type of research. Often it

does not fit the format of journal articles, which means that writing extensive documentation of how data has been constructed is not very rewarding for individual researchers. This dilemma is shared with other fields of historical statistical reconstruction. Even so, historical national accounts often have better documentation than that of official statistics. One solution would be to redefine historical statistics as a separate academic field, and to create new academic journals specializing in this field. These could then also publish detailed documentations.

Since historical national accounts deal with quite extensive quantities of material, it is almost inevitable (according to the laws of probability) that they should contain errors, even if much time has been spent on double-checking and calculating series in different ways. Such errors can be found in most studies dealing with a large amount of quantitative material, and this is further aggravated by the lack of documentation and transparency. For countries where different researchers have constructed different historical GDP series, these often deviate substantially from each other. In spite of this, the impression is often given that the estimates are very accurate.

Feinstein and Thomas (2002) argue that the publication of new historical data should present the margin of errors in a transparent way. This is seldom done however, and the reader cannot gain any idea of how reliable various estimates are.

The users of historical national accounts must be much more conscious of the weaknesses and assumptions underpinning various series. What historical national accounts usually provide is in statistical terms the expected value of various aggregates. For example, if there is a 40 per cent probability that the GDP per capita of Sweden in the year 1000 was 600 dollars and 60 per cent probability that it was 800 dollars, the expected value of the two numbers is 720 dollars ($0.4 \times 600 + 0.6 \times 800 = 720$). Even if we knew that only a GDP per capita of 600 or 800 dollars is possible, and never a GDP per capita of 720 dollars, presenting the number 720 dollars minimizes the squared error in the estimate. The most important thing is, however, that the expected value presented is not biased. For example, if we know that the calculated estimate probably underestimates the

actual value, it is better to increase the estimate, even if it is based on very rough judgments.

Deciding the actual margin of error is very difficult. Instead, Feinstein and Thomas (2002) propose that researchers should present subjective margins of error for various series based on their calculation methods and assessments of the material. If the margin of error of an aggregate series is to be calculated we also need to estimate a correlation matrix of the different errors, which might also be based on the subjective judgments of the researcher. For example, the root mean square error (RMSE) of the sum of the estimates of A and B is calculated as:

$$RMSE_{A+B} = \sqrt{(RMSE_A)^2 + (RMSE_B)^2 + 2Corr(E_A, E_B)RMSE_A RMSE_B}$$

$Corr(E_A, E_B)$ is the correlation between the two errors E_A and E_B which can range from -1 (perfect negative correlation) to $+1$ (perfect positive correlation). If there is no correlation this term is set to zero. The above formula shows that a correlation closer to $+1$ increases the margin of error of the sum of the two estimates. If there is no correlation between the errors of the individual series, this in turn means that the margin of error of the sum is reduced. We can also calculate a coefficient of variation of the error, as the RMSE divided by the estimate, which can be presented as a percentage.

Assume, for example, that the estimate of the GDP of Norway is 100 billion dollars and of Sweden 100 billion dollars. Assume, furthermore, that the RMSE in both instances is 10 billion dollars, which means that the coefficient of variation of the error for each country is 10 per cent. The estimated GDP for Norway and Sweden together is then 200 billion dollars. The estimated margin of error of this sum depends on the correlation between the two errors. Using the formula above, if the correlation is zero, then the estimated RMSE of the sum is 14 billion dollars and the coefficient of variation 7 per cent. If the correlation is $+0.5$ then the RMSE is 17 billion dollars and the coefficient of variation 8 per cent. If the correlation is $+1$, then the RMSE is 20 billion dollars and the coefficient of variation 10 per cent. Under the very unlikely cir-

cumstances that there is a perfect negative correlation, the RMSE of the sum would be zero.

This reasoning can be extended to the sum of many estimates. For example, assume that we estimate the GDP of 100 countries, encompassing the whole world, at 100 billion dollars each. The estimate of global GDP will then be 10,000 billion dollars. Assume that the RMSE of each individual country's estimate is 10 billion dollars and the coefficient of variation 10 per cent. If the errors of the individual countries are perfectly correlated then the coefficient of variation of the error for global GDP is also 10 per cent. However, if the errors of the individual countries are not correlated with each other, then the coefficient of variation for the error of global GDP is just 1 per cent.

The point is that given that there is no perfect correlation, the sum is relatively more accurate than its individual components. The various errors partly even each other out. That is one of statistical theory's important insights. Despite the problems of reconstructing historical national accounts for individual countries, estimates of global GDP might actually be more accurate than most or even all of the estimates individual countries. A major problem, however, is whether or not there is a systematic bias across countries, which renders the correlation of their errors close to unity. One such bias complicating our analysis of global GDP in the preindustrial period is Maddison's assumption of a 400-dollar subsistence level.

The 400-dollar subsistence level versus a real wage

There are divergent interpretations of per capita economic growth in the Middle Ages and the early modern period in Western Europe, one, Malthusian, assuming stagnation or even decline and one, Smithian, assuming steady growth. While Maddison advocates the Smithian view, Clark (2009) criticizes Maddison's assumptions of continual economic growth in the Middle Ages and the early modern period in Western Europe. Different indicators point in different directions. The real wage series supports the Malthusian view, while this indicator is heavily criticized by Maddison (2007). Historical national accounts have been used to support either one

or the other of the views. The quandary is that these series display quite different developments, despite the fact that they claim to measure the same variable (GDP per capita).

To reconstruct global GDP back to the Middle Ages or even earlier, we need data on population, the agrarian sector and the non-agrarian sector. All three are problematic to estimate. Maddison and others use outdated population data, which is further discussed by Janken Myrdal in this book. An important check for the agrarian sector is whether or not it yields reasonable estimates of the implied calorie consumption, but that is sometimes forgotten, and many estimates are too low. The non-agrarian sector is probably the most difficult component to calculate. An indicator that has been used, the rate of urbanization (Persson 2008: 170), might be inappropriate given that in some countries most of the non-agrarian sector was located in the countryside.

The Smithian viewpoint mainly rests on Maddison's assumption of a 400-dollar subsistence level in 1990 Geary-Khamis dollars. This is based on the data showing that the poorest countries only had a GDP per capita at this level, or slightly above, in that year. In fact, Maddison uses that to estimate the GDP per capita for most countries in the year 1000 CE. As Gregory Clark (2009) puts it:

One crucial element is his assumption that the basic subsistence GDP per capita of all societies is \$400 (1990 international prices). This is the fundamental constant in Maddison's world, the basic unit of human existence. Any society without a sophisticated production technology, without significant urbanization, and without a substantial rich class, or just where nothing is known, is assigned this minimum. Thus around 1000 AD the various parts of the world are mostly assumed to have incomes either of \$400 (uncivilized) or \$450 (civilized)... What is that subsistence income in real terms? In 1990 US \$ prices, a pound of white bread cost \$0.70. So Maddison's \$400 is the equivalent of 1.6 lbs of wheaten bread per person per day, or 1,500 kcal. That is an extraordinarily low income, rarely observed in practice. Since most societies have inequality, the poorest in such a subsistence economy would have lived on the equivalent of much less than that daily 1.6 lbs of bread. So if the

poorest people spent nothing on clothing, heat, shelter, light, and consumed only the cheapest form of calories such as bread, they would still be engaging in hard physical labor on a diet well below 1,500 kcal in the Maddison vision of subsistence.

Since many Western European countries had a GDP per capita of over 1,000 dollars in 1820, Maddison draws the conclusion that there was substantial growth in Western Europe during the Middle Ages and the early modern period. The assumption that the subsistence level was 400 dollars in 1990 Geary-Khamis dollars distorts the comparison of Western European countries with the rest of the world in the early modern period and before (Jerven, 2012 and 2013). It seems unlikely that a country like Sweden on the periphery of Western Europe had a GDP per capita around 1800 that was twice the level of poor African countries in 1990. It is likely that the estimates for 1990 undervalue the actual GDP level for the poorest countries. For example, it should be considered that the new revisions of international guidelines in 1993 recommended the inclusion of all goods production in GDP, also the proportion that is only consumed by the producers themselves. Poor countries implementing this have revised their data upwards.

An alternative to calculating GDP is to use the income approach. This is applied by Gregory Clark (2010) for the English economy for example, by calculating the incomes derived from wages, capital and land retrospectively as far back as the Middle Ages. His estimate of English GDP per capita in the Middle Ages is set much higher than Maddison's, but is also higher than the corresponding estimate for England based on the production approach presented by Broadberry et al. (2010). Clark's series displays stagnating GDP per capita in England between the Middle Ages and the early nineteenth century, which runs counter to the views of most other economic historians.

The main predicament when applying the income approach to the preindustrial period is that most of the incomes have to be estimated theoretically, since only a small part of the economy was monetized. Clark makes the assumption that the ratio of capital income essentially followed wages and rent values, with a correction for long-term changes in the profit rate; and because of the risks

he adds 3 per cent. It is also possible that his rent values are overestimated. According to him, his series “measures rental values when land was rented in a competitive market, not the average rents paid by land occupiers which would often be lower because of customary leases” and that the estimated rent values “are much higher before 1820 than in the recent series of Michael Turner, John Beckett and Bethany Afton” (Clark 2002: 201). Altogether, this implies that his estimated property incomes may be overestimated. Clark (2010) estimates the wage share in 1200 at 47.8 per cent, which was even lower than its share in 1860 at 65.1 per cent.

There is a difference in estimating theoretical production values and incomes. For example, while estimates concerning theoretical values for building houses for own-use may be questionable, they are still based on actual production activity. However, when the contribution of capital or labor is included in the estimation, the researcher may be led astray since their contribution must first of all be measured in terms of how much they contribute to physical output. Estimating physical output is necessary before anything can be said about incomes. Even in developed countries today some of the incomes must be imputed as well. The category of mixed income concerns production units where the laborer also owns the capital. It is a sum of labor and property income. In a previous study, I have tried to estimate property income in agriculture in late twentieth-century Sweden (Edvinsson 2005). Since the self-employed constituted a large proportion of the labor force, most of the labor income must be calculated by using the wage rate of agricultural wage earners. Property income cannot be estimated directly, but only indirectly, as the value added less the estimated labor income. The net property income thus estimated was negative for the late twentieth century. The only conclusion that could be drawn was that both capital and labor in modern Swedish agriculture are paid significantly below their market values. In other words, not knowing physical output and using the income approach for Swedish agriculture in the late twentieth century would significantly overestimate value added if it was assumed that capital and labor were paid their full market values. Assuming market rates for the Middle Ages must be even more questionable.

A variant of the income approach is the estimation of agricultural production according to the so-called demand approach. For example, Malanima (2010) presents annual estimates for Italy back to 1300 and Álvarez-Nogal and Prados de la Escosura (2011) for Spain back to 1270, using the approach for agricultural output, while other activities are approximated from the rate of urbanization. The demand approach was advocated by Allen, based on positing a demand curve for agricultural products (Allen 2001: 13). Consumer theory requires that own price, income, and cross-price elasticities of demand add up to zero. Agricultural production is accordingly calculated from the development of real wages and the real prices of agricultural and non-agricultural products. The change in income is set equal to the change in the wage rate, and rests on the assumption of no change in the wage share or labor input per worker. The problem is, of course, that the assumption of a constant wage share has no empirical backing, even for the modern period. Theoretically it is motivated by the Cobb-Douglas function in neoclassical growth theories, but there are also other functional specifications that do not rest on constant shares in the incomes of labor and capital. It is also quite likely that the share of labor income was much higher in the Middle Ages than it is in a fully developed capitalist economy.

Both the 400-dollar approach and the identification of income with wages rest on shaky assumptions. To overcome the gap between Smithians and Malthusians, new measures involving more direct indicators of production are necessary. One path forward would be to apply a kind of expenditure approach, where reasonable estimates are made of consumption and investment patterns to assess the level of GDP per capita. The expenditure approach can also be used to cross-check whether various estimates are reasonable, such as in the quote by Clark that even the assumption that people only live on bread must entail a GDP per capita above 400 dollars.

One example can illustrate the expenditure approach. Most studies show that food consumption in Sweden, as well as in other European countries, was better in the late Middle Ages than in the early nineteenth century. An indicator of the food nutrition standard is average heights. For Sweden, archeological findings record that the average height among men was 170–174 centimeters in the Middle

Ages, and 170–176 centimeters in the Viking Age (Gustafsson et al., 2007). An average height of 172 centimeters is recorded for conscripted men born in the 1890s, and a height of 173 centimeters for conscripted men born around 1910 (Öberg, 2014: 17). In 1900, food consumption made up around one third of GDP. In that year GDP per capita was 2,202 (1990 Geary-Khamis) dollars (Edvinsson 2013). Food consumption alone therefore contributed around 700 dollars of the GDP per capita. A similar calculation for the early nineteenth century shows that food consumption contributed around 600 dollars, consumption of clothes 100–150 dollars, housing around 50 dollars, wood products (mostly firewood) around 50 dollars, and building and construction around 50 dollars of the GDP per capita. Given the climatic conditions in Sweden it is unlikely that people could survive having much worse clothing, housing and warming in the Viking Age or the Middle Ages than in the early nineteenth century. Assuming that other parts of GDP contributed to less than 50 dollars per capita, GDP per capita in Viking age and medieval Sweden should have been around 1,000 dollars, which is at the same level as in the early nineteenth century.

This very simple application of the expenditure approach shows that the GDP per capita in Sweden probably did not change much before the nineteenth century. Any growth in some parts of GDP, such as trade and the public sector, was mostly offset by a decline in food standards. More importantly, the estimate proposed here is much above Maddison's assumption for Sweden of 400 dollars for the Viking Age and 650 dollars for the year 1500. This gives further support to Clark's view that no society could have survived on a 400-dollar subsistence level in 1990 Geary-Khamis dollars, even if we allow that different climatic conditions can be accompanied by different subsistence levels (Jerven, 2012: 119).

Summary and conclusions

This paper focuses on the methodological questions and dilemmas of reconstructing historical series of global GDP. Various problems stem in part from the endeavor to make international comparisons. If we want to compare countries and world regions we need common

definitions and methods. Sometimes differences must be assumed to be of minor importance, otherwise comparisons cannot be made. Data collection is extremely time-consuming even for a single country or a single branch for a short period. Estimating world GDP in the last centuries or millennia necessitates certain shortcuts.

It must be emphasized that constructing historical national accounts is not an exact science. It is inevitable that certain assumptions must be made and accepted. Even modern national accounts are not very exact, and are based on much guesswork, such as, for example, estimating the extent of illegal activities to be included in GDP, or other activities prone to a high degree of tax evasion. The solution may lie in publishing all the documentation and calculations that underpin the historical national accounts.

Interpreting history is never a neutral act and neither is the critique of such interpretation. Which methods and definitions to use is not just a purely objective question, but is also dependent on the purpose to which each series is to be used. Official national accounts are not socially neutral, as they may appear, and are adapted to the needs of the social community of the present day, not least its economic policy. Using the same definitions and methods to construct macroeconomic series back to the Middle Ages, or even the dawn of mankind, unavoidably introduces anachronistic elements. Writing history on the basis of the definitions of official national accounting is in a sense partly writing history from the perspective of the social system that has conquered the whole world in the last two centuries, namely the capitalist system. Putting a price tag on all the goods and services produced in societies mainly based on self-sufficiency could still be done, however, if there were at least some market activities. However, economies with no market activity, such as hunters-gatherers, cannot be valued in current prices. Similarly, some productive activities, such as unpaid domestic services, currently excluded from official estimates of GDP, are not priced. There is, therefore, a need to develop new alternative measures that are not necessarily based on valuations using market prices.

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