



THE ANALYSIS OF LARGE SOCIAL SYSTEMS IN GLOBAL HISTORY: A METHODOLOGICAL APPROACH

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Resumo: This article begins from the assumption that in a rapidly globalizing world, an important frontier in the understanding of the history of human society is the recognition and definition of large social systems. We begin with an attempt to define the 'large social system', proceed with analyzing the best methodology for defining and measuring such a system. It is argued that large social systems often have particular economic or institutional characteristics which act as 'sinews' for the system. At the same time, a large social system is often to be found hiding in 'plain sight' as it were. This is because its scale does not easily conform to a single nation or culture—the usual units of analysis for historians of human society. Thus, simply identifying a large social system, and defining its temporal and geographical boundaries, can bring significant insights. Identifying key mechanisms which actuate the system is often integral to the definition process.

Of course, there are existing methodologies which can be of greater and lesser use for defining and explaining the workings of large social systems. We consider four of these: so-called 'Global History', World History, Comparative History, and New Institutionalism. The pros and cons of each of these methodologies are briefly assessed, and some new methodologies and guidelines are suggested for moving forwards with this exciting frontier in historical, economic, and social science.

Keywords: Global History; Large Social System; Comparative History

ANÁLISIS DE GRANDES SISTEMAS SOCIALES EN LA HISTORIA GLOBAL: UN ENFOQUE METODOLÓGICO

Resumen: Este artículo presupone que en un mundo que se globaliza rápidamente, el reconocimiento y la definición de Grandes Sistemas Sociales constituyen una importante frontera para la comprensión de la historia de la sociedad humana. Comenzamos por definir el "gran sistema social", luego analizamos la mejor metodología para determinar y mensurar dicho sistema. Argumentamos que los grandes sistemas sociales a menudo tienen características económicas o institucionales particulares que actúan como "tendones" del sistema. Al mismo tiempo, es frecuente encontrar un "gran sistema social" no perceptible a primera vista. Esto se debe a que su escala no coincide con una nación o cultura: las habituales unidades de análisis para los historiadores. Por lo tanto, la simple identificación de un "gran sistema social" y la definición de sus límites temporales y geográficos pueden constituir una gran contribución para el conocimiento de las sociedades humanas. La identificación de los mecanismos que accionan el sistema suele ser parte integral del proceso de definición.

Por supuesto, existen metodologías que revisten mayor o menor utilidad para definir y explicar el funcionamiento de los "grandes sistemas sociales". Consideramos cuatro ellas: la llamada 'Historia Global', la Historia Mundial, la Historia Comparada y el Nuevo Institucionalismo. Evaluamos brevemente los pros y los contras de cada una de estas metodologías y sugerimos algunas nuevas metodologías y directrices para avanzar con esta apasionante "frontera" en las ciencias históricas, económicas y sociales.

Palabras-clave: Historia Global; Gran sistema social; Historia Comparada.

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1. Large Social Systems vs. Comparative History: Definitions

The mandate for this essay is to ‘stimulate discussion on the uses of comparative history for understanding the making, development, and crises of large social systems’ (with reference to how comparative history mattered in my own research into the long-term history of slavery). This mandate is broad and therefore dangerous. Essays on such topics often veer into platitudes or pontification and are seldom worth the reader’s time. So going forward we must be as precise as possible, and make sure that we are adding some useful concepts to the historian’s toolbox.

This essay proposes that the study of Large Social Systems is one of the most fruitful labours that active researchers can engage in today. Because the analysis of Large Social Systems remains a young and ill-defined field, its objects and methodologies remain uncodified and in danger of being hijacked by a particular agenda. This essay will grapple with a total of five variables pertaining to the historical analysis on a large scale:

- Comparative History
- Large Social Systems
- World History
- Global History
- and
- New Institutionalism

Normally, five variables might seem too many for a single article to take on. But when we point out that the analysis of Large Social Systems is our overall goal, and that World History (or World Systems Analysis) will be treated only cursorily for reasons described below, then we are left with only three principal concepts to introduce and grapple with, viz., Comparative History, Global History, and New Institutionalism.

Taking these in turn, this essay will argue in parts one and two that *Comparative History* is necessary for Large Social Systems analysis, but is perhaps not the best methodology. It will further argue in parts two and three that we need to distinguish both Comparative History and Large Social Systems analysis from the emerging discipline of *Global History* and the older discipline of World History.

In part four, this essay will distinguish Large Social System analysis from *New Institutionalism*—a school of historiography with which it has much in common, but from which I argue it should also remain distinct. Part five will bring the preceding analyses to bear in a case study of Greater Mediterranean Slavery. The final part will summarize our findings and suggest opportunities for further research into Large Social Systems analysis.

Let us begin by taking the mandate above, and distinguishing our first two variables: Comparative History and Large Social Systems. The first variable is **Comparative History**. What is comparative history and why is it useful? On the surface of it, the idea of comparative history sounds so broad as to be tautological. Is not every history comparative in some way? Our first observation is, that whether something can count as comparative has to do with expectations. Expectations are in turn founded on what has come before—if most studies are based on, say, a single country, or a single century, or single industry, then whatever breaks this mould will be seen as comparative. And we could leave it at that. But we should be more specific if we want our term to retain scientific value. Probably the safest definition of Comparative History, which maximizes its epistemological value, is that it expressly compares two or more units, with the aim of offering new causal explanations for the phenomena in question. The rationale for being comparative, then, is to show that *explanations based on a single unit of study are insufficient to describe the whole*, while those based on two or more units can highlight patterns which were not evident in the case of a single unit. This can be either a) because a single unit did not reveal the pattern at all, or b) because the pattern, given only a single unit of analysis, did not seem scientifically significant.

Our second term of analysis is **Large Social Systems** (hereafter LSSs for short). At first this unit of analysis might seem as ‘meaningless’ as the idea of Comparative History. However, it should be noted that an LSS is an *object* comprised of actors interacting with other actors and objects across time, while comparative history is a *methodology* operating on two or more distinct objects. The idea we have of a Large Social System is that of a movement or development in human society, which might be so large, as to have eluded detection as a distinct

process or unit. It is literally the forest that we cannot see for the trees. In the nineteenth century, modern history writing was born in a European nationalist context, meaning that historians often ignored obvious and parallel developments which were going on in neighbouring countries. And it was not until recently that some Western historians began to take seriously the history of countries outside of the West, and that non-Western history departments gained the resources to conduct world-class research. Between these path dependencies, there are many Large Social Systems which remain to be discovered. This in itself is very exciting, because it means that there remain many new and important discoveries waiting to be made about the workings of human society over time.

An example will serve to concretize the correspondences and distinctions between an LSS and the objects of Comparative History. A few months ago, the present author was at a conference where someone presented on the development of newspapers in Belgium in the 1840s. They had meticulously researched all the Belgian dailies with reference to one another, and looked for purely 'Belgian' reasons why the newspaper blossomed at the time that it did, citing Belgian independence, laws, financial regulations, etc. But the thought had clearly not occurred to this researcher to look into similar developments going on in Paris and other French cities, at precisely the same time. In this way, Comparative History would have been very useful for discerning what was in fact a larger social system than the researcher originally envisaged. Clearly, a comparative study of say, the newspaper industry in France and Belgium, would reveal many processes which were particular to each country, but others which were shared by both industries. And this I would argue is the main value of Comparative History. But if we move further, then the notion of comparative history becomes a bit distorted, because the true development of the newspaper industry in Belgium cannot be understood without reference to a dozen or more national newspaper industries within which the Belgian industry developed. Even if most Belgian newspapermen were not directly aware of the other European newspaper industries outside of the Francophone world (and most Belgian papers at the time were in French), the fact is that the printing machinery, reporting techniques, freedoms of the press, public expectations regarding 'news', legal restrictions, etc., would all have developed

within a European-wide nexus, which had an impact on all of the participating national industries.

So here is where the notion of a specifically ‘comparative history’ begins to look too limited, and instead we have to focus on the limits of a LSS itself. In this case, we can say that the entire Western newspaper industry of the 1840s was part of a single LSS, which is the global history of newspaper printing. We might easily dismiss this as too large to analyze. In fact, the newspaper industry had a very definite beginning in northwest Europe in the years around 1700 (with a few antecedents stretching back two or more centuries before), and it was long limited to a few countries, with few active offices and presses, especially before the late nineteenth century. So tracing the contours of the global newspaper industry, at least before say 1900, might be a scientifically beneficial exercise. This would entail discovering the limits of an LSS, and it would entail a great deal of ‘comparative history’. But the main work of an historian tracing the global newspaper industry before 1900 would be to describe the existence of presses, offices, and editors, and to explain why the newspaper industry LSS was limited to the boundaries that it in fact observed.

From this, we arrive at our working definitions:

A **Large Social System** is an analyzable section (noun) of human society whose origins and development are more satisfactorily explained with reference to a *larger geographical focus* (space) and/or a *longer temporal frame* (time) than was originally evident. A Large Social System will usually contain an internal logic which operates according to a discernible set of rules or institutions. The aim of Large Social System Analysis (verb) is to discover the geographical and temporal limits of the LSS in question, and the rules which govern its development. To trace these limits, one has to define the core institutions or ‘sinews’ which signal the presence or absence of your system, e.g. printing presses producing newspapers, or slave markets. While Comparative History is certainly necessary for discerning the contours of an LSS, often the types of features one is looking for in a given culture or political unit are very broad, and so the idea of doing a ‘comparison’ in a meaningful sense, is perhaps of limited use, as will become evident below.

Comparative History is a type of analysis (verb) which explicitly compares two or more discrete units of analysis, for the purpose of finding causes and 'laws' which are not evident from the analysis of a single unit. Historians' natural bias is to assume that their unit of analysis (e.g. the Francophone newspaper industry in Belgium in the 1830s) works according to endogenous factors, when in fact, the principal contours might have been determined by exogenous factors (e.g., the history of newspaper printing in France). The primary goal of Comparative History is to distinguish endogenous from exogenous factors in such a case. Whereas Comparative History can be used to describe the contours of an LSS, the fact is that so many 'comparisons' might be necessary to trace an LSS, that Comparative History remains strongest when it is used to do the work just described.

2. World History and Global History: A Brief History

For students reading this essay, we should begin this section by defining the term 'historiography'. Historiography is the word that professional historians use to describe the act of writing about history (i.e., as a verb). The term can also be used as a noun, i.e., the historiography of a particular subject means the scientific literature that has been written about a given problem in history.

When attempting to assess the value and impact of our two main terms 'Comparative History' and 'Large Social Systems', our next task is to relate these to the already existing historiographical traditions of 'World History' and 'Global History.' Even if we can analyze LSS or do Comparative History without referring to the entire world, still, the historiography which relates to such large-scale topics tends to come under the headings of World or Global History. Historians familiar with these subjects will recognize that historiographers have long made a technical distinction between terms 'World History' and 'Global History.' Because of these technical differences, the two terms are not interchangeable, although students who are first coming to the topics might naturally think them synonymous.

Before we briefly define World and Global History, it will come as a surprise to many students to think that professional historians have not been writing about World or Global history for more than a few decades. In fact, these disciplines

were only invented in the 1970s and 2000s, respectively. When we think about it further, however, a number of major milestones in historiography will serve to underline the newness of the entire historical discipline as a mass endeavour. For one thing, just like the modern newspaper, modern scientific history has a very specific birthplace. While there were always chroniclers who attempted to use scientific methods with more or less success in all of the literate parts of the world, there was no such thing as a sustained scientific community of historians, until these began to develop in the universities of Northwest Europe during the later nineteenth century. While we might credit individuals such as Edward Gibbon or even some renaissance writers such as Leonardo Bruni with inventing the paradigm of modern historiography, it is true that these writers remained relatively alone, without a community of equally accomplished peers, for centuries after their death. In order to do science, one generally has to be part of an active researching community.

While the rise of ‘professional’ historiography occurred in a handful of universities in a handful of countries in the later nineteenth century, this historiography remained very spare by modern standards until World War II. It was the birth of the modern socialist state, and the corresponding expansion of the university systems in the Western and other developing countries, that spurred the growth of the modern history department.² And at the beginnings of this expansion, most historians still tended to write in a nationalist vein. As universities continued to expand, and as new faculty were recruited from the less ‘privileged’ classes, a generation of historians embraced Marxist or semi-Marxist principles, as a means of critiquing previously existing ‘bourgeois’ studies of nation, king, and ‘Great Men.’ The *Annales* School in France, which began already in the 1930s, was a pioneer of this movement and remained influential through the 1980s. Typical of *Annales*-style analysis was an emphasis on material goods and processes, which were akin to Marx’s ‘means of production.’ Studies in this vein tend to start with ‘the land’ and ‘means of subsistence’, and proceed to describe

² SCHOFER, Evan; MAYER, John. The worldwide expansion of higher education in the twentieth century. *American sociological review*, v. 70, n. 6, p. 898-920, 2005. Also GEIGER, Roger. **Research and relevant knowledge: American research universities since World War II.** Piscataway, NJ: Transaction, 2004.

how this 'Structure' created a 'Superstructure' of Ideas and Political Systems. (Both 'Structure' and 'Superstructure' are Marxist terms).³ As more women were admitted into intellectual life in the 1960s and 70s, they too demanded a history of women which set out to critique previously sexist studies based on the assumption that men's lives were normative. As more people of colour became historians from the 1990s, they in turn demanded a historiography which was de-centered from the idea that white people were normative. In this way, new generations of historians wished to see themselves reflected in the historiography, and the science of history tended overall to improve by this means.

With this background in mind, we can define our main terms. The term '**World History**' was given a technical meaning by the appearance of Immanuel Wallerstein's seminal *The Modern World-System: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century*, in 1974.⁴ In this volume, Wallerstein attempted to give a materialist-structuralist interpretation to the origins of modern global capitalism. His central theory is that by exploiting a global periphery, the most economically developed regions of capitalist Europe managed to create 'wealth-extracting systems' which enabled Europe to gain more and more wealth, while other regions of the world such as Asia, Africa, and South America were relegated to exploited backwaters. Wallerstein's insights proceed very much from the Annales school of historiography, mentioned above, from the Marxist-Materialist notion that Structure (economics) determines Superstructure (politics, ideology, and culture.) Thus, Wallerstein's ideas are Marxist-derived, particularly the idea that it is economic processes or means of production which underly anything like a 'global system.'

From Wallerstein's insights have followed a number of other influential theories and schools of historiography, including those of Post-Colonial studies. These especially have carried forward the idea that there is a 'Centre' and a

³ The key study is that of Braudel of the Mediterranean, originally published in French in 1949 as: BRAUDEL, Fernand. **La Méditerranée et le monde méditerranéen à l'époque de Philippe II**. Paris: Librairie Armand Colin, 1949. For the current English edition see BRAUDEL, Fernand. **The Mediterranean and the Mediterranean world in the age of Philip II (Vol. 2)**. Berkeley, Los Angeles, Londres: University of California, 1995.

⁴ For a modern edition see WALLERSTEIN, Immanuel. **The modern world-system I: Capitalist agriculture and the origins of the European world-economy in the sixteenth century (Vol. 1)**. Berkeley, Los Angeles, Londres: University of California, 1991.

'Periphery' with 'Metropolises' which tend to dominate, exploit, and dictate, while the colonies tend to be exploited, to have colonized peoples, who might or might not retain some agency to resist or otherwise counter the impulses of the colonizing society. But the structurally antagonistic nature of this relationship—the assumption that colonialism is exploitation akin to Marx's exploitation of the proletariat by the bourgeoisie—follows somewhat from Wallerstein's Structuralist interpretation of the World System. In general, those who follow Wallerstein's analysis and its offshoots are lumped into a school called 'World Systems Theory.'

Another very influential model of the World System was put forward by Janet Abu-Lughod in 1989, entitled *Before European Hegemony: The World System A.D. 1250-1350*.⁵ Abu-Lughod's book was a direct response to Wallerstein's book, and the *Annales* School's tendency to see World History as beginning with the so-called 'rise of European World Capitalism' in the sixteenth century. In her book, Abu-Lughod points out that the Islamic world had created a massive trading empire, and, really, a 'World System' very similar to that described by Wallerstein, which stretched from Mozambique all the way along the Indian Ocean and Persian Gulf littorals, which also encompassed India and Southeast Asia to Indonesia. Abu-Lughod was at pains to point out that this system existed independently of Europe, but she also did not play up the idea of centre and periphery in the way Wallerstein had done, and her implication was that Arab traders were less exploitative than their European counterparts. Whether or not this is true, Abu-Lughod's work marked the beginning of an attempt on the part of historiographers to 'de-centre' the narrative of World history from focusing on Europe.

The fall of the Berlin Wall in 1989 and the subsequent breakup of the Soviet Empire had a curious effect on the intellectual world: it made Marxism and any theory derived from it seem passé, almost overnight. The intellectual mood of the 1990s was set by Francis Fukuyama's famous essay 'The End of History', which argued that with the fall of communism, the long process of global history had

⁵ ABU-LUGHOUD, Janet. **Before European hegemony**: the world system AD 1250-1350. Nova York: Oxford University, 1991.

come to an end.⁶ The argument was that, since capitalism and democracy had now triumphed, that the rest of world history would be a relatively harmonious movement towards the unification of the world under a capitalist and democratic regime. And even in the aftermath of the 9/11 attacks, indeed until the Great Recession of 2008, almost every Western politician acted as though they were living in such a world. In the light of subsequent history this now seems naive. There were of course countering voices, such as Samuel Huntingdon, who argued in his book *The Clash of Civilizations and the Remaking of World Order*, that fundamental cultural differences meant that there could not be, at least for a long time, any real detente between certain areas and peoples of the world.⁷ But Huntingdon's argument for a 'Clash of Civilizations' seemed too simplistic and determinist to many historians, who sought elsewhere for explanations of why the world was the way it was. (N.B., it should appear from this essay that LSS analysis is a good strategy for those wishing to counter to Huntingdon's model.)

As Marxism and associated theories fell, hard, during the later 1990s, the very concept of theory itself also went out of fashion. This entirely unforeseen event occurred not only in historical studies, but in most other fields of the social sciences and humanities. Suddenly, people who wished to do 'large scale' history felt that the older materialist theories such as those of Wallerstein and Abu Lughod were no longer appropriate for their endeavours. Particularly hard hit were economic history, and theories of class, and economic inequality—ideas strongly associated with Marxism. One form of exploitation which survived the Marxism purge was the notion of identity exploitation, based on the newer Foucaultian insight that language is a carrier of power.⁸ Thus, during the early 2000s, almost all of the studies which gained serious attention were smaller-scale studies detailing the histories of some group which could be identified as a 'traditional minority'. Queer people, women, people of colour, religious minorities, etc., were

⁶ Originally published as FUKUYAMA, Francis. The end of history?. **The national interest**, v. 16, p. 3-18, 1989.

⁷ Originally published as HUNTINGDON, Samuel. The clash of civilizations. **Foreign affairs**, v. 72, n. 3, p. 22-49, 1993.

⁸ A key study which brought this concept of language as power, as well as the concept of punishment as a performance of power, to the attention of an anglophone audience is FOUCAULT, Michel. **Discipline and punish: the birth of the prison**. New York: Pantheon, 1977.

the groups upon which historians now focused most of their energies. A problem with this vast literature was that in the relative absence of theory, it was often seen as enough to write about 'minority' groups simply for the sake of writing about minority groups.

While history was in danger of devolving into an atheoretical study of underprivileged people for its own sake, other historians were focused on the evolving concept of '**globalization**'. This was a major buzzword of the 1990s and early 2000s, which was based on the idea that economic globalization would create greater wealth for everyone. This idea also flowed from Fukuyama's notions of the 'End of History'. This, in turn, was based on an understanding of the Bretton Woods system set up by the US and UK in the aftermath of World War II, which included such institutions as the UN, the IMF, and the World Bank, and whose purpose was to integrate the global economy into one capitalistic and democratic whole. Spurred by the creation of the European Union and NAFTA in the early 1990s, and the 'fall' of Communism mentioned above, it seemed as though the globe was indeed reaching a new threshold of integration. Historians naturally began looking backwards in time, in an effort to find the 'real' beginnings of modern globalization.⁹

Seeing that this movement had been gaining momentum from the later 1990s, in 2005, a number of researchers founded the *Journal of Global History*. This is seen by some in the field as a foundational moment in its development.¹⁰ The term **Global History** was deliberately chosen by the journal's editors, in contradistinction to the earlier term World History. The major aims of Global History were:

- 1) To move beyond the geographical confines of the nation state
- 2) To move beyond traditional periodizations: ancient, medieval, early modern, modern
- 3) To emphasize connections and connectedness over boundaries

⁹ That this trend is ongoing can be deduced from the recent appearance of the Past & Present Supplement 'The Global Middle Ages', Volume 238, Issue supplement 13, November 2018.

¹⁰ For a recent summary of the current state of global history by one of the founders of the *Journal of Global History*, see VRIES, Peer, *The Prospects of Global History: Personal Reflections of an Old Believer*. **International Review of Social History**, v. 64, n. 1, p.111-121, 2019; see also the response article by ANTUNES, Catia, *An Old Practitioner Still in Search of the métier d'historien* Response to Peer Vries "The Prospects of Global History: Personal Reflections of an Old Believer". **International Review of Social History**, v. 64, n. 1, p. 123-127, 2019.

4) To avoid any major theoretical paradigm

During this foundational phase, the practitioners of Global History spent a good deal of time defining and defending their field. This defensiveness arose from the fact that—as it appeared—no one can really be considered an ‘expert’ on global history. As a result, most historians were sceptical of self-appointed ‘global historians’. After all, one can be an expert in fourteenth-century English cities, in the circle of advisors around FDR in the 1930s, or women and punishment in nineteenth century France, but who can claim to be an expert on the whole world? A related criticism is that many historians find it difficult to see how one can do meaningful primary research in ‘global history’. The main tools of the global historian seem, therefore, to be mere secondary literature—previously published works in which more specialized historians have consulted and digested various primary sources. The charge then, is that so-called ‘Global Historians’ will necessarily be generalists, who do not consult primary sources, who are not experts in any given field, and who rely on the published work of real experts in order to form their opinions. Further complicating the Global History problem is the notion that Global Historians are suspicious of the idea of a ‘master narrative’ or of an overarching theory. Whereas the World Systems crowd can point to a reliance on a Wallersteinian interpretation of economic relations as a basis for their observations, Global History has been dogged by the fact that, in the absence of any overarching theory, it can easily mean ‘all things to all people,’ which is to say, that it can collapse into meaninglessness.

The appearance in 2016 of the volume *The Prospect of Global History* seemed to herald a new era in the emergence of Global History.¹¹ As the editors of this volume assert, the turn towards Global History had only grown and grown since the foundation of the *Journal of Global History* in 2005, and the need for larger-scale, bigger picture studies was increasingly manifest. The editors further made the bold assertion that the time for defending the field was over: it was now time to apply it. It was the publication of this book, along with several

¹¹ BELICH, James; DARWIN, John; FRENZ, Margret; WICKHAM, Chris (Ed.). **The prospect of global history**. Nova York: Oxford University, 2016.

accompanying volumes, which sparked the editors of the current special issue to analyze the potential uses of LSS and Comparative History. In the introduction, the editors of *The Prospect of Global History* suggest three distinct methodologies for doing global history. One is the analysis of what I would call LSSs, though the editors define them, to my mind, less precisely than we attempt here. Another is the history of globalization, which has been introduced above. And the third is a more specific methodology of looking at ‘connectedness’—this has a specific historiography of its own as discussed below. Despite this potential messiness, some are now claiming that, after years of struggling to establish itself as a viable subject or school, Global History has arrived as a discipline.

3. Comparative History and LSS Analysis: Their Relation to World History and Global History

Having provided an outline of World and Global History, we can now turn to the major work of relating these fields to our main notion of a Large Social System.

As should be apparent from the previous discussion, we do not really need to worry about the relation of LSS analysis to World History. World History is formally done within the confines of World Systems theory, and is a particular kettle of fish which has more or less had its day. World Systems theory undoubtedly has value: but it is tied to a particular time and place and mode of analysis which, though it purports to be of global import for understanding global history, can have only a limited impact for explaining large scale phenomena in global history in general. Besides, as a Marxism-derived theory, World Systems Theory (and thus World History) labours under the difficulty that all such theories have: Marxism (and Weberianism, and other isms) point to particular ‘engines’ of development in the history of human society, and expect us to find them particularly illuminating: e.g. ‘revolution’ or ‘class struggle’—and in fact, decades of study shows that these things, once thought central to the evolution of history, are often only ancillary to much more cogent explanations. In other words, while I believe that LSSs have ‘engines’ – the older notion that a theory should privilege a particular engine now seems too limited.

The main question left to us becomes: how do we relate LSS analysis to Global History? And the answer here, I think, is that some aims of these methodologies and schools are similar. The sticking point revolves around how precisely one defines Global History. If one defines Global History as a school of practitioners with the ultimate goal of writing a 'history of the Earth and particularly its human systems', then it is simple enough to define LSS analysis (and Comparative History) as components of Global History. In this way, we can say that Global History is a catch phrase for studies which utilize LSS analysis and Comparative History. LSS analysis and Comparative History are more modest, insofar as they identify particular components of Global History, and seek to explain how these evolved and worked, without necessarily referring to any larger goal or concept of Global History. One is almost tempted to go further, and suggest that LSS analysis and Comparative History are distinct methods, while Global History is most properly a field of study.

In practise, the *Journal of Global History* over the past few years has been overwhelmingly focused on the processes of modern globalization. This is one of the three 'sub-fields' of Global History that the editors of *The Prospect of Global History* identify as a major methodology for the discipline. If we define Global History as the process by which the modern world became economically, culturally, and (to some extent) politically interconnected, then fine, this does work, and is useful. But defined in this way, Global History as a field is very much narrower, in fact, than either Comparative History, or Large Social Systems analysis, as methodologies. And its limitations in this regard should be clearly acknowledged by the proponents of Global History.

If, as some have done, one wishes to define Global History more narrowly as a methodology, rather than a subject, then the best definitions of Global History in this vein emphasize it as the study of interconnectedness across borders. This methodology has the largest historiography currently associated with Global History for the early modern period. If we define Global History as a methodology of looking for interconnectedness across borders, or 'network analysis', then we can see it as based on discerning large scale movements of people, goods, ideas, and techniques, often relating them to empires of land or sea. The idea is to find

movements and interconnectedness where little was assumed to exist. Defined this way, the aims of Global History are quite different from Comparative History defined above, in that it looks in some detail at discrete units, to find causalities. LSS analysis is closer to Global Historical network analysis, but the object of LSS analysis is a particular system, whereas the object of Global Historical analysis is often, in practise, tied to particular political units (despite what its practitioners might wish to argue!). So while some have argued that Global History as network analysis is an abstract methodology, in fact, this form of Global History has been strongly tied to the emergence of early modern global empires and trading systems, which itself smacks strongly of the older World Systems analysis. Conceptually and methodologically, it is probably best, therefore, to separate LSS analysis from this version of Global History, as well.

Overall, I would argue that the editors of this special issue have hit upon something important by emphasizing LSS analysis rather than Global History per se. While Global History might have been a crucial catch phrase for igniting the study of Large Social Systems, it might prove to be the studies of LSS or studies based on Comparative History, which end up having the most real explanatory power, and for which historiography is best suited. As the editors of *The Prospect of Global History* recognized, true Global History is not a goal which can be reached: understanding 'the whole world' might be possible, but perhaps only in a superficial way. And furthermore, it seems as though Global History in this largest sense might always be forced to rely on syntheses, and might not evolve a methodology or probative model of its own. While others will argue otherwise, it seems to me that Global History as it now stands suffers from four possible shortfalls that make it incompatible with general LSS analysis, based on how one approaches the topic. It is either too specifically 'global', too broad (and superficial), too tied in with the history of twentieth-century 'globalization,' or too reliant on World History, to provide a methodology for equipping young researchers to go out and discover previously unremarked causalities and systems.

4. LSS Analysis and New Institutionalism

To those who are familiar with the methodologies of New Institutionalism, it might at this point look as though my advocacy for LSS analysis is merely an application of this line of thinking. Sections four and five will refer to my own case study of a particular LSS, in order to suggest that while there are indeed similarities, I would strongly caution against subsuming LSS analysis under the heading of New Institutionalism. The LSS which I have most successfully analyzed was a system that I termed 'Greater Mediterranean Slavery'.¹²

A major goal of LSS analysis is to break down path dependencies on theoretical paradigms which tend to linger, zombie like, for many decades longer than they should. In the post-theory era, LSS analysis can provide large-scale causal explanations which are based not on *a priori* notions of how the world should work, but rather on *a posteriori* observations of how the world has, indeed worked. But I also realize that this type of work is rare for several reasons. First, there are disciplinary prejudices against large-scale history which we have detailed in our definition of Global History above. Also, historians tend to be inductive empiricists who start with individual primary sources, rather than with big-picture theories. In this regard, researchers who attempt to trace LSSs will need to have something of the deductive mentality of a historical sociologist, geographer, or an economist. These disciplines tend to work from large scale theories, and then work down to specifics and specific evidence as necessary. I myself am familiar with some economics and economic history, and therefore I am used to the idea of the big-picture or macro analysis. The difficulty, of course, is that with no pre-existing model for historians or historical sociologists to turn to in the post-theory era, one has to be able to look at the big picture, and its workings, by deducing the main 'laws' or parameters which constrained the system. Each LSS, in other words, will have its own characteristics, and it is up to the historian of an LSS to deduce which of these characteristics is decisive in limiting the shape and size of the LSS as it evolves over time.

¹² FYNN-PAUL, Jeff. Empire, monotheism and slavery in the greater Mediterranean region from antiquity to the early modern era. **Past and Present**, v. 205, n. 1, p. 3-40, 2009.

To perform this work, one of the most helpful frameworks is that of Douglass North, whose name is associated with the idea of 'New Institutional History.'¹³ An 'Institution' has been defined by North and his followers (perhaps somewhat unhelpfully) as 'the rules of the game.' While this can seem confusing at first, North and his colleagues are attempting to get at the ideas which I detailed above: in every system, there are particular parameters or rules which constrain actors into particular channels; and these constraints and channels in turn help to define the system. North's ideas are derived from so-called 'game theory', which evolved during the Cold War, and later became popular with economists attempting to characterize human behaviour in a given microeconomic situation. For example, if two businessmen have the option to buy a particular company, what determines whether they will buy it or not? As it turns out, if you set particular rules, then you can work out the probability of the actors doing a particular action in a given standardized situation. Whether one is analyzing a formal game, or the odds of a CEO making a particular decision in a given situation, game theory requires that you set certain 'parameters', so that the odds of a given action can be deduced. These are the basis for North's 'rules of the game' definition of institutions.

Economists further noticed that when analyzing a given microeconomic situation, there are two main types of rules that one has to grapple with. One of these is a formal set of rules which cannot be transgressed without some obvious penalty: these they named 'Formal Institutions.' But in addition to the Formal Institutions, there are myriad 'Informal Institutions' which might not be written, but which nonetheless can have a major limiting impact on behavioural options. For example, in modern boardrooms, it is expected that negotiators will conform to certain dress codes. Going against this dress code will not break any formal rules of the game, but anyone who routinely flaunts the given dress code, except in very exceptional circumstances, will be ostracized from the wider business community

¹³ NORTH, Douglass; WEINGAST, Barry. Constitutions and commitment: the evolution of institutions governing public choice in seventeenth-century England. **The Journal of Economic History**, v. 49, n. 4, p. 803-832, 1989.

and marginalized as an actor in major negotiations. So informal institutions often matter as much or more than formal ones.

For historians attempting to analyze a Large Social System, it might behoove them to think about the implications of formal and informal institutions. For example, by far the most common study of a Large Scale System that exists on the market today is that of a particular formal institution. This is the easiest to do because formal institutions that survive for a long time period tend to resist change in a few key ways, which means that they tend to either build similar buildings, or set up similar schools, and/or hold to a particular code or set of laws and practises, which makes elements of this Formal Institution relatively easy to identify in sources across dispersed times and places. For example, a history of the Catholic Church, or more specifically a history of the Jesuit Order, or a history of the Jewish People, or of Buddhism, Freemasons, etc., are all relatively obvious and easy to do if one accepts a certain level of abstraction. In fact, such studies come so naturally, given the way that human brains work and culture is transmitted, that many people who read such studies might remain unaware that they are even reading a study of a Large Scale System—they simply see it as a history of an institution or idea in which they are interested.

The idea of an LSS study becomes much more difficult when we attempt to think about the history of a particular informal institution. For example, the history of wearing beards, or of laughter, or of the handshake, or of masculinity, or ritual violence, or histories of honour and dishonor, these have been attempted, with varying degrees of success.¹⁴ Of course, the difficulty here is that it is often much more difficult to find evidence, since data collected will almost invariably have been picked up for some other purpose (e.g. in court records). But there is also a danger that collected evidence will be biased in some way, leading the historian to focus on particular aspects of their subject, while ignoring others. And there is always the danger that the ‘informal institution’ under question will be either too broad or too trivial to say anything meaningful about the topic at hand.

¹⁴ DOSSEY, Leslie. Wife beating and manliness in late antiquity. **Past and Present**, v. 199, n. 1, p. 3-40, 2008.

By this point it might seem as though I am arguing that LSS analysis and New Institutionalism are one and the same. After all, it has been argued above that tracing the history of a single ‘institution’ can be the basis of an LSS study. Nothing could be further from the truth. New Institutionalism is by this time a school of thought, like World Systems theory, with its own founding texts, biases, assumptions, goals, and methodologies, and I believe it is therefore dangerous to equate New Institutionalism with LSS analysis, or to suggest that LSS analysis is merely a facet or an application of New Institutionalism. For one thing, New Institutionalism arose from a sub-discipline of economics—game theory—and in the hands of Douglass North, Avner Greif, and other practitioners close to the theoretical core of New Institutionalism, the economic theory involved is the essence of New Institutionalism.¹⁵ To my mind, as an historian, the results of such direct attempts to apply hardcore economic theory to historical analysis have been challenging, but often deeply flawed. To draw from my own sub-field of medieval economic history: while some are beguiled into thinking that Greif’s *Institutions and the Path to the Modern Economy: Lessons from Medieval Trade* is so brilliant that they simply lack the expertise to understand what Greif is driving at, in fact, I would argue that what many others have suspected is true: the emperor has no clothes. Many of the conclusions drawn by Greif in this volume, as well as those drawn by other practitioners such as the late Larry Epstein are indeed valuable because they are challenging, but this stems from the fact that based on their models, the authors in question are willing to draw too-neat, and too-sweeping conclusions, which in my experience are often proven false by a careful reading of the primary sources involved.¹⁶

A recent conversation with a banker about the validity of Piketty’s findings might serve to illustrate the dangers of applying economic theory derived from modern sources too closely to the realities of economic history. Over dinner, this banker (with an economics degree) strenuously objected to Piketty’s assertion that

¹⁵ GREIF, Avner. **Institutions and the path to the modern economy**: Lessons from medieval trade. Nova York: Cambridge University, 2006.

¹⁶ For an example of this New Institutionalist model-based logic, which to my mind has always seemed slippery, and difficult to reconcile with reality, see e.g. the introduction to EPSTEIN, Stephen; PRAK, Maarten (Eds.). **Guilds, innovation and the European economy, 1400–1800**. Nova York: Cambridge University, 2008.

capital has grown at a rate of (say—the figures here are only examples) 7% over several centuries, because, this banker pointed out, Piketty estimated that the global economy was worth Y in 1650, and growing at 7%, the global economy should now be worth Z, but in fact it was 50% less than Z. So using the beginning and end figures, he argued that returns on capital could not possibly have averaged 7%, but must have been closer to 3.5%. I pointed out that, in fact, all of my studies of earnings on capital have shown that they were closer to 7% per annum, and so Piketty is in the main correct. Wealthy people who invested tended to earn this amount, and I have the sources to prove it. But my interlocutor was keen to argue for a general picture using an overall model, while I was arguing from specific sources. Certainly the beginning or end figures might have been wrong. But more importantly, all these figures might have been right—including the interest rate! This is because the vagaries of global history, uneven economic growth globally, etc., might have caused the overall capital stock to grow more slowly, even if, on average, invested capital grew at 7%. This is where inductive reasoning can trump pure deduction.

More recently, some historians and historical sociologists have attempted to move away from this ‘economic modelling’ version of New Institutionalism, and instead asserted something akin to an historians’ version of Institutionalism. In practise, taking their cue from North and Weingast’s article ‘Constitutions and Commitment’, these tend to isolate a particular political institution (for example, parliamentary democracy, or an urban town council), and look at the economic effects of these across time and space—often, a single effect such as low interest rates will be singled out.¹⁷ Other notable studies include Botticini and Eckstein’s argument that educational norms and institutions tended to create human capital amongst Jewish communities during the medieval period.¹⁸

But this highlights a final problem with adopting or even advocating New Institutionalism for LSS analysis is that many recent studies of Large Social Systems expressly reject the notion of ‘institutions’, even while they utilize many of

¹⁷ STAVASAGE, David. **States of credit: Size, power, and the development of European polities.** Princeton: Princeton University, 2011.

¹⁸ BOTTICINI, Maristtela; ECKSTEIN, Zvi. **The chosen few: How education shaped Jewish history, 70-1492.** Princeton: Princeton University, 2014.

the methodologies of LSS analysis that I advocate in this article. Two studies which spring to mind are Robert C. Allen's *The British Industrial Revolution in Global Perspective*, and Peter Clark's *The Oxford Handbook of Cities in World History*.¹⁹ As stated, both Allen and Clark argue vehemently against the notion that the system or process they are describing (factory-based industrialization and urbanization) has aught to do with 'institutions'. In this case, both Allen and Clark appear to hold that 'institutions' means 'political institutions'—and so this illustrates that New Institutionalism runs the risk of being too closely associated with political constitutions, systems, or norms, in order to be valuable for a more general methodology of LSS analysis.

Our arguments for keeping New Institutionalism and LSS analysis distinct can be summarized in three points. First, New Institutionalism was born from a hardcore of economic theory which is not only offputting for most historians, but often leads to problematic, model-derived assertions which seem to clash with historians' source-based conclusions. While this is by no means unfruitful, neither is it the main purpose of LSS analysis, which is to look at historical sources and secondary literature to trace a specific historical process. LSS analysis is therefore primarily an *inductive* exercise, while New Institutionalism began as a *deductive* (and highly economics-focused) exercise. Secondly, even if economic historians have begun to adapt New Institutionalist methodologies to gel with their own source material and the existing secondary literature—thus turning it into more of an inductive exercise, the results of this enquiry have thus far tended to remain limited to discovering a particular economic effect of a particular (political) institution. While such studies have proven to be extremely valuable, and they are opening up new scientific vistas, the goal of such studies is not the main goal of LSS analysis. LSS analysis is therefore potentially much more varied than what New Institutionalism has heretofore produced. For example, LSS analysis can and should be open to discovering cultural effects, intellectual effects, or political effects of the existence of institutions or practises—not just economic effects.

¹⁹ Allen, Robert. **The British industrial revolution in global perspective**. Cambridge: Cambridge University, 2009. CLARK, Peter. (Ed.). **The Oxford handbook of cities in world history**. Oxford: Oxford University, 2013.

Thirdly, it should be pointed out that the purpose of LSS analysis is to find a system itself, which may well consist of a 'single' institution, or chains of institutions, and to look at the rules by which this evolved over time. As stated, New Institutionalism tends to focus on the (economic) effects of a given institution. LSS analysis should, arguably, focus on the internal logic of the system in question, at least as much as looking at the effects of this system. Thus, while LSS analysis is certainly indebted to some ideas derived from New Institutionalism, it should remain firmly within the realm of historical studies, and should keep the LSS itself as a focus, rather than adopt the methodologies of the existing New Institutional School.

5. LSS Analysis Case Study: Greater Mediterranean Slavery

Before proceeding to our general conclusions, a few specifics about my analysis of Greater Mediterranean Slavery might help to clarify the points made in section four above. In this case, it can be said that I was tracing the history of a formal institution with informal components. For most of the time period I analyzed, that is from classical times until the early twentieth century, slaves might have begun their journeys in places with few 'formal' institutions, but they usually ended up in places where sales and property were formally recognized by written legal systems. So while there were informal elements in the buying, selling, and transporting of slaves, it is nonetheless true that since ancient times slavery in the Greater Mediterranean has been more or less formal.

That being said, one of the reasons why the history of Greater Mediterranean Slavery had not had a history of its own, is because the formal institutions that I was tracing existed across many boundaries of time and space, and were expressly not recognized by any participants as being part of a long-standing system. Thus, my LSS might have escaped the net of New Institutionalism working on a particular formal institution. While Catholic bishops, for example, might have existed in many countries and left many types of record, they nonetheless were highly aware of being part of a centralized system, which in turn makes it easier for historians to consider the 'Catholic episcopacy' as a unit with a specific history. But since slavery existed in many countries, and was taken for

granted by most participants as being simply something that happened in such and such a way, it took a much greater leap of historiographical imagination to see the evolution of slavery as in some ways conforming to the definition of a single Large Scale System.

And while I have characterized my quest as one to ‘trace a formal institution’ – my purpose was not necessarily to trace the effects, let alone the economic effects, of this institution, but rather to trace how a network of institutions, laws, assumptions, ideas and practises came together to influence the ebb and flow of slaves across time. My goal was to define how groups of people came to be designated as slaves or were enabled to be slave-takers, and the concept of ‘institutions’ was sometimes useful in this regard. In this way, I was able to discern how ‘networks’ of institutions worked in tandem, according to a logic which was not tied to any one institution narrowly defined. So while institutions were an essential part of my analysis, the normal modes of New Institutionalism were only tangentially useful. Once again, I would argue that it is best to leave the labels and schools behind, and focus on the internal logic of the system itself, wherever this may lead.

With these caveats aside, how does one go about tracing the actors and ‘sinews’ of an LSS? In the case of Greater Mediterranean Slavery, my curiosity began when I uncovered some references to slaves and slaveholders in the archives of Manresa, a town outside of Barcelona. In a meticulous cadastral survey dated to 1408, the city government listed the names of every slave and slaveholder in the city, along with the wealth and occupations of the owners, and the value of the slaves. Several thousand spreadsheet entries later, I was able to calculate that the slave population in Manresa around 1408 was roughly one percent of the population.²⁰ I became intrigued for two reasons: first, because slavery had been largely phased out in Catalonia in the eleventh and twelfth centuries, and was replaced with serfdom or other types of feudal tenure. And secondly, because some writers had identified a ‘Black Sea Phase’ of slavery in southern Europe which lasted from the mid-fourteenth century until the mid-

²⁰ FYNN-PAUL, Jeff. Tartars in Spain: renaissance slavery in the Catalan city of Manresa, c. 1408. *Journal of Medieval History*, v. 34, n. 4, p.347-359, 2008.

fifteenth century. So it appeared as though ‘my’ slaves and slave owners were part of this Black Sea Phase of Mediterranean slavery, and it became my goal to trace the contours of this system in space-time. When and why did Catalans suddenly start importing slaves from the Black Sea, after two hundred years without slavery? As it turned out, Black Sea slavery went into high gear after the Black Death made labour expensive, but the resistance of guilds to the importation of slaves helped to ensure that this experiment with Black Sea slaves was on the decline already by the 1420s. The fall of Constantinople to the Turks in 1453 then made Black Sea slaves even more expensive, and after that time slavery in southern Europe became, for practical purposes, moribund. Southwest Iberia, as we now know, continued to import limited numbers of slaves, but from the 1440s these were mostly west Africans.

My desire to discover the temporal and geographical contours of Black Sea slavery in southern Europe led me to a number of further questions and discoveries. I had already traced the chronology of the phenomenon and the economic logic behind it. But why did southern Europeans buy slaves from the Black Sea, and why didn’t they enslave anyone else? As it turned out, a big part of the answer had to do with ideology—and specifically with the religious ideology which was so important in the medieval Mediterranean. By the later middle ages, it was largely taboo for Christians to enslave one another, and it had almost always been taboo for Muslims to enslave one another. Therefore, contemporaries reasoned that the only enslavable people were non-co-religionists. Alongside the religious angle was one of practicality: since, by the later middle ages at least, Christians and Muslims tended to live in organized polities, it was easier to come by slaves from politically disorganized areas which tended not to be Christian or Muslim – i.e. the areas where pagans lived were used as slave stocks. Furthermore, since they were not united by a single monotheistic faith, ‘pagans’ were in general happy to enslave people from the next tribe over, while Christians or Muslims—even if they lived thousands of miles from one another—usually would not, out of religious scruple. This combination of political disorganization, pagan willingness to enslave captured enemies, and monotheistic scruples, thus determined which, in the fourteenth and fifteenth centuries, was the ‘easiest’ group to enslave. In

economic terms, of course, 'easiest' usually means 'cheapest.' So low prices also drove this trade. As it turned out, the two largest pagan areas which were accessible to Christians in the later medieval period were, yes, the Black Sea, and also sub-Saharan Africa, which Europeans—particularly the Portuguese—began to reach in the mid-fifteenth century.

By using a combination of economic logic, and the religious logic of the day, I was able to arrive at an overall picture of how the slave economy of the late medieval Mediterranean worked. When, in my head, I ran this model backwards and forwards in time, I realized that the regions which provided the majority of slave stock—which I dubbed 'Slaving Zones'—remained stable for long periods, and that they also expanded or contracted according to a discernible logic. Religion and political organization were the main 'rules of the game' or institutional characteristics, which determined the ebb and flow of the Slaving Zones. While these systems were characterized by remarkable stability, paradigms sometimes shifted, as with the introduction of Islamic and (later) Christian scruples into the system. To complete my picture, I coined the term 'No-Slaving Zones' to point out that many rulers or religious leaders proscribed slave taking from certain regions: for example, ancient Egyptian pharaohs generally forbade military slave raids on their own territory, and later medieval (Latin) Christian and Muslim authorities forbade or discouraged slave taking from their respective religious groups. (For a complete picture of the LSS in question, please see the article 'Empire, Monotheism, and Slavery'.)

6. Conclusions

To conclude, we can summarize our main observations about the five concepts related to 'big picture' historiography discussed above. The primary goal, of course, is to discern which methodology is best for analyzing the geographical, temporal, physical, and cultural contours of a Large Social System.

Global History still seems to have its own agenda, based either on the expansion of empires, or the history of modern globalization. It is thus an expansion of **World Systems** theory, but in many ways still dependent on World Systems' original hypothesis about the creation of a European Capitalist hegemony

from the sixteenth century. Work done under the Global History banner has been fruitfully expanded geographically and temporally to cover the Chinese and Islamic ‘empires’ and sometimes ancient networks of exchange, but again, there remains a danger of it relegating itself merely to a description of modern ‘globalization’ in the twentieth and nineteenth centuries.²¹ Overall, as explained above, Global History remains a mixed bag, still unsure of whether it is a subject or a methodology, freighted with some baggage and particular research vectors, and difficult to untangle. The verdict: Very useful and illuminating in many instances, but not as universally applicable as Belich, Darwin, et al. suggest; and it should be kept separate from either Comparative History or Large Social Systems Analysis.

Comparative History is useful as a method where one expressly compares similar systems in two (or more) different countries or polities or cultural units. In this way, many of the assumptions which are made by specialists in one or both regions are often revealed to be based on a false sense of endogeny. Specialists in a given region, language, etc., tend to exaggerate endogenous path dependencies, and rely on these to supply the majority of their causal explanations for the developments they study. Because historians are often discouraged from looking at parallels beyond their ‘comfort zones’ of expertise, we as a profession are burdened with a literature in which, I estimate, over half of all explanations are falsely attributed to endogenous factors. There is much work to be done here. The usefulness in defining Comparative History as a method in this way, is that it encourages researchers to undertake explicitly comparative analyses, which they otherwise might not have done.

New Institutionalism, as we have seen, began as a highly technical, model-based economic theory derived from game theory. Applied as a model, it often results in too-abstract descriptions of reality which do not jibe with primary sources. When applied in a less theoretical way, it has proven especially fruitful at determining some (usually economic) effects of the existence of particular institutions. For LSS analysis, it is very useful to borrow the basic idea of finding ‘institutions’ and tracing them, their actors, and their effects. Nonetheless, New

²¹ This based on the recent heavily twentieth-century focus of the *Journal of Global History*.

Institutionalism has remained largely a *deductive* exercise, while it has been argued here that LSS analysis should be an *inductive* exercise. The focus on contours and internal development of the system in question therefore distinguishes LSS analysis from the main goals of New Institutionalism.

What one is attempting to do when one discovers and describes a **Large Social System**, is look at a form of interaction which carried on for a long time, across institutional or political boundaries, but which was previously unrecognized as a system. The Greater Mediterranean Slavery system, discussed above, is one such example. A further case in point is the ‘discovery’ or rather re-conceptualization of the highlands of Asia as ‘Zomia’ by Willem van Schendel in 2002.²² The fact that this idea of Zomia has rapidly been taken up by so many scholars is proof that the concept has significant scientific value. In this case, a number of political, social, and cultural characteristics held in common suggested that this region be considered a single transnational entity. This had not been previously considered because Zomia crossed both national boundaries, and those of traditional ‘area studies’ disciplines. Likewise, what characterized Zomia was a conglomeration of institutions—or even a lack of institutions, which was partly determined by geography. So the typical New Institutional method of tracing a particular ‘institution’ would have been almost useless in the discovery of Zomia. In this case, it was a geographer, rather than an historian or anthropologist, who discovered this LSS, presumably because the paradigms he was trained in helped him to ‘think big’. Moving further afield, it might be helpful to reflect that LSS discovery and analysis does not only occur in the social sciences. The recent discovery of Laniakea, the supercluster of galaxy clusters which dominates our corner of the universe, is a very useful analogue. Our home supercluster is strikingly beautiful—it has an obvious symmetry—but it is so large, and composed of so many smaller units, that until the advent of modern supercomputers it remained hidden in plain sight.²³

²² VAN SCHENDEL, Willem. ‘Geographies of knowing, geographies of ignorance: jumping scale in Southeast Asia’. **Environment and Planning D: Society and Space**, v. 20, n. 6, p. 647–68, 2002. See the special issue on Zomia in *Journal of Global History*, v. 5, 2010.

²³ COURTOIS, H  l  ne. **Finding Our Place in the Universe: How We Discovered Laniakea – the Milky Ways Home**. Cambridge: MIT, 2019.

On the subject of supercomputers, it would be tempting and stylish—and it would certainly give this article more ‘hits’ and citations in google scholar—if I were to argue that the pursuit of Large Social Systems is a ‘future trend’ which will be aided by the availability of Big Data and supercomputers. Administrators across the globe are positively salivating at the prospect of turning social scientists and historians into big data crunchers and practitioners of the ‘digital humanities’. While we can hope that historians begin to pursue LSS discovery and analysis more actively, it seems doubtful that computers and big data will be of much use in this regard. The fact is, when we are looking for LSSs in global history, what will be required is people who have a very broad and deep understanding of the main contours of global history. The case of Zomia helps to confirm this. In the case of Greater Mediterranean Slavery, there are no datasets in existence which could help one to discover slaving patterns stretching across the dozens of countries or regions in question. Building such a database would be virtually impossible, due to limited sources prior to the nineteenth century in most parts of the world, and in any event it is unlikely that any programme would discern patterns not already deductible before the data was entered.

Finally, since the present author is keen to push the idea of LSS research, we can conclude this article with a few ideas as to where to look for the next big discoveries. By this point it should be apparent that in order to discover the true significance of an LSS, one will often have to move beyond one’s original sub-field. Historians tend to do either cultural, political, intellectual, or economic history, but an LSS is often revealed by effects that consistently relate two or more of these subfields. For example, the consistent and persistent economic impact of a particular cultural institution across linguistic and political boundaries might be crucial to discovering an LSS. Or the educational effects of a given political arrangement might be the key to discovering why a given system evolved as it did. Since individual researchers are often uncomfortable crossing these boundaries, the opportunities for ‘big’ discoveries are correspondingly large. At any rate, a list of Large Social Systems waiting to be discovered might include the following.

- 1) The fact that ‘barbarians’ periodically overcame empires, by similar means which were repeated for a space of some five thousand years, needs to be

analyzed in more detail. Specifically, barbarians periodically gained an advantage over empires by means of barbarian peoples absorbing the military technology of empires, without having to pay taxes in order to raise armies. This phenomenon happened time and again from ancient Egypt, through Rome, through Ming China. Thus far, it has only been written about in the Roman context, to my knowledge.²⁴ Also, the fact that supplies of dangerous 'barbarians' remained relatively limited to certain regions, and to limited times in history, deserves further study. The end of the 'barbarian age' came with the expansion of the state systems of Russia, the Islamic World, and China into central Asia and the end of independent nomadic regimes in the sixteenth and seventeenth centuries. Here, one is not looking at a particular institution, but the dynamics of technological adoption working in tandem with fiscal systems. Together, however, they create a discernible LSS with different permutations.

2) The creation of a system of serfdom and a 'second slavery' in Russia and the Slavic lands from the end of the middle ages through the nineteenth century was the subject of the great Brenner Debate in the late 1970s and early 1980s, but this was debated in the specific (and now very dated) context of agrarian Marxism.²⁵ My idea is that the absence of town councils in the cities of Orthodox Europe is what enabled landlords to continue a harsher form of feudalism for longer than it did. In Western Europe, the town councils of independent cities were key in pushing the idea of 'liberty' which also helped to end serfdom by the later middle ages. To this day, Russia and Russians enjoy fewer political liberties, and the absence of town councils, and the absence of a parliament in Russia until very late, (and it was always based on weak foundations), may have something to do with this.

3) The long-term history of monogamy, and its effects on women's equality. It seems as though monogamy was a very odd institution invented by the Greeks and Romans. But it also seems as though Graeco-Roman women had more

²⁴ A good starting point is WARD-PERKINS, Bryan. **The Fall of Rome and the End of Civilization**. Oxford, Nova York: Oxford University, 2006.

²⁵ ASTON, Trevor; PHILPIN, Charles. **The Brenner Debate: Agrarian Class Structure and Economic Development in Pre-industrial Europe**. Cambridge, Nova York, Melbourne: The University of Cambridge, 1987.

freedom than women elsewhere in the world. The European middle ages inherited monogamy, and passed it through the centuries until eighteenth- and nineteenth-century women were the first to gain the vote and anything like political equality. Although monogamy was much decried by First- and Second-wave feminism as the bane of women's freedom, a global perspective reveals that the much more common alternative of (elite) polygamy might well be worse for women's rights. What is the long-term relationship between women's legal rights and monogamy, on a global scale?

These are just a few of the Large Social Systems which are waiting out there for an historian or group of historians to study. The fruits of such studies will, to my mind, result in some of the most insightful and meaningful history that has been done since the 'Age of Theory' collapsed in the 1990s. But now, instead of being enslaved to some nineteenth-century paradigm of how social systems worked—and allowing some version of Marxism or Weberianism to dictate the engines of history—historians of Large Social Systems hold, for the first time, the promise of discovering the 'real' rules by which human societies have operated. Though it may seem very surprising in our age of supercomputers, the organization and even the very existence of many fundamental social systems of human history still remain hidden, like Zomia, or like Laniakea, in plain sight. This is both humbling, and exciting. It is a call to recognize that work of the professional historian is really just beginning.

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Recebido: 10/06/2019
Aprovado: 22/06/2019