

## Big History Research: A First Outline\*

*Fred Spier*

### Abstract

*This contribution provides a first outline of how to define big history research, including an overview of the types of research that could profitably be undertaken. Practical issues are also discussed, such as how to obtain funding, where to publish the results and whether the research results might have practical applications. Because this contribution is, to my knowledge, the first attempt to outline big history research, my observations should be considered preliminary. I hope that they will stimulate a healthy and vigorous discussion about big history research, the one that will lead to formulating a big history research agenda that will successfully be pursued worldwide.*

### General Outline: What is Big History?

For a fruitful discussion of big history research we first need to address the question of what ‘big history’ is. In September 2010 a group of big historians defined big history as ‘the attempt to understand, in a unified and interdisciplinary way, the history of the Cosmos, Earth, Life, and Humanity’. This joint formulation was a part of the effort in founding the International Big History Association (IBHA).<sup>1</sup>

The word ‘unified’ is extremely important, because it means that big history is more than the sum of its parts. This is by itself not at all exceptional. All academic fields are more than a sum of their parts. In other words, these fields exhibit a distinct type of complexity that legitimizes their existence. Usually this includes a theoretical approach that holds the promise to successfully tackle a set of research questions, questions which cannot be investigated equally successfully by looking only at the parts. This is very much the situation, for instance, in the fields of astronomy, geology and the social sciences, although in the latter case there is no consensus yet about core theories. But even if a disci-

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\* This essay has benefitted from insightful commentary by Jonathan Markley, Robert King, Esther Quaedackers, Barry Rodrigue and John de Vos.

<sup>1</sup> Big historians who took part in the discussion about how to define big history included: Walter Alvarez, Craig Benjamin, Cynthia Brown, Lowell Gustafson, David Christian, Barry Rodrigue and myself. These big historians constituted the provisional board of the International Big History Association. More about this may be found at the IBHA website: [www.ibhanet.org](http://www.ibhanet.org).

pline does not yet have a shared general theory, the idea of employing a theory for mapping and explaining its field is usually not contested.

However, there are some exceptions within the walls of university life. The academic study of history as well as other branches of the humanities, such as the study of art and literature, have established themselves without a core theory and are very reluctant to use any such theories. This is usually explained by saying that these fields are too complex or subjective for any general theory to be useful. To be sure, individual humans, human societies, and their expressions, are among the most complex subjects that are studied in academia. However, it seems to me that the main reason of why these fields continue to exist without a core theory is that they are dealing with identities. Histories written by academic historians, for instance, are mostly local, regional and national in scope, while for the European Union members this now also involves the history of their nations within the context of European history. The academic histories about nations and regions outside of what is seen as one's own society often deal with the period that these societies began to interact with the own society. In fact, the interactions during such a period often helped to shape the identity of the society that performs these studies. Historical studies that do not bear any direct relation to the identity of the academics that produced them are actually rare. It seems to me that they form only a tiny minority of all academic histories.

Furthermore, the results of academic histories are sometimes meant for consumption by larger numbers of people that make up their own society. This especially pertains to the history taught at schools or shown in documentaries. In such situations, story-telling usually works the best. Yet there are limitations to story-telling. While the sources on which the story is based can be checked, it is not usually made explicit what the criteria were for deciding what to include in the story and what to omit, as well as how to tell the story. Sometimes even the research questions that led to the story may be unclear. As a result, many academic histories stimulate virtually endless discussions, which reinforce the impression that these objects of study are extremely complex.

Most, if not all, historical research tends therefore to be impressionistic in nature. It may have a central thesis, but it almost always lacks a core theory that could be used to structure and test such a theory. In other words, the traditional approach to history is not unified.<sup>2</sup> If this is an accurate description of academic history, one may wonder how we could possibly achieve a unified approach in big history, within which human history only constitutes a small sub-field.

Yet, if big history is to become fully-fledged academic field that includes research, big historians must be able to show how, and to what extent, big history is unified. In other words, what type of results could big history research

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<sup>2</sup> This is not at all a new point of view. In the 1960s, German sociologist Norbert Elias argued along these lines, first in German (1969) and later published in English (2005).

contribute that are both valuable and different from the results that are already being obtained within its sub-fields, most notably astronomy, geology, biology, climatology, archeology and human history? What is it that might unify big history and thus would legitimize a big history research agenda?

### **The Need for a Big History Theory**

In my opinion, the unification of big history requires a viable theory, or a set of theories, that helps to structure, summarize and explain big history in a unifying way. If this sounds very abstract, let us consider some of the advantages that a core theory of big history might offer, if it could be found.

First of all, such a core theory would allow us to systematically structure the historical narrative along its theoretical lines. The theory would thus help to clarify why the story is told the way it is. It could also help to discover deficiencies, as well as to find indications for how our story could be improved. The theory could also illuminate in a systematic fashion why certain aspects were selected to become part of the story while others were omitted.

In the second place, a theory-driven approach to big history would make practitioners aware, in a systematic fashion, of important aspects that are missing. Furthermore, such a core theory could be applied to aspects of big history that have not systematically been examined yet. This also could include testing whether the core theory is applicable to these aspects. If the theory failed to be useful in such circumstances, even after a long and careful investigation, it would be in deep trouble. This type of research might therefore lead to a better understanding of both the research subject and its theory.

In the natural sciences, theories are often tested by predicting either the outcome of new experiments or the occurrence of certain phenomena. If these predictions fail, the theory may be in jeopardy. Because it is impossible to do systematic experiments while studying history, historians lack this predictive power. But that does not mean that theories of history cannot predict anything. Instead of predicting the outcome of experiments, or the ways natural processes have evolved, good theories of history should be able to help us predict the past of circumstances that have not yet been examined but may be similar, ranging from the development of galaxies to the ways human societies have changed over time. I encountered such examples during my research in Peru on religion and politics. For instance, a general theory about the emergence of early priesthood in human history helped to throw new light on the development of the early Inca state, while a general theory about the dynamics of the Catholic church that was developed for the Southern Netherlands allowed me to recognize similar processes in colonial Peru.<sup>3</sup>

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<sup>3</sup> For my Peru research see Spier 1994.

If all of this is correct, big history research should to a considerable extent be based on an agenda that is driven by a core theory. This makes one wonder whether such a theory may already exist. Although a few proposals have been made, most recently in my book – *Big History and the Future of Humanity* (2010) – it seems fair to say that no consensus has emerged yet about such a core theory. But this does not mean that the goal of achieving such a synthesis is elusive. Until today, only very few academics have devoted time and energy to formulating such a theory. It may well be that, while the big history field expands, more scholars will make such efforts, which will more likely than not lead to the successful formulation of such a theory.

### **In Search of a Big History Theory**

Let us now explore in very general terms how we might proceed in search of a big history theory. While looking for a unified view of big history, the first thing to do is to look for components of the theory that are shared by all aspects of history. For instance, in my big history theory, the main players are matter, energy, complexity and disorder, as well as favourable ('Goldilocks') circumstances. These aspects can be found in all historical studies.

In the second place, we could systematically compare certain portions of big history, such as the division of labor in human societies with what happens within and among biological cells. Such detailed comparisons are rarely made today, as a result of academic specialization.<sup>4</sup> We could look for similarities and difference, while we could also examine whether the current biological and social theories share certain aspects. If that were to be the case, we might be on our way to constructing a more general theory. We could then extend our comparison to lifeless objects such as galaxies and solar systems, and examine whether our theory is also relevant in those cases. If that did not happen, our theory would not be a general big history theory. Yet it might be applicable as a specific theory for certain levels of complexity with their associated emergent properties. But perhaps, while extending our comparisons, we might stumble into unexpected similarities that all these portions of big history share. If that were to be the case, we would be on our way to construct a theory of big history.

Let me now summarize my own approach, as outlined in chapter one of my book *The Structure of Big History* (1996). Then, I argued that a general theoretical framework for big history must be applicable to the entire range of phenomena that have occurred in cosmic history, from the smallest particles to the most complex configurations. As a result, we can expect that such a theory will exhibit, by necessity, a very high level of generality. This is very similar to the level of generality that currently exists in the natural sciences. For each lev-

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<sup>4</sup> For such comparisons, for one of such exceptions see, *e.g.*, the contribution by Grinin, Korotayev, and Markov to this issue of the Almanac; see also Grinin, Markov, and Korotayev 2011.

el of emerging complexity we may well need additional theories, such as the theory of natural selection for life or plate tectonics for geology. Yet all these additional theories must fit within the general theory.

Finally, let us not forget that all the divisions that we see in big history, such as the distinction between life and non-living nature, are to some extent mental constructs invented by humans. However different these fields may be, they are never entirely unconnected or separated by unbridgeable abysses. For instance, although life is surely different from rocks or stars, they all consist of matter and energy, and thus share certain aspects. A similar argument can be made for all aspects of big history.

If all of this is correct, then the search for general principles outlining how big history works, and thus unifying and simplifying the entire story as much as possible, must stand out as a primary goal. A considerable part of big history research efforts must, therefore, be aimed at, or at least be connected to, the development of big history theories. These theories can then be utilized and tested while trying to find new, and better, answers to particular questions. This is similar to how currently biological research is linked to the theory of evolution; geological research to plate tectonics; and astronomical research to big bang cosmology. In our case, the general big history theory must overarch all of these specialized theories, which, in doing so, would become special cases of the general theory.

All of this may sound very ambitious. Yet this ambition is not new. It was already formulated by Alexander von Humboldt more than 150 years ago in the introduction to his series of books *Cosmos*, in which he tried to summarize all of nature and its history. Von Humboldt may not have succeeded at the time, but that does not mean that achieving such a synthesis is totally impossible. Baron von Humboldt was restricted in his efforts by the much more limited scientific knowledge about the past that existed at that time. Today, by contrast, we can build on more than 150 years of research into many different fields of history, which has yielded so many wonderful fruits, while we have virtually instant access to large portions of this knowledge, thanks to the emergence of the Internet. I think, therefore, that the time is now ripe to pick up this unfinished task and challenge each other to see whether we can do better.

### **Types of Big History Research**

If the above thesis is correct, then the first and most important subject of big history research is:

*1. Big 'big histories': exploration of big history theories.*

This involves finding general patterns and mechanisms in big history that help us to understand it better, and thus explain it in more simple ways. Such research could be both descriptive (qualitative) and quantitative. All these studies could be undertaken in the form of interdisciplinary projects. All the theo-

ries employed must be connected to empirically observable reality. I see several areas of potential interest:

*1a.* Make an inventory of all the theories that have been used in large-scale approaches to history and select those that hold the greatest promise. Such decisions should always be open to revision. Yet, if we were able to agree temporarily on a common theoretical paradigm, or just on a few common principles, this would greatly strengthen our research position. If such decisions cannot be made, we must point out what the weaknesses are of the current theories, while seeking to develop new and better ones.

*1b.* Critically examine existing theories of big history. In the case of my big history theory, this would include refining the analysis of how energy flows through matter, while it is producing certain effects (in our personal case, how food is keeping us alive while enabling us to do the things that we do). This refinement is important, because the outflow of energy that can be seen as waste, from the point of view of the regime through which it is flowing (for example, our bodies), is often useful for other regimes. For instance, solar energy is a waste product for the Sun but high-quality energy for us in the form of energy captured by plants and transformed by animals. In addition, many microorganisms thrive on our waste products. In fact, the entire food pyramid can be considered in terms of such energy flows. This requires much more systematic attention.

*1c.* Systematically compare theories in different fields of big history, such as comparing the process of human collective learning with natural selection in biology, as suggested by David Christian. This may enrich both fields and, perhaps, also general big history theories.

*2. Use big history theories and insights for interdisciplinary research projects.*

Areas of interest might include:

*2a.* A research project based on my emerging theory of big history of combining the existing literature for numerical and descriptive data on matter and energy flows, the generation of entropy, and ‘Goldilocks circumstances’, all the way through big history and systematizing the results, in the hope that this will show the emergence of new unexpected patterns. I am convinced that a large amount of such data already exists, scattered throughout the academic literature. The results could also be used to improve the theory. Such a project would preferably include collaboration with US astrophysicist Eric Chaisson (Tufts University and Harvard University), who is the major pioneer in the field of energy flows in big history.

*2b.* Form small interdisciplinary big history teams, while dealing with specific problems. While considering the origin of humans, for instance, or our current situation concerning the use of natural resources, we could form teams with specialists from many different fields, including unusual choices such as cosmologists, psychologists, paleoanthropologists and cultural anthropologists,

to see whether this would add extra value to the analysis. My strong suspicion is that in most cases there will be such an unexpected added value.

We should, however, not underestimate the extent to which such interdisciplinary efforts may already have started, most notably, perhaps, in System Earth science, including long-term climate studies. In all such cases, we should first carefully consider what has been achieved already before making any grand plans ourselves.

2c. One wonders whether there might be a role for big historians as research counselors for interdisciplinary projects of many kinds, including the business world, thus contributing our wide-ranging knowledge to provide unexpected, and hopefully productive, new angles on how to tackle certain more limited research questions. I have experienced such productive situations quite often in informal settings over the past fifteen years, during a great many meetings with scholars, ranging from astronomers to anthropologists. We may want to establish this on an institutional basis.

### 3. Little 'big histories'.

3a. Placing a certain research subjects within a big history perspective, preferably, but not always, all the way back to the big bang, and see whether this enriches our understanding of that particular subject. In the Netherlands, we require our students to do this during our big history courses.<sup>5</sup> They may pick any object of their choice, the history of which they must trace back all the way to the big bang. Their choices range from iPhones to 'my little brother because he is so funny'. Some students are puzzled in the beginning, but the great majority of them produce essays that are often enlightening and sometimes very entertaining. This approach clearly helps them to better understand both big history and their subject. I understand that this approach is going to be a major component of big history teaching at Dominican University in San Rafael, California (USA), where Cynthia Brown is the leading inspiration.

Little big histories are not only useful didactic tools but can also become serious research projects. The study by Jonathan Markley (2009) on the history of grass offers such an example. But more Little big histories exist, such as the book *Lima* (1992) by Juan Gunther Doering and Guillermo Lohmann Villena, which begins with a short explanation of how plate tectonics had formed the landscape on the Peruvian Pacific coast and how that had in turn influenced the entire ecology as well as its human habitation during all of its history. The work by Alfred Crosby on the Columbian exchange is a much better known example, which explains how plate tectonics led to the current configuration of continents, which deeply influenced the biological resources available to their inhabitants. Another recent example is British geologist Jan Zalasiewicz's book *The Planet in a Pebble: A Journey into Earth's Deep History* (2010), in which he traces

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<sup>5</sup> For the Dutch big history courses, this idea was first proposed by Esther Quaedackers and Marcel Koonen. This idea emerged independently elsewhere also.

the history of one little pebble through the eons of time as part of changes of the environment it found itself in during its entire existence. This contribution provides a first outline of how to define big history research, including an overview of the types of research that could profitably be undertaken. Practical issues are also discussed, such as how to obtain funding, where to publish the results and whether the research results might have practical applications. Because this contribution is, to my knowledge, the first attempt to outline big history research, my observations should be considered preliminary. I hope that they will stimulate a healthy and vigorous discussion about big history research, the one that will lead to formulating a big history research agenda that will successfully be pursued worldwide.<sup>6</sup>

3b. It might be a good idea to make an inventory of the already-existing studies of this kind. I would not be surprised to find that many more such studies already do exist.

4. *Study of the history of big history.*

This could include the following research questions:

4a. How did our current big history field emerge? Who were important players and forerunners? In which ways has big history been used, and by whom?

4b. What have been the various approaches (theoretical, regional, thematic, style of narrative, etc.) to big history and how was their reception? How can we explain this?

4c. Have similar approaches to history also emerged elsewhere in the world?

5. *Reinterpreting traditional origin stories from a big history perspective.*

How and to what extent can we reinterpret origin stories of traditional cultures in the light of big history – by analyzing them as answers to big questions that people have posed in such societies? Why were some of these answers codified and are known to large numbers of people today, while many other origin stories became marginalized and were partially, or sometimes almost entirely, lost? What does this tell us about the history of both those societies and our own society?

## Funding

How to obtain funding for big history research will, in all likelihood, very much depend on the type of big history research that is proposed. Because big history is not yet an established discipline, its practitioners will find it hard to apply for grants in established scientific organizations, because these are usually organ-

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<sup>6</sup> Little big histories: Crosby (1972, 1986), Markley (2009), Zalasiewicz (2010), Doering and Lohmann Villena (1992); for a recent illuminating summary by Juan Gunther Doering see <http://www.usmp.edu.pe/webingles/novedades/index.php?pag=ivuc>.



ized along established academic lines. As a result, we must be creative in how to obtain funding and seek new venues, including perhaps wealthy sponsors, business corporations or other individuals and organizations willing to support big history research. Of course, we should try to apply for grants at established academic organizations also, if only to make clear that we exist and have a novel, and hopefully exciting, research agenda. The IBHA might serve as a major platform for promoting such a research agenda, for instance, by posting exciting research results on its website on dedicated research pages.

Perhaps, we could add a contact page on the IBHA website from where potential sponsors could send us a message showing interest in potentially supporting IBHA members' research proposals. A further dialogue might lead to productive forms of collaboration. Such a discussion would involve building and maintaining contacts with all these different types of sponsors. The IBHA members must be able to make very clear what type of research they want to undertake so that sponsors can decide whether it would be worthwhile for them to fund such types of big history research.

### **Doing the Research**

Most, if not all, big history research should preferably be initiated and executed within universities and across disciplines, hopefully with a global orientation and participation, in the form of PhD projects and beyond. In addition to the research results, an important objective for this focus is to make sure that we train new generations of big historians. We should also look for opportunities to forge cooperation with business corporations or any other organization that may be interested in performing portions of the research.

An important outcome of the research should also be raising the profile of young talent, who hopefully may become eligible for big history university positions. In order to achieve such an objective, it is extremely important that such candidates also gain sufficient experience in teaching big history, because teaching is primarily what universities want their faculty to do.

All of this will involve building contacts with universities, independent academics and business corporations worldwide. The IBHA may help to foster such global contacts.

### **Reporting Results**

We are basically starting from scratch, and thus have the opportunity to use the latest technology for reporting our research results. However, we should not ignore the existing ways of reporting them either. More traditional venues for reporting research results include established academic journals and books. In addition, the IBHA is planning to start its own journal, which may become a place where big history research is reported.

But we may also consider other options, most notably the IBHA web site. It might very much enhance spreading the results of big history research and scholarship, if we could place at least the summaries of our results on the IBHA web site on dedicated pages. This would be very effective in spreading such knowledge worldwide and may attract potential sponsors, especially when we indicate very clearly who had sponsored successful types of big history research.

All of this will involve building contacts with publishers as well as constructing a big history research section on our web site. Furthermore, we would promote big history research at conferences, both the IBHA conferences and panels during other conferences that we find appropriate. This will involve organizing such panels as well as obtaining the necessary funding.

In addition, we may consider presenting the results of big history research via Internet media such as Twitter and Facebook, online video sites and audio presentations, as well as encouraging interactions with interested persons and organizations, through Skype or similar channels. And, more likely than not, there may be a great many other options on the Internet that may evolve somewhere in the near future. All of this will involve a continuing exploration of new media. I see this as particularly important.

### **Practical Applications**

Many sciences have practical applications. One may wonder, therefore, to what extent big history might also offer more than only general insights into how things have gone the way they went. While it is often said that history repeats itself, it remains difficult to draw clear lessons from history. But, perhaps, our more general theories, if we can find them, might come to the rescue. Perhaps, we will be able to offer more than impressionistic insights. Perhaps, we will uncover mechanisms in history that may help people to make better decisions in specific situations in daily life. If we could achieve such results, it would represent a major added value to our research projects. As always, the proof of the pudding will be in the eating. Right now, we have barely begun preparing the pudding of big history research. Let us start doing that first and then decide how it tastes.

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