Archaeological and Palaeoenvironmental Time-series Analysis

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Abstract

The effects of modern climate change will be felt for centuries to come. Planning for that future right now is very difficult, however. We do not know how human societies respond to climate change over the long term. Modern and historically recent cases cannot provide us with a solid basis for making predications about the future because modern climate change has not been going on long enough to see its full effects. Instead, we need to look to the archaeological record for examples of long-term human responses to climate change.

Despite more than a century of effort, though, archaeologists have made limited progress in understanding past human-environment dynamics. Archaeological and palaeoenvironmental datasets have improved markedly, but attempts to link those records have so far been unconvincing. The primary reason for this is a lack of appropriate quantitative tools. Archaeological and palaeoenvironmental data contain idiosyncrasies—namely temporal autocorrelation and chronological uncertainty—that undermine statistical methods. Given the seriousness of modern climate change, we need to rectify this situation.

In this dissertation, I lay the groundwork for developing a quantitative toolkit for analyzing long-term human-environment dynamics. The dissertation is comprised of four studies involving time-series methods. The first two look at the impact of climate changes on the Classic Maya using two types of time-series analysis, and the last two use simulations to probe the limits of these methods. Together, the four studies demonstrate that the idiosyncrasies of archaeological and palaeoenvironmental data create challenges for quantitative analyses. Reviewing the studies, I identify the main methodological challenges and sketch out some potential solutions, illuminating a path for future methodological development.

Keywords: Archaeology; Time-series Analysis; Climate Change; Palaeoenvironment; Human-environment Dynamics

Dedication

To my family.

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Chapter 1. Introduction

1.1. Overview

Climate scientists predict that over the next 100 years global average temperatures will increase by at least 1.5°C, sea level will rise by at least 20cm, and droughts, forest fires, and storms will occur with increasing frequency and intensity (IPCC 2013). These changes will be challenging for many people. And while our species is likely to survive, our social, economic, and cultural institutions might not. We do not yet know how complex human societies respond to significant climate changes like the one we now face, making planning for the future difficult. The crucial question is not whether our species will survive, but what will happen to our societies and institutions as the climate changes.

Our genus, *Homo*, has been around for at least 2.8 million years (Collard & Wood 2015, Finlayson 2005, Maslin et al. 2015, Trinkaus 2005) and throughout that time adapted to numerous changes in the Earth's climate. Over the Pleistocene, a period that began around 2.5 million years ago, the climate changed wildly (deMenocal 2004, Maslin et al. 2014, Trauth et al. 2007). In Africa, the geographic origin of *Homo*, forests became grasslands and then forests again, massive lakes vacillated between dry and brimming, deserts expanded and contracted. In the high latitudes of Europe and Asia, glaciation intensified. Most of the continental Northern Hemisphere was laden with sheets of ice kilometers thick that would periodically extend into the south almost to the Mediterranean before receding again. Yet through all of it, *Homo* persisted, eventually giving rise to modern humans about 200,000 years ago.

The last 200,000 years were similarly tumultuous (Blome et al. 2012, Carto et al. 2009). Modern humans survived no fewer than three Ice Ages, the earlier two of which we experienced while still mostly living in Africa, and the last one occurred after we

expanded into Europe and Asia around 50,000 years ago. Then, at the end of the last Ice Age 11,000 years ago the Holocene began—i.e., the modern geological epoch often characterized as relatively stability compared to the Pleistocene. Despite its comparative stability, though, it too has had its vicissitudes (deMenocal et al. 2000, Kuper & Kröpelin 2006, Mayewski et al. 2004, Roberts 2013). So far, there have been several hemisphere-scale climatic shocks, notably including the Younger Dryas, the Medieval Warm Period, and the Little Ice Age. Some of these shocks involved temperature swings of 5–10°C over parts of the Northern Hemisphere, changes that transpired in mere decades (e.g., Alley 2000). Yet, here we are, with a global population that pushed past 7 billion people in 2014, clearly thriving as a species in spite of major climate changes. However, *that* we have survived past climate changes says very little about the potential impacts of climate change on human societies.

On its own, survival is a pretty low standard for evaluating the impact of a perilous event. If you were a safety conscious person looking to purchase a car and every manufacturer only told you the odds that *someone* might survive a crash in a given vehicle, you would probably think you lacked sufficient information to decide which manufacturer to trust, even if the odds favoured survival. You might want to know how often crashes occurred, how many people survived different kinds of crashes, and what state the survivors were in afterwards. Similarly, knowing only that humanity has managed to survive climate changes before is hardly sufficient information with which to plan for the future of modern climate change.

Rather, to plan effectively we need to know more about the range of likely impacts of climate change on human societies. There are many potential impacts and societies that could be affected. Climate change, too, is geographically and temporally diverse, with local effects that can be quite different from global averages, effects that are dependent on local conditions. Therefore, to make useful predictions that might apply generally to human societies, we need comparably diverse information on which to base those predictions. We need to know how different societies occupying various environments under different ecological conditions have responded to various local and global changes. In other words, we need a diverse database of examples. However, such a database cannot be created by studying modern societies alone.

The upswing in modern global temperatures characteristic of anthropogenic global warming only began within the last two hundred years (IPCC 2013, Karl & Trenberth 2003). As a result, the current episode of climate change has not been going on long enough to see its long-run impact on human societies. Likewise, information about historically recent societies can only provide limited evidence for the impact of climate change because rapid climate changes like the ones we are experiencing have no recent historical analogues (Salinger 2005). Instead, as a number of scholars have argued in recent years (e.g., Caseldine & Turney 2010, Costanza et al. 2007a, de Menocal 2001, Mitchell 2008, Orlove 2005, Van de Noort 2011), we have to predict the future by using what we know about the more distant past. We need to step back and take a wide-angle view of human responses to climate change over long periods of time. With such a view, we could build the database we need for predicting the long-run impact of modern climate change. The archaeological and historical records are the only archives with a sufficient time depth and diversity to create that database.

Unfortunately, our understanding of past human-climate dynamics is limited. The vast majority of the work can be described as hand wavy, involving unsubstantiated narratives and informal visual correlations between climatological and archaeological records. Some studies have gone further and used statistical methods, but very few of those have subjected both the climatological and archaeological records to statistical analysis, and none of them has addressed the two most important sources of bias—namely, temporal autocorrelation and chronological uncertainty. The term "temporal autocorrelation" refers to the fact that observations from one time-step are related to the observations from the next time-step, like daily temperature or population levels. "Chronological uncertainty" refers to the errors associated with assigning dates to observations. Chronological uncertainty is a major problem for the most commonly used chronometric method in archaeology and palaeoclimatology—radiocarbon dating. Thus, it affects much of what we know about the past. Together, the two sources of bias produce questionable statistical findings. Consequently, there is much uncertainty about the impact of climate change on past human societies, meaning that we lack sufficient evidence for making predictions about its future impacts.

To improve our predictions, we need better analytical tools. The tools will need to meet three key criteria. First, they need to account for the idiosyncrasies of palaeoenvironmental and archaeological data, including temporal autocorrelation and chronological uncertainty, especially radiocarbon dating uncertainty. Second, they need to enable us to test hypotheses involving different combinations of climate variables—i.e., they need to be formal statistical models designed to make predictions about a given dependent variable using various combinations of independent variables. Third, the models need to be comparable so some of them can be eliminated, falsifying the hypotheses on which they were based. Having tools that meet these criteria would allow us to test hypotheses about interactions between human societies and climate change over the long term, improve our understanding of past societies, and provide a solid basis for making predictions about the future. Laying the necessary groundwork for developing such tools is the primary focus of this dissertation.

1.2. Project Aims and Objectives

In this dissertation, I begin the process of developing a quantitative toolkit for analyzing past human-environment interaction using archaeological and palaeoenvironmental data. I have several specific objectives. One is to highlight the need for using quantitative methods to assess past human-environment interaction. Another is to demonstrate that existing quantitative methods are complicated by the two idiosyncrasies mentioned earlier, temporal autocorrelation and chronological uncertainty—specifically radiocarbon dating uncertainty because it is by far the most commonly used chronometric method in archaeology. The third objective is to adapt existing quantitative methods to account for the idiosyncrasies. The fourth objective is to apply these adapted methods to two high profile case studies involving the Classic Maya, simultaneously showcasing the methods and improving our understanding of human-environment interaction among the Classic Maya. The fifth objective is to explore the efficacy of the methods by conducting computer simulations that probe their limits under different experimental conditions with varying degrees of radiocarbon dating uncertainty.

Since both the archaeological and palaeoenvironmental records are essentially comprised of *time-series* data, I focus on methods for *time-series analysis*. A time-series is a set of sequential observations where the order of the observations matters (Chatfield 2009, Pickup 2014). In archaeology, an example might be a series of demographic estimates based on the numbers of sites occupied at different times, while palaeoenvironmental examples include proxy measurements for temperature or rainfall at a series of depths in a sediment core. Since the order of these observations is important, a special body of statistical techniques is used to analyze them. These methods can be divided into two broad categories (Pickup 2014). One contains a suite of techniques designed to identify cyclical patterns in time-series data, often referred to as *frequency-based* analysis. The other category comprises modeling approaches for *regression* and *forecasting*. Together, they are key tools for understanding relationships between phenomena that change over time, like environmental and sociocultural conditions.

However, palaeoenvironmental and archaeological time-series are idiosyncratic, mostly because they contain temporal autocorrelation and radiocarbon dating uncertainty as I mentioned earlier. Together, these two idiosyncrasies have the potential to confound time-series methods. Temporal autocorrelation is problematic for two main reasons. First, it leads to autocorrelated error terms in regression models, often causing inflation in correlation coefficients and statistical significance (Pickup 2014). This gives rise to spurious correlations and overly optimistic significance levels. Second, failing to account for expected autocorrelation can lead to identifying ostensibly meaningful trends that are really just random drift. If the values at a given time affect subsequent values in the series, the mean of the observations will have some persistence such that it can drift up or down over time creating a path. The path can look indistinguishable from a trend caused by external forcing even though it has been created by nothing but autocorrelation. Fortunately, many time-series methods have been adapted to handle autocorrelation since it is prevalent in real world data—e.g., daily temperatures, stock prices, and so on. Thus, as long as the correct methods are used, it can be accounted for.

Chronological uncertainty, on the other hand, is more troublesome for statistics, especially when it relates to radiocarbon dates (Blaauw 2010, Carleton et al. 2014, Mudelsee 2014, Parnell et al. 2011, Telford et al. 2004b). Radiocarbon dates are calibrated to account for the historic variation in environmental carbon isotope ratios. They can be off by centuries if they are not calibrated. Unfortunately, though, calibrated radiocarbon dates have highly irregular, multimodal distributions that reflect our uncertainty about the date of a given event. Unlike unimodal probability distributions, these highly irregular distributions cannot be adequately described by *point estimates* like the mean or median value. The mean, for example, may actually be no more likely for a given date distribution than any number of other potential values with equal or higher probabilities. So, focusing exclusively on the mean, or median, of a radiocarbon date distribution injects substantial bias into a given analysis. Furthermore, the fact that we cannot confidently pin a given observation to a specific date means the observations in a radiocarbon dated time-series can "float" in time. For any pair of observations our uncertainty about their true chronological position means they might be closer together or farther apart depending on which dates we use for them. Consequently, the time between observations in a radiocarbon dated time-series is uncertain and inter observation times can be compressed or expanded in irregular ways along the series.

The chronological uncertainty of radiocarbon dated time-series creates two main problems for statistical analyses. One is that it undercuts the assumption that observations are paired. In most statistical applications of use to archaeology, the primary objective is to compare one dataset with another, such as two time-series. But, if the dates for the observations in one or both series are uncertain, the observations in one series might not line up in time with any observations in the other series. The two series will appear to have gaps compared to one another. Without complete pairs of observations, standard techniques like regression are not possible. Correcting for the mismatches entails either removing solitary observations or interpolating between them to create complete pairs. Both options add bias.

The other main problem chronological uncertainty creates has to do with calculating slopes. Recall that calculating a slope in a time-series involves dividing the change in the value of observations by the time between them. Slope calculations are

essential in statistical analyses because many methods involve rates, which are just slopes by another name. Because the observations can float in time due to chronological uncertainty, the denominator in the rate calculation can vary because the time between observations is uncertain.

Aiming to explore how temporal autocorrelation and chronological uncertainty affect time-series methods, I conducted four studies. The first two investigated the impact of climate change on the Classic Maya. The other two studies used simulation to explore the effect of chronological uncertainty on the methods employed in the empirical analyses. Together, the four studies improve our understanding of how the idiosyncrasies of archaeological and palaeoenvironmental data affect established time-series methods and shed light on important aspects of human-environment dynamics among the Classic Maya.

The Classic Maya have become a prominent case study for research into past human-environment interaction. The Maya people live near the isthmian portion of the North American continent (see Figure 1-1). During the Classic Period, which archaeologists define as spanning approximately 350–900 CE, they lived in city-states ruled by divine kings (Coe 2011). The Classic Period is primarily known for its elaborate temples, massive pyramids, finely crafted artifacts, and logographic inscriptions sometimes referred to as “hieroglyphics” (Coe 2011, Houston et al. 2001, Martin & Grube 2000, Sharer & Traxler 2006). It is also well known for its end—i.e., the famous collapse of the Classic Maya, a period lasting around two centuries from 900–1100 CE during which many cities were abandoned and the institution of divine kingship crumbled (Demarest et al. 2004, Turner & Sabloff 2012). The collapse has inspired a considerable amount of research, much of which has focused on the role of drought and climatic change (e.g., Aimers & Hodell 2011, Brenner et al. 2002, Cowgill 1964, Curtis et al. 1996, Dahlin 2002, Douglas et al. 2015, Dunning et al. 2012, Gill 2000, Gill et al. 2007, Haug et al. 2003, Hodell et al. 2005a, 2007; Kerr 2001, Lucero et al. 2011, Oglesby et al. 2010, Peterson & Haug 2005, Sabloff & Willey 1967, Shaw 2003). This focus has resulted in numerous high-quality palaeoenvironmental proxy datasets from the Maya region. Together, the high-quality palaeoenvironmental data and the significant

archaeological interest in the region made the Classic Maya ideal for my research into time-series methods and past human-environment interaction.

Figure 1-1 Map of Classic Maya Area

In the first case study, I tested the well-known hypothesis that cyclical droughts contributed to periodic social and political upheaval throughout the Classic period (Brenner et al. 2002, Curtis et al. 1996, 1998, Hodell et al. 2001, 2005b,a, 2007). The hypothesis was based on a frequency-based time-series analysis of a drought record from the middle of the Yucatan Peninsula, which is the central region of Classic Maya civilization. The hypothesis claims that two major drought cycles peaked at times corresponding to several major Classic Maya historical events. However, the frequency-based method used to find the drought cycles cannot account for missing data, temporal autocorrelation, or chronological uncertainty, all characteristics of the drought record. So, in the first study, my co-authors and I sought to explore the effect of autocorrelation and chronological uncertainty on frequency-based techniques and to retest the cyclical drought hypothesis with a method that could account for those effects.

In the second study, I investigated the hypothesis that a trend toward increasing dryness throughout the Classic Period led to increased levels of conflict (Kennett et al. 2012). The hypothesis was based on visual comparison between palaeoclimatic drought proxy time-series and a historical record of Classic Maya conflict levels derived from writing on monuments. Early in the Classic Period, Maya kings began commissioning epigraphic inscriptions in stone monuments erected during ritual events (Coe 2011, Martin & Grube 2008). Since the decipherment of the Classic Maya language, many of these inscriptions have been translated revealing that they often describe historical events, including conflicts between city-states. So, the monument record contains time-series data about conflict levels, which were evidently increasing throughout the Classic Period. However, since the climate-conflict hypothesis was based only on a visual comparison, there was no way to be certain that a correlation really existed. So, in the second study, my co-authors and I evaluated the impact of climate change on Classic Maya conflict using a time-series regression method. By comparing regression models involving different palaeoenvironmental proxies as covariates, we quantitatively tested the hypothesis that climate change drove conflict levels.

In the third and fourth studies I conducted simulation experiments with the goal of evaluating the impact of radiocarbon dating uncertainty on the time-series methods used in the previous two studies. Both simulation experiments involved the use of artificial radiocarbon dates and synthetic time-series data containing pre-determined patterns. In the third study, I experimented with the frequency-based method used to re-evaluate the cyclical drought hypothesis in the first paper. By modulating the chronological uncertainty in the synthetic time-series data, I aimed to identify the conditions under which radiocarbon dating uncertainty overwhelms our ability to detect cycles in time-series. The objective was to determine whether frequency-based methods are capable of producing reliable results when used on palaeoenvironmental and archaeological time-series dated with calibrated radiocarbon dates. Then, in the fourth study, I conducted a series of simulation experiments involving artificial time-series and the regression method used to evaluate the climate-conflict hypothesis. By modulating the chronological uncertainty and several other simulation parameters, I aimed to explore how chronological uncertainty affects our ability to correctly identify pre-determined correlations between the synthetic time-series. The main objective was to determine

whether it was possible to recover the pre-determined relationships between the synthetic time-series despite the presence of radiocarbon dating uncertainty.

Together, these four studies lay the foundation for developing a new set of quantitative tools for analyzing past human-environment interaction. They demonstrate the need for the tools and explore how the idiosyncrasies of archaeological and palaeoenvironmental data affect quantitative analyses. They also present two early versions of tools that will allow archaeologists and palaeoenvironmental scientists to analyze their data while accounting for the idiosyncrasies of their respective records. These tools, and the theoretical and methodological understanding gained by studying them, have the potential to advance archaeological research on past human-environment interaction.

1.3. Dissertation Outline

The remainder of this dissertation is organized into several chapters. Chapter 2 briefly maps out the history of human-environment research in archaeology. It explores the trajectory of the field and establishes where my dissertation fits into the existing literature. Chapters 3 through 6 present the four studies that comprise the bulk of my PhD research. The first study, which re-examined the hypothesis that cyclical droughts affected Classic Maya history, was published in 2013 in the journal *Quaternary Science Reviews* (Carleton et al. 2014). The second study, which re-examines the hypothesis that increasing dryness led to increased levels of conflict among the Classic Maya, was published in 2017 in the journal *Quaternary Science Reviews*. The last two studies, both of which involve massive computer simulations designed to probe the limits of the methods used in the first two studies, have been submitted to peer reviewed journals and should be published in the near future. The final chapter of this dissertation, Chapter 7, synthesizes the major findings of the four studies and discusses their implications for past human-environment research in archaeology.

Chapter 2. Background

2.1. Overview: A long history of interest

The first person to publish on the relationship between past societies and the environment seems to have been a Danish geologist named Japetus Steenstrup (1813–1907). In the middle of the 19th century, Steenstrup studied peat bogs in Denmark using the principles of geological stratigraphy laid out by Charles Lyell a few decades earlier (Birks & Seppä 2010, Trigger 2006). The peat bogs had been accumulating organic material and preserving it for thousands of years, resulting in a long record of vegetation history. While studying the layers of preserved organic matter, Steenstrup discovered that the composition of Denmark's forests changed substantially during what we now call the Quaternary Period. He also found artifacts in the bogs and correlated them with Christian Thomsen's Three Age System. Consequently, Steenstrup was able to associate, for the first time, changes in past human societies with past environmental conditions. His work appeared in *Denmark Oldstid* (1843), a summary of the prehistory of Denmark by Jens Worsaae who was arguably the first professional archaeologist (Trigger 2006). About 18 years later, a more detailed summary of Steenstrup's work, integrated with faunal evidence of past climate changes, appeared in a book titled *General Views on Archaeology* written by Adolph von Morlot (1861), a Swiss geologist turned archaeologist (Grapes 2008). Morlot was an early promoter of the term Quaternary and a strong proponent of the theory of the Ice Age, which was a controversial idea at the time (Grapes 2008). In his book, Morlot discussed the possibility that the past changes in Denmark's forest cover might have been caused by human deforestation. Thus, the earliest intersection of palaeoecology and archaeology occurred at the dawn of modern archaeology. It not only resulted in evidence for past changes in climate and additional empirical stratigraphic support for the Three Age System, but also inspired speculation about the relationship between past societies and past environmental conditions.

Despite the precocious start, little was published in the remaining decades of the 19th century or the beginning of the 20th that indicates any substantial interest in climate change among archaeologists of the day. It was not until the mid 20th century that the study of past climates received substantial attention in archaeology. At that time, palynology was burgeoning and scholars began to realize the potential for pollen evidence, combined with other environmental proxies, to provide them with considerable detail about past environments (e.g., Eiseley 1939, Sears 1932). Some prominent archaeologists were also advocating ecological explanations for cultural variation and change, and that helped foster the development of environmental archaeology as a sub-discipline (Trigger 2006). Graham Clark, for example, laid out a theory of human ecology intended to provide a basis for archaeological interpretations (Clark 1954). His work placed the means by which people fed themselves and exploited their environments at the foundation of human society, explicitly linking human societies to their environments. Other notable researchers, including Joseph Caldwell, Gordon Willey, Lewis Binford, and E. O. Wilson, argued along similar lines (Binford 1962, Caldwell 1959, Willey 1953, Wilson 1975). This view influenced the so-called New or Processual Archaeology. The processualists thought that archaeologists could use human ecology to illuminate the social and ecological processes that produced the cultural variation they observed in the archaeological record. Taking inspiration from White (1959), Binford stated the case for studying past environments pithily when he wrote that culture is humanity's "extra-somatic means of adaptation" (Binford 1962). The phrase highlighted the tight, evolutionary coupling Binford and the other processualists saw between culture and the environment. According to this view, human-environment interaction has been responsible for much of human history on time scales relevant to cultural and biological evolution. By extension, the notion that culture was an adaptation necessarily meant that significant changes to past environments would have caused changes in the past societies that inhabited them as well—i.e. the human-environment relationship was a prime mover of cultural change.

In the 1980s and 1990s, several archaeological theorists began to question the assumptions of the theories espoused by processual archaeologists (Trigger 2006). The theoretical focus started to shift toward understanding social causes for material cultural variation, toward seeing human agency as a prime mover that drove social change, and

toward substantial skepticism about what archaeologists could actually learn about the past. Some highly influential archaeologists like Ian Hodder, Christopher Tilley, and Michael Shanks were critical of ecological approaches to archaeology, starting a new theoretical movement called "post-processualism" (Hodder 1982a, Shanks & Tilley 1992). Despite their criticism, environmental research in archaeology continued (e.g., Arnold 1992, Broughton 1994, Kennett & Kennett 2000, Kirch & Ellison 1994). In fact, it became more prominent (see Figure 2-1 below) and even the critical post-processualists studied how people interacted with their environments. Tilley, for example, conducted a project that was intended to understand how landscapes shaped human experience, albeit in a highly subjective way (Tilley 1994). Similarly, Hodder embarked on one of the largest interdisciplinary excavations ever conducted, one that included numerous researchers charged with collecting and analyzing environmental evidence (Hodder 1996, 2005). The biggest difference for environmental archaeology between the mid and late 20th century was probably that some prominent scholars of the latter period saw environmental causes of human social change as having played a secondary role to social causes. Nevertheless, environmental archaeology continued to advance, developing and refining methods for ancient pollen analysis, dietary analysis, geochemical and isotopic analysis, micro-scale geological analysis, and spatial analysis (Aitken 1990, Bernhardsen 1999, Bradley 1999).

Today, the methodological advances continue and archaeological interest in the relationship between past climate change and changes in past societies is experiencing something of a boom. According to a search on Web of Science for "environment" and "archaeology", the number of archaeological papers published annually that involve past human-environment interaction has increased from fewer than 10 before the 1970s up to around 300 each year since 2009 (www.webofscience.com, searched 2016-09). As an annual percentage, the fraction of all archaeological research that involves past human-environment interaction has risen from approximately 0.3% in 1959 to 10% in 2015. The rate of publication looks almost exponential, with the major incline occurring sometime around 1990 (see Figure 2-1).

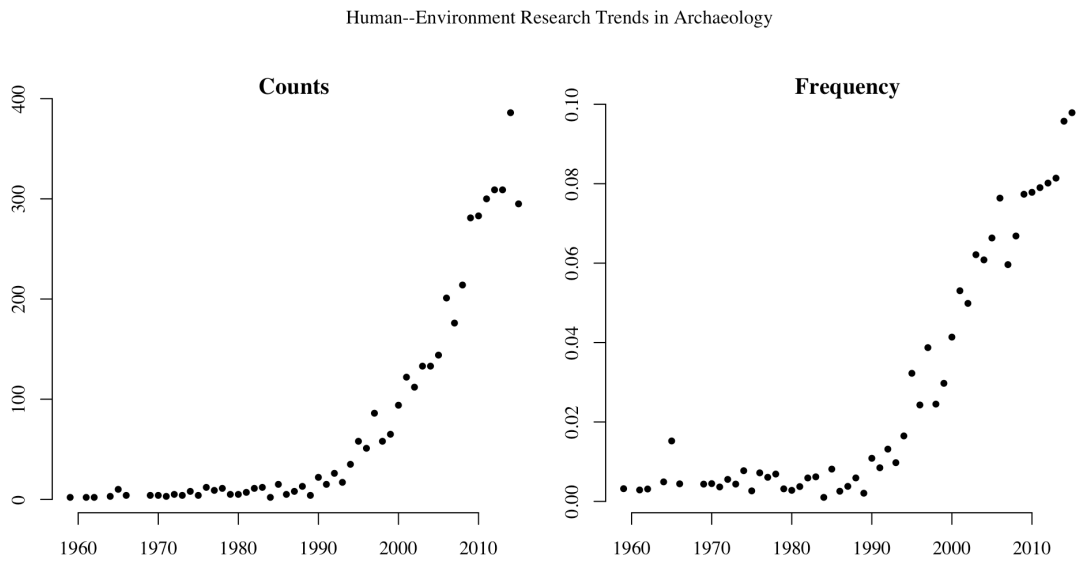

Figure 2-1 Research Trends

That was the year the Intergovernmental Panel on Climate Change (IPCC) published their first report (Houghton et al. 1990). The IPPC is an international body of scientists under the auspices of the United Nations established to provide the world with an objective scientific view of climate change (www.ipcc.ch). That they were assembled in the first place suggests society at large was becoming increasingly concerned about modern climate change. And, as many scholars have pointed out, archaeologists are often inspired to look for evidence of contemporary concerns in the past (e.g., Shanks 1992). Moreover, the widespread concern about modern climate change has led to a proliferation of palaeoenvironmental data as climate scientists have attempted to understand anthropogenic global warming. At the same time, Internet access was becoming commonplace, which facilitated collaboration and data sharing and giving rise to large, freely accessible online databases (e.g., www.ncdc.noaa.gov/data-access/paleoclimatology-data/datasets). So, the increased archaeological interest in past human-environment interaction has probably been facilitated by a combination of contemporary climate change zeitgeist and a marked increase in the amount of palaeoenvironmental data available to study.

Many archaeologists would argue, though, that the increase in interest reflects more than just convenience or our tendency to project the present into the past (e.g.,

Caseldine & Turney 2010, Costanza et al. 2007a, Van de Noort 2011, van der Leeuw & Redman 2002, van der Leeuw et al. 2011). For one thing, significant climate changes surely impacted past people, placing understanding past human-environment interaction rightly within our purview. In addition, an increasing number of scholars think that understanding past human-environment dynamics can help us to understand the present. This line of reasoning has led several scholars to look to the archaeological record for examples of past human responses to climate change (e.g., Caseldine & Turney 2010, de Menocal 2001, Kirch 2005, Rainbird 2002). They argue that we can learn something from these societies that might help motivate people to take action to mitigate modern climate change and perhaps even help us adapt to it.

2.2. The theoretical developments of the last century

There has been a great deal of theoretical innovation in relation to human-environment interaction in the past. In the first few decades of the 20th century, scholars drew only tenuous and naïve connections between cultural traits and environmental conditions. Some argued that cultures advanced toward complexity in a predictable way and that cultures in different environments had particular traits—e.g., that "difficult" environments like the Arctic produce "distinctive" cultures like the Inuit—but precisely how environmental conditions were tied to suites of cultural traits or perceived levels of complexity was never fully explained (Bennett 1944). Archaeologists like Alfred Kroeber, a student of France Boas, argued that cultural areas overlapped with geographic areas in a meaningful way, but the causal linkages were only vaguely defined (Kroeber 1939). Another student of Boas, Julian Steward, also highlighted correlations between culture and the environment—specifically between economic activity and environmental conditions (e.g., Steward 1937, 1963). But, again, he was vague about the linkages involved.

By the middle of the 20th century, ideas about human-environment interaction had become more refined. Leslie White, for example, thought that the human-environment interaction was primarily mediated by technology. Culture, he famously formulated, was quite literally the product of technology and environmental conditions and was, in general, an adaptation to the environment (White 1943). Around the same

time, Betty Meggars argued that environment was a limiting factor for cultural complexity (Meggers 1954). In her view, a given culture's potential suite of traits and its complexity were functionally limited by what the environment had to offer, which is how environmental conditions could determine cultural traits.

Perspectives on human-environment interaction became even more nuanced in the 1960s, with the introduction of Systems Theory to archaeology. Now-famous scholars like Sally and Lewis Binford (Binford 1968, Binford & Binford 1968) and Kent Flannery (Flannery 1968) argued that cultures were like living organisms with subsystems that extracted energy from the environment. Systems Theory included ideas about homeostasis and equilibria between social and environmental systems. It also led to theories about environmental catastrophe that could lead to cultural system imbalances and eventually societal collapse. Alongside systems theory, evolutionary ideas about culture were further developed. Some archaeologists saw cultures as adapting to different environments while others, most notably Lewis Binford, argued that culture itself was a human adaptation in the Darwinian sense (e.g. Binford 1965).

Through the 1970s and 1980s, though, scholars began to heavily criticize the focus on environmental forces behind cultural changes that was common in the preceding decades. Several polemics were written about *environmental determinism* (e.g., Baker 1962, Trigger 1971, Zubrow 1972)—i.e., the notion that the environment is the primary driving force behind cultural variation and change. Critics argued that the ecological approaches to archaeology prominent since the beginning of the century had neglected to account for the role of culture in human-environment interaction. Deterministic approaches failed to recognize that individuals had *agency*—i.e., the power to make choices and actively manipulate their own circumstances. They also failed, the critics argued, to account for the idiosyncrasies of archaeological data, namely that the patterns in the record are the convoluted product of site formation processes and past ideology (Baker 1962, Hodder 1982a,b; Leone et al. 1987, Zubrow 1972). Thus, the record could not be taken at face value, which meant that ecological models of cultural change were problematic because the cultural changes they were intended to explain might have been mirages—the "real" cultures were obscured by site formation processes and the past manipulation of symbols and ideology. So, archaeologists began

to focus more on the role of culture and agency in determining the patterns that appeared in the archaeological record. They also began to look at how culture and agency affected human-environment interaction (e.g., Bryson 1994). This reorientation meant archaeologists were looking at the human-environment relationship from the human side instead of generally drawing the causal arrow the other way.

Recently, interest in understanding the ways in which sociocultural and economic systems are robust, or *resilient*, to environmental change has become the main focus of attention (e.g., Butzer 2005, Dearing 2008, Dunning et al. 2012, McAnany & Yoffee 2009, Redman 2005, Redman & Kinzig 2003, Rosen & Rivera-Collazo 2012, Thompson & Turck 2009). Resiliency Theory was developed by ecologists to better understand adaptability in ecosystems. At the core of this theory is something called an *adaptive cycle*, which is intended to describe the process of a species colonizing a niche, absorbing biomass and energy, suddenly releasing that biomass back into the ecosystem, and then reorganizing to take advantage of a new ecosystem regime (Allen et al. 2014, Gunderson & Holling 2002). In most archaeological applications of the concept, the adaptive cycle is intended to be viewed over long time scales and provide an explanation for the repeated cycles of civilization that some scholars claim to have occurred throughout human history—i.e., the rise and fall of several prominent historical civilizations like the Classic Maya. Resiliency Theory also involves notions taken from complexity science—i.e., the mathematics of complex systems—like regime shifts, tipping points, and non-linear causality. It is considerably more nuanced than earlier formulations of human-environment dynamics, and many archaeologists have taken up the idea. In so doing, they have begun to emphasize human adaptation and the importance of socioecological context for determining given historical outcomes.

2.3. The analytical and methodological lag

In contrast to the situation with regard to theory, the basic methods used for assessing the human-environment dynamics of past societies have hardly changed in over 150 years. In Morlot's (1861) book, written more than a century ago, he synthesizes faunal and floral evidence for past climates derived from peat bogs in Denmark. His climatic reconstruction is delivered in three sections, corresponding to the ages of Thomsen's

Three Age System, namely the Stone, Bronze, and Iron Ages. In each section, Morlot describes the floral and faunal evidence, compares that evidence to the modern environment of Denmark, and discusses how the environment must have been different in each age to account for the differences in flora and fauna. He also describes the archaeological evidence corresponding to each age and muses about how past societies interacted with the different environments. He speculates that human industry caused significant deforestation and describes how people may have made use of the different plant species available to them in each age.

Archaeological papers from the early and mid 20th century were more quantitative than Morlot's book but hardly any more analytically sound. Quantification of environmental indicators became more common. In particular, pollen counts and frequencies were used to classify environments, an inference that relies on a constant relationship between suites of plant species and certain prevailing environmental conditions (Erdtman 1943). Early on, pollen spectra became a cornerstone of ancient pollen studies and appeared commonly in archaeological papers (e.g., Clark 1954, Deevey 1944, Hedberg 1954, Hill & Hevly 1968). They were used as a tool to visualize changing frequencies of pollen types over time and to define chronological boundaries between pollen zones—i.e., suites of pollen types that represent distinct biomes. Pollen zones combined with dating methods like varve counting, dendrochronology, and eventually radiometry provided ecologists with a sequence of changing biomes, distinguishing colder periods from warmer ones and wetter periods from drier ones. However, the increasingly quantitative approach to pollen and climatic reconstruction had little effect on the assessment of the relationships between reconstructions, or between those reconstructions and archaeological sequences. Palaeoclimatic reconstructions only provided fodder for archaeological postulation and subjective narratives. The postulates and narratives became increasingly sophisticated, well beyond the simple musings of Morlot, but they still consisted mainly of unverifiable, unfalsifiable stories.

More recent studies have benefited from high-precision, high-resolution climate data that has led to a clearer picture of past climate than Morlot could possibly have imagined. Climate scientists have discovered dozens of new proxies for past

environmental conditions, far beyond plant detritus in peat bogs, pollen frequencies, and lakebed varves (Cronin 2013). New climate archives, like ice cores, corals, and layers of mollusc shells have given us an unprecedented view of past environments, with more detail and greater geographic coverage than anything scientists had in the mid-1900s (e.g., Ainis et al. 2014, Bailey & Craighead 2003, Black et al. 2004, Brennwald et al. 2004, Simonsen et al. 2011, Steig et al. 2000, Thompson et al. 2013). The databases continue to grow (see the National Oceanic and Atmospheric Administration's website for examples [www.ncdc.noaa.gov]), but the basic analytical process in studies of past human-environment interaction continues to be reminiscent of Morlot's work.

To evaluate recent work on the topic, I conducted a systematic review of the literature. In a sample of 110 archaeological human-environment interaction studies published between 2003 and 2013 in several academic journals (see Appendix A), I looked for the main approaches used by archaeologists to identify and interpret past human-environment interaction. I found that scholars have generally taken one of two approaches. In one approach, the authors begin by presenting their climate data and a regional archaeological review. Then they interpret the climate data in an attempt to reconstruct past climate regimes with a focus on subjectively defined climate shifts. The shifts are often used to define the boundaries between broad chronological periods characterized by supposedly distinct climates. In the other approach, authors use archaeological periods to provide the chronological boundaries, like Morlot did, and reconstruct climate regimes corresponding to those periods. The end result in the vast majority of cases is a narrative about past climate change and archaeology. The authors typically guide the reader through a story, moving from one chronological period to the next, narrating possible relationships between the archaeological data and the climatic reconstruction, much like Morlot did, with few if any examples of rigorous quantitative analysis or attempts to falsify hypotheses. So, it seems the addition of abundant quantitative data and a sharper image of past environments has not spurred analytical development; it has simply enabled more detailed narratives.

The narratives also often come with illustrations to aide the storytelling. One illustration is virtually emblematic of recent archaeological human-environment research: the multilayered time-series diagram (e.g., Kennett et al. 2012). In these diagrams, each

layer displays a climatic or archaeological proxy time-series depicted by a wiggly line with time proceeding to the left or right along the bottom of the diagram. The “wiggles” are formed of peaks and valleys that represent highs and lows in a given proxy (e.g., Migowski et al. 2006, Morales et al. 2009, Smith et al. 2008). Authors highlight, arbitrarily, apparent correspondences among the wiggles in the various layers of the diagram. Then, they use those correspondences to inspire a causal narrative about past human responses to climate change. While these diagrams are made possible only by the availability of quantitative proxy records requiring sophisticated scientific methods and apparatuses to produce, they are merely ornamental. For every apparent correspondence between records, invariably another slice of time can be found where the relationship is reversed or nonexistent. So, without the aid of quantitative measures of correlation, the wiggle matching diagrams are just visual aids to mostly subjective narratives, making most of the recent research unconvincing.

Some authors have attempted to quantify human-environment interaction, but they have not been especially convincing. In about 14% of the review sample, authors have attempted to subject their data to quantitative analyses, but the attempts are lacking in several ways. One problem is that authors have patchily applied quantitative methods to different aspects of each analysis. In more or less every paper, we are left guessing about one important aspect or another. In some papers, authors used statistical methods to defend their chronologies but neglected to defend their identification of significant climatic events, leaving us to wonder whether the climate changes they discuss were substantial deviations from the norm and significant enough to warrant consideration (e.g., Plunkett et al. 2013). In others, authors used statistical methods to identify significant climatic changes but then failed to provide a defense of their chronologies, raising the question of how chronological uncertainty might have biased those results (e.g., Gulyás & Sümegi 2011a,b). In nearly all of these studies, we are expected to trust the authors’ subjective assessments of correlations between records. Three papers in the sample included quantitative measures of correlation, but those authors neglected to defend their chronological boundaries or their identification of significant climatic changes (Asmussen & McInnes 2013, Smith et al. 2008, Wang et al. 2014). Thus, some sporadic attempts have been made to apply rigorous methods, but

even these attempts have fallen flat because most scholars have neglected to account for important sources of bias, casting doubt on their findings.

2.4. Summary and context for the present work

In sum, archaeological research into past human-environment dynamics has improved in two ways since Morlot's day, but lagged behind in another. One improvement is that archaeologists, and other scientists studying the distant past, have access to unprecedented levels of detail about past environments. Numerous proxies are available with new ones surfacing regularly, all used ingeniously to infer past environmental conditions (Bradley 2013). New palaeoenvironmental records based on these proxies are published frequently and many datasets are warehoused online in freely available, easily accessible formats (e.g., http://www.ncdc.noaa.gov/data-access/paleoclimatology-data/datasets). The data are quantitative, increasingly precise, and often have high temporal resolution.

The other improvement is that archaeologists are thinking about causality in a more sophisticated way than ever before. Monocausal explanations are giving way to multi-causal models that often include feedback, tipping points, social and cultural context, and resilience.

Analytical developments, in contrast, have not kept pace. Archaeologists are still largely using a narrative approach to understanding how past societies responded to climate changes. Attempts to quantify the human-environment relationship have been piecemeal, properly addressing some aspects of a given analysis while ignoring others. The quantitative studies have also largely failed to address temporal autocorrelation and chronological uncertainty, two crucial idiosyncrasies of archaeological and palaeoenvironmental data. Thus, we have not been using the available data to its greatest advantage for testing the many hypotheses and sophisticated causal models about human-environment interaction that have arisen over the last century.

Clearly, there is a need for analytical development in relation to the use of archaeological and palaeoenvironmental data in the study of human-environment

interaction. We need to develop methods that are designed to deal with the idiosyncrasies of our data and use them to get away from the now century old narrative approach to understanding past human-environment interaction. Essentially, we need a new toolkit for studying past human-environment dynamics.

This dissertation lays the groundwork for developing that toolkit. It is the first step toward closing the gap between the newly available data and the newly developed theories. Hopefully, this work will allow us to eventually test those theories with the data available, leading to further theoretical developments and a better understanding of past human-environment interaction—indeed a better understanding of human-environment dynamics in general. In the next chapter, I will demonstrate the need for new tools with a case study involving drought and the Classic Maya.

Chapter 3. A reassessment of the impact of drought cycles on the Classic Maya

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Statement of Contributions of Joint Authors

Carleton, W. (candidate): research design; data collection; data analysis; co-wrote the manuscript.

Campbell, D. (committee member): research design; supervised data analysis; co-wrote the manuscript.

Collard, M. (senior supervisor): overall supervision; research design; co-wrote the manuscript.

This Chapter is the accepted version of the journal paper referred to above prior to any copy-editing, formatting, or typesetting by the journal.

3.1. Abstract

The study reported here challenges the widely discussed hypothesis that cyclical droughts had a major impact on the Classic Maya. This hypothesis was developed by Hodell et al. (Hodell et al. 2001, 2005a) on the basis of the results of time-series analyses of cores from Lake Chichancanab in the Yucatán peninsula. The analyses indicated that the Maya region was affected by two drought cycles during the 1st millennium CE, one with a periodicity of 208 years and another with a periodicity of 50 years. The timing of the droughts was such, Hodell et al. argued, that they were likely

responsible for several important socio-political events, including the collapse of Classic Maya society. In our study, we investigated two potentially important problems with Hodell et al.'s analyses: their use of interpolation to make their data regularly spaced, and their reliance on radiocarbon point estimates to generate age-depth models. We found that interpolation biased Hodell et al.'s results and that when it is avoided there is no evidence for a 208-year drought cycle in the Lake Chichancanab dataset. We also found that when the errors associated with the relevant radiocarbon dates are taken into account, there is no evidence for any drought cycles in the Lake Chichancanab dataset. Together, our analyses indicate that both the 208-year drought cycle and the 50-year drought cycle identified by Hodell et al. are methodological artifacts. The corollary of this is that the drought cycle hypothesis lacks an empirical basis and needs to be treated with skepticism.

3.2. Introduction

This paper reports a reassessment of an influential hypothesis concerning the impact of climate change on Classic Maya society. The traditional territory of the Maya-speaking people is located close to the middle of the isthmian portion of the North American continent (Figure 3-1). Mayanists usually divide this area into three loosely defined regions (Sharer & Traxler 2006). The Highlands is formed by the Chiapas highlands of Mexico and the elevated part of Guatemala. The Southern Lowlands consists of the southern portions of the Mexican states of Campeche, Quintana Roo, the Petén of northern Guatemala, and Belize. The Northern Lowlands comprises the rest of the Yucatán Peninsula. The Classic period of Maya history began around 250 CE and ended about 900 CE (Sharer & Traxler 2006). Conventionally, the Classic period of Maya history is divided into the Early Classic (ca. 250-600 CE), Late Classic (ca. 600-800 CE), and Terminal Classic (ca. 800-900 CE) (Sharer & Traxler 2006).

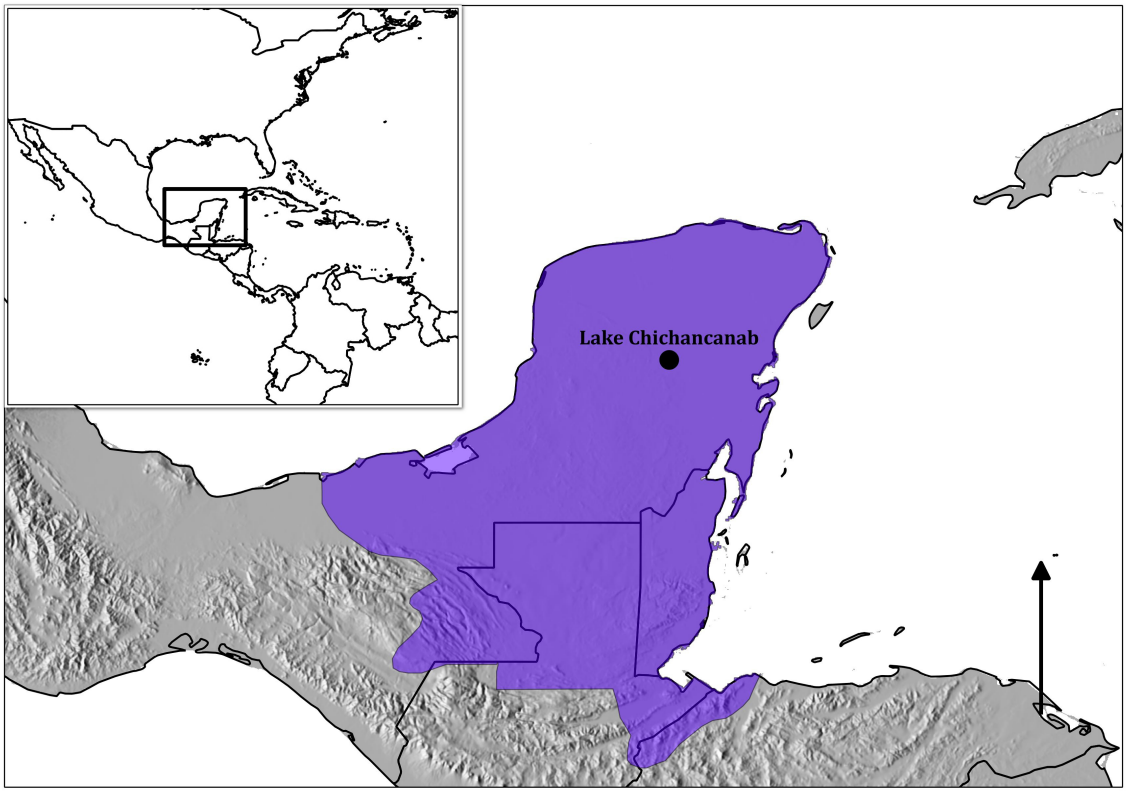

Figure 3-1 Map of Study Area

The Classic Maya have attracted the interest of archaeologists, art historians, epigraphers, and linguists for several reasons. First, their socioeconomic system was among the most complex in prehispanic North America. They engaged in intensive agriculture, specialized craft production, and long-distance trade, and they lived in city-states ruled by divine kings (Coe 2011). Classic Maya city-states normally comprised several civic-ceremonial centers and a large number of villages that were connected by a road network and, in some cases, causeways (Chase & Chase 2001). Second, the material culture of the Classic Maya is unusually rich. They constructed large stone step-pyramids, built elaborate temples and palaces, and erected ornately carved stone stelae (Coe 2011). They also created high-quality polychrome pottery, intricate jade funerary masks, and fine lithic artifacts, including a range of what seem to be primarily aesthetic or ceremonial objects (Coe 2011). Third, the Classic Maya had one of the few well-developed writing systems in the Americas (Houston et al. 2001). Their writing has been deciphered, and the texts and inscriptions that have been translated provide an often

remarkably detailed history of political events, conflict, and mythology (Martin & Grube 2008). Lastly, the Classic Maya developed a sophisticated system of calendars based on celestial movements (Rice 2007).

The hypothesis we tested concerns the impact of drought on the Classic Maya. There is a long tradition of invoking drought as a cause of the disappearance of the distinctive traditions of the Classic Maya between 900 and 1100 CE—an event that is often referred to as the "Classic Maya collapse" (Demarest et al. 2004, Gunn et al. 2002). Today, most Mayanists accept that drought was involved in the collapse, but opinions differ about the number of droughts involved, and the causal relationship between drought and collapse (Aimers 2007, Iannone et al. 2013, Turner & Sabloff 2012). Some authors have argued that the Maya region was subject to a series of intense droughts that placed stress on resources, rapidly lowering the carrying capacity of the environment (Haug et al. 2003, Kennett et al. 2012). The reduction in carrying capacity caused a decrease in population from starvation and migration to less-affected areas, and this in turn led to the decline of the most affected lowland cities. Others have argued that a "mega-drought" was responsible for the collapse (e.g., Faust 2001, Gill 2000). First outlined by Gill (2000), this hypothesis posits that between 800 and 1000 CE the Maya lowlands were affected by a severe drought that peaked around 922 CE. The great length and severity of the mega-drought brought about thirst, famine, and disease, killing the majority of the lowland Maya. Still other authors have argued that drought's role in the collapse was mediated by ideological change (e.g., Lucero 2002, Lucero et al. 2011, Moyes et al. 2009). One of the obligations of the divine kings was to ensure good harvests by correctly performing rituals and currying favor with supernatural forces. Another of their obligations was to maintain a certain level of peace and prosperity for their subjects. When the droughts occurred, crops failed and water stores were depleted leading to food stress and increased conflict between polities. Consequently, the system of divine kingship was perceived to have failed, leading people to abandon it along with other Classic Maya traditions. Lastly, some authors have placed more emphasis on environmental mismanagement by the Maya, which made Classic Maya society unsustainable and less resilient to the effects of drought (e.g., Culbert 1973, Diamond 2005, Dunning et al. 2012, Iannone et al. 2013). According to these models, the Classic Maya expanded into marginally productive areas to cope with population increase. The

expansion involved clear-cutting and more intensive agricultural practices, which caused soil erosion and nutrient loss. Then, when drought occurred, the already fragile, unsustainable agricultural system could no longer support the population and consequently society collapsed.

The hypothesis we tested also posits that drought caused the collapse of Classic Maya society, but differs from the foregoing hypotheses in that it views the collapse as only one of a number of sociopolitical events that were caused by drought. Developed over the last 20 years by David A. Hodell and his collaborators (Hodell et al. 1995, 2001, 2005a; Yaeger & Hodell 2008) on the basis of results of analyses of sediment cores from lakes in the Yucatán peninsula, the hypothesis contends that the Maya region was subject to two drought cycles during the 1st millennium CE. The primary cycle was driven by solar activity, and had a periodicity of around 208 years. Droughts in this cycle caused the site abandonments that preceded the emergence of the Classic Maya at 250 CE, the temporary decline of the important centre of Tikal around 670 CE, and the collapse of Classic Maya society between 900 and 1100 CE. The second drought cycle had a periodicity of about 50 years. These higher-frequency droughts governed the tempo and pattern of the collapse. The collapse began in the Southern Lowlands with the onset of drought conditions around 900CE, ebbed for roughly 50 years when a drought was skipped, and then continued in the Northern Lowlands as the 50-year drought cycle reengaged.

The drought cycle hypothesis has been influential. It has not only affected thinking about the Classic Maya (e.g., Dunning et al. 2002, 2012; Haug et al. 2003, Lucero et al. 2011, Masson 2012, Turner & Sabloff 2012, Wahl et al. 2006) but also influenced discussion about the impact of climate change on the sustainability of current human social, economic, and political systems (e.g., de Menocal 2001, Diaz & Trouet 2014). However, it is possible that it has been accepted too readily. The reason for this is that the analyses that indicated that the Maya region was subject to cyclical droughts during the 1st millennium CE are potentially problematic.

The analyses in question involved applying a time-series method to sediment density data from the bed of a large lake in the Yucátan peninsula named Lake

Chichancanab (Hodell et al., 2001, 2005). Hodell et al. (2001, 2005) used variation in the sediment density as a proxy for variation in the ratio of evaporation to precipitation. The rationale for this was that sediment density reflects the ratio of evaporation to precipitation because calcium and sulfate ions precipitate out as the mineral gypsum when the water level drops, and gypsum is much denser than the organic matter that usually forms the bulk of lake-bed sediment. In order to assign calendar ages to their measurements of sediment density, Hodell et al. (2001, 2005) used age-depth models based on radiocarbon dates. In the 2001 analysis, they found evidence of several drought cycles in the sediment density time-series. The most important of these had a periodicity of 208 years at the 95% confidence level. The other significant peaks identified in the analysis were at 50 years and 39 years. The results of the 2005 analysis differed somewhat from the results of the 2001 analysis. The 2005 analysis identified significant peaks at 213, 50, and 27 years rather than at 208, 50, and 39 years. But Hodell et al. (2005) argued that the differences were not meaningful.

There are two main potential problems with Hodell et al.'s (2001, 2005) analyses. One of these concerns a procedure that was necessitated by their choice of time-series analysis technique. The method of time-series analysis Hodell et al. (2001, 2005) used is usually referred to as the Blackman-Tukey (BT) method after its developers, Ralph Blackman and John Tukey (1958). The BT method is a parametric, frequency domain time-series analysis technique that is designed to find periodic functions (Blackman & Tukey 1958). The BT method is effective with data that are regularly sampled (Kay 1988). However, it cannot be applied to time-series that contain irregular inter-observation times (Chatfield 2009). Because the Lake Chichancanab time-series, like most palaeoenvironmental time-series, are irregularly sampled, Hodell et al. (2001, 2005) had to interpolate the data prior to analyzing them with the BT method. This is a problem because interpolation has been shown to artificially increase autocorrelation in time-series (Horowitz 1974, Levy & Dezhbakhsh 1994, Rehfeld et al. 2011), and autocorrelation is what the BT method uses to identify periodic components in time-series. Thus, it is possible that the signal of cyclical drought identified by Hodell et al. (2001, 2005) is an artifact of interpolation rather a real feature of the data.

Hodell et al.'s (2001, 2005) treatment of the radiocarbon dates obtained from the Lake Chichancanab cores is the other main reason to be skeptical of their claim to have found evidence of the occurrence of major droughts every 208 years and smaller but still damaging droughts every 50 years. The age-depth models they created were based on point-estimates of calibrated radiocarbon date distributions. Although this approach is common in palaeoclimate studies, it is flawed (Telford et al. 2004b). Point estimates are inadequate descriptors of calibrated radiocarbon date distributions because the latter are typically multimodal and highly irregular (Parnell et al. 2011). Any single point estimate of such a distribution will fail to adequately describe the true calendar date represented by the radiocarbon assay. In fact, multiple calendar dates may be similarly probable because of the multimodal nature of the calibrated radiocarbon date distributions. As a result, multiple age-depth models are possible for any radiocarbon-dated time-series. Any single age-depth model is, therefore, only one possible estimate of the true, unknown temporal structure of a given time-series. In effect, the time-series could be compressed or expanded in time by using different highly probable age-depth models to define its temporal structure. Neglecting this uncertainty has the potential to result in biased estimates of the true temporal structure of a radiocarbon-dated time-series. Since it is the temporal structure of the series that time-series methods are designed to study, the bias could greatly affect the results of a time-series analysis. Thus, it is possible that the periodicity identified by Hodell et al. (2001, 2005) in their drought proxy time-series is also an artifact of their treatment of radiocarbon dates.

Given how influential the drought cycle hypothesis has been, there is a need to determine whether or not the foregoing concerns are valid. With that in mind, we carried out a three-part study involving the dataset from Lake Chichancanab that is publicly available. In the first part of the study, we reanalyzed the dataset with Hodell et al.'s (2001, 2005) research protocol to ensure that the dataset was suitable for evaluating the impact of Hodell et al.'s (2001, 2005) methodological choices. In the second part of the study, we investigated the effect of interpolation on Hodell et al.'s (2001, 2005) results. In the final part of the study, we evaluated the impact of Hodell et al.'s (2001, 2005) failure to account for radiocarbon date errors on their results. Together, the analyses show conclusively that the findings underpinning the drought cycle hypothesis—that

droughts occurred every 208 and 50 years in the Maya region during the 1st millennium CE—are methodological artifacts.

3.3. Replication of Hodell et al.'s (2001, 2005) analyses

As only one of the datasets analyzed by Hodell et al. (2001, 2005) has been made publicly available, it was necessary to begin by assessing its suitability for evaluating the impact of Hodell et al.'s (2001, 2005) methodological choices on their results. We accomplished this by reanalyzing the dataset in question with the research protocol that Hodell et al. (2001, 2005) employed, and comparing the significant peaks we obtained with the significant peaks they reported.

The dataset that Hodell and colleagues have released consists of a time-series from a core from Lake Chichancanab that is designated CH1 7-III-04 (see Figure 3-2). The time-series consists of sediment density measurements, which, as explained earlier, are thought to reflect changes in gypsum concentration and therefore changes in precipitation. The measurements were taken at 0.5 cm intervals along the core between 4.5 cm and 286.5 cm in depth, resulting in a total of 564 data points. However, following Hodell et al. (2005) methods, we only considered the 99 points between approximately 120 and 170 cm depth, which corresponds roughly to the time leading up to and including the Classic Maya collapse. Each point has a calendar date derived from Hodell et al.'s (2005) age-depth model, which was based on a regression of the median calibrated radiocarbon dates of 15 AMS assays on the depth of the carbon samples. The dataset was obtained from the website of the National Oceanic and Atmospheric Administration (www.ncdc.noaa.gov).

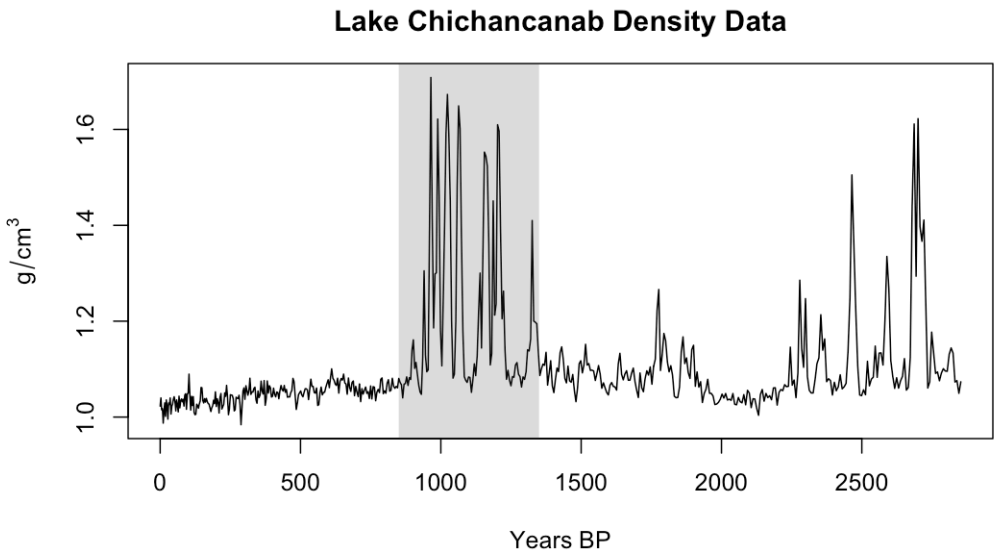

Figure 3-2 Density time-series from Lake Chichancanab (CH17-III-04).

Note The gray area shows the section of the series that was analyzed.

In line with Hodell et al.'s (2001, 2005) description of their methods, we used the BT method to derive a power spectrum and identify periodic functions in the sediment density data. A power spectrum is a function that describes the contribution of different periodic components to the total variance in a signal—peaks in the spectrum denote the frequencies of potentially significant periodic signal components. To begin, following Hodell et al. (2005), we removed a weak linear trend from the series by subtracting a straight-line function that was fit to the data by least squares. Linear de-trending is common in frequency-based time-series analyses because it eliminates unimportant variation when searching for periodicity. Subsequently, a Bartlett window that incorporated a third of the series was used in the calculation of the autocorrelation function. Lastly, we tested the power spectrum for significant peaks by comparing it to the power spectrum of a random, white noise time-series. Because palaeoenvironmental data usually contain some degree of background autocorrelation, comparison with a red noise spectrum rather than a white noise spectrum has been recommended (Mann and Lees, 1996). However, Hodell et al. (2001, 2005) compared their empirical power spectra with a white noise spectrum, so we used a white noise spectrum in our comparison. A significance level of 95% was used to identify significant peaks. We

carried out this set of analyses in the kSpectra software package (www.spectraworks.com) because it has greater significance testing functionality than the software that Hodell et al. (2001, 2005) used, Analyseries (Paillard et al., 1996). We did, however, replicate the analyses with Analyseries to ensure that the spectra the programs produced were the same. The results of the two sets of analyses were identical.

The significant peaks in our BT power spectrum were similar to the peaks of the mean spectrum presented in Hodell et al. (2005). The peaks we obtained correspond to periods of approximately 232, 46, and 25 years. Specific differences between the peaks we identified and those Hodell et al. (2001, 2005) found at 213, 50, and 27 years are minor. They are likely accounted for by the fact that we had access to only one of the four time-series Hodell et al. (2005) used to obtain an average spectrum. The close similarity between our results and those obtained by Hodell et al. (2005) indicates that the CH1 7-III-04 core data is suitable for evaluating Hodell et al.'s (2001, 2005) methodological choices.

3.4. Evaluation of the impact of interpolation on Hodell et al.'s (2001, 2005) results

In the second part of the study, we investigated the effect of the interpolation step in Hodell et al.'s (2001, 2005) research protocol. We did this in two ways. First, we applied the method Hodell et al. (2001, 2005) used to a simulated time-series that was created in such a way that we could be sure it did not contain any periodic components. Subsequently, we reanalyzed the gypsum concentration time-series data from the CH1 7-III-04 core with a method of time-series analysis that does not require regularly spaced data, and therefore does not require irregularly-spaced data to be interpolated prior to analysis.

The simulated time-series we created is based on white noise. By definition, such a time-series contains no periodic components. The series contained 100 observations with a mean of zero and a standard deviation of one. The length of the simulated series approximately mirrors the length of the section of the Chichancanab series analyzed by

Hodell et al. (2005). The observation times for the random series were also generated randomly. Beginning with an observation time of zero for the first observation in the time-series, each observation time was then generated by incrementing the previous observation time by a random value drawn from a log-normal distribution with a mean of five and a standard deviation of one. This process created the effect of monotonic increasing irregular inter-observation times. Next, five experiments were conducted in which portions of the white noise series comprising from 10 to 50% of the total number of points were removed. Each subsample was interpolated and resampled at regular intervals. To search the eight interpolated series for periodicity, the power spectrum for each simulated series was estimated using the BT method implemented as per Hodell et al.'s (2001, 2005) description of their analyses. We then compared the results of these experiments to the power spectrum of an evenly spaced white noise signal of the same length, which is what would be expected if interpolation had no impact on the BT power spectrum.

Figure 3-3 shows the relationship between low-frequency autocorrelation and the percentage of the simulated series that is derived from linear interpolation after some portion of it was randomly removed. Increasing the percentage of a randomly generated series that is derived from interpolation increases low-frequency autocorrelation. The autocorrelation functions also become more sinusoidal as greater percentages of the white noise series are interpolated (see the bottom panel of Figure 3-3). Figure 4 shows the effect of this increasing autocorrelation on the BT power spectrum. Using the BT method for transforming an artificially inflated autocorrelation function into a power spectrum resulted in spurious peaks, primarily in the low-frequency end of the spectrum where the power becomes concentrated (Figure 3-4). These spectra decline exponentially toward the high-frequency end of the spectrum. In contrast, the spectrum of the evenly spaced white noise series is relatively flat, which indicates that power is evenly distributed between frequencies. If a white noise spectrum were used as the benchmark for identifying statistically significant peaks, the spurious peaks in the spectra of the interpolated simulation series would be considered significant. This demonstrates that interpolation effectively inflates the BT method's Type I error rate, i.e. the rate of obtaining false positive results. Thus, our simulation demonstrates that interpolation of the kind employed by Hodell et al. (2001, 2005) does indeed increase autocorrelation

and the Type I error rate, and supports the idea that the periodicity identified by Hodell et al. (2001, 2005) may have been artificially imposed by interpolation.

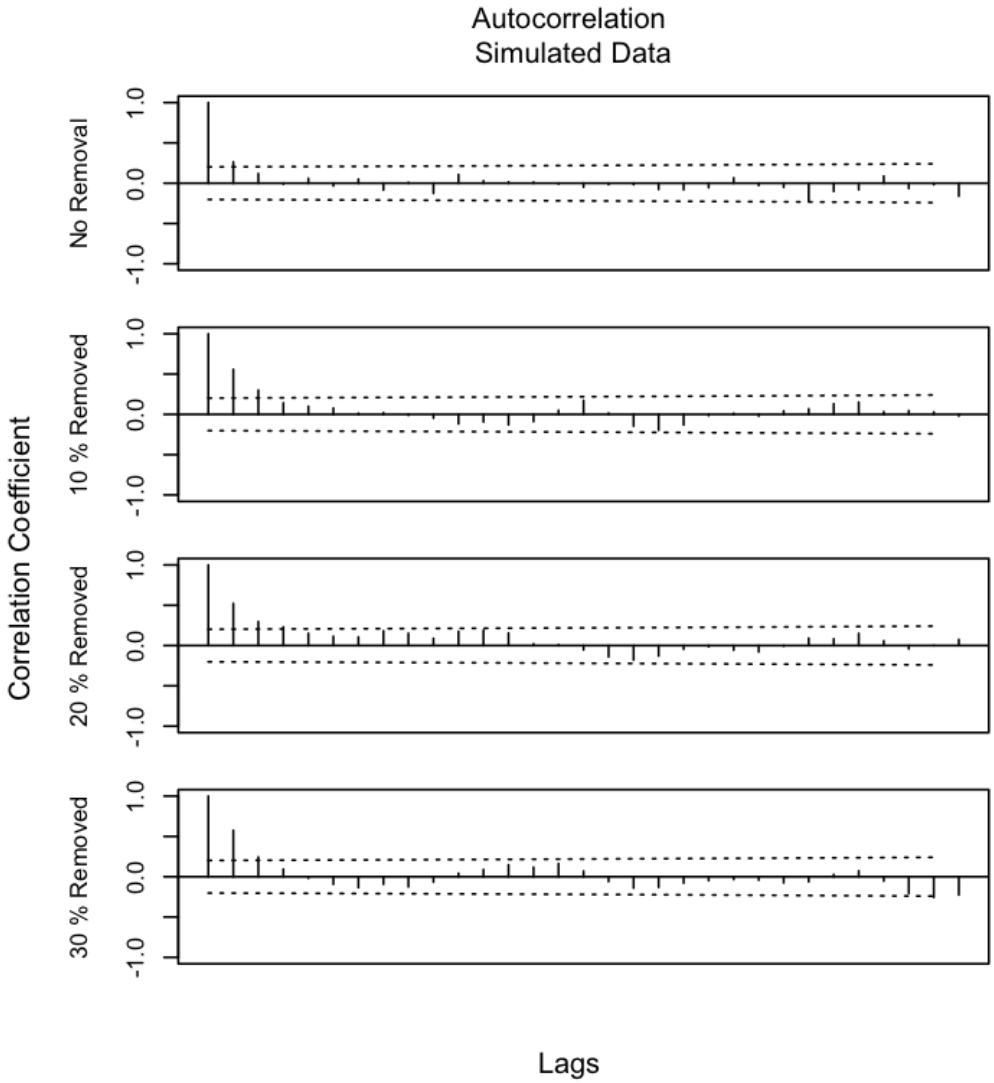

Figure 3-3 Simulated autocorrelation functions

Note Sample of the results of the white noise simulation demonstrating the effect of incrementally increasing the percentage of the series derived from interpolation on the autocorrelation function. The autocorrelation function describes the correlation between a series and itself at different lags. The top panel shows the autocorrelation function of an evenly spaced white noise process for comparison and the dashed lines indicate theoretical 95% confidence levels. Any vertical lines that are above the dashed lines indicate statistically significant correlations when the series is compared to itself after being shifted by a given lag distance. Since the underlying process used to generate these white noise time-series is random, there should be no significant correlations between a series and itself at any lag beyond the first (a series will correlate perfectly with itself if it has not been shifted by a lag). What this sample of results shows is that the autocorrelation function increases for low lag distances as the simulated series is subjected to greater amounts of interpolation.

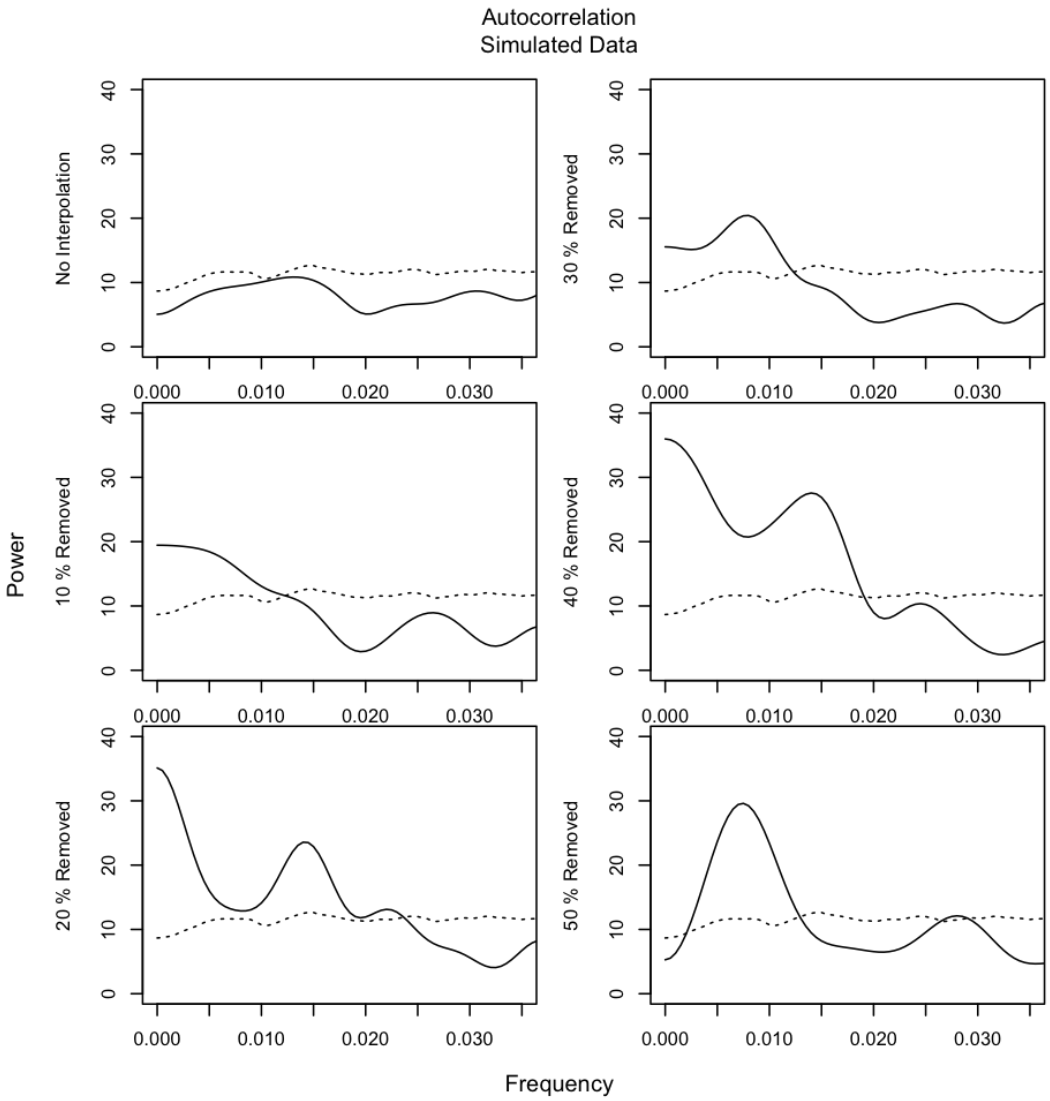

Figure 3-4 Autocorrelation simulation power spectra

Note Sample of the results of the white noise simulation demonstrating the effect of incrementally increasing the percentage of the series derived from interpolation on the BT estimate of the power spectrum. The top left panel shows the power spectrum of an evenly spaced white noise process for comparison. The dashed lines indicate the 95% confidence level of the BT spectrum for an evenly spaced white noise process, estimated from 100 bootstrap iterations. As with the autocorrelation functions in Fig. 3, no obvious or statistically significant features should be visible in the spectrum of a white noise process. What this sample of results shows is that increasing the percentage of a series that is subject to interpolation increases the size and distinctiveness of features in the BT spectral estimates.

The alternative method of time-series analysis we used to reanalyze the sediment density time-series data from the CH1 7-III-04 core is called Least Squares Spectral Analysis (LSSA) (Vaníček, 1971). LSSA differs from other frequency-based methods of time-series analysis in that it does not rely on autocorrelation functions or Fourier transforms. Instead, it uses the least squares principle to sequentially fit sinusoidal functions of incremental frequencies to a time-series. This means that LSSA does not require regularly spaced data and, therefore, can handle irregularly spaced time-series without interpolation.

The LSSA analysis involved two steps. First, we iterated through a set of evenly spaced frequencies fitting a sinusoid by least-squares to the time-series. Each sinusoid was subtracted from the time-series before the next fit was performed. This procedure removed the variation caused by components at each frequency, thereby partially mitigating what is often called “spectral leakage”. Spectral leakage occurs when a periodic component of the underlying signal generating process lies between frequencies that were assessed by least-squares, resulting in partial fits by sinusoids of nearby frequencies.

In the second step of the analysis, we searched for significant peaks in the LS-spectrum. To do this, we compared it to both a white noise LS-spectrum and a red noise LS-spectrum. We used two null hypothesis spectra because, as explained earlier, there is a difference between the procedure for identifying significant peaks employed by Hodell et al. (2001, 2005) and the currently recommended best practice. To reiterate, Hodell et al. (2001, 2005) identified significant peaks in their empirical spectra by comparing them to white noise spectra, whereas the currently recommended best practice is to use red noise spectra to identify significant peaks in the spectra of palaeoenvironmental data. Red noise spectra are preferable for significance testing because they reduce the potential for the background autocorrelation often contained in palaeoenvironmental datasets to give rise to false-positive results in the low-frequency range (Mann & Lees 1996). To run the two tests, we simulated ensembles of white and red noise time-series that contained no other periodic functions and calculated their LS-spectra. Each ensemble contained 5000 simulated time-series. The white noise time-series were calculated by drawing from a normal distribution with mean and variance

equal to those of the section of the Chichancanab time-series. The red noise time-series were generated following the methods outlined in Schulz and Mudelsee (2002). Again, each simulated time-series had the same observation times as the original Chichancanab series. We then compared the Chichancanab LS-spectrum to the 95th percentile of the distributions of simulated white and red noise LS-spectra. Peaks in the Chichancanab spectrum that were higher than the 95% levels of the simulated LS-spectra were considered statistically significant.

Figure 3-5 shows the results of comparing the LS-spectrum of the Chichancanab series to the LS- spectrum of a white noise process at the 95% confidence level. Using the white noise null spectrum, we identified 5 significant peaks centered at 492, 250, 167, 63, and 46 years. Only the 46-year cycle in the LS-spectrum corresponds roughly with a peak from Hodell et al.'s (2001) analyses, namely their putative 50-year cycle. However, the 46-year cycle appears significant compared to white noise at the 95% confidence level whereas it only appeared significant at the 80% level in Hodell et al. (2001). More notably, the 208-year drought cycle is absent in the LS-spectra. These results suggest that the 208-year drought cycle identified by Hodell et al. (2001, 2005) was a spurious peak caused by their interpolation procedure.

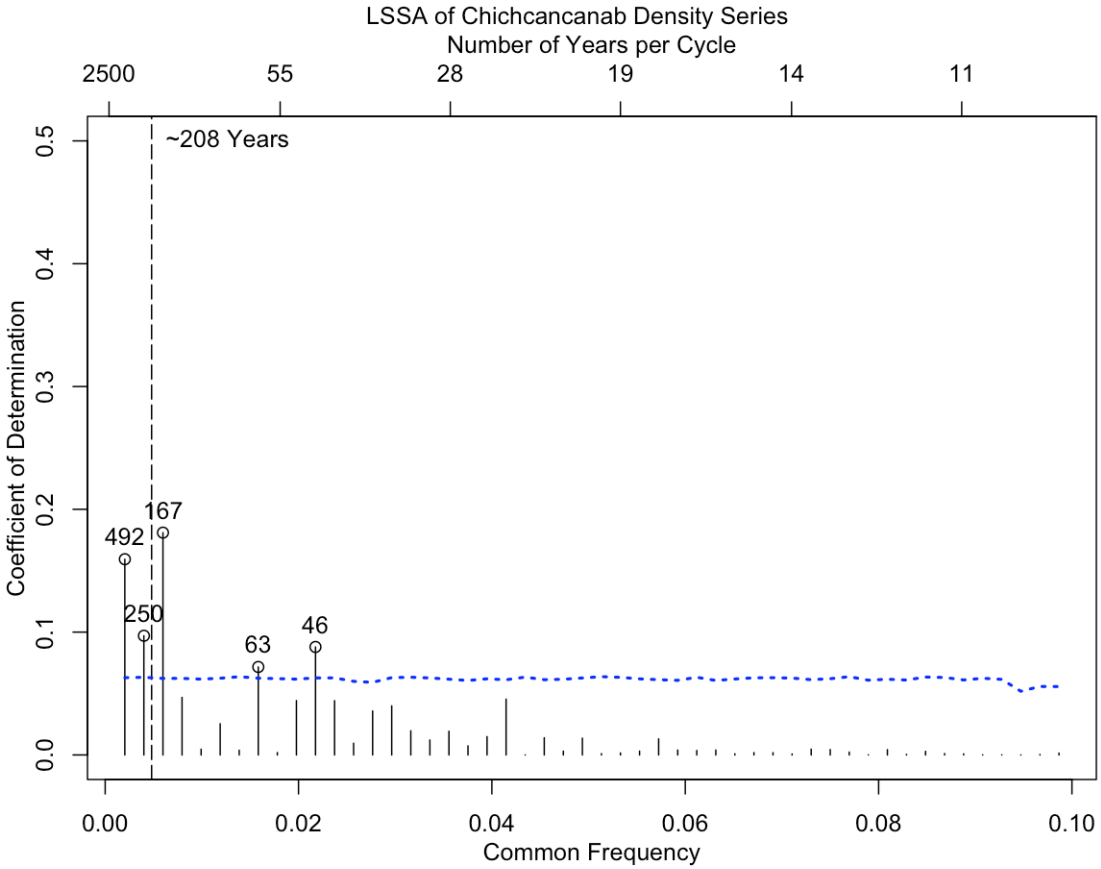

Figure 3-5 LSSA Spectrum of Chichancanab density series

Note LSSA spectrum of the Lake Chichcancanab density time-series compared to the LSSA spectrum of a white noise process. Vertical lines in the LS spectrum denote the frequencies of the sinusoids that were fit to the series and their heights indicate the coefficient of determination of the regression. Higher vertical lines indicate frequencies for which the fits between the relevant sinusoids and the series are better. These higher vertical lines, or 'peaks', in the LS spectrum indicate potential cyclical components in the series. The short-dash line shows the 95% level of the null hypothesis spectrum. Peaks that are higher than the short-dash line are statistically significantdthese peaks have been circled and labeled with the length of the relevant period (in years). The vertical long-dash line shows where a 208-year peak would be, if it were a feature of the time-series.

Figure 3-6 shows the results of comparing the LS-spectrum of the Chichancanab series to the LS- spectrum of a red noise process at the 95% confidence level. Using the red noise null spectrum, the LSSA indicated that the only statistically significant peaks in the Chichancanab spectrum were at approximately 46- and 24-years. The peaks at 492, 250, and 167 years identified in the white noise comparison were not significant when

the red noise null spectrum was employed. Thus, the red noise comparison also suggests that the 208-year drought cycle identified by Hodell et al. (2001, 2005) was a spurious peak caused by their interpolation procedure.

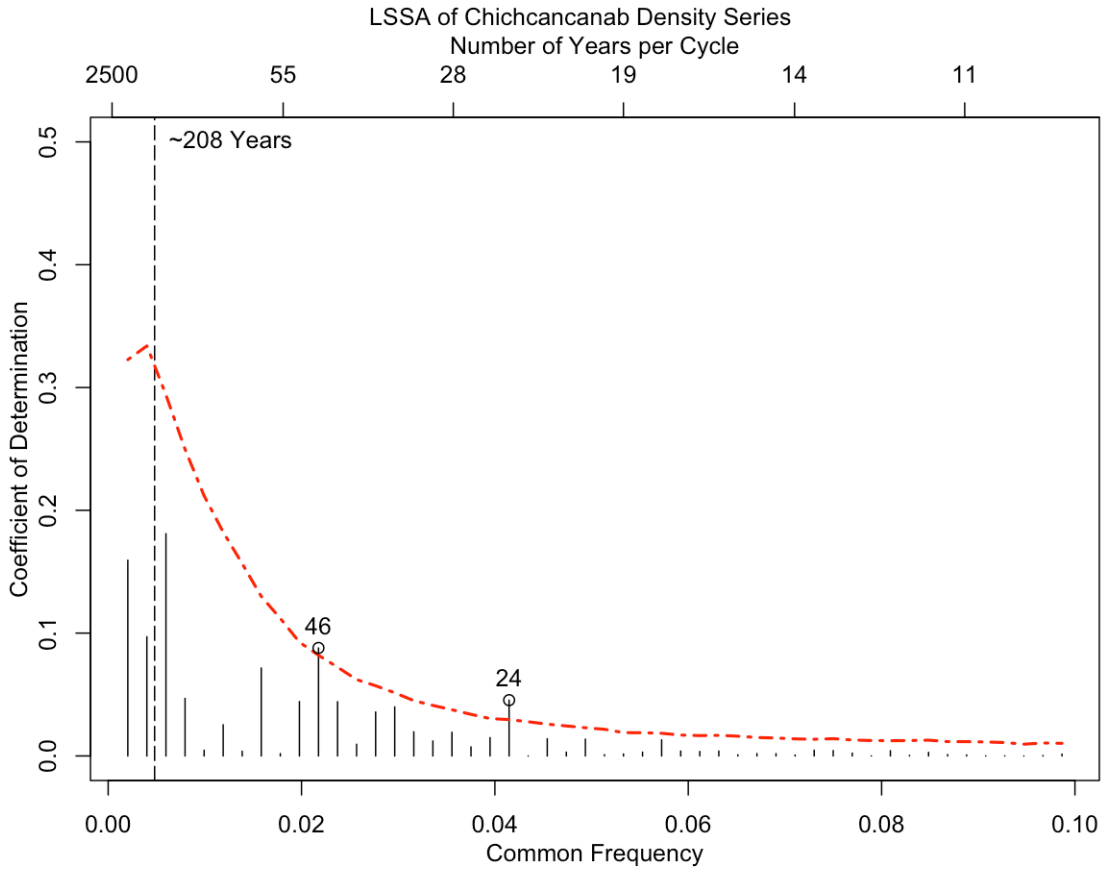

Figure 3-6 LSSA spectrum of Chichancanab density series with red noise null

Note LSSA spectrum of the Lake Chichcancanab density time-series compared to the LSSA spectrum of a red noise process. Vertical lines in the LS spectrum denote the frequencies of the sinusoids that were fit to the series and their heights indicate the coefficient of determination of the regression. Higher vertical lines indicate frequencies for which the fits between the relevant sinusoids and the series are better. These higher vertical lines, or 'peaks', in the LS spectrum indicate potential cyclical components in the series. The shortdash line shows the 95% level of the null hypothesis spectrum. Peaks in the LS-spectrum that are higher than the short-dash line are statistically significantdthese peaks have been circled and labeled with the length of the relevant period (in years). The vertical long-dash line shows where a 208-year peak would be, if it were a feature of the time-series.

Taken together, our simulation and LSSA analyses show that Hodell et al.'s (2001, 2005) findings were indeed greatly affected by interpolation. Hodell et al.'s 50-

year drought cycle does not seem to be a product of interpolation, but their 208-year drought cycle, which they thought to be considerably more important than the 50-year drought cycle, is clearly an artifact of interpolation.

3.5. Evaluation of impact of radiocarbon date errors on Hodell et al.'s (2001, 2005) results

In the last part of the study, we evaluated the impact of Hodell et al.'s (2001, 2005) failure to account for radiocarbon date errors. We accomplished this by reanalyzing the gypsum concentration time-series data from Hodell et al.'s CH1 7-III-04 core with LSSA in combination with a bootstrap simulation to account for error in the calibrated radiocarbon dates. The simulation was necessary because the calibration process used to convert radiocarbon dates into calendar dates produces multimodal posterior probability distributions, so their errors cannot be modeled analytically (see Appendix B).

We began by calibrating the AMS dates reported by Hodell et al. (2005) in OxCal (Ramsey & Lee 2013) with the INTCAL09 (Reimer et al. 2009) curve. Next, the calibrated date distributions (i.e., multimodal posterior probability density functions) were resampled within one standard deviation of their means—the sampling occurred non-uniformly with replacement in accordance with the relative probabilities of each calendar date specified by the calibrated radiocarbon date distributions. If a sample of dates violated the stratigraphic relationships of the carbon samples, it was discarded and a new sample was drawn. Then, a new age-depth model was created for each sample of dates by using a monotonic polynomial function in R (R Core Team 2016). The new age-depth models were used to create an ensemble of 5000 simulated time-series. Following Hodell et al. (2005), only the sections of the simulated time-series dating to between 600 and 1200 CE were used in further analysis so that the results would be comparable. Each of the 5000 simulated series was then analyzed using LSSA, allowing us to explore the effect of the true chronological error of the age-depth model on a frequency-based analysis. As in the previous analysis, statistical significance was assessed using both white and red noise LS spectra derived from a bootstrap simulation—an additional 5000 iterations were used to find the 95th percentile of the white and red noise LS-

spectra for each simulated series. Adjusting the confidence levels for multiple comparisons would not have been straightforward because of spectral leakage between frequencies. Consequently, they were not adjusted and the values should be viewed as point-wise estimates that constitute a best-case scenario for supporting Hodell et al.'s (2005) findings. The analyses were performed in R (see Appendix B for code) and run on Westgrid's Bugaboo High Performance Computing Cluster (www.westgrid.ca).

The LSSA–bootstrap simulations found no significant periodicity in the gypsum concentration time-series data. Figures 3-7 and 3-8 show the proportion of the simulation over which a signal component with a given frequency was significant compared to white and red noise null spectra respectively at the 95th percentile of confidence. Neither the red nor white noise tests identified signal components with frequencies that correspond to a 208-year period. Both simulations identified statistically significant peaks corresponding to periods of roughly 50 years, but in less than 20% of the simulated LS-spectra when compared to white noise, and less than 10% when compared to red noise. Other peaks were identified as statistically significant in the simulations (see Figures 3-7 and 3-8), but they occurred even less frequently. Thus, once calibrated radiocarbon date error is taken into account, there is no strong evidence for Hodell et al.'s 208-year drought cycle or for their 50-year drought cycle. Indeed, there is no strong evidence for any periodicity in the sediment density time-series at all.

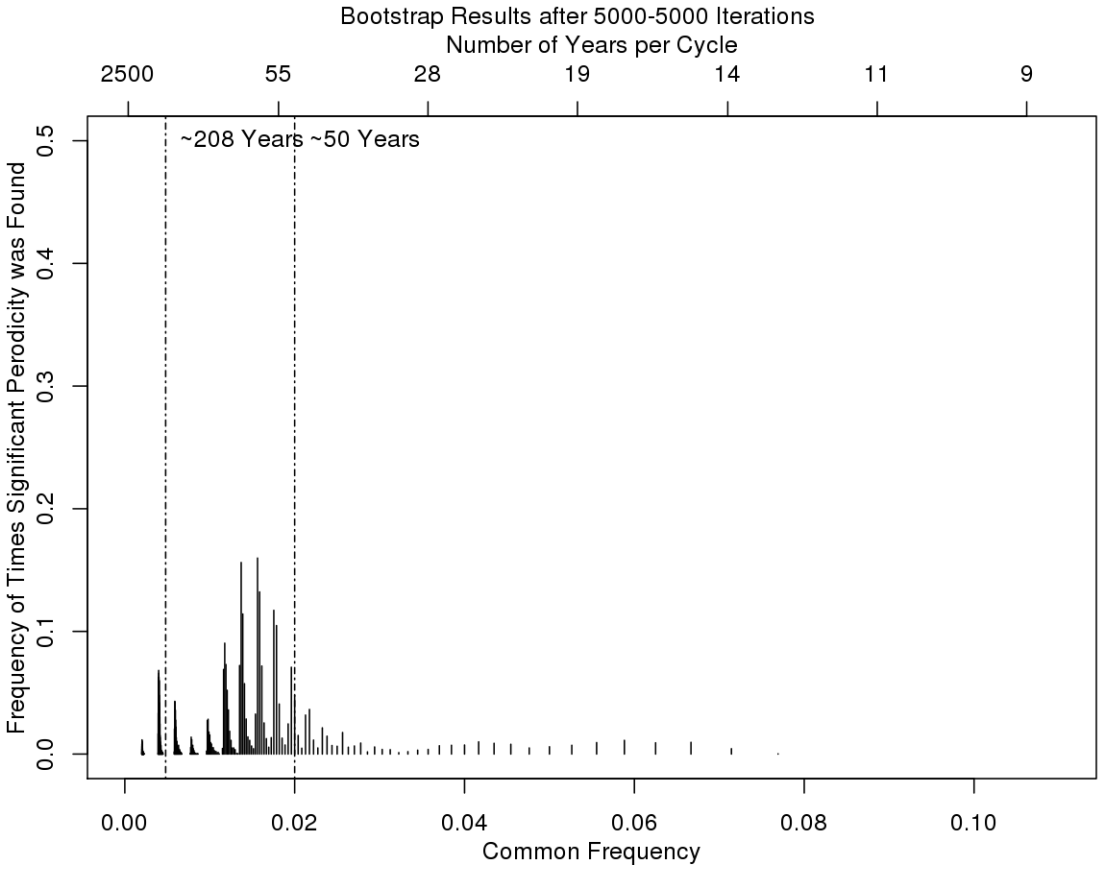

Figure 3-7 Bootstrap results with white noise null

Note Results of the bootstrap simulation when the Chichancnab density series LS-spectrum was compared to a white noise null spectrum. The vertical long-dash lines show where a 208-year peak and a 50-year peak would be, if they were features of the time-series. Unlike the LS-spectrum results in Fig. 3-5 and 3-6, the peaks in this plot show only the percentage of the simulation runs that identified a particular frequency (shown on the x-axis) as statistically significant. It does not show the strength of the correlation between the relevant sinusoid and the series, only the relative likelihood that a given frequency is significant despite chronological error.

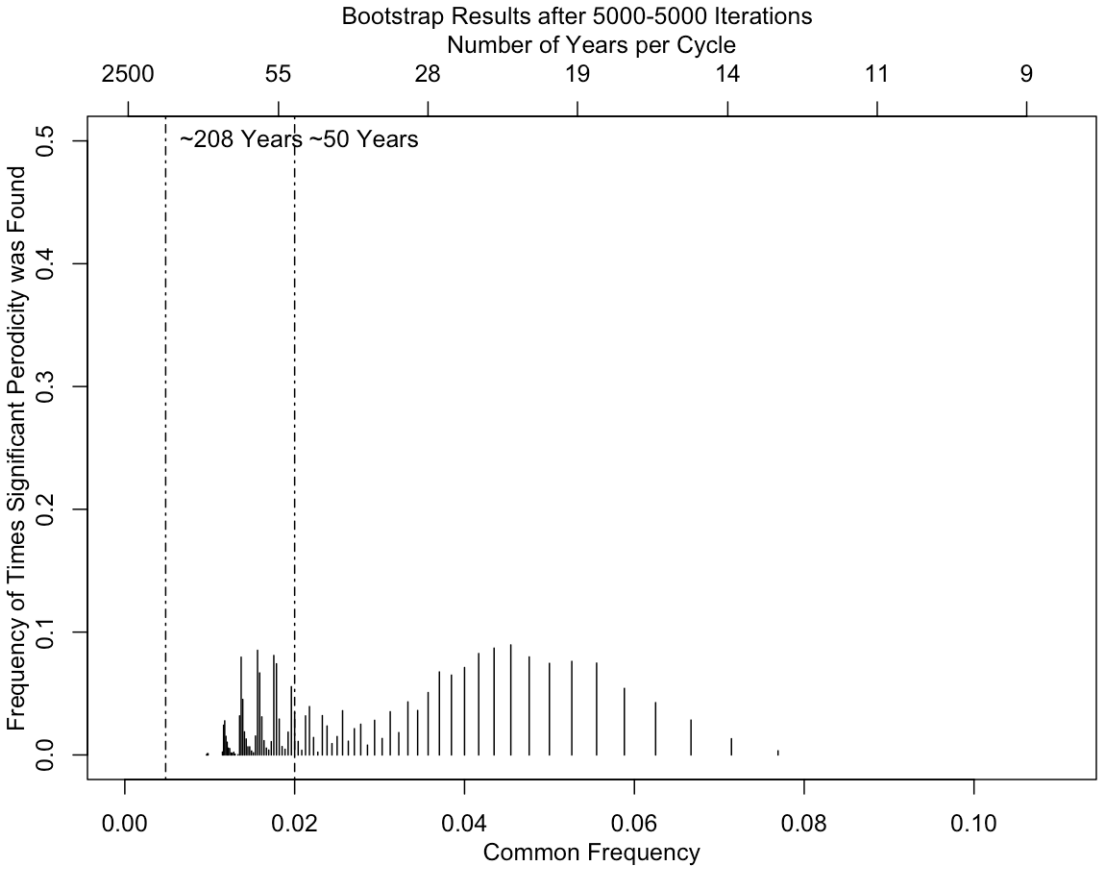

Figure 3-8 Bootstrap results with red noise null

Note Results of the bootstrap simulation when the Chichancanab density series LS-spectrum was compared to a red noise null spectrum. The vertical long-dash lines show where a 208-year peak and a 50-year peak would be, if they were features of the time-series. Unlike the LS-spectrum results in Fig. 5 and 6, the peaks in this plot show only the percentage of the simulation runs that identified a particular frequency (shown on the x-axis) as statistically significant. It does not show the strength of the correlation between the relevant sinusoid and the series, only the relative likelihood that a given frequency is significant despite chronological error.

3.6. Discussion

Our study casts doubt on Hodell et al.'s (2001, 2005) claims regarding the impact of cyclical droughts on the Classic Maya. Our first set of analyses confirms that the sediment density data derived from Hodell et al.'s (2001, 2005) CH1 7-III-04 core are suitable for assessing the impact of their choice of time-series analysis method and their failure to take into account the errors associated with the radiocarbon assays they used

to generate their time-depth models. Our second set of analyses suggest that the interpolation step in Hodell et al.'s (2001, 2005) research protocol inflated low-frequency periodicity in the sediment density data power spectra and caused the method of time-series Hodell et al. (2001, 2005) employed to identify false peaks at 208 years. Our third set of analyses suggests that Hodell et al.'s (2001, 2005) core sediment density data do not contain any peaks when the data are not interpolated and the errors associated with the radiocarbon assays are dealt with appropriately. Thus, our analyses do not support the existence of a 208-year drought cycle or a 50-year drought cycle in the Maya region during the 1st millennium CE. Consequently, they also do not support the hypothesis that periodic droughts repeatedly caused important sociopolitical events among the Classic Maya.

Hodell et al. (2001, 2005) are not alone in suggesting that drought cycles were an important influence on prehispanic Maya history. A number of authors have argued that Maya society went through repeated cycles of growth, regional integration, decline, and disintegration during the 1st millennium CE, and suggested that periodic severe droughts were a major factor in these cycles (Dunning et al. 2012, Gill et al. 2007, Masson 2012, Turner & Sabloff 2012). Our study does not speak to the existence or otherwise of recurring sociopolitical cycles in Maya history. However, it casts doubt on the idea that such cycles were driven by drought cycles. Major droughts that affected the whole Maya lowlands may have occurred during the 1st millennium CE, as Hodell et al. (1995, 2001, 2005) and other researchers have suggested (e.g., Curtis et al. 1996, Haug et al. 2003, Kennett et al. 2012). But our analyses indicate that, if such droughts did occur, they did not do so with a regular periodicity. A corollary of this is that, if there were sociopolitical cycles in prehispanic Maya history, the primary driver must have been something other than drought cycles.

The results of our study also have some important implications for future work in archaeology and palaeoenvironmental studies involving the analysis of time-series. One concerns interpolation. Many types of archaeological or palaeoenvironmental proxy time-series are sedimentary in nature (Gornitz 2009), like Hodell et al's (2005) lakebed cores, and they will almost always be irregularly sampled because of natural variation and taphonomic processes. Our study shows that interpolating such time-series can create

methodological artifacts, and that these can lead to misinterpretation of the time-series. Therefore, our study suggests that, when analyzing archaeological or palaeoenvironmental time-series data, methods designed to handle irregular inter-observation times directly should be used (e.g., Bretthorst 2003, Lomb 1976, Schulz & Stattegger 1997, Vaníček 1971, Zechmeister & Kürster 2009).

Equally importantly, our study demonstrates that dating error must be taken into account when analyzing archaeological and palaeoenvironmental time-series that are dated with radiocarbon assays. Many archaeological and palaeoenvironmental studies rely on calibrated radiocarbon assays to date time-series. As discussed earlier, the calibration procedure produces date distributions that are often highly irregular and multimodal, which means that the probability distributions cannot be adequately described by point estimates. Our study demonstrates that frequency-based analyses are greatly affected by irregular temporal errors. This is because periodic functions of different frequency will fit the time-series data better or worse depending on which point estimates are used. The effect would also be important for studies that attempt to correlate palaeoenvironmental and archaeological data. In such cases, the problem with chronological uncertainty may be compounded when two or more time-series are involved. Chronological uncertainty is often cited as a problem for such work (e.g., Aimers & Hodell 2011, Bryson 1994, Caseldine & Turney 2010, Hodell et al. 2007, Iannone et al. 2013), but its effects have never been explored empirically before. Our results show that ignoring temporal uncertainty can introduce significant statistical bias into time-series analyses and result in specious conclusions about palaeoclimate systems and their effects on human societies. Until an analytical solution to this problem is developed, irregular temporal errors can be accounted for by a simulation-based approach, like the one we used.

A third, somewhat less obvious issue that our study raises is the importance of using the correct null-spectra when searching power spectra for significant peaks. In our second and third sets of analyses we compared the data from Lake Chichancanab to both white noise and red noise spectra in order to identify significant peaks. As we explained earlier, we used two null spectra because the current recommended best practice differs from the way Hodell et al. (2001, 2005) tested for significant peaks. To

reiterate, the current recommended best practice is to compare palaeoenvironmental spectra to a red noise spectrum (Mann & Lees 1996) whereas Hodell et al. (2001, 2005) compared the Lake Chichancanab data to a white noise spectrum. It is clear from our analyses that the choice of null spectrum can greatly affect the results of time-series analyses carried out to identify periodic functions. The peaks identified by the white noise test were concentrated toward the low-frequency end of the spectrum, whereas the peaks identified by the red noise test were in the high-end. Thus, the selection of null spectrum can influence which set of significant peaks that are identified and where in the spectrum they are more likely to occur. If the wrong one is selected, peaks may be incorrectly identified as significant—specifically, incorrectly specifying a white noise null spectrum will cause low-frequency peaks to be spuriously identified as significant, and it will likely cause potentially significant high-frequency peaks to be missed altogether. As others have pointed out, climate time-series will naturally contain low-frequency autocorrelation (Mann & Lees 1996). This background autocorrelation is a result of the similarity between observations in a time-series that is due entirely to temporal proximity—the amount of rainfall today is expected to be similar to the amount of rainfall tomorrow, for example. When transformed into a power spectrum, this autocorrelation creates a distribution that declines exponentially with increasing frequency so that there is always more power—i.e., higher peaks—in the low-frequency end of the spectrum. As a result, low-frequency peaks are to be expected and should not be considered significant unless the power of the peak is sufficiently high that it stands out against the background autocorrelation. Thus, assuming that peaks which are higher than those expected from a completely random, uncorrelated series—i.e., a white noise null hypothesis—ignores the nature of the climate processes that created the observations. In such cases, where autocorrelation is expected because of the nature of the underlying processes, an appropriate null hypothesis should account for those expectations—i.e., a red noise null hypothesis. Without setting such a benchmark for identifying significant peaks, any relatively high peaks in the spectrum could be arbitrarily selected leading to spurious causal inferences, as was the case with Hodell et al.'s (2001, 2005) analyses. Many archaeological time-series can also be expected to contain low-frequency autocorrelation, and should also be compared to a red noise null hypothesis.

3.7. Conclusions

In the study reported here, we re-evaluated the empirical basis of the widely discussed hypothesis that cyclical droughts played a major role in Classic Maya history, causing several important events, including the disappearance of the Classic Maya's distinctive traditions between 900 and 1100 CE. Hodell et al. (2001, 2005) developed this hypothesis on the basis of time-series analyses of lake-cores. These analyses suggested that the Maya region was affected by two drought cycles during the 1st millennium CE, one with a periodicity of 208 years and another with a periodicity of 50 years.

Our study was motivated by two concerns about Hodell et al.'s (2001, 2005) analyses. One was that, because the method of time-series analysis they employed requires regularly-spaced data, they interpolated their data prior to analysis. This is potentially problematic because interpolation is known to introduce low-frequency periodicity in time-series data by artificially increasing autocorrelation. The other cause for concern is that Hodell et al. relied on radiocarbon date means to generate time-depth models, and radiocarbon date means are not necessarily the best estimates of dated events due to the multimodal nature of most radiocarbon date errors.

Our study had three parts. In the first, we replicated Hodell et al.'s (2001, 2005) results using data from their 2005 study. In the second part of the study, we examined the effects of interpolation through a simulation-based analysis and a reanalysis of Hodell et al.'s Chichancanab data using a method of time-series analysis that does not require regularly-spaced data. In the third part of the study, we used a bootstrap-based resampling procedure to investigate the effects of ignoring the dating error.

Our exploration of the effects of interpolation clearly show that the 208-year drought cycle Hodell et al. identified is an artifact of interpolation. The results of our assessment of the effects of ignoring the dating error are equally decisive. They return no evidence of drought cycles in the Maya region during the 1st millennium CE, which indicates that the 50-year drought cycle identified by Hodell et al. is an artifact of their reliance on point estimates of calibrated radiocarbon date distributions.

Given that both of the putative drought cycles appear to be methodological artifacts, and the Chichancanab data contains no other significant periodicities, our results have obvious implications for current thinking about the role played by cyclical drought in Classic Maya history. Clearly, it cannot be argued that drought periodicity was the cause of anything in the vicinity of Lake Chichancanab during the 1st millennium CE, since there is no evidence for such periodicity. The corollary of this is that discussions of Classic Maya history that invoke drought cycles to explain sociopolitical events should be viewed with scepticism (e.g., Dunning et al. 2012, Gill et al. 2007, Masson 2012, Turner & Sabloff 2012).

Our results also have important implications for future archaeological and palaeoenvironmental work involving time-series data. They indicate that we need to be more conscious of the idiosyncrasies of our data and the analytical decisions we make to cope with them. Most time-series of archaeological or palaeoenvironmental data can be expected to contain natural autocorrelation, irregular inter-observation times, and chronological uncertainty. All of these characteristics pose challenges for time-series analysis because they introduce biases and have the potential to generate spurious results. Future research needs to involve evaluations of their effect on time-series analyses, particularly the impact they have on uncertainty. We need to better understand the uncertainties involved in analyzing past human-environment interactions so that we can evaluate the level of confidence that should be given to our interpretations, especially if they could affect modern discourse about climate change.

3.8. Acknowledgements

We thank Jess Munson for her assistance with this paper. Our research would not have been possible without access to computational clusters and technical support provided by WestGrid (www.westgrid.ca) and Compute Canada (www.computecanada.ca). We are grateful to both institutions. We received financial support from the Social Sciences and Humanities Research of Canada (767-2011-0159 and 895-2011-1009), the Canada Research Chairs Program, the Canada Foundation for Innovation, the British Columbia Knowledge Development Fund, and Simon Fraser University.

Chapter 4. Increasing regional temperature exacerbated Classic Maya conflict over the long term

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Statement of Contributions of Joint Authors

Carleton, W. (candidate): research design; data collection; data analysis; co-wrote the manuscript.

Campbell, D. (committee member): research design; supervised data analysis; co-wrote the manuscript.

Collard, M. (senior supervisor): overall supervision; identified question; research design; co-wrote the manuscript.

This Chapter is the accepted version of the journal paper referred to above prior to any copy-editing, formatting, or typesetting by the journal.

4.1. Abstract

The impact of climate change on conflict is an important but controversial topic. Some researchers contend that climate change exacerbates conflict within and among human societies, while others have challenged the hypothesis on the grounds that the available evidence is problematic. One important issue that needs to be resolved is the impact of climate change on conflict over the long term. With this in mind, we

investigated the relationship between climate change and conflict among Classic Maya polities between 363 and 888 CE. We compiled a list of mentions of conflict carved into dated monuments, and then obtained a series of temperature and rainfall records for the region. Subsequently, we used a recently developed time-series method to investigate the impact of the climatic variables on the frequency of conflict while controlling for trends in monument number. We found that there was a substantial increase in conflict in the approximately 500 years covered by the dataset. This increase could not be explained by change in the amount of rainfall. In contrast, the increase was strongly associated with an increase in summer temperature. These finding have implications not only for Classic Maya history but also for the debate about the likely effects of contemporary anthropogenic climate change.

4.2. Introduction

Concern is growing among policy-makers that the current episode of anthropogenic climate change will increase conflict within and among human societies. For example, the European Commission recently advised that climate change will intensify social and political tensions, leading to more conflict (European Commission 2013). Similar warnings have been issued by the Intergovernmental Panel on Climate Change and the US Department of Defense in the last few years. The IPCC has cautioned that climate change will exacerbate conflict at a range of scales from personal violence to civil war (Adger et al. 2014), while the DoD has classified climate change as a threat multiplier, suggesting it could lead to political instability and increased terrorism (US Department of Defense 2014). However, a close examination of the scientific literature reveals that there is still uncertainty about the impact of climate change on conflict. The idea that climate change increases conflict levels has been supported by several studies (Hsiang & Burke 2014, Hsiang et al. 2013), but this body of work has been heavily criticized (e.g., Buhaug et al. 2014, Hsiang et al. 2014, Hsiang & Meng 2014, Meierding 2013, Salehyan 2008, 2014; Scheffran et al. 2012; Theisen et al. 2011, 2013). Consequently, at the moment it is not in fact clear that present and future global warming can be expected to lead to more conflict.

One important issue that requires clarification is the nature of relationship between climate change and conflict over the long term. A number of studies have compared historical conflict and climate records (Bai & Kung 2010, Jia 2014, Tol & Wagner 2010, Zhang et al. 2006, 2007b,a, 2010, 2011a), but these studies suffer from important methodological shortcomings. Very few of them use formal modeling techniques, while those that do employ formal modeling techniques utilize methods that are not well suited to analyzing time series count data, which casts doubt on the reliability of their results. In addition to these problems, work to date has focused solely on data from China and Europe. Consequently, convincing evidence for a worldwide relationship between climate change and conflict over the long term is currently lacking.

With this in mind, we carried out a quantitative analysis of the influence of climate change on conflict among the Classic Maya over a period of several hundred years. The Maya people occupy a region close to the middle of the isthmian portion of the North American continent (Fig. 1). The Classic period of Maya history began around 250 CE and ended between 900 and 1100 CE (Sharer & Traxler 2006). It is during the Classic period that the Maya constructed most of the extensive cities and massive pyramids that have made them famous. They also developed one of the few writing systems in the Americas (Houston et al. 2001) and began a tradition of recording historical events on stone monuments. The inscriptions that have been translated provide often remarkably detailed accounts of their myths and political events, including conflicts between city-states (Martin & Grube 2008).

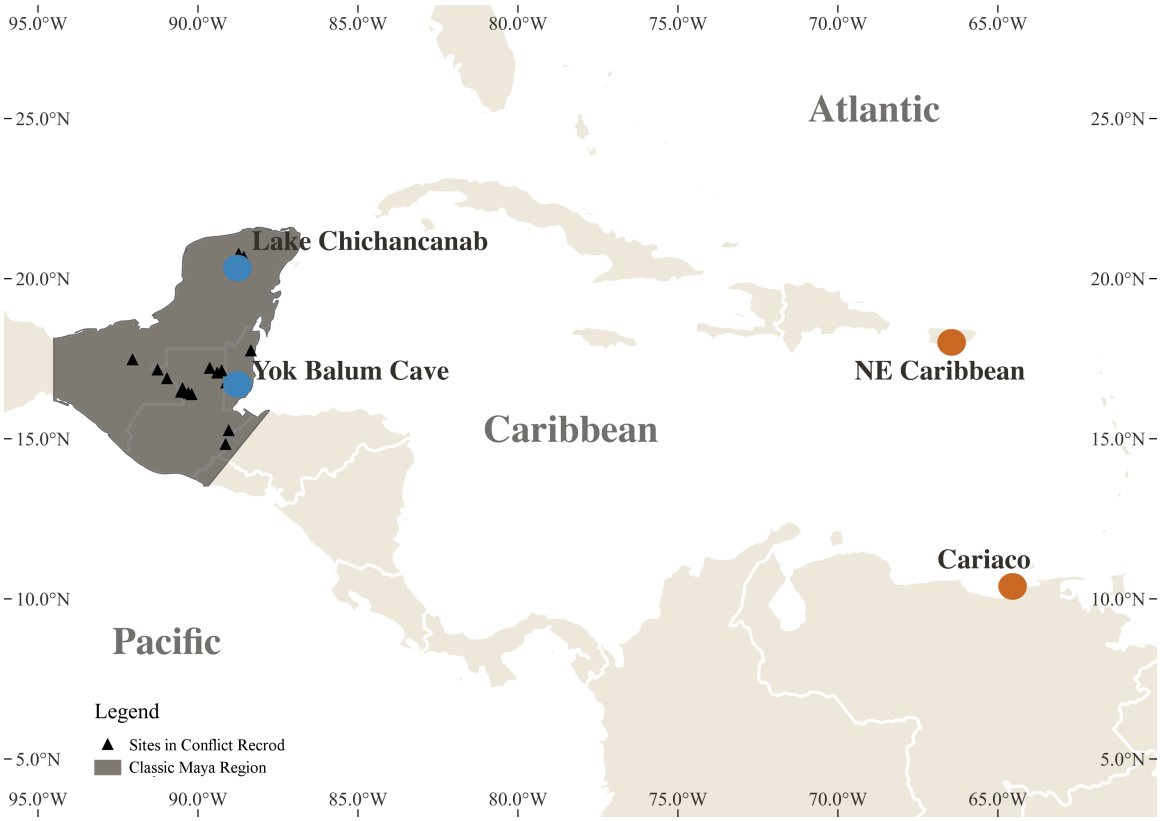

Figure 4-1 Map of study area

Note This map shows the Classic Maya region (shaded red-brown) and the source locations of the palaeoclimate proxy data (the rainfall proxies are blue and the temperature proxies are red-orange).

Inter-polity conflict was an important feature of Classic Maya life (Brown & Stanton 2003, Chase & Chase 2003a, Culbert 1991, Hassig 1992, Houston 1993, Inomata & Triadan 2009, Webster 2000). This is indicated by numerous mentions of conflicts between city-states in the epigraphic record and artwork depicting scenes of violence (e.g., Chase & Chase 1989, 2003a; Culbert 1991, Houston 1993, Miller 1986). For instance, epigraphers have identified a century-long power struggle between two of the major southern city-states, Tikal and Calakmul (Martin & Grube 2008). This struggle embroiled numerous Maya centers, and involved both direct confrontations and proxy conflicts between client polities (e.g., Martin 1993). Among the conflict events mentioned in the epigraphic record are demands for tribute, captive takings, human sacrifices, deliberate destruction of monuments and temples, and large coordinated attacks that

may have been timed to coincide with astronomical events and therefore are often called “star wars” (Webster 2000).

Scholars have long been interested in Classic Maya conflict, and a number of potential drivers have been proposed, including status rivalry, captive taking, resource acquisition, agricultural shortfalls, and drought (Webster 2000). To date, however, none of these factors has been shown to correspond to changes in past conflict levels through quantitative analysis. Recently, Kennett et al. (Kennett et al. 2012) argued that increasing dryness from 600-900 CE drove Classic Maya conflict, based on a comparison between conflict levels and an oxygen isotope rainfall proxy from Yok Balum Cave, Belize. Their argument, however, was based only on a visual comparison between the palaeoclimatic and conflict data, which means the association they identified between increasing dryness and conflict may be more apparent than real. This raises the question of whether evidence for the impact of climate change on Classic Maya conflict actually exists.

To assess whether Classic Maya conflict was driven by climate change, we compiled a time series of conflict levels from the Classic Maya historical record and then obtained published high-resolution palaeoclimate proxies for temperature and rainfall. Subsequently, we used a recently developed time series regression technique called the Poisson Exponentially Weighted Moving Average (PEWMA) method (Brandt et al. 2000) to construct a set of statistical models, each of which used a different climate proxy as a covariate and included monument numbers to control for the possibility that conflict trends reflected only trends in the number of erected monuments. Lastly, we compared the models to each other and to a null model without any climate variables.

4.3. Materials and Methods

The conflict time series we analyzed consists of 144 unique conflict events that are inscribed on Classic Maya monuments from more than 30 major Maya centres, described in dozens of scholarly works (see Fig. 4-2 and Appendix C). The inscriptions are mainly from sites in the Southern Maya Lowlands, a region formed by the southern portions of the Mexican states of Campeche, Quintana Roo, the Petén of northern

Guatemala, and Belize. Many of the conflict records were taken from Kennett et al.'s (2012) dataset, which itself was drawn from the Maya Hieroglyphic Database Project (http://mayadatabase.faculty.ucdavis.edu/). The remaining conflict records were identified in the course of a systematic search of literature (see Appendix C). The records describe specific historical events and are associated with Classic Maya calendar dates that are precise to the day in many cases. An illustrative example comes from an altar at Caracol, a large civic-ceremonial centre in southern Belize. It states that the ruler of Caracol "decapitates/attack holy Mutal ajaw [a lord connected to Tikal, another important centre]" in 820 CE (Kennett et al. 2012). We turned the 144 dated conflict events into a time series of conflict levels with a 25-year resolution by counting the number of events that occurred in each 25-year period spanning approximately 350-900 CE. The size of the interval was chosen to be consistent with previous work on Classic Maya conflict (e.g., Kennett et al. 2012), but we explored the effect of the interval size in a sensitivity analysis (see Appendix C).

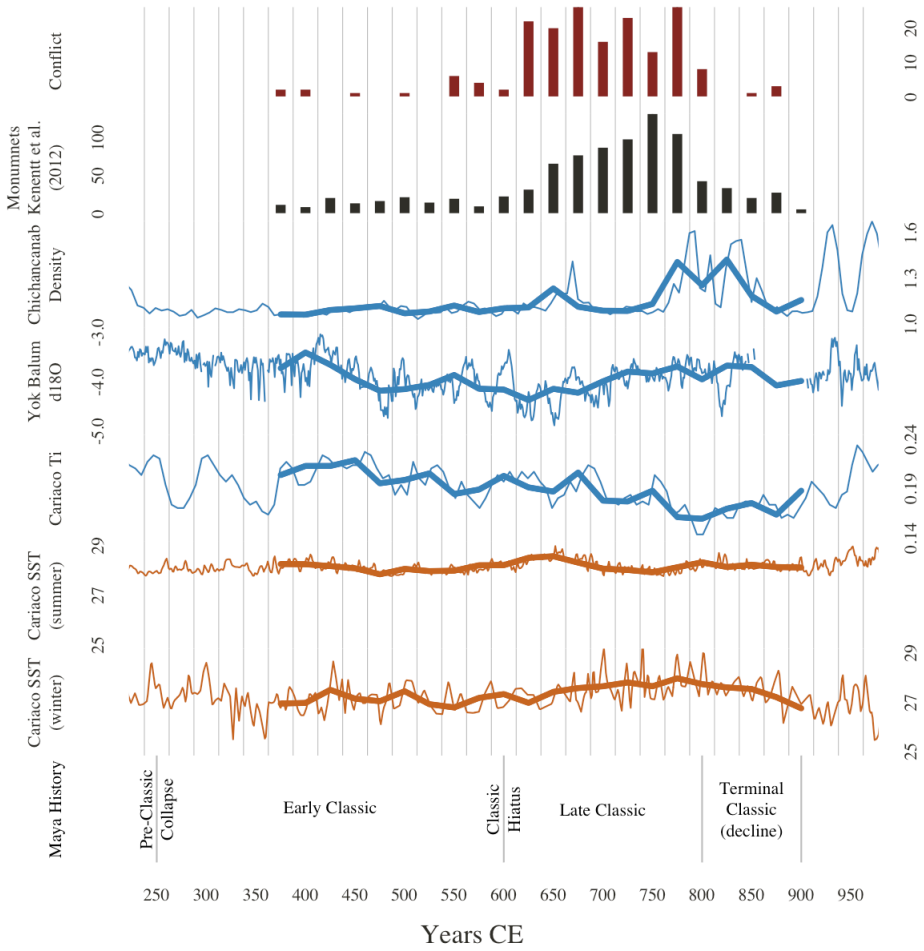

Figure 4-2 Data

Note Conflict and Climate Proxy Data (25-year resolution). The first row is conflict counts with a rug plot of conflict counts at 1-year resolution. The second row is monument counts taken from (Kennett et al. 2012). The next five rows are the climate records with the thick lines showing the data at 25-year resolution and the thin lines showing the raw palaeoclimate data. The last row shows the approximate boundaries of Classic Maya historical periods according to (Sharer & Traxler 2006).

Next, we created comparably-binned palaeoclimate time series from two sets of temperature records and three sets of rainfall records (See Fig. 4-2 and Appendix C). The nearest temperature records we could find with sufficient resolution are sea surface temperature (SST) reconstructions from the Cariaco Basin, including summer and winter estimates (Wurtzel et al. 2013). The rainfall data included the Yok Balum oxygen isotope record, a titanium concentration record from the Cariaco Basin, and a sediment density

record from Lake Chichcancanab (Haug et al. 2001, Hodell et al. 2005a, Kennett et al. 2012). Caution must be exercised when extrapolating palaeoclimatic reconstructions over large geographic areas because local conditions may not be strongly correlated with conditions farther afield. However, climatological processes in the Cariaco basin are known to be related to the conditions in Maya region (Bhattacharya et al. 2015, Haug et al. 2001, Knudsen et al. 2011) and our own comparisons of the Cariaco basin SST reconstructions with nearby temperature time-series suggest that the former adequately reflect a regional trend in temperature change throughout the Caribbean (see Appendix C). Including both temperature and rainfall records allowed us to simultaneously test the drought hypothesis and the hypothesis that increases in temperature drove Classic Maya conflict.

In the next step of the study, we compared the conflict record to the five palaeoclimate records using the PEWMA method, which is a Poisson regression technique (Brandt et al. 2000). Poisson regression was appropriate because it takes into account two important characteristics of conflict time series. The first is that conflict time series always comprise counts of positive integer numbers because there is no such thing as a fractional or negative conflict. The Poisson distribution is suitable for such data because it is discrete, meaning it has only integer-valued outcomes, and it can never be negative. The second characteristic of conflict time series that favours Poisson regression is that the time between conflicts often follows an exponential distribution (e.g., Helmbold 1998, Houweling & Siccama 1985, Mansfield 1988, Richardson 1944, 1960; Sarkees et al. 2003, Tang et al. 2010). This means that the average number of conflicts in a given span of time conforms to a Poisson distribution. Thus, a Poisson regression model is appropriate for estimating the mean conflict level while testing for the influence of covariates, such as climatic variation. We opted to use PEWMA over standard Poisson regression because it is designed to model autocorrelated count data. Looking at the conflict record, it was clear that the data contain autocorrelation, suggesting that there was “momentum” in Classic Maya conflict levels (see Fig. 3 and SI Fig. 1). We used the Akaike Information Criterion (AIC) to compare competing models, specified with different covariates (Akaike 2011). Models with lower AICs involve less information loss than those with higher AICs and, thus, are better approximations of the underlying process (Akaike 2011, Burnham & Anderson 2004, Kuha 2004).

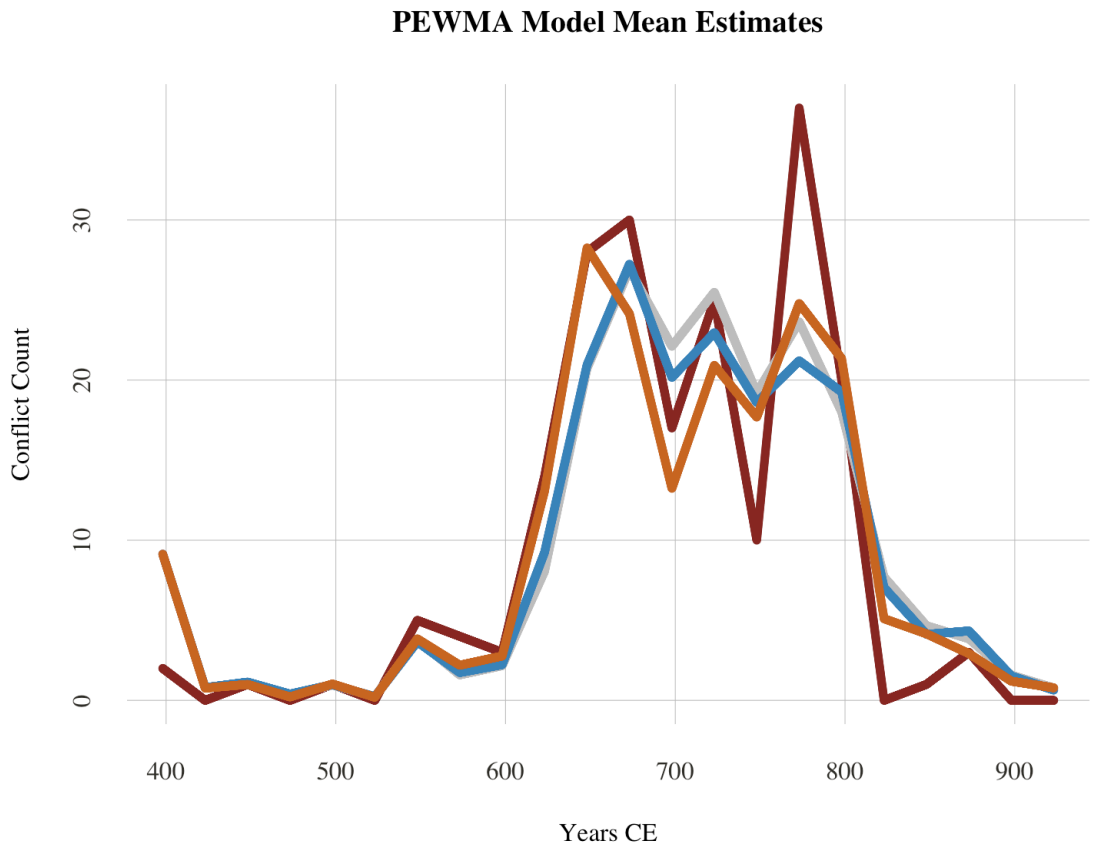

Figure 4-3 PEWMA model prediction results

Note PEWMA Model Predictions. This figure shows the PEWMA model predictions compared to the 25-year conflict count data. The 25-year conflict data is represented by the black line; the model predictions using the benchmark model are represented by the grey line; the predictions using the Yok Balum rainfall proxy are represented by the blue line; and the predictions using the Cariaco SST winter estimates are represented by the orange line.

In the regression analyses, we compared the rainfall and temperature models with each other and with a null model in which variation in conflict was not affected by changes in temperature or rainfall. We constructed the null model using just a constant and a time-series of monument numbers. As such, the null model allowed us to control for the possibility that the variation in conflict was only a function of previous conflict levels and/or of changes in monument use—i.e., that there was a change in the practice of inscribing monuments or the number of groups erecting monuments rather than a

change in the number of conflicts. We then estimated a separate model for each climate proxy, using the proxy as the predictor variable in the model along with the time-series of monument numbers and a constant. We reasoned that if reduced rainfall drove conflict, the models with rainfall proxy predictor variables would have the lowest AICs. Alternatively, if temperature drove conflict, then the models with temperature predictor variables would have the lowest AICs. For any of the climate predictors to be accepted as a potential driver of conflict, the model containing it had to substantially outperform the null model. To negate the potentially biasing effects of arbitrarily selecting bin edge locations, we ran the analysis 25 times, shifting the bin edge locations by +1 years each time. We also performed several additional sensitivity tests looking at how temporal bin widths affected our results and explored whether the increase in time alone could explain the increase in conflict levels (see Appendix C for details).

4.4. Results

The change in conflict levels between 350 and 900 CE was considerable. The number of conflicts increased from 0-3 every 25 years in the first two centuries to 24 conflicts every 25 years near the end of the period. This increase cannot be explained by change in the amount of rainfall. None of the rainfall proxies had AICs that were substantially and consistently lower than the null model (See Appendix C and Table 4-1), which indicates that variation in rainfall had no long-term effect on Classic Maya conflict. In contrast, the temporal variation in the number of conflicts might be explained by changes in temperature. One of the temperature proxies—the Cariaco basin winter SST—was no better than the null model in any analyses. But the model involving the Cariaco basin summer SST was consistently hundreds to thousands of times more likely than the null model, depending on where the bin edges were located (Appendix C). This finding indicates that the increase in conflict is best explained by a combination of past conflict levels, increasing numbers of monuments, and increasing summer temperatures. These results were robust to changes in data bin edge locations and width (Appendix C). In sum, our analyses indicate that summer temperature increases led to increased levels of Maya conflict during the Classic Period while rainfall variation, in contrast, had little or no effect (see Fig 4-3).

4.5. Discussion

Two issues need to be addressed before discussing the implications of our findings. First, we need to consider the possibility that the political nature of the epigraphic record biased the results of our study. Not surprisingly, propaganda was an important tool of the Classic Maya elite (Lucero 2003, Marcus 1974, Rice 2009, Sanchez 2005). One way they spread propaganda was by carving politically favourable inscriptions into monumental architecture. In these inscriptions, the Classic Maya elite told stories about their lineages, connections to the gods, and their conflicts with other elites in order to increase their political capital (Martin & Grube 2008). To improve the political effect of these narratives, they would sometimes leave events out of the record. Consequently, individual conflict events are often not corroborated at multiple locations, especially at the centres that lost a given conflict, unless recording it served the ruler's political narrative in the long run (Webster 2000). This means that the conflict record at any given centre is probably biased in favour of the elites at that centre. This in turn raises the possibility that the epigraphic record as a whole does not accurately reflect the variation in conflict levels.

While the political nature of the epigraphic record may be a problem when investigating some issues—e.g. the relationship between particular polities or conflict history at a solitary centre—we think it is unlikely to have negatively affected our results. This is because the use of propaganda can be expected to have been widespread among Classic Maya elites. Essentially, if every leader was recording events that favoured their own historical narrative, then these biases should counter balance each other when all conflict mentions are combined into a single record. While the leader of the losing side in a battle might have been motivated to ignore the loss, the leader of the winning side can be expected to have recorded it. Consequently, taking all recorded conflicts together should produce a relatively unbiased proxy for conflict levels. Thus, there is no reason to believe that the political nature of the conflict record is responsible for our key finding—that temperatures and conflict levels increase in a correlated manner through time.

The second issue we need to address is the possibility that the conflict record contains a temporal bias that favours the Late Classic. This potential bias has two possible sources. One involves our knowledge of the Classic period. As is often the case in archaeology, we have more information about younger periods than older ones. In part this is because younger deposits tend to cover older ones, making older material less visible and harder to find. This is especially true for the Classic Maya because Late Classic structures were often built over earlier ones, covering the earlier architecture. In addition, the epigraphic and architectural evidence indicates that the number of elites was increasing throughout the Classic Period, and this likely resulted in greater numbers of inscribed monuments being built during the Late Classic (Demarest et al. 2014, Fox et al. 1996, O'Manksy 2013). Taken together, these factors mean that we generally have more information about the Late Classic than the Early and Middle Classic. The other possible source of temporal bias involves conflict events at the beginning of the record. Some of the conflicts dated prior to about 550 CE are recorded on monuments that were erected many years after the events in question, raising some concern about the reliability of the record before that time (Webster 2000). It is possible that only the most prominent conflicts were remembered long enough to be recorded. This could have biased the record by making it look as if there were fewer conflicts in earlier periods than actually occurred. Together, these two potential sources of bias mean that the Late Classic may be over-represented in the epigraphic record, making it possible that the increase in conflict indicated by our dataset is an artifact.

However, there are reasons to think that our results were not in fact negatively affected by the temporal bias. To begin with, our models account for the bias by including the number of monuments as a covariate. The time-series of monument numbers can be expected to reflect the amount of archaeological information we have from the Classic period, including any over-representation of material dated to the Late Classic. If the latter were responsible for our results, then the increase in monument numbers would explain the increase in conflict entirely with no improvement from adding a temperature covariate. But this is not what we found. The models that included the temperature covariate outperformed all other models. While the increase in monument numbers and temporal autocorrelation in conflict levels explain some of the variation in conflict levels at any given time, adding the temperature covariate greatly improved the

models according to the AICs. In fact, the models that incorporated the temperature covariate were on average several thousand times more likely to explain conflict levels than the null model which included only a constant and the number of monuments (see Table 4-1). Thus, we think it is unlikely that overrepresentation of Late Classic conflict events in the epigraphic record accounts for our findings.

Table 4-1 Summary of PEWMA analysis results

Model Name	% of times a given model beat the benchmark	Median AIC weight-based comparison
benchmark	NA	NA
Yok_d18O	0	1
Chich_Dens	36	19.3
Cariaco_Ti	52	172.1
Cariaco_ssts	100	545490.4
Cariaco_sstw	16	10.6

Note. This table contains a summary of the results of the PEWMA analyses obtained after shifting the temporal bins 25 times by +1 year each time. The second column indicates the percentage of the 25 analyses that a given model outperformed its benchmark (i.e., the model with only a constant and the time-series of monument numbers). The third column indicates the median of the AIC-based likelihoods for each model computed for each of the 25 analyses—the median was used to account for outliers in a small sample. These values indicate how many times more likely a given model is than the benchmark—*i.e,* a given model is 'x' times more likely than the benchmark to explain conflict levels, where 'x' is the value in the third column.

That our key result is not an artifact of a temporal bias in the conflict record is further supported by the fact that several lines of archaeological evidence indicate conflict increased from the Middle to Late Classic. One of these lines of evidence is fortifications. The Late Classic is notable for defensive walls at several civic-ceremonial centers and for settlements located on highly strategic terrain, such as hilltops and elevated areas with commanding views of the surrounding landscape. Examples include Mayapan, Chunchucmil, Dos Pilas, and Punta De Chimino, and Aguateca, all of which contain well-known evidence of Late Classic fortification (Borgstede & Mathieu 2007, Dahlin 2000, Demarest et al. 1997, Rice & Rice 1981, Russell 2013, Webster 1976, 1978, 2000). At Chunchucmil, for example, a rubble wall was erected during the Late

Classic that runs over top of other Classic period architecture (Dahlin 2000). The rubble was robbed from nearby buildings, suggesting the wall was urgently constructed in response to a novel impending threat. Weaponry is a second line of evidence that indicates that conflict increased from the Middle to Late Classic. Aoyama (2005) conducted a typological and microscopic use-wear analysis of stone spear, dart, and arrow points at Aguateca, a site near Copan in western Honduras that is famous for its evidence of Classic period conflict. He concluded that the points were often used as weapons and that the proportion of the lithic artifacts classifiable as weapons increased during the Classic period, indicating an increase in conflict levels. According to Ayoama, his lithic analysis corroborates the archaeological and epigraphic evidence from the region around Copan, which also point to increasing levels of conflict through time. The last line of evidence involves direct evidence of violence in the form of destruction and violent death (e.g., Barrett & Scherer 2005, Demarest et al. 2016). For example, at Cancuen located in northern Guatemala, Demarest and colleagues discovered a mass grave containing the massacred bodies of an entire royal court dated to the Late Classic (Demarest et al. 2016). They also discovered unfinished defensive walls, scatters of spear and dart points, and evidence for rapidly abandoned buildings. Evidence of this sort is rare, but illustrates severe conflict toward the end of the Classic period, especially in the Peten and Pasion regions of northern Guatemala. Together, these various lines of archaeological evidence support the idea that the trend we observe in the epigraphic data reflects a real trend toward increasing conflict levels.

It appears, then, that the increase in conflict events in the epigraphic record reflects a real increase in Classic Maya conflict levels. Thus, the relationship we identified between conflict and temperature is not the product of biases in the conflict record. Consequently, we can now consider the implications of our findings.

The literature on climate change and conflict suggests there are two potential mechanisms by which the increase in temperature could have led to greater conflict (e.g., Anderson 2001, Hsiang & Burke 2014, Salehyan 2014, Van Lange et al. 2016). One is psychological. Several recent studies have found evidence that regional heat waves coincide with waves of violent crime (Anderson 2001, Hsiang & Burke 2014, Hsiang et al. 2013, e.g., Van Lange et al. 2016). This relationship between increasing

temperature and interpersonal violence has been argued to be the result of a psychological link between heat and aggression because there is no connection between crime-related economic gains and increased temperature (Anderson 2001). The possibility that there is a psychological link between temperature and violence is also supported by a study that found that baseball pitchers target the bodies of batters more often when it is hot than when it is cold, and by a study that discovered that car-drivers use their horns more in hot weather than in cold weather (Kenrick & MacFarlane 1986, Larrick et al. 2011). Together, these studies suggest it is possible that increased average summer temperatures served to make the Classic Maya more combative and therefore more prone to engage in raiding and warfare.

The other potential mechanism is economic and involves maize, which was the staple crop for the Classic Maya. Our conflict time-series shows the strongest interaction with the high-resolution summer Cariaco SST record spanning approximately June-August (Wurtzel et al. 2013), which overlaps substantially with the primary agricultural season for maize in the Maya region (Sharer & Traxler 2006, Webster 2000). Researchers have long been aware that heat stress can reduce maize yields by inhibiting the growth and development of kernels. This has been observed in maize from North America, Africa, and Europe (Barnabás et al. 2008, Cheikh & Jones 1994, Crafts-Brandner & Salvucci 2002, Hawkins et al. 2013, Jones & Thornton 2003, Lobell et al. 2011). While the research is ongoing, recent work involving African maize indicates that temperature has a nonlinear effect on maize productivity (Lobell et al. 2011). Up to 30°C, maize yields improve as temperature increases. If temperature rises above 30°C, however, yields decline precipitously. Specifically, for each day spent above 30°C, maize yields drop by 1%, even under optimal moisture conditions (Lobell et al. 2011). Under drought conditions—caused by decreased rainfall or increased evapotranspiration—the effects are worse still because of the role of water in mitigating heat stress. Because the effect occurs even with careful water management, ultimately there is little a Classic Maya farmer could have done to maintain or improve yields if temperatures were too high during the growing season even for only a few days.

While both mechanisms are feasible, we think the psychological one is less likely than the economic one. Given the 25-year resolution of our analysis and the slight

increase in average temperature over the 600-year study period, we suspect that the economic mechanism provides a more compelling explanation for the relationship we identified between temperature and conflict.

The nonlinear effect of heat stress on maize yields suggests the following possible scenario for the Classic Maya, we think. Throughout the Classic period, average temperature fluctuated between 28ºC and 29ºC (Supplementary Figure 2 in Appendix C). During periods when the temperature was around 28ºC or less, maize yields were reasonably stable, with the exception of occasional drought caused by reduced precipitation. Periods of food shortage were infrequent and, when they occurred, brief. Consequently, there was relatively little conflict caused by resource stress. Intermittently, however, average summer temperatures rose, which occurred the first time at around 325 CE, then at around 550 CE and again at around 750 CE (Supplementary Figure 2 in Appendix C). As the average temperature increased to 28.5-29ºC the number of crop growing days with temperatures above 28.5-29ºC increased, too. Initially, the increases led to larger yields for several years or even decades, which raised the carrying capacity and therefore allowed population size to increase (Culbert & Rice 1990). However, as temperature continued to rise, the region experienced more days at or above 30ºC, which meant that crop shortfalls occurred more frequently. In addition, large-scale deforestation throughout the Classic period caused by urban expansion and agricultural intensification might have led to increased evapotranspiration, worsening the effect of increasing regional temperatures by reducing soil moisture availability (Oglesby et al. 2010, Shaw 2003). As a consequence of this, the recently expanded population experienced longer, more frequent periods of food shortage, leading to increased levels of conflict.

Food shortages among the Classic Maya might have led to conflict via two pathways. One of these involves starvation. While there is no direct archaeological evidence for starvation as far as we know, it is theoretically possible that maize yield shortfalls propelled Classic Maya rulers and their followers to attack nearby city-states and steal their food. Conflict over food might have been especially prominent during the Terminal Classic, a period of several decades leading up to the so-called collapse that began in the southern lowlands around 900 CE. The recently identified impact of

deforestation and droughts during the Late and Terminal Classic might have combined to reduce crop yields so substantially that starvation was a real threat (Oglesby et al. 2010). This hypothesis is consistent with recent research indicating that resource scarcity drives interpersonal conflict by making the need for resources outweigh the personal costs of violence (Allen et al. 2016). For most of the Classic period, however, we suspect the starvation scenario is not particularly likely because potential combatants would have been suffering the same food shortages, given the regional effect of temperature change, making stealing food an unsustainable long-term strategy. Furthermore, without draft animals, transporting enough maize through the jungle on foot over potentially hundreds of kilometers to support an entire population after a conflict would be difficult for the victor, even if some transport of maize over long distances might have been possible (Drennan 1984a,b; Sluyter 1993). It is also worth noting in connection with this that epigraphic and iconographic data indicate that tribute extracted from clients or demanded after conflict was often paid in the form of elite goods like cacao, jade, feathers, fine polychrome pottery, and cotton textiles, rather than large amounts of staple resources like maize (Chase et al. 2008, Foias 2002, Inomata 2001). In light of these points, we think the starvation scenario is unlikely to account for the long-term trend in conflict levels.

The other pathway that might have linked food shortage to conflict involves kingly legitimacy. Like many ancient kings, Classic Maya rulers had to demonstrate their legitimacy in order to retain their power (Iannone et al. 2016). One of the main sources of legitimacy among Classic Maya rulers was their ability to ensure prosperity for their states, especially agricultural prosperity (Iannone 2016, Lucero 2002). Because maize was the primary staple crop for the Classic Maya, local maize yields likely would have been directly linked to the perceived legitimacy of the rulers. We can see that the Maya rulers' identities during the Classic period became increasingly tied to maize because many rulers adopted special epithets that included the Maya term for maize (Tokovine 2013). With their legitimacy tied to maize yields, any declines in yields could have created a "crisis of legitimacy" (Iannone 2016), which the rulers needed to overcome by reaffirming or accruing cachet. The available evidence indicates that Classic Maya elites had several ways to accrue cachet, including building monuments, bestowing titles and land on client rulers, exacting tribute from clients, hosting ritual festivals, and

successfully attacking other elites (Chase & Chase 1998, Inomata 2006, LeCount 2001, Marcus 1974, Sanchez 2005, Webster 1975, 2000). Importantly for present purposes, however, a decline in maize yields would have made some of these tactics difficult. With less maize, a ruler could not have relied as heavily on opulent festivals or fed large labour forces. Consequently, he or she would have had to place more emphasis on bestowing rewards and assailing others.

While the economic mechanism provides a plausible explanation for how temperature change impacted Classic Maya conflict frequency over the long-term, internal conflict dynamics must also be considered. To reiterate, elite competition was an important source of conflict (O'Manksy 2013, Webster 2000). It is clear from the monument record that Classic Maya kings and elites were in competition with one another over resources and power (Martin & Grube 2008, Sharer & Traxler 2006). As such, much of the increase in conflict may have been caused by political ratcheting, whereby rulers engage in conflict as retribution for past transgressions leading to escalating conflict levels over time irrespective of resource shortfalls. Such ratcheting would be reflected in the serial dependence of the record because conflict would beget conflict. Conflict levels might also have increased with the proliferation of elites vying for power because of the polygamous marriage rules for elites and their system for status inheritance (O'Manksy 2013). The proliferation led to an increase in the number of polities competing for power and resources, as indicated by the increase in the number of named political entities in the epigraphic record—the so-called "emblem glyphs" (Marcus 1976). These internal processes would have created "momentum" in conflict.

The relatively strong performance of the null model, which includes no external variables, suggests that internal conflict dynamics, such as political ratcheting and elite proliferation, may indeed have been responsible for much of Classic Maya conflict (Fig. 4-2 and SI). However, the approximation still falls short of the observed conflict levels. According to our analysis (Fig. 4-3 and SI), including the summer Cariaco SST record produced a better approximation of past conflict levels than was possible using internal dynamics alone. Hence, the internal dynamics of Classic Maya conflict are insufficient to explain all of the temporal variation in conflict—it is necessary to look at external forces, too. We envisage a situation in which early in the Classic period relationships among

elites were often tense but only rarely reached the point at which conflict was deemed preferable to peace. As the population grew, and the number of competing elites increased, conflict became an increasingly common part of Classic Maya life for political reasons, as mentioned earlier. Critically, however, temperature also began to rise in the early Classic period and crop shortages became more common, leading to resource stress and more frequent crises of legitimacy. Because the strategies available for responding to the legitimacy crises became limited as crop failure became more common—e.g., the rulers did not have the maize required to hold large feasts or feed the corvee labour forces needed to build impressive monuments—the threshold to conflict was reduced. In other words, conflict became an increasingly important tool for regaining and maintaining legitimacy. Consequently, rulers decided more often to attack their neighbours, sometimes in order to acquire the resources necessary to feed their communities but more often to maintain or increase their political capital. Memories of past conflict fuelled this process by decreasing tolerance of words and actions of competitors. Thus, conflict levels increased in part because conflict begets conflict and in part because maize crop failures were occurring more frequently, creating crises of legitimacy for the elite. Eventually, the growth in conflict became explosive, rising from 0-3 per quarter century to 24 per quarter century.

Our findings have several implications. One concerns our understanding of the Classic Maya. Most of the literature about the impact of climate change on the Classic Maya has focused on drought caused by rainfall shortages. Droughts have been implicated in the demise of the Classic Maya civilization and argued to be a driver of cultural change (e.g., Kennett et al. 2012, Dunning et al. 2012, de Menocal 2001, cf. O'Manksy 2013). Even though several rainfall proxies indicate that droughts occurred during the Classic period (e.g., Kennett et al. 2012), our results indicate that drought might have had less of an impact on Classic Maya society than previously thought. While individual droughts may have contributed to specific conflicts, our results show that the trend in conflicts cannot be explained by rainfall shortages. Instead, the key environmental variable seems to have been temperature. The effects of temperature on conflict levels could have been further exacerbated by deforestation, soil depletion, and rainfall shortage in certain cases. But the overall long-term trend in conflict levels is still best explained by a combination of internal conflict dynamics and temperature. This may

lead to new insights about the patterns that have been documented in the Classic Maya archaeological and epigraphic records.

Our results also have policy implications. Most obviously, our results indicate that it is necessary take into account the long-term, potentially nonlinear, effects of climate change on conflict. Over the short-term, the effects in some areas might appear benign, as with initially increasing maize yields. But, over longer time scales the effects could be dire, contributing to substantial increases in conflict and violence. Perhaps more perniciously, though, our results also imply that we need to consider the interaction between our current political ideology and the impact of climate change. In the Maya case, the increase in conflict levels might not have been an inevitable outcome of climate change had their political ideology been different. The symbolic connection between maize yields and power might have driven their leaders into conflict unnecessarily. So, perhaps we need to look more closely at the role political ideology may play in determining the long-term impact of climate change on our societies. If we ignore the long-term effects and the role of ideology in determining outcomes we could drastically underestimate the scale of the problems caused by climate change and miss opportunities to adapt by addressing problematic ideologies.

Our study has implications for the role of archaeological data in discussions about modern climate change too. Several scholars have argued that archaeology can contribute to the discussion about contemporary climate change and some policy organizations like the IPCC have recently begun including archaeological case studies in their reports. The idea here is that the archaeological record contains important examples of past societies affected by climate changes, which can serve as a basis for improving our predictions about future impacts and persuade people to take action. Our results reinforce this notion, but they also underscore archaeology's ability to shed light on another critical issue—namely the long-term effects of climate change. The impact of temperature on Classic Maya conflict appears to have been significant at the centennial scale, something we could only see with long-term records. So, in addition to being a source for case studies, archaeology is important because of the long-term, time-transgressive vantage point it affords us. In fact, since long-term effects can be quantitatively and qualitatively different than short term ones, the archaeological record

is a crucial source of information about human responses to climate change. Needless to say, the same holds for the palaeontological record and current attempts to predict the impact of climate change on non-human animals.

With regard to future research, we think at least three avenues could be explored. One involves determining the extent to which the pattern we identified holds for the whole Maya region. As we explained in the Materials and Methods section, the conflict record pertains mostly to the Southern Maya Lowlands, with relatively few inscriptions from elsewhere. That said, some archaeological evidence in the form of defensive architecture points to an increase in conflict at the end of the Classic period in the northern Yucatan as well, suggesting that the trend might be the same (e.g., Dahlin 2000, Webster 1978). So, future research should aim to determine whether the pattern we identified pertains to the whole Maya region by collecting more epigraphic data or perhaps using construction dates for defensive architecture as a proxy for increased militarism. Another avenue for future research involves our hypothesis about the causal pathway from temperature to conflict. While it seems plausible that increasing temperature could have caused maize yields to decline thereby precipitating greater levels of conflict, this hypothesis needs testing. One possible test would be to compare a proxy for maize yields, such as pollen frequencies in sediment cores, to palaeoclimate records using the PEWMA method. This test could be conducted in the near future as high-resolution pollen records are being collected from the region by a team led by David Wahl of the University of California Berkeley (Pers. Comm. August 10, 2015). The third and final avenue for future research involves finding local high-resolution temperature records. While the temperature increase we identified appears to have been a regional phenomenon that probably affected all Classic Maya centres, local records might expose some important variability. A better understanding of that variability could improve the predictions of our model and reveal important variation in the climate-conflict relationship.

4.6. Acknowledgements

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Chapter 5. Radiocarbon dating uncertainty severely undermines our ability to identify cycles in palaeoclimate data

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Statement of Contribution of Joint Authors:

Carleton, W. (candidate): research design; data collection; data analysis; co-wrote the manuscript.

Campbell, D. (committee member): research design; supervised data analysis; co-wrote the manuscript.

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5.1. Abstract

The Earth's climate is a complicated system driven by cyclic and acyclic forces that likely impact human societies at a variety of temporal scales. Creating accurate models of past and future climate change requires that we distinguish the cycles from the trends. Therefore, it is crucial that we are able to identify cyclical patterns in palaeoclimate proxy records so that we can build an accurate picture of the Earth's climate dynamics and its effect on human societies. However, securely identifying cycles in palaeoclimate proxy time-series data is challenging, especially when the records are dated with radiocarbon assays. This is because radiocarbon dates have highly irregular uncertainties that can create challenges for the standard statistical methods used to identify cycles in time-series data. In this paper, we present the results of a large

simulation study in which we explored the impact of radiocarbon dating uncertainty on our ability to identify cycles in time-series records dated with radiocarbon. We conducted a series of simulation experiments involving thousands of artificial time-series with known cyclical patterns. We found that, at best, we could correctly identify cycles only 42% of the time. We also found that the rate of false positive findings can be as high as 90%. Together these findings raise significant concerns about the reliability of previous research and our ability to ever convincingly identify cycles in radiocarbon dated time-series.

5.2. Introduction

Identifying cycles in palaeoenvironmental data is important for understanding human-environment interaction. Determining which aspects of past climate change are driven by natural cycles is crucial for understanding how those changes affected human biological and cultural evolution. It is also vital for producing accurate predictions of future climate change, which is in turn important for planning to adapt to those changes. However, there is a potential problem. A recent re-evaluation of the widely-accepted hypothesis that cyclical droughts had a profound impact on the Classic Maya found that the drought cycles were likely a spurious finding caused, in part, by radiocarbon dating uncertainty (Carleton et al., 2014). Here we report the results of a simulation study in which we sought to ascertain whether Carleton et al.'s results reflect a more general problem with the impact of radiocarbon dating uncertainty on the identification of cycles in palaeoclimatic records. Our main objective was to estimate the degree of confidence that ought to be placed in cycles identified in radiocarbon dated time-series.

Scholars have long been aware that aspects of the Earth's climatic system appear to be cyclical. In the 1920s, Milankovich proposed that the climate was affected by *orbital forcing*—i.e., periodic variations in the Earth's orbital parameters spanning tens to hundreds of thousands of years that cause climate change by affecting the amount of sunlight reaching the Earth. The cycles are thought to have been a major driver behind the rhythmic glaciation of the northern hemisphere throughout the Quaternary period from 2.58 million years ago until the present (Appenzeller et al. 1998). They are a

defining component of the present climatic regime and probably had a significant impact on the evolution of many species, including humans, over the last few million years.

Since the identification of Milankovich Cycles, many other climate cycles have been proposed. Scholars claim to have found cycles in past sea surface temperatures, rainfall, storm activity, fire regimes, and more using a host of climate proxy records, including dust influx in alluvial deposits, variations in sediment compositions in lakebeds, isotopes in cave speleothems, and tree ring thicknesses (e.g., Baldini et al. 2008, Delworth & Zeng 2016, Desprat et al. 2003, Feliks et al. 2010, 2013; Franke et al. 2013, Gámiz-Fortis et al. 2002, Jiang et al. 2013, Rutherford & D'Hondt 2000, Wu et al. 2009). These cycles range from the millennial scale to the decadal, and they have influenced the way scholars think about the Earth's climate (e.g., Bond et al. 1997, Delworth & Zeng 2016, Omta et al. 2015).

The identification of these cycles has also led to speculation about the impact of cyclical climate change on human-environment dynamics. One famous example is *Bond Cycles* (Bond et al. 1997, 2001)—i.e., periodic ice rafting in the North Atlantic that led to changes in sea water density and circulation patterns causing significant Northern Hemisphere cooling. Some of the so-called *Bond Events* are well known, such as the 8.2 ky (kiloyear) event and the Little Ice Age. Bond Events have been linked to important archaeological events, like the abandonment of many large villages in the Levant around 8200 years ago, the abandonment of many Bronze Age cities in the Near East 4200 years ago, and the collapse of the Norse colonies in Greenland 500 years ago. These hypothetical connections raise the possibility that several important episodes in human history were caused by climate cycles.

Some scholars have gone even further, arguing that historical processes are inherently cyclical, driven largely by natural cycles. Recently, for example, environmental scientists have proposed something called "Panarchy Theory," which characterizes human-environment interaction as a dynamic relationship involving cycles at different spatial and temporal scales interacting to produce complex cyclical patterns (Allen et al. 2014, Gunderson & Holling 2002). According to Panarchy Theory, these interacting cycles explain the rise and fall of societies throughout history. Several archaeologists

have since taken up the idea, claiming to have found archaeological evidence in support of Panarchy Theory (e.g., Gronenborn et al. 2014, Rodriguez & Anderson 2013, Rosen & Rivera-Collazo 2012, Thompson & Turck 2009, Zimmermann 2012). Other scholars have made similar arguments, without specific reference to Panarchy Theory, echoing the notion that human history is cyclical and to some extent driven by environmental cycles (e.g., Chase & Chase 2013; Hodell et al. 2001, 2005a; Masson 2012, Ur 2010, Zhang et al. 2006). Their arguments are reminiscent of a centuries old idea that history involves inevitable cycles of increasing complexity followed by decline (Collingwood 1927). Some have placed more emphasis on environmental cycles and others have placed more emphasis on social and political forces. Nevertheless, the idea that historical cycles exist is widespread and so is the notion that these cycles are related to natural environmental cycles.

While doubts have been expressed about some of these claims (e.g., Akkermans et al. 2015, Berger & Guilaine 2009, Mercuri et al. 2011, Weninger et al. 2006, Zhang et al. 2011b), there are reasons to expect that natural environmental cycles might affect human societies. Like many other animals, for instance, humans are diurnal. So, our behaviour and biochemistry are affected by day-night cycles, which demonstrates that cyclical patterns in the environment can lead to cyclical patterns in biology. Furthermore, numerous cultural practices are cyclical, like equinox festivals and agricultural seasons. Even warfare has been known to happen at certain times of year (e.g., Foxhall 2000, Hurtado & Hill 1990). Thus, it seems plausible that cyclical climate changes at a variety of temporal scales might have cyclical effects on long and short-term patterns in human history.

As plausible as it may be, though, the hypothesis that human history has been affected by cyclical climate change needs to be tested. Testing it requires confidently identifying natural environmental cycles in the first place—a critical part of determining whether they affect human societies over the long term. So, we first need to search for cycles in palaeoenvironmental data. However, as we indicated earlier, there is reason to think that radiocarbon dating uncertainty undermines our ability to securely identify cycles in many palaeoenvironmental datasets.

Radiocarbon dating is basically a two-step process. The first step involves estimating the ratio of ^{14}C to ^{12}C and ^{13}C in a sample of organic carbon, and the second step involves calibration. Radiocarbon dates are calibrated to account for the historic variation in environmental carbon isotope ratios. Without calibration, radiocarbon dates can be off by centuries (Aitken 1990, Bronk Ramsey 2008). Importantly, the two steps involve different kinds of errors. The first step results in estimated ages that have normally distributed temporal errors, often with magnitudes less than ±100 years, depending on the age of the carbon. In contrast, the second step yields ages with highly irregular, skewed, multimodal temporal errors (see Figure 5-1). As several scholars have pointed out, these highly irregular errors make calibrated radiocarbon dates challenging to analyze (Blaauw et al. 2007, e.g., Blaauw 2010, 2012; Mudelsee 2014, Parnell et al. 2011, Telford et al. 2004b). The multimodality of the errors means that the arithmetic average does not necessarily represent the most likely real date for a given radiocarbon assay. Instead, multiple dates can be likely candidates. Consequently, means are of questionable use for describing calibrated radiocarbon dates.

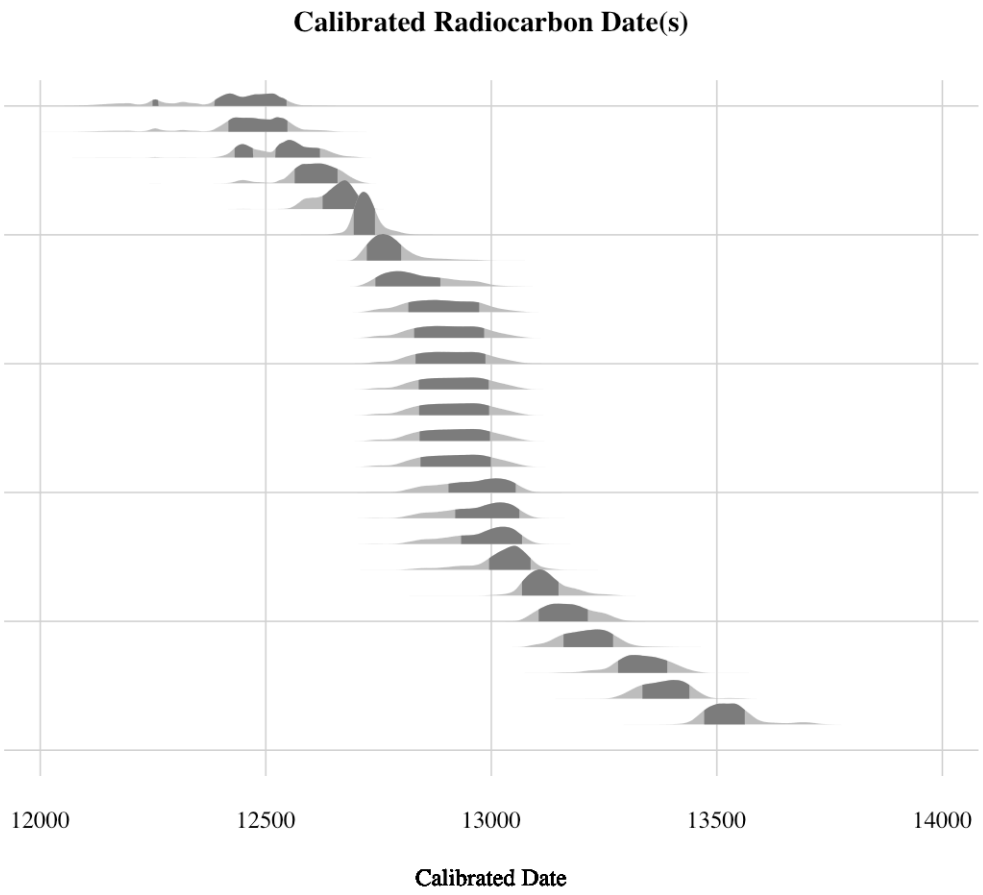

Figure 5-1 Example of a series of calibrated radiocarbon dates

The highly irregular errors associated with calibrated radiocarbon dates create two major analytical problems. First, the assumptions of any statistical methods that rely on the arithmetic mean of a sample will be violated by the temporal uncertainty of calibrated radiocarbon dates. Second, observations in a time-series—e.g., measurements from a sediment core—will float in time because they cannot be confidently pinned to a specific date. Together these problems have important consequences for palaeoenvironmental time-series analyses, mostly via the effect they have on age-depth models (Carleton et al. 2014, Mudelsee 2014, Telford et al. 2004a,b).

Age-depth models are mathematical functions that relate the age of a given sedimentary layer to its depth, and they are the chronological backbone of most

palaeoclimatological records (Bradley 2013). Since it would be prohibitively expensive and time consuming to precisely date every layer in a sediment core, palaeoclimatologists instead sample the sediment for datable material, like organic carbon. The samples are then dated, providing dates for the layers in which the carbon samples were found. These dated layers act as chronological anchors for the core. Correlating the dates of the anchors to their depths and then interpolating between them provides a time stamp for every depth measurement in the sediment core. That way, any observations of the sediment, or climatological proxies it contains, can be assigned an age based on its depth. But, if the anchors used in the interpolation are not fixed in time because of chronological uncertainty, then the interpolation function can be compressed or expanded along the length of the sediment sample. And if the anchors are radiocarbon dates, as they often are, then the temporal compression and expansion can be highly irregular because of the irregular, multimodal temporal distributions of calibrated radiocarbon dates. Consequently, the amount of time that apparently passed between observations in the sediment sample will be different depending on temporal location of the anchors and it can vary along the length of the series. Thus, several properties of the underlying climatological process of interest will appear to be different depending on the anchors—e.g., the rate of change in the climate variable or, crucially for studying cycles, the wavelength and frequency of any waveforms.

Identifying cycles in time-series data involves a suite of techniques often referred to as *frequency-based* time-series methods (Pickup 2014). The most common frequency-based methods are *time-invariant*. They involve fitting *sinusoids*—i.e., mathematical wave functions made up of sine and cosine waves—with constant frequencies, amplitudes, and phases to entire time-series (Oppenheim & Schafer 2010). These methods yield *frequency spectra*—i.e., plots showing the original signal decomposed into cycles of different frequencies with peaks representing the most powerful cyclical components present in a given time-series. Unfortunately, however, the methods rely on chronological stability. If the chronological anchors used to build an age-depth model can float in time, as they can with radiocarbon dates, then for any time-series or part thereof the parameters of the sinusoids that might fit the data can be different depending on the temporal positions of the anchors. Compressions in time

cause the frequencies to be higher, while expansions cause them to be lower, creating uncertainty about the wavelengths of any cycles identified in the time-series.

These uncertainties raise the possibility that cycles in palaeoclimate time-series cannot be securely identified using established frequency-based methods. This in turn raises questions about the current state of research. While some cycles have been identified in records dated with techniques other than radiocarbon, like Milankovich Cycles found in ice cores, radiocarbon dating is highly prevalent among studies intended to identify cycles in palaeoclimate data (e.g., Bond et al. 1997, Cacho et al. 1999, Desprat et al. 2003, Langdon et al. 2003, Obrochta et al. 2012, Sorrel et al. 2012, Wu et al. 2009). It is also more often the case that cycles on timescales relevant to human societies—like Bond Cycles—are identified in records dated with radiocarbon, especially in archaeological contexts where carbon is the most readily available datable material. So, how much confidence can we place in the cycles identified? With this question in mind, we conducted a simulation study to determine how much confidence can be placed in the cycles identified in radiocarbon dated time-series. The simulation involved creating thousands of time-series with a single known cyclical pattern and autocorrelated noise to reflect the kinds of climate time-series we are interested in analyzing. Throughout the simulation, we varied four parameters of the time-series while keeping the others constant to explore the effect of calibrated radiocarbon dates on our ability to recover the known cycle. The parameters we varied were 1) the location of time-series along the radiocarbon calibration curve, 2) the number simulated radiocarbon dates involved, 3) the amount of noise in synthetic time-series, and 4) the frequency of the cyclical pattern. To find the cycle, we used a method called Least-Square Spectral Analysis, the same method we used to analyze the Maya drought record (Carleton et al. 2014). At the end, we estimated a false-positive rate for each combination of simulation parameters—i.e., how often we should expect to find spurious cycles.

5.3. Methods

To investigate the effect of radiocarbon dating uncertainty on frequency-based methods, we followed the bootstrap approach we used to test the hypothesis that drought cycles caused periodic socio-political upheaval among the Classic Maya

(Carleton et al. 2014). The approach involved sampling the calibrated radiocarbon date distributions of a given time-series repeatedly, and then re-estimating its age-depth model to compile a large ensemble of likely age-depth models. Next, each of the age-depth models in the ensemble were then used to date the original series of observations creating an ensemble of time-series that reflects the chronological uncertainty in the radiocarbon dates. Each series was then analyzed using the LSSA to find candidate cycles. The candidate cycles were then compared to a sensible benchmark for statistical significance—i.e., a *null frequency spectrum* that reflects our expectations about how a random time-series with no cycles would appear. Critically for palaeoenvironmental time-series, the null spectrum had to account for autocorrelation since climate processes are typically autocorrelated, resulting in a *red-noise* background (Schulz & Mudelsee 2002). Red-noise produces a characteristic frequency spectrum with exponentially decreasing power as frequencies increase (Schulz & Mudelsee 2002). So, we identified peaks in the frequency spectrum as statistically significant—i.e., indicative of potential cycles—only if they exceeded the red-noise background spectrum.

Using the R statistical programming language (R Core Team 2016), we ran a series of simulation experiments, each of which explored how a set of variables affected the identification of a single cycle in a synthetic time-series (see SI for scripts and functions). Each experiment involved a set of fixed parameters that were the same for every experiment and a set of variable parameters.

The experiments proceeded in several steps. In the first, we constructed an ensemble of 1000 synthetic time-series using a simple sine function. The time-series each spanned 1000-year period, a free parameter that could be either of two ranges corresponding to two parts of the INTCAL-13 calibration curve—i.e., a function used to calibrate radiocarbon dates (Reimer et al. 2013). One part of the curve spanned 13,000–14,000 BP, while the other spanned 14,000–15,000 BP (see Figure 5-2). These ranges were selected because the slope of the calibration curve in the earlier part is nearly twice that of the later part, allowing us to explore the effect of the curve's slope on our findings, which was necessary because high slope regions tend to produce calibrated dates with lower variances (Buck et al. 1994).

The number of observations in the time-series was fixed at 300 evenly spaced samples of a sine wave. The sine wave had fixed amplitude and phase, both set to one, but a variable frequency determined by the number of sine wave cycles present in the series, a free parameter. We varied the number of cycles between 5, 10, or 40 cycles, which corresponded to periods of 200, 100, and 25 years respectively because of the fixed series length. The larger period was chosen because it corresponds to cycle lengths of interest in the human-environment interaction literature already (e.g., Hodell et al. 2005a) and because any longer periods would not produce reliably detectable periodic signals in a time-series spanning 1000 years. The shortest period was chosen because any shorter periods would be beyond the resolution of most archaeological records.

To adequately reflect the autocorrelation present in palaeoclimate records, we also added a red noise term to the sine wave. The red noise was created with an R function that generates realizations of an auto-regressive processes, namely *arima.sim()*. We set the autocorrelation between adjacent observations to 0.7, but varied the amount of noise by modulating the variance of process. The variance was scaled so that the signal-to-noise ratio could be 100, 10, or 1—with 100 being the clearest signal and 1 being the nosiest.

Then, we selected radiocarbon dates from the calibration curve that would be used in subsequent steps to create age-depth models for the series. There could be 5, 15, or 25 dates, evenly spaced along the calendrical time axis of the curve. To derive dates in radiocarbon time, we looked up the radiocarbon dates in the curve that corresponded to the calendrical dates, a process sometimes called *back-calibration*. Those back-calibrated dates became the synthetic radiocarbon assays for the time-series. We then set the error of the simulated radiocarbon dates to a standard deviation of ± 50 years, a fixed parameter corresponding to a common magnitude of error returned by dating labs. Setting these errors to a constant value was necessary to isolate the errors introduced by calibration—i.e., the irregular errors we were interested in.

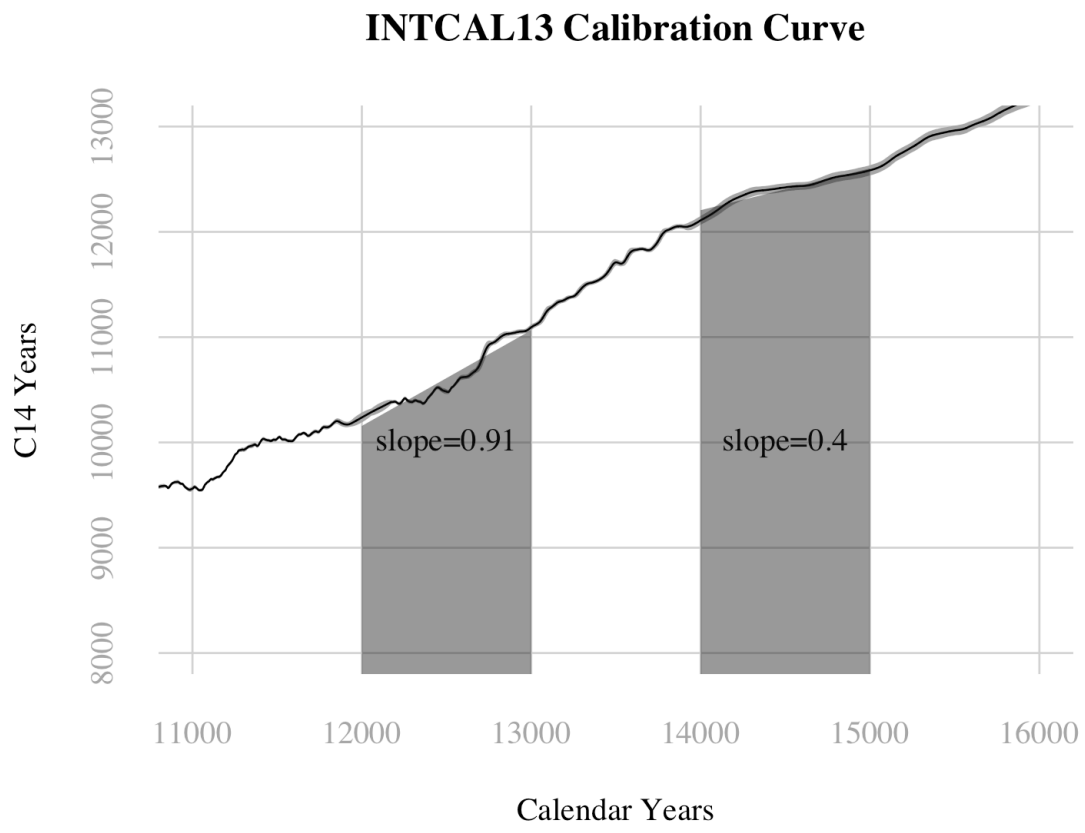

Figure 5-2 Calibration curve test regions

In the second step, we estimated a null hypothesis for the experiment using Least Squares Spectral Analysis (LSSA) (Vanícek 1971). Like other frequency-based methods, LSSA produces a frequency spectrum. To reiterate, peaks in the spectrum denote potentially significant periodic components of the time-series. To determine which of the peaks are statistically significant, we created another ensemble of 1000 time-series, but this time the observations did not include any waveforms—i.e., each series was only a realization of the auto-regressive process we used in the first step to add noise to the sine wave. Then, we used LSSA to produce an ensemble of frequency spectra, each estimated at the same set of frequencies. Next, we combined the 1000 red noise LS-spectra to produce a point-wise 95% confidence level that acted as the null spectrum for the experiment—i.e., the 95% quantile of the distribution of pure red noise

LS-spectra at each frequency above which any peaks would be considered statistically significant.

In the third and final step, we used LSSA and the null spectrum to search for statistically significant cycles in the 1000 synthetic time-series from the first step while exploring the effect of calibrated radiocarbon dating uncertainty on the results. To do so, we created 2000 age-depth models for each synthetic time-series, resulting in 2,000,000 synthetic time-series. First, we bootstrapped the calibrated date distributions associated with each synthetic series using a Gibbs sampler to produce the radiocarbon dates needed for age-depth modeling (Buck et al. 1992, Geman & Geman 1984). The sampler was constrained to ensure that the dates selected always resulted in a monotonic increasing age-depth model for each series. This was done to simulate the conditions of a sedimentary environment in which the stratigraphic relationships among dates were known and constant. Then, we constructed the age-depth models by fitting a monotonic spline to each set of bootstrapped radiocarbon tie-points and we sampled the age-depth models at regular intervals to give a date to each of the 1000 observations in every time-series. Next, we estimated LS-spectra for each of the 2,000,000 series, building a list of the frequencies that surpassed the 95% confidence level denoted by the null spectrum. Given the rounding errors and uncertainties in real and simulated climate data, however, we expected to rarely if ever recover the exact frequency of the known cycle. So, to make the experimental results useful, we established an acceptable error window of ± 20% of the cycle's period. The error window meant that, for instance, the target period for a 25-year cycle was 25 ± 5 years, corresponding to a target frequency range of 0.03–0.05. Any significant frequencies in the LS-spectra outside the window were considered false positive findings while any found within the target window were considered hits, allowing us to calculate the true-positive hit rate of each experiment given its fixed and free parameters.

5.4. Results

Permuting all possible values for the four free parameters yielded 54 experiments, the results of which are illustrated in Figures 5-3 to 5-8. Across all experiments, the hit rates were low. Around two thirds of the experiments yielded hit

rates of less than 10% with a maximum hit rate of 42% in a single experiment. Conversely, the false positive rates were large, reaching 90% in two-thirds of the experiments. So, the majority of the cycles identified were false positives-i.e., spurious cycles.

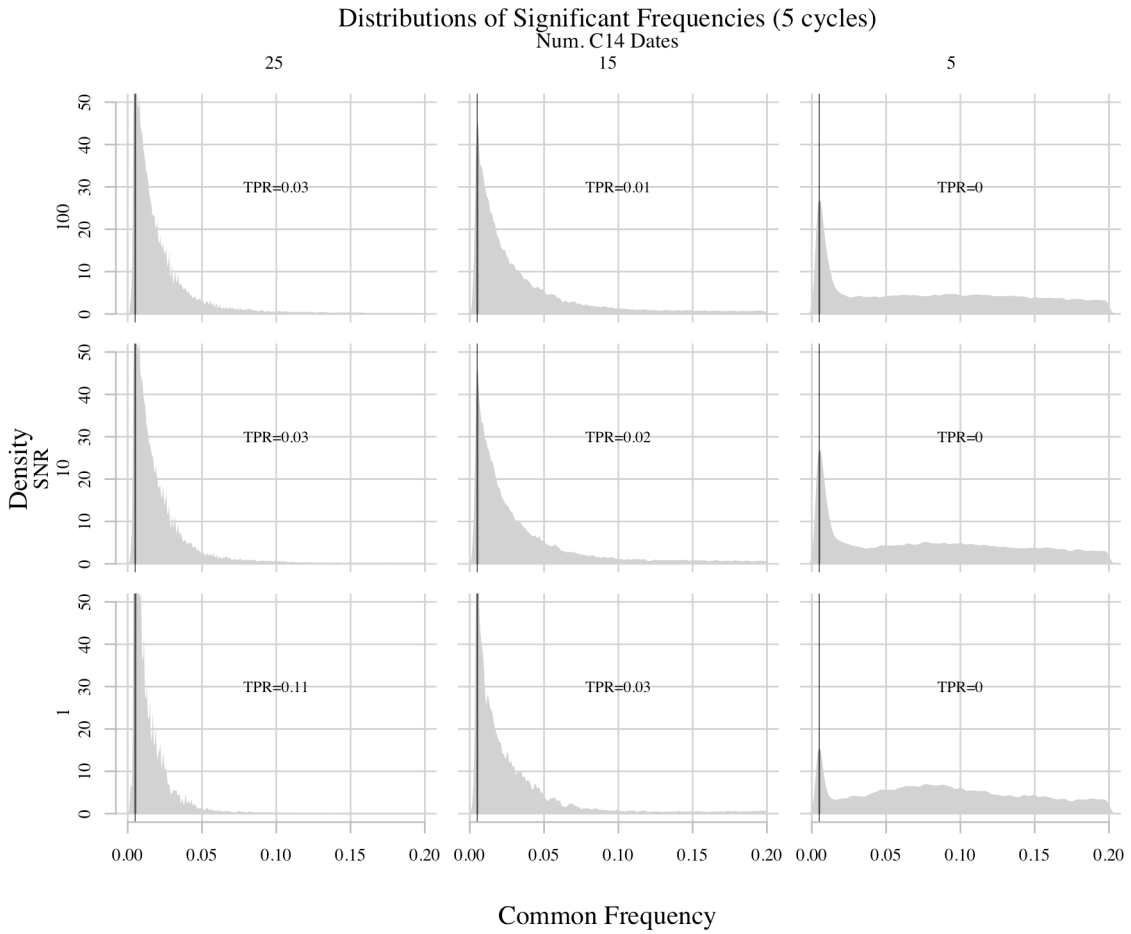

Figure 5-3 Simulation Results: 12000–13000 BP, 5 cycles

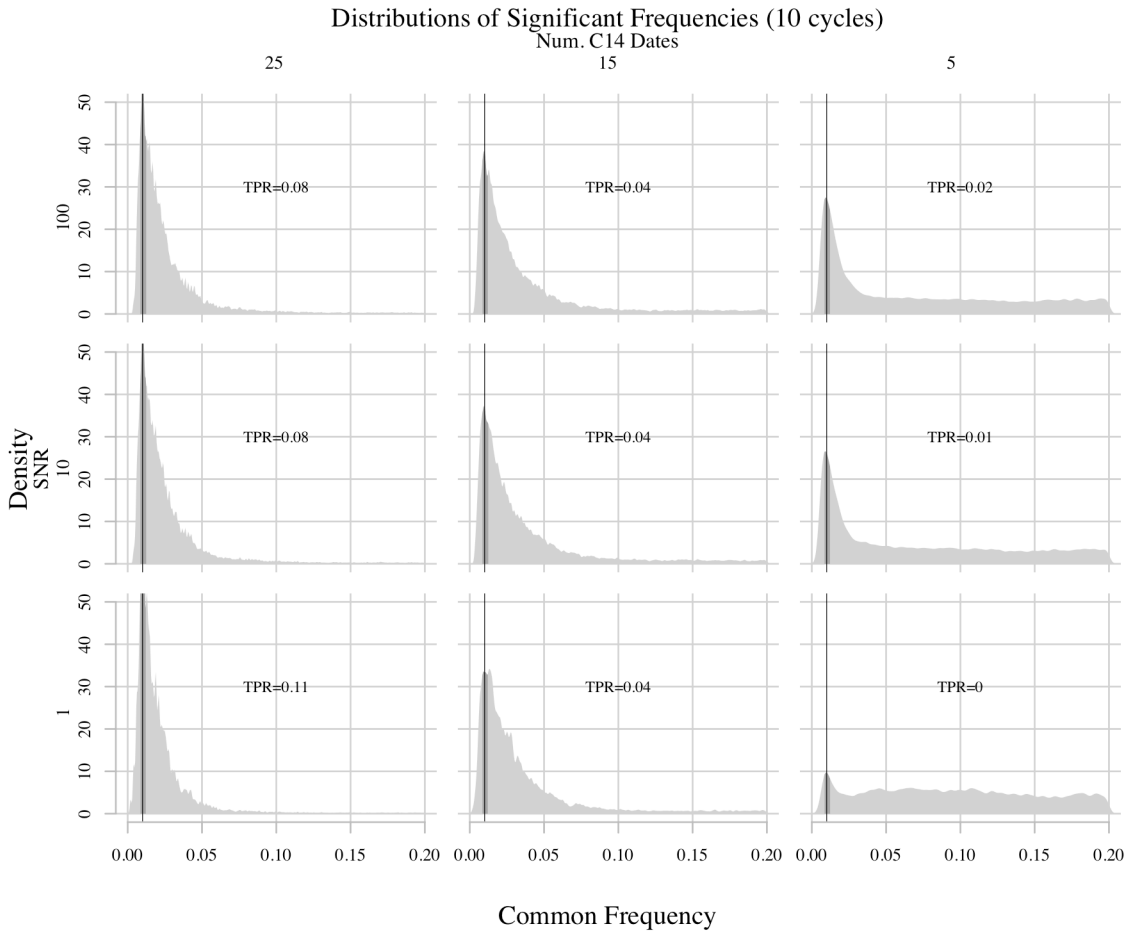

Figure 5-4 Simulation Results: 12000–13000 BP, 10 cycles

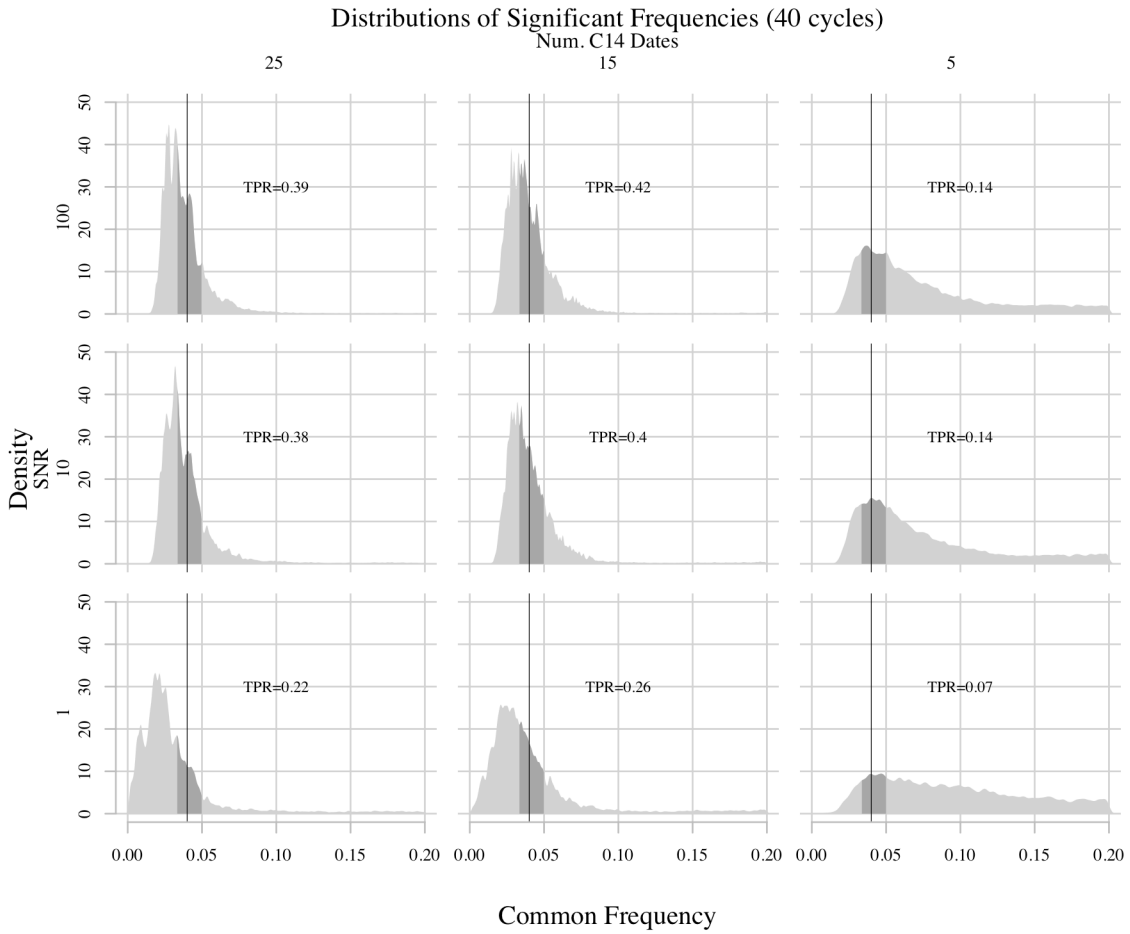

Figure 5-5 Simulation Results: 12000–13000 BP, 40 cycles

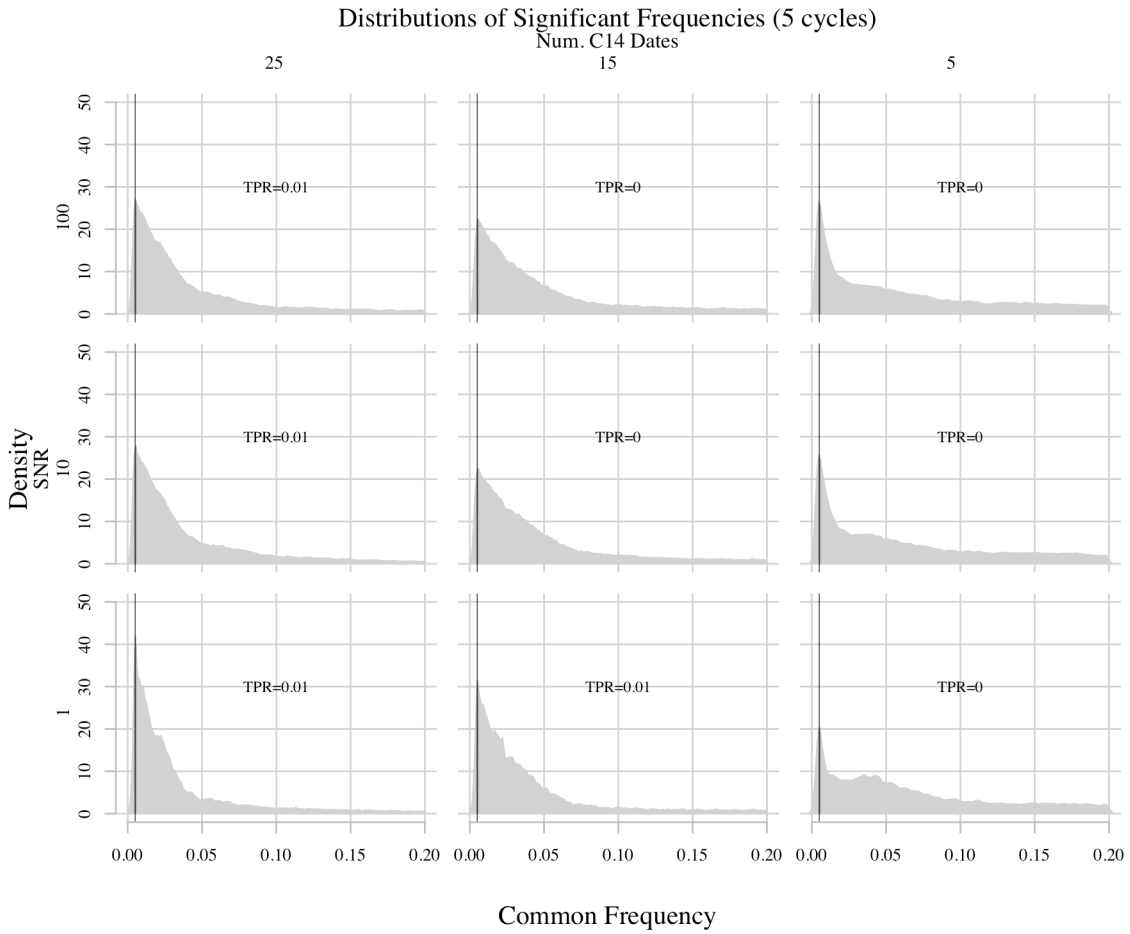

Figure 5-6 Simulation Results: 14000–15000 BP, 5 cycles

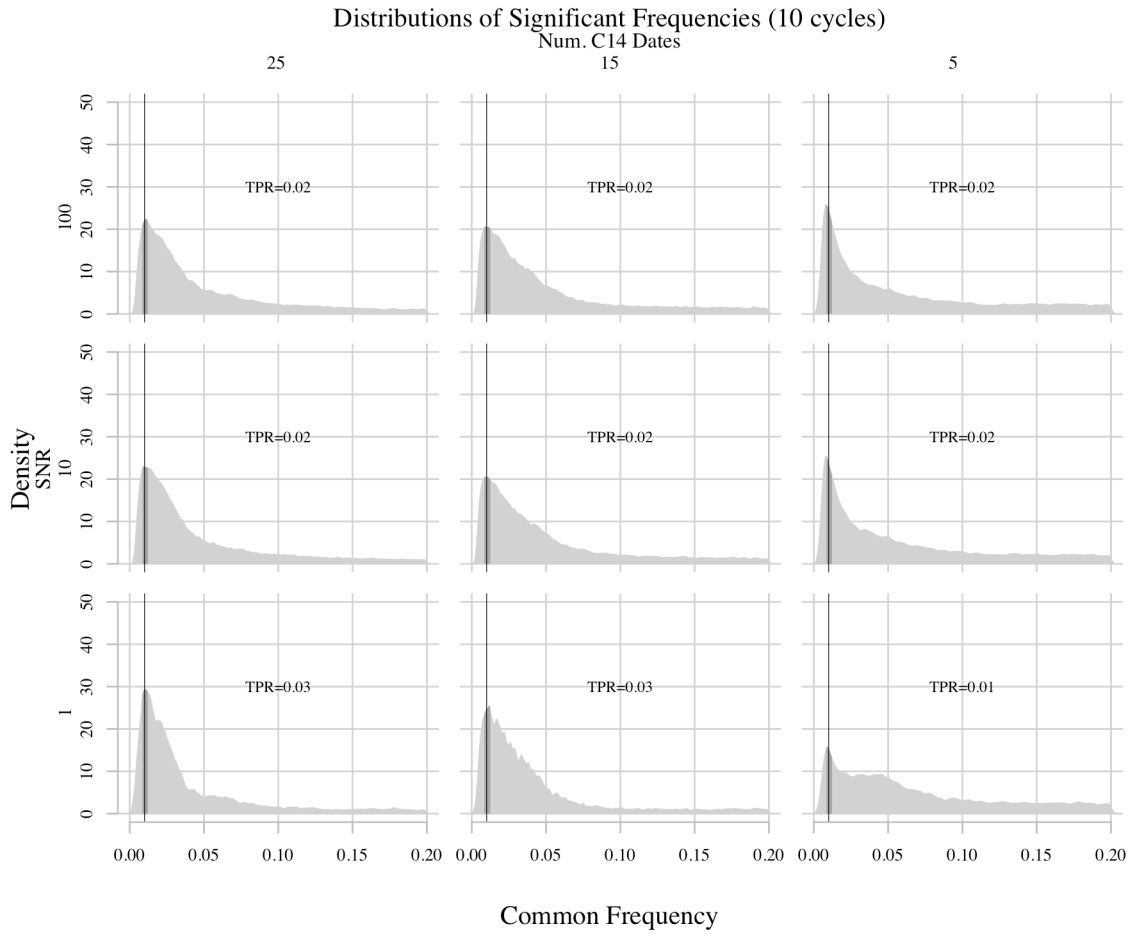

Figure 5-7 Simulation Results: 14000–15000 BP, 10 cycles

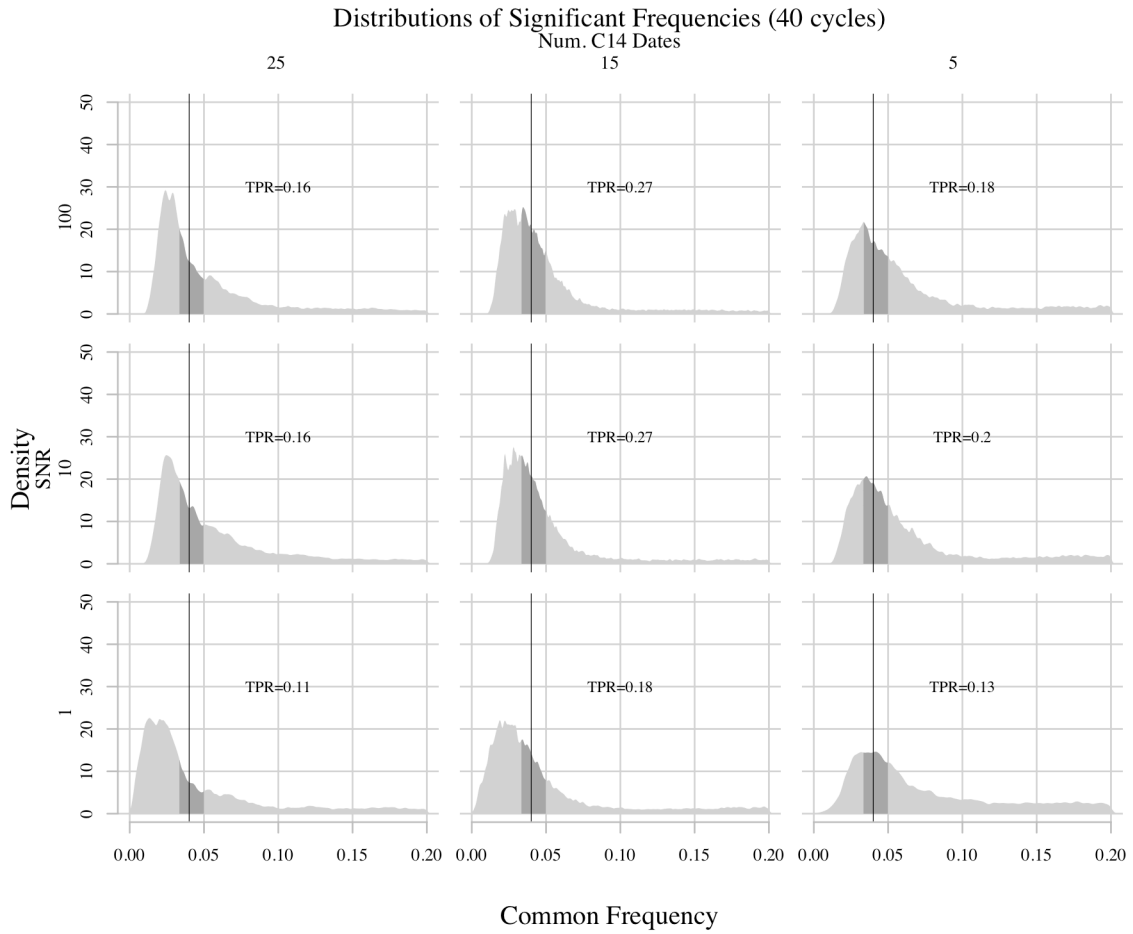

Figure 5-8 Simulation Results: 14000–15000 BP, 40 cycles

A number of other patterns can be identified in the results. One involves the slope of the INTCAL-13 curve. The lower slope portion of the curve that spanned 14,000–15,000 BP resulted in greater numbers of false positives. The distributions of statistically significant frequencies were fatter—i.e., contained greater variance—compared to results the experiments involving the same set of parameters but in the high-slope region of the curve. This finding is unsurprising because radiocarbon dates from the low-slope portion of the calibration curve have greater variance, which means that they could move in time to a greater extent, creating larger magnitude temporal compressions and expansions in the time-series.

Another pattern concerns the number of complete cycles in the synthetic time-series—i.e., the frequency of the cyclical pattern. Increasing the number of cycles in a given series increased the true-positive hit rate. This is also not particularly surprising. The frequency of the sine wave would be expected to affect our analysis by determining the number of full cycles observable in the time-series, with more cycles being easier to detect and distinguish from background noise than fewer cycles. If, for instance, the series contained less than a complete cycle, we would not be able to distinguish the signal from a long trend, or from part of a waveform with a very long wavelength. In contrast, greater numbers of complete cycles would fit the sinusoids better in the least-squares sense, producing clearer peaks in the frequency spectra that would be distinct from the red-noise background. Thus, we expected greater numbers of cycles to improve the chances of true positive results. Holding the other parameters constant, this expectation was met: going from 10 cycles to 40 cycles in the experiments involving the high-slope portion of the calibration curve increased the hit rate the most, an average increase of approximately 27%.

A third pattern involves the signal-to-noise ratio (SNR), which indicates the clarity of the underlying sine wave compared to the variance of the red noise. Increasing the noise in the synthetic time-series increased the variance in the distributions of statistically significant frequencies across the board, meaning more false positive findings. However, the greatest increases in variance occurred when we changed the SNR from 10 to 1—i.e., when the noise went from being one-tenth the strength of the sine wave to being as strong as the wave. In contrast, SNRs of 100 made little difference in the hit rate compared to SNRs of only 10, where the sine wave was only 10 times stronger than the noise. This is surprising. To illustrate the difference, we created two time-series plots, one showing a synthetic time-series with an SNR of 100 and the other an SNR 10 (see Figure 5-9). Obviously, the signal with an SNR of 100 is clearer than the other one. Thus, our findings indicate that beyond a low threshold, increasing the clarity of the synthetic signal does nothing to improve our ability to recover a simple sine wave from a time-series dated with radiocarbon. This pattern underscores the relevance of radiocarbon dating uncertainty—i.e., even a clear signal, such as one with an SNR of 100, is likely to be obscured by the error associated with calibrated radiocarbon dates.

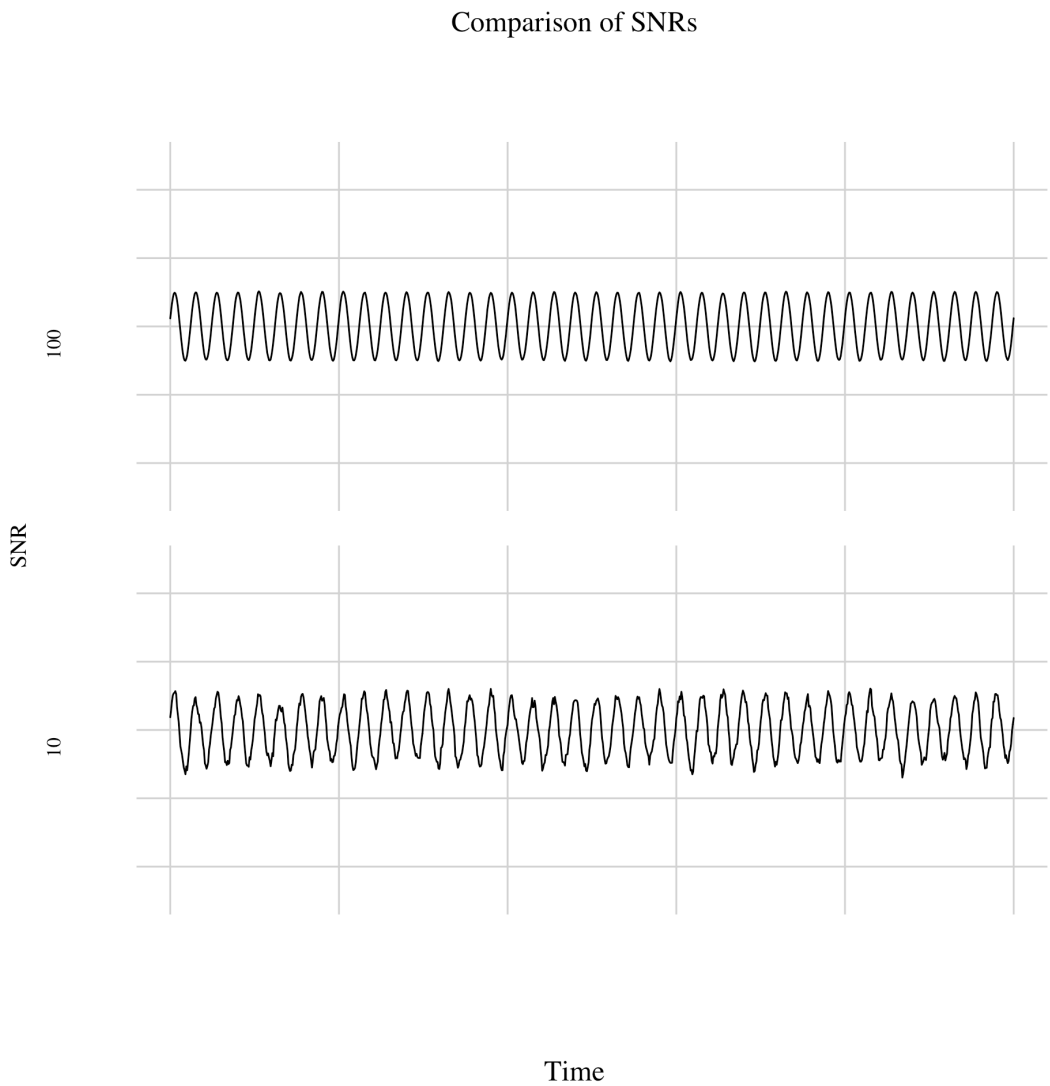

Figure 5-9 Comparison of SNRs

Note The top plot shows a sine wave with very little red-noise, corresponding to a SNR of 100, meaning that the variance of the wave was 100 times greater than the variance of random red noise. The bottom plot shows a sine wave with an SNR of 10, which means that the wave has a variance only 10 times greater than that of the red-noise—i.e., a noisier signal.

The last, and in some ways most interesting, pattern pertains to the number of radiocarbon dates used in the age-depth models. Prior to running the experiments, we had assumed that we would obtain better results with more dates. However, the results show a more complicated relationship between numbers of dates and experimental hit

rates. As expected, experiments involving fewer dates had higher variances and lower hit rates compared to experiments involving more dates. Having fewer dates meant that there were fewer constraints on the age-depth model, resulting in fatter tailed frequency distributions with more high-frequency false positives, as shown in Figures 5-3 to 5-8. But, the relationship appears to be asymptotic. Increasing from 5 to 15 dates improved the hit rate more rapidly than going from 15 to 25 dates. In other words, contrary to what we had assumed, there are diminishing returns for increasing the numbers of radiocarbon dates. So, using more than 15 dates—i.e., a dating density of 0.015, or one date every 66 years in a 1000-year series—appears to have less impact on hit rates than the other variables.

5.5. Discussion

These results indicate that calibrated radiocarbon dating uncertainty can undermine our ability to identify cycles in time-series data. The uncertainty is so irregular that the average date, or even the expected value—i.e., a weighted average—cannot adequately describe the real calendar date of a calibrated radiocarbon assay, as others have noted (e.g., Telford et al. 2004a). Consequently, in statistical analyses we need to explore how this uncertainty affects our results by trying a sample of likely dates and re-running a given analysis many times. Unfortunately, in the case of cyclical patterns in time-series data, the main parameter of interest is the frequency of the waveform, and changing the time between observations substantially alters it. The highly irregular uncertainty of calibrated radiocarbon dates causes time-series to compress or expand in time differentially along the length of the series when we try to account for that uncertainty by resampling the dates. Consequently, the frequency of any waveform in the series is altered as well, very often leading to spurious findings.

Our results have important implications for research on climate change. The majority of published palaeoclimate time-series are based on radiocarbon dates from archives like sediment cores (see the database at www.noaa.gov). With such a high false-positive rate, we can expect that at least some of the cycles identified in those datasets—e.g., Bond Cycles—could be false-positive findings. In fact, the proportion could be as high as 90%. The rate might also be higher since we used a lenient window

of error—i.e., ±20%—for counting a true positive finding. A narrower window of error would, of course, paint a worse picture. It is also important to remember that real climate data are much more complicated than a simple waveform plus red-noise. Using real-world data and not knowing exactly what cycle(s) to look for would make the process all the more difficult.

A corollary of this is that recent claims about cycles in human history should be reconsidered (e.g., Chase & Chase 2013, Gronenborn et al. 2014, Hodell et al. 2005a, Thompson & Turck 2009, Ur 2010, Wu et al. 2009, Zhang et al. 2006, Zimmermann 2012). Some of these studies used qualitative methods, which would lead to substantial error when attempting to identify cycles in palaeoenvironmental and historical data (e.g., Chase & Chase 2013, Gronenborn et al. 2014, Rosen & Rivera-Collazo 2012, Thompson & Turck 2009, Ur 2010, Zimmermann 2012). If a statistical approach is likely to yield spurious cycles, a qualitative approach is at least as likely to yield unreliable results, probably more so. The quantitative studies should be reevalauted, too, since none of them accounted for chronological uncertainty or analysed multiple times-series (Hodell et al. 2001, 2005a; Wu et al. 2009, Zhang et al. 2006). Considering the very high false positive rate we identified in our simulation, it seems more likely than not that a single analysis would yield a false positive finding. Thus, it is very likely that these studies' conclusions are based on spurious cycles.

With regard to future research, our results suggest a way to overcome the problems caused by radiocarbon dating uncertainty. The distributions of significant frequencies we found are mostly unimodal with the modes close to the underlying frequency of the known, artificial sine wave. This observation suggests that while a single analysis of a real climate record is likely to yield a spurious finding, multiple analyses of multiple records should converge to a modal frequency, which likely represents the true underlying cycle if there is one to find. We suggest that, in the future, multiple time-series of climatic observations should be collected whenever possible, whether that entails repeated observations from the same climate archive or measurements of the same proxy in multiple archives. That way, several time-series indicating the same underlying climatic phenomenon could be searched for cycles, increasing the likelihood that the results would converge to a modal frequency, or

frequencies in the case of multiple cycles. Exactly how many time-series would be required to find the mode or modes will be the subject of future simulations research.

Our results also indicate that investments in dating beyond about 15 dates per 1000 years should be distributed among several time-series. In our simulations, having more than 15 dates had relatively little impact on our ability to recover the known synthetic cycle compared to other factors, like the SNR. The reason for this likely has to do with the relationship between chronological uncertainty and the other sources of uncertainty in our simulation. Strictly speaking, additional chronological anchors will decrease the chronological variance in the age-depth model, but the gains in precision are not matched by equal gains in our ability to find cycles given the frequencies and error margins we included in our simulation and factors like the SNR. Thus, if chronometric resources are limited for a given project, it makes more sense to increase the number of dated time-series than to increase the number of dates per time-series beyond a density of about 0.015 per year.

5.6. Conclusions

Identifying cycles in palaeoclimate time-series data is important for understanding past, present, and future climate change and its effects on human societies. But, finding cycles in palaeoclimate records dated with radiocarbon—the most common dating method—is challenging because of the highly irregular uncertainty that is characteristic of calibrated radiocarbon dates. Therefore, understanding how its irregular uncertainty affects our ability to confidently identify climatic and cultural cycles, if they exist, is critical. With this in mind, we carried out a large simulation study in which we assessed the impact of radiocarbon dating uncertainty on Least-Squares Spectral Analysis, a method well suited for identifying cycles in archaeological and paleoenvironmental time-series. The simulation involved searching for a known cyclical pattern in several million synthetic time-series. Our main finding is that we could only correctly identify the known cycle 42% of the time under optimal conditions, which included a simple cyclical signal with very little noise, numerous complete oscillations of the underlying wave pattern, and a large number of radiocarbon dates from a high-slope portion of the INTCAL-13 calibration curve. Most of the time, however, the situation was

considerably worse. The majority of the experiments we ran resulted in only a 10% true positive rate, which means that 90% of the cycles we identified were false positives. Still, cycles probably do exist in the climate system and we need to identify them to build sound models of past, present, and future climate change. Fortunately, our simulation study also indicated that there are promising ways forward that might allow us to overcome the problems with radiocarbon date uncertainty, but more work needs to be done.

Chapter 6. The effect of radiocarbon dating uncertainty on the utility of the Poisson Exponentially Weighted Moving Average (PEWMA) time-series regression method for human-environment interaction research

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Statement of Contributions of Joint Authors

Carleton, W. (candidate): research design; data collection; data analysis; co-wrote the manuscript.

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6.1. Abstract

Statistical time-series analysis has substantial potential for improving our understanding of past human-environment interaction. However, radiocarbon dating—the most common chronometric technique in archaeology and palaeoenvironmental science—creates challenges for established statistical methods. These methods rely on the assumption that observations occurred at precisely known times, and this assumption is clearly violated when calibrated radiocarbon dates are used because they have highly irregular temporal errors. As a result, whether established time-series methods can be used on records dated with radiocarbon is unclear.

With the foregoing in mind, we conducted a large simulation study to investigate the impact of chronological uncertainty on a recently developed time-series method. The method is a type of regression involving a prediction algorithm called the Poisson Exponentially Weighted Moving Average (PEMWA). The method is designed for count time-series data, like the numbers of archaeological sites or artifacts or historical events, making it applicable to a wide range of potential questions about past human-environment interaction. Our main finding is that the PEWMA method is fairly robust to chronological uncertainty. When two time-series are correlated with a coefficient of 0.25, the method is able to identify that relationship correctly 20–30% of the time, providing the time-series contain low noise levels. With higher correlations of around 0.5, it is capable of correctly identifying correlations despite chronological uncertainty more than 90% of the time.

6.2. Introduction

This paper concerns the use of time-series regression in research on past human-environment interaction. The primary sources of information about past human and environmental conditions are the archaeological and palaeoenvironmental records, respectively. These records contain observations with an inherent temporal ordering. Thus, *time-series*—which are simply ordered sets of observations—can be easily compiled from the archaeological and palaeoenvironmental records. This means time-series regression methods could be used to improve our understanding of past human-environment interaction. However, chronological uncertainty in archaeological and palaeoenvironmental records could potentially complicate the use of the methods. In particular, the chronological uncertainty associated with the most common chronometric method used in the dating of both records—radiocarbon carbon dating—could severely undermine our ability to confidently identify statistical relationships between the records. This is because radiocarbon dates have highly irregular uncertainties associated with them that undermine the assumptions of many standard statistical methods, including time series analysis (Blaauw 2010, Blaauw et al. 2007, Carleton et al. 2014, Telford et al. 2004a,b). To investigate this possibility, we conducted a large simulation study in which we investigate the impact of radiocarbon dating uncertainty on a time-series

regression method called the Poisson Exponentially-Weighted Moving Average (PEWMA) algorithm (Brandt et al. 2000).

6.3. Background

Time-series data have to be analyzed carefully because the order in the sequence of observations matters. There are two traits a time-series can have that highlight the importance of the temporal ordering. One is *non-stationarity*, which describes time-series with statistical properties that vary through time (Pickup 2014). The mean or variance of the series, for instance, could be different in one interval than in another. This difference could come about because of a slowly varying trend or a discontinuity—i.e., a seemingly instantaneous jump or drop in the average. These types of changes lead to changes in the parameters of the statistical distributions that describe the time-series. So, the observations in one part of a series could have a different distribution than observations in another part, violating the common statistical assumption that observations are identically distributed.

The other troublesome trait is *autocorrelation*, which means the observations in the series correlate with themselves at a given lag (Pickup 2014). For example, the sequence of annual global average temperatures from 1950–2000 might correlate with the sequence of average temperatures from 1951–2001, indicating an autocorrelation with a lag time of 1 year. Autocorrelation can be caused by a trend or cyclical patterns, which can in turn be caused by some external force or by persistence in the phenomenon itself. Ambient temperatures, for instance, are in part autocorrelated because important environmental processes that affect temperature, like the Earth's distance from the sun, vary smoothly over time, causing temperature readings taken closely in time to be similar (Cronin 2013). But, they are also autocorrelated because temperatures today actually affect temperatures tomorrow—i.e., since half the Earth does not instantly lose its heat at sunset thereby resetting the temperature every day, any residual heat contributes to the next day's temperature (Trenberth et al. 2009). Regardless of what drives it, autocorrelation leads to dependence among the observations in a time-series, which—like non-stationarity—violates a common statistical assumption, namely that observations are independent.

Archaeological and palaeoenvironmental time-series typically have both traits (Carleton et al. 2014, Mudelsee 2014, Schulz & Mudelsee 2002). They will usually be non-stationary, since almost any environmental or cultural phenomenon you might think of changes over time—e.g., yearly temperatures, or population demographics. They will also typically contain autocorrelation. Thus, archaeological and palaeoenvironmental data can be expected to violate the assumptions of many classical statistical methods. Consequently, we need special methods to find correlations between past human and environmental conditions.

Fortunately, these methods already exist because statisticians, mathematicians, and engineers have been working with non-stationary, autocorrelated time-series for a long time (Klein 1997). As a result, many established time-series methods are designed specifically to handle non-stationary and autocorrelated data (Chatfield 2009, Mudelsee 2014, Pickup 2014). However, time-series of archaeological and palaeoenvironmental observations are idiosyncratic in another way that potentially undermines even these established methods—often we are uncertain about the times associated with archaeological and palaeoenvironmental observations (Aitken 1990, Bradley 2013, Cronin 2013). That is, the time-series contain *chronological uncertainty*.

Contemporary time-series are usually recorded at precisely known times, like stock prices or radio signals, because the observations are being made as the data are generated. In contrast, looking into the deep past involves reckoning at a distance through a foggy lense—i.e., there might be centuries or millennia between the observation time and the data generation time, and our estimates of the latter are uncertain. We usually make chronometric estimations by proxy using radiometric methods that rely on measuring isotopes of unstable elements that decay at a constant rate (Taylor & Aitken 1997). Despite the accuracy of some of these methods—like Uranium series dating with errors of ±10 or 20 years—even the best methods still yield dates with uncertainty. Consequently, many palaeoenvironmental and archaeological time-series contain temporal uncertainty.

The most common chronometric method, radiocarbon dating, is particularly problematic. Radiocarbon dates have to be calibrated to account for historic changes in

levels of environmental radiocarbon isotopes. The calibration process results in chronometric errors that are highly irregular and entail ranges of potential dates that can span centuries (Bronk-Ramsey et al. 2006, Buck et al. 1996, Ramsey et al. 2006, Telford et al. 2004a,b). Point estimates—i.e., mean ages—cannot be used to describe these highly irregular distributions because they often contain multiple modes and are highly skewed. Most statistical methods are, therefore, undermined by calibrated radiocarbon dating because most methods rely, at least to some extent, on point estimates. Time-series methods are no different, raising concerns about our ability to use them for identifying correlations between archaeological and palaeoenvironmental time-series.

In our study we used a recently developed time-series regression method called the Poisson Exponentially Weighted Moving Average (PEWMA) (Brandt et al. 2000). As the name implies, the PEWMA algorithm estimates a regression model for Poisson processes—i.e., a process that produces a series of integer numbers. Importantly, the method accounts for autocorrelation in the Poisson process, which is a critical trait of archaeological and palaeoenvironmental time-series, as we mentioned earlier.

The PEWMA method is useful for many archaeological, historical, and palaeoenvironmental applications because count data is a common in these fields—e.g., counts of artifacts, sites, or first appearance dates of species in the fossil record. One example of its potential use would be for testing hypotheses about the effect of climate change on population levels. A fairly prominent method for estimating past population levels in archaeology and palaeoecology is to use counts of radiocarbon dated contexts binned into given periods (Rick 1987). So, the resulting time-series can be interpreted as the number of archaeological contexts per period—e.g., the number of hearths, or burials, or occupation sites—which is used as a proxy for levels of overall human occupation intensity. The PEWMA method could be used to compare changes in the demographic proxy to changes in palaeoenvironmental time series data.

Another example involves testing hypotheses about historical processes using counts of historical events. For instance, we used the PEWMA method in a previous study to test the prominent hypothesis that climate change exacerbates conflict within and between human societies over the long term (Chapter 3). In that study, we

compared a time-series of Classic Maya conflict levels to several palaeoenvironmental proxies. The time-series of interest was a historical record of conflict events inscribed into monuments along with Classic Maya calendar dates. Using the PEWMA method, we compared the conflict record with several palaeoenvironmental records including temperature and rainfall proxies. We found that temperature was the only variable that appeared to correlate with conflict levels. Increases in temperature might have led to increases in conflict among the Classic Maya, we averred. However, while the conflict time-series contained very little chronological uncertainty, all of the palaeoenvironmental records were dated with radiocarbon. Thus, the palaeoenvironmental time-series we used as independent variables in our regressions definitely contained chronological uncertainty. This prompted us to ask how robust the PEWMA method is to chronological uncertainty.

The simulation we carried out involved creating thousands of pairs of artificial palaeoclimatic and archaeological time-series with known relationships and then testing for those relationships with the PEWMA algorithm. The regressions were set up with the synthetic archaeological time-series as the dependent variable and the synthetic palaeoenvironmental time-series as the independent variable. We used error-free dates for the artificial archaeological time-series so that we could limit the sources of error and see the effects more clearly. This analytical control also had the benefit of allowing us to compare the simulation results to our previous work on the Classic Maya (Carleton et al., 2014) because the dependent variable in that study was a historical record with very little chronological uncertainty. Thus, only the synthetic palaeoenvironmental time-series contained chronological uncertainty. To explore the effect of this uncertainty, we used a bootstrap. We resampled the set of synthetic calibrated radiocarbon dates used to date the palaeoenvironmental time-series thousands of times, running a separate PEWMA analysis each time. Throughout the simulation we varied several parameters while keeping everything else constant. The parameters included the variance of the time-series, the number of synthetic radiocarbon dates used to date the series, and the strength of the correlation between the artificial archaeological time-series and the synthetic palaeoenvironmental data. Varying the parameters allowed us to see how radiocarbon dating uncertainty in the palaeoenvironmental series affected our ability to find the known relationships between the time-series in each pair.

6.4. Methods

Using the R statistical programming language (R Core Team 2016), we ran a series of simulation experiments, each of which explored how a set of variables affected the outcome of a PEWMA regression analysis. The PEWMA algorithm is a special kind of time-series filter that can be used to model Poisson processes containing autocorrelation. Poisson processes produce integer count time-series (Kingman 1993), a very common type of time-series found in archaeological data as noted earlier—e.g., counts of sites per century or counts of animal bones per stratigraphic layer and so on. To model an empirical time-series, the PEWMA algorithm uses an *observe-then-predict* mechanism, which as the phrase suggests involves first observing some data and then making a prediction based on that observation. It filters through a given count series one observation at a time, updating its predictions for the next time based on previous observations. It can account for autocorrelation in the count data by discounting the information from older observations as it filters through the series, estimating the optimal amount of discounting to apply. More discounting implies less autocorrelation in the observed data because older values in the series have a lower impact on subsequent values—less discounting implies the converse, of course. The algorithm can also be fed covariates to see whether they improve its predictions of the time-series of interest. To estimate the statistical parameters for a model, the algorithm uses maximum likelihood, which means we can use Akaike's Information Criterion (AIC), a measure of information loss, to estimate the goodness of fit of a given model (Akaike 2011, Pan 2001, Wagenmakers & Farrell 2004). Models with a lower AIC involve less information loss, meaning they fit the observed time-series better.

With the simulation, we aimed to determine how calibrated radiocarbon dating affects the PEWMA algorithm. To do so, we ran a series of experiments involving a set of fixed parameters that were the same for every experiment and a set of variable, or free, parameters. The experiments proceeded in several steps.

First, we created 1000 synthetic palaeoenvironmental time-series spanning a thousand-year period, from 12000 to 13000 calibrated years BP, a fixed parameter of the simulation. We created the observations in each series using a linear function with a

slope of 0.01, also a fixed parameter. This function was chosen to simulate an environmental process that increased gently over the 1000-year period of the series—i.e., a synthetic environmental signal. We then added autocorrelated random error with a fixed autocorrelation of 0.7, creating noise in the synthetic environmental signal. The autocorrelated noise was generated using an R function called *arima.sim*. In each experiment, we controlled the amount of noise by tuning the standard deviation of the arima.sim function. The standard deviation could vary freely between one of three values, namely 1, 0.1, and 0.01. Increasing the standard deviation increased the level of noise, thereby decreasing the *signal-to-noise* ratio of the synthetic palaeoenvironmental observations—i.e., the variance of the autocorrelated noise increased relative to the variance of the signal. We then dated the observations by selecting radiocarbon dates from the INTCAL-13 calibration curve from 12000–13000 BP (Reimer et al. 2013). There could be 5, 15, or 25 dates evenly spaced along the calendrical time axis of the curve, a free parameter intended to help us determine whether having more dates improved our regression results. To derive dates in radiocarbon time, we looked up the radiocarbon dates in the curve that corresponded to the calendrical dates, a process sometimes called *back-calibration*. Those back-calibrated dates became the synthetic radiocarbon assays for the time-series. They stood in for the uncalibrated radiocarbon measurements that we might receive from a dating lab in a real investigation. We then set the error of those simulated radiocarbon dates to a standard deviation of ± 50 years, a fixed parameter corresponding to a common magnitude of error returned by dating labs. Setting these errors to a constant value was necessary to isolate the errors introduced by calibration—i.e., the irregular errors we were interested in.

In the second step, we created 1000 synthetic archaeological time-series using a PEWMA filter in reverse. Instead of iterating over an existing count time-series to estimate its statistical parameters, you can use the algorithm to produce a time-series by feeding it the parameters you want the series to have. So, to simulate an archaeological process that was affected by environmental conditions, we fed in each of the synthetic environmental series created in the previous step. To do that, we sampled each 1000-year environmental series 200 times at regularly spaced intervals and used them as covariates in the creation of 1000 PEWMA count time-series, creating 1000 time-series pairs. By tuning the correlation parameter, we could test whether the strength of the

correlation between the environmental series and its paired count series affected our results given radiocarbon dating uncertainty. The correlation parameter varied between 0.75, 0.5, 0.25, and 0—the last of these indicated no correlation allowing us to estimate the false positive error rate of the simulation. The PEWMA filter also allows you to set the autocorrelation parameter for the count series. This parameter indicates the degree of persistence in the underlying Poisson process—i.e., the degree to which future values are dependent on previous ones. We fixed this parameter at 0.6 for the simulation, corresponding to the default settings for *Pests*, the R software package written by the developer of the PEWMA method (Brandt et al. 2000).

Then, in the third step, we created 2000 age models for each of the 1000 synthetic environmental series. Most palaeoenvironmental time-series are dated with age models—i.e., mathematical interpolations between chronometric estimates anchored to certain parts of a series (Aitken & Stokes 1997, Bradley 2013). The most common kind of age modeling involves sediment depths and radiocarbon dates. To date a time-series of observations from a lakebed sediment core, for example, palaeoenvironmental scientists interpolate between calibrated radiocarbon dates from a set of carbon samples at different depths along the core. The depth of the carbon sample and its calibrated date become chronological anchors. By relating the age of the carbon sample to its depth, the ages of the layers between the anchors can be estimated. To simulate this process, while accounting for chronological uncertainty we used a bootstrap. The bootstrap was necessary because the mean, or any other point estimate, is insufficient for describing the true underlying date of a calibrated radiocarbon date distribution (Mudelsee 2014). This is because the calibration process yields highly irregular, multimodal distributions (Bronk-Ramsey et al. 2006, Buck et al. 1996, Ramsey et al. 2006, Telford et al. 2004a,b). Consequently, multiple equally likely age models are possible, meaning that choosing only one model results in a highly biased estimate of the ages associated with each observation in a given time-series. So, using a bootstrap allowed us to explore the effect of that bias by exploring the different likely age depth models that could be produced using a set of calibrated radiocarbon dates. The bootstrap involved calibrating the synthetic radiocarbon dates from the first step using R and then randomly sampling the calibrated distributions. We sampled them with replacement using a Gibbs sampler (Buck et al. 1996, Geman & Geman 1984)—a tool

that allowed us to randomly sample a sequence of radiocarbon dates with the constraint that the order of the dates in the time-series had to be preserved, mimicking sedimentological relationships among them. Then, we used a monotonic spline to interpolate between the sampled radiocarbon dates, assigning a time stamp to each of the observations in a given synthetic environmental series. In the end, for every experiment, involving every combination of fixed and free parameters, we had an ensemble of 2000 synthetic environmental time-series paired with each simulated archaeological count time-series.

In the last step of each experiment, we used the PEWMA algorithm to create regression models with the synthetic archaeological series as dependent variables. For each archaeological series, we created 2000 PEWMA models. In each model, a given archaeological series was compared to one of the environmental series from its partner bootstrap ensemble. So, since each of the 1000 archaeological series was paired to an ensemble of 2000 bootstrapped environmental series, we ran a total of 2,000,000 PEWMA analyses for each experiment. In each analysis, a given synthetic environmental series was used as a covariate for predicting its partner archaeological series. To determine whether including the environmental series improved a given model, we created another PEWMA model for each archaeological series that included only a constant and no covariate. The models with no environmental covariate acted as *benchmarks* for identifying statistically significant results. We reasoned that if the AIC of a given model with an environmental covariate outperformed its benchmark, the PEWMA algorithm had successfully identified the underlying correlation—or, in the case of no underlying correlation, erroneously identified one. For each of the 1000 synthetic archaeological series, we had 2000 PEWMA results, which meant we could calculate the percentage of the analyses that yielded a positive result—i.e., the *hit rate*. We then tallied these percentages to create a distribution of hit rates for every experiment.

6.5. Results

Permuting all possible values for the free parameters yielded 36 experiments, the results of which are shown in Figures 6-1 to 6-4. There are several important patterns in these results. The least surprising pattern involves the correlation between synthetic

environmental and archaeological series. The correlation parameter had, by far, the clearest impact on hit rates. The method generally had a hit rate of less than 50% when the correlation was 0.25. Depending on the values of the other parameters, the hit rate varied between 20 and 40%. But, when the correlation increased to 0.5 or higher, the hit rate rose as high as 90% in experiments where the signal-to-noise ratio (SNR) was 100. As the correlation increased, the modes of the hit rate distributions increased and the variances generally decreased, meaning the method consistently performed better in experiments with higher correlations. Thus, when the environmental impact was greater, the PEWMA algorithm was better able to identify the underlying correlation despite radiocarbon dating uncertainty. This was an unsurprising finding since, intuitively, stronger relationships should be easier to see.

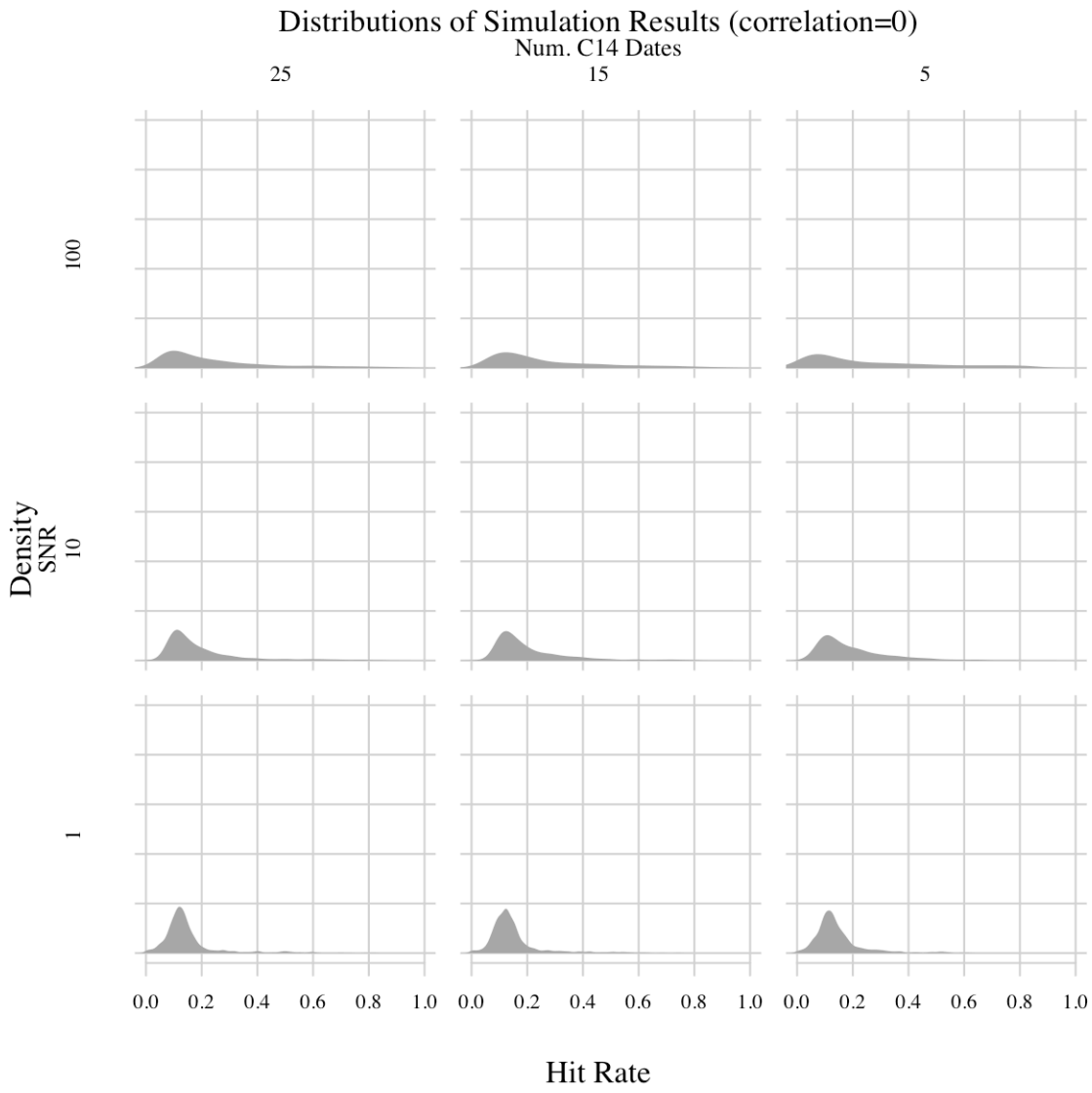

Figure 6-1 PEWMA simulation results; correlation = 0

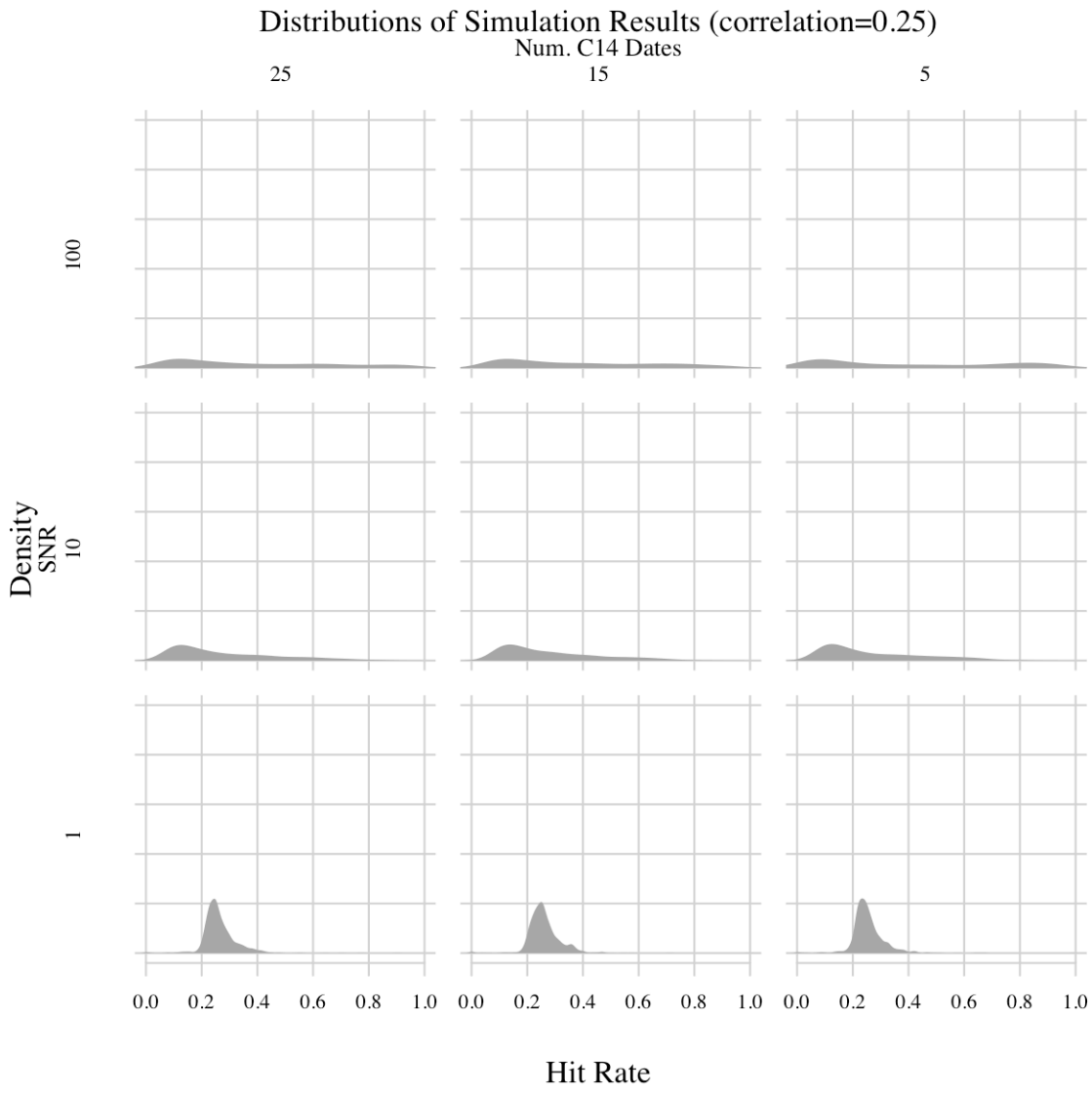

Figure 6-2 PEWMA simulation results; correlation = 0.25

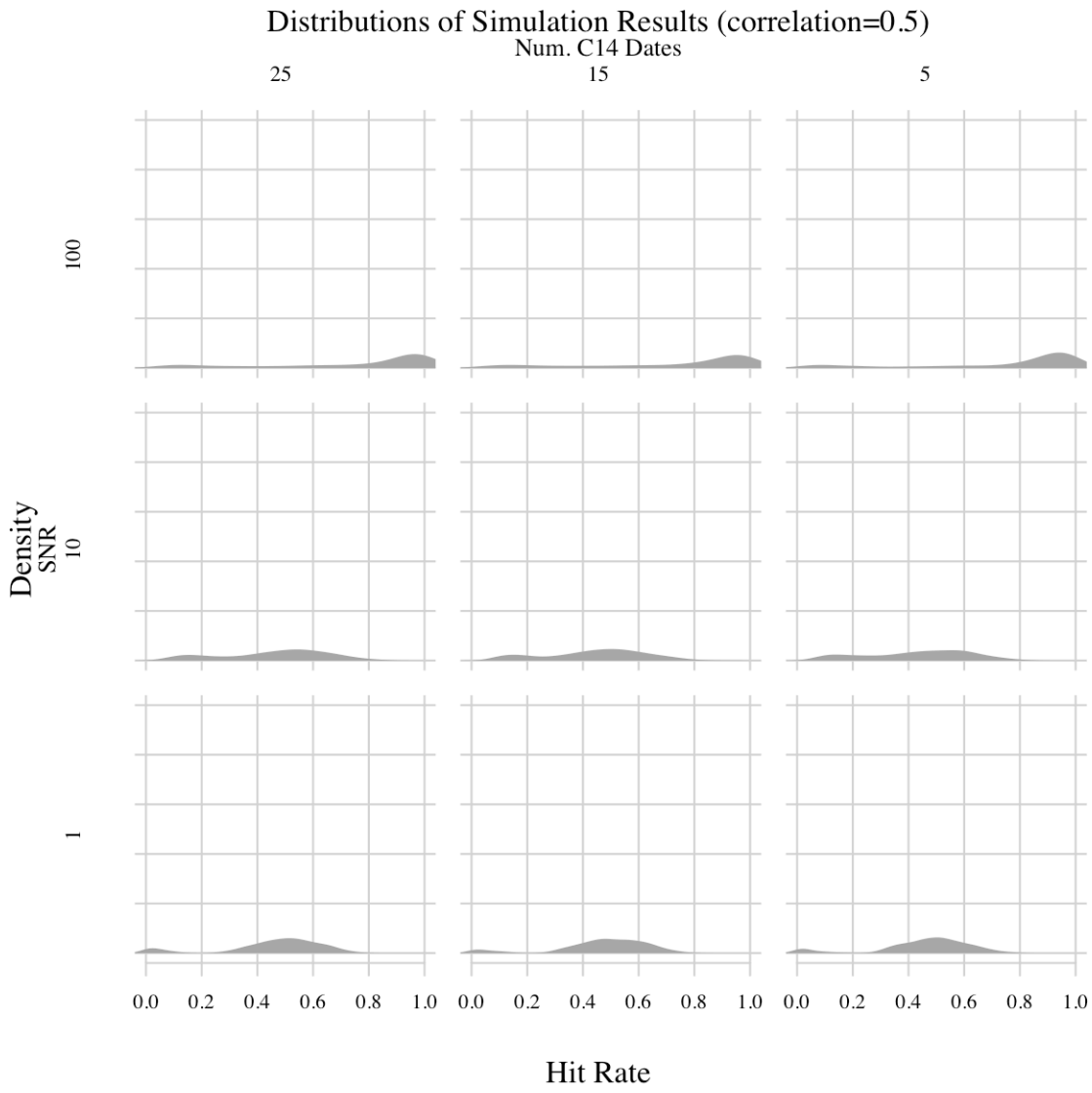

Figure 6-3 PEWMA simulation results; correlation = 0.5

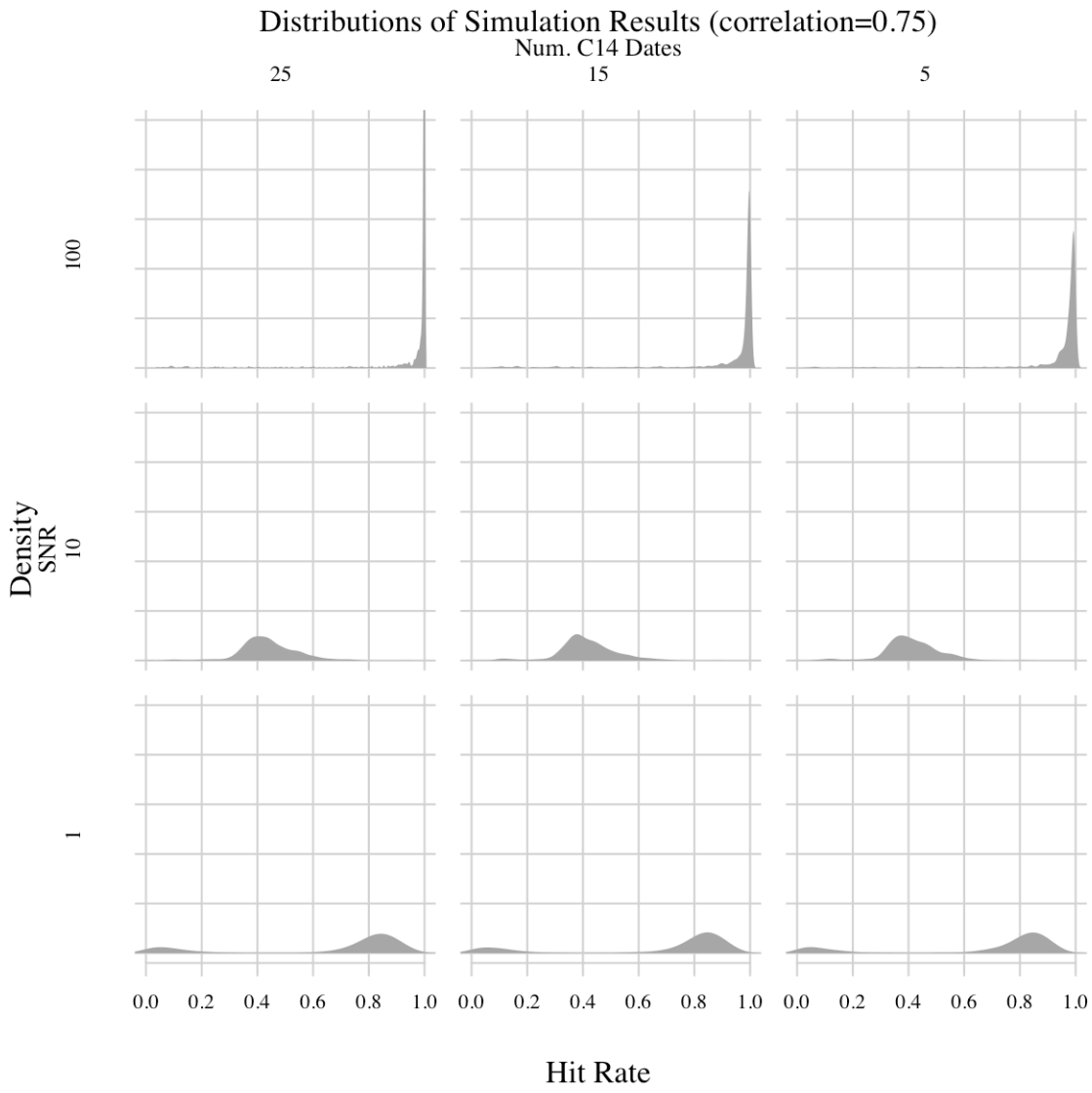

Figure 6-4 PEWMA simulation results; correlation = 0.75

Another unsurprising pattern involves the SNR. Holding the other parameters constant, we found that increasing the SNR from 10 to 100 generally improved the hit rate. When the SNR was 100, the PEWMA analysis was able to correctly identify the underlying correlation more than 80–90% of time in experiments with correlations of 0.5 or 0.75. Dropping the SNR to 10, though, reduced the hit rates. For the strongest correlation we explored—0.75—an SNR of 10 reduced the hit rate from greater than 80% to between 30% and 60%. For the lower correlation values, the hit rate was

similarly reduced, but the distribution was also spread out across a greater range of values, indicating more variability in the hit rate as the SNR decreased. This finding makes sense since the climate data would be noisier, leading to a less clear relationship between the synthetic environmental series and the synthetic archaeological series.

Lowering the SNR further to 1, though, yielded ostensibly counterintuitive results. Interestingly, the hit rates seemed to improve somewhat. For example, in experiments where the correlation was 0.75, reducing the SNR to 1 increased the mode of the hit rate distribution to more than 80%. At first glance, this result would suggest that noisier environmental data somehow made it easier to see an underlying correlation. But this effect was caused by the fact that the autocorrelated noise we added to the main climate signal was included in the creation of the synthetic archaeological count data. So, increased environmental noise translated into increased noise in the archaeological data, too. Thus, when the correlation of a given experiment was strong, the increased variance of the environmental data resulted in higher overall co-variance of both time-series—both were noisy, but strongly correlated. Consequently, the primary mode of the hit rate distribution shifted upward. Still, the hit rate distributions generally show higher variance as the SNR decreases, even in experiments with high correlations, which is more in line with the expectation that more noise should make it harder to see underlying relationships. In addition, a second mode appeared in the experiments with SNRs of 1 and correlations of 0.5 or 0.75. That smaller secondary mode in the hit rate distributions was much lower, around 10% or less. It indicates that the chances of failing to see the underlying correlation increased with very low SNR values, even in experiments with high correlations. So, overall, the effect of SNR values on the simulation was as expected, namely that more noise reduced the power of the method.

One surprising pattern involves the false positive rate of the PEWMA method. By setting the correlation of some experiments to zero, we were able to determine how often random variation resulted in spurious correlations. Overall, the modes of the hit rate distributions hovered around 10%, irrespective of the experimental parameters. Thus, the most common false positive hit rate for the PEWMA method appears to be around 10%. This false positive rate was unexpectedly low. Given the impact of radiocarbon dating uncertainty on other time-series methods we have explored (see

Chapters 3 and 5), we were expecting to see more spurious correlations. So, a false-positive rate of about 10% seems very low—quite acceptable for archaeological applications. The hit rate distributions, however, are skewed to the right for experiments with higher SNRs, indicating greater numbers of spurious correlations. This finding makes sense considering those experiments involve synthetic environmental series with a straight, clearly increasing trend—i.e., strong signals with low noise. Holding that trend stable while allowing the synthetic archaeological series to fluctuate around it increased the chances that the two would align by chance. If, in contrast, the environmental series fluctuated more, we would expect to see fewer hits because chance concordances would occur less often. Indeed this is what we see. Decreasing the SNR led to noisier environmental series, which spuriously correlated with the synthetic archaeological series less often—like a mounted archer trying to hit a moving target instead of a stationary one. Despite the difference caused by the SNR, though, the primary result is that the frequency of spurious correlations appears to have been low throughout the simulation, around 10%, even after accounting for radiocarbon dating uncertainty.

The last pattern is also surprising and it involves the number of radiocarbon dates. Surprisingly, the number of radiocarbon dates used to date the time-series had little effect on the experimental hit rates. Irrespective of the correlation and signal-to-noise ratios, the distributions of hit rates were almost identical whether the series were dated with 5, 15, or 25 synthetic radiocarbon dates. So, increasing the number of radiocarbon dates is unlikely to affect the accuracy of a PEWMA regression analysis even when using a bootstrap to account for dating uncertainty. This is quite surprising given our previous experience with radiocarbon dating uncertainty and its negative impact on time-series analyses (see Chapters 3 and 5). In a sense, the lack of significant effect is quite good news. It suggests that testing hypotheses involving regression models is possible despite the uncertainty introduced by radiocarbon dating, which as we stated earlier is the most common dating method in archaeological and palaeoenvironmental applications.

6.6. Discussion and Conclusions

Time-series analysis has considerable potential to help us understand past human-environment interaction. However, its application and ultimate use could be undermined by the widespread reliance on calibrated radiocarbon dates for age-depth models. Calibrated radiocarbon dates have highly irregular uncertainties, as we mentioned earlier, that fail to meet the assumptions of many statistical methods. These highly irregular uncertainties might make it very difficult to find correlations between archaeological and palaeoenvironmental records.

With this in mind, we conducted a large simulation study in which we explored the effect of calibrated radiocarbon date uncertainty on a recently developed Poisson regression-based method for time-series regression called PEWMA. To test the effect of calibrated radiocarbon date error on the PEWMA method, we simulated thousands of archaeological and palaeoenvironmental time-series with known correlations and then analysed them with the PEWMA algorithm. The simulation involved bootstrapping synthetic calibrated radiocarbon dates used to date the simulated palaeoenvironmental time-series. We resampled the date distributions thousands of times rerunning the PEWMA regression each time to see if changing the dates affected the regression results.

Our results show that the PEWMA method is fairly robust to chronological uncertainty from calibrated radiocarbon dates. Overall, the simulation showed that a real underlying correlation between the synthetic time-series could be identified 20–90% of the time depending on the combination of simulation parameters in a given experiment. The most likely cases, with the most realistic parameters, yielded true positive rates of around 30–50%. Thus, the PEWMA method was often able to successfully identify an underlying correlation despite the presence of chronological uncertainty in the synthetic palaeoenvironmental time-series.

Two of the main findings of the simulation are important to note even if they are unsurprising. One is that the method was better able to identify the underlying correlation when the synthetic environmental series had a lower noise-to-signal ratio. The other is that stronger underlying correlations are easier for the PEWMA method to identify. When

the correlation was 0.25 the hit rate was between 20% and 40%, increasing to as high as 90% with correlations of 0.5 and 0.75.

The third main finding—that the false positive error rate of the method is roughly 10%, on average—is more surprising. We were expecting the highly irregular chronological errors of radiocarbon dates to warp the time-series in ways that could cause many spurious correlations and therefore a high false positive rate. Instead, the 10% false-positive rate suggests that finding spurious correlations is actually unlikely—in the context of archaeological research at any rate.

The last, and perhaps most surprising finding, was that increasing the number of radiocarbon dates used to date the time-series had no noticeable effect. The simulation results were largely consistent whether 5, 10, or 15 radiocarbon dates were used. This was surprising because it seems like adding more dates should reduce chronological uncertainty by increasing the number of chronological anchors for the age-depth models. Thus, we expected that more dates would improve our ability to find underlying correlations. The counter-intuitive result raises an important question about how chronological uncertainty affects the PEWMA method.

One possible explanation is that chronological uncertainty is not relevant at all because using more dates seemed to have no impact on the results. This possibility, however, can be easily dismissed by looking at the results of a single bootstrap iteration. Recall that the simulation was broken down into experiments. Each experiment involved a combination of simulation parameters that was constant throughout a given experiment. Within each experiment, 1000 pairs of synthetic time-series were analyzed using the PEWMA algorithm. Let us call them *top-level pairs*. Each top-level pair was subjected to a chronological bootstrap, which resulted in 2000 *sub-pairs* of time-series. Each sub-pair only differed from the others because different chronological anchors—i.e., dates sampled from calibrated radiocarbon date distributions—were used to create their age-depth models. So, if chronological uncertainty was irrelevant, we would expect the PEWMA analysis results to have been identical between sub-pairs. That is, we would expect that the PEWMA method would either succeed *or* fail 100% of the time for a given top-level pair because the sub-pairs only differed due to chronological

uncertainty. What we saw instead was that each top-level result was a percentage ranging from zero to one, indicating the percentage of the 2000 sub-pairs for which the PEWMA method was able to identify the underlying correlation. Therefore, we can be sure that chronological uncertainty had an effect, which means that another explanation is required.

A more likely explanation is that chronological uncertainty has an effect, but it is not as important as the other variables, namely the signal-to-noise ratio and the strength of the underlying correlation. So, large differences in the signal-to-noise ratio and the strength of the underlying correlation will mask the effect of chronological uncertainty to some degree. Consequently, had we included chronological uncertainty in the archaeological time-series as well as the palaeoenvironmental time-series, we might have seen a greater effect. So, to some extent, these results should be considered optimistic, since archaeological time-series generally do contain chronological uncertainty. Still, since the effect we see in the simulation results is small, similar amounts of chronological uncertainty in the archaeological time-series should be expected to only slightly decrease the true-positive rate of the PEWMA method.

Taken together, a low false-positive rate of 10% combined with a true-positive rate that ranges from 20%–90% suggests that the PEWMA method is suitable for archaeological research. A low false-positive rate means we are reasonably unlikely to be fooled into thinking correlations exist when they do not—i.e., the method has a high *specificity*, a statistical term describing the rate of true-negative findings. A high specificity is ultimately the most important trait when investigating past human-environment interaction since spurious correlations abound in the real world and filtering out unlikely hypotheses is an important part of scientific research. On the other hand, that we might miss important correlations because of chronological uncertainty is clearly a problem that should be addressed with more methodological work.

These findings have implications for our previous research on Classic Maya conflict (Chapter 4). As we explained earlier, the present simulation study compliments our earlier use of the PEWMA method for testing the hypothesis that climate change drove Classic Maya conflict. The conflict record we analysed in that paper was a set of

dated conflict events recorded by the Classic Maya in inscriptions on stone monuments at various civic-ceremonial centres in the Maya region. To look at changing levels of conflict through time, we binned the data into wider temporal intervals—i.e., intervals wide enough to show variation through time rather than a string of mostly zeros and ones corresponding to years with no conflict and years in which a conflict occurred, respectively. The interval we selected was 25 years, corresponding to intervals used in previous studies (e.g., Kennett et al. 2012). At the same time, we binned the palaeoenvironmental data so that we could look at the average temperature or rainfall proxy values in each temporal bin compared to the number of conflicts in each bin. However, binning data potentially introduces a bias often referred to as the "bin edge" bias (Silverman 1986). The bias comes from assuming the bin edges and widths are appropriate for describing the underlying patterns in the data. They might not be. Other bin edges and widths might also be appropriate, which means opting to use one set of edges and widths over another possible set introduces a bias. Changing the bin edge locations and widths could potentially change the results. To evaluate the effect of the bin edge bias, we conducted a sensitivity analysis in which we changed the locations of the bin edges several times and re-ran the PEWMA analysis. Changing the locations of the bin edges meant re-binning the conflict record, which allowed us to evaluate the impact of bin edge locations on our results. This sensitivity analysis indicated that our primary finding, that increases in temperature corresponded to increases in conflict at the centennial scale, was largely unaffected by bias in the temporal bins. But, more importantly, the bin shifting exercise also allowed to us to explore some of the effect of chronological uncertainty on our findings.

The sensitivity analysis allowed us to evaluate some of the impact of chronological uncertainty on our PEWMA analysis by comparing chronological *what-if* scenarios. Re-binning the data caused some data points to move into neighbouring bins, which was like assuming that a given observation was dated to an earlier or later interval. So at the same time that the re-binning exercise was looking for the impact of using arbitrary temporal bins, it was evaluating what would happen if some palaeoenvironmental observations were dated to times other than those we assumed at first. However, it was a fairly limited evaluation of the PEWMA method. The present simulation looked specifically, and more completely, at the effect of chronological

uncertainty in the palaeoenvironmental time-series by performing bootstraps to evaluate a very large number of what-if scenarios. The results suggest that the PEWMA method is robust to chronological uncertainty—in fact, chronological uncertainty appears to be the least important of the parameters we investigated. Thus, we can be more confident that our findings in the Classic Maya case study were not the result of chronological uncertainty.

To appreciate the implications of our simulation results more generally, we can think in terms of conducting *blind* analyses—i.e., real studies with no prior information about the existence, or non-existence, of an underlying relationship between human and environmental conditions. So, imagine we set out to conduct a real analysis and planned to use the PEWMA regression method. Our simulation suggests that having at least 5 radiocarbon dates with which to date the palaeoenvironmental series is sufficient. Spending resources on more dates would likely make little difference in the results. This means, for instance, that most of the readily available online palaeoenvironmental time-series come with sufficient numbers of radiocarbon dates to create reliable PEWMA models. The largest, and most popular, online source for palaeoenvironmental time-series is the NOAA website (www.noaa.gov). Perusal of their catalogue revealed that many of the time-series they curate come with more than five radiocarbon dates. Consequently, our hypothetical analysis could involve the existing palaeoenvironmental data, and if we need to gather a new dataset our chronometric costs would be low.

We could also be confident that our PEWMA analysis would be able to identify an important relationship if it existed, at least much of the time. Since correlations of 0.25 or greater were recoverable between 20% and 90% of the time, failing to find a relationship could suggest that there was no important relationship to find. So, if we hypothesized that rainfall variation, for instance, was strongly correlated to the rise and fall of Classic Maya socio-political complexity, then the PEWMA method should be able to identify such a relationship given a proxy time-series for past rainfall and one for socio-political complexity. If it failed to identify a relationship, one possible reason is that the correlation is quite low, at least according to our simulation results. Thus, failing to find a correlation might simply indicate that the underlying relationship is not very important anyway, falsifying the hypothesis that a strong relationship existed. On the

other hand, for low to moderate correlations the method could miss a true relationship 50% of the time or more. A simple way to overcome this problem would be to test the hypothesis with additional time-series since that would increase the chances of finding a true-positive correlation. So, with some replication we could be fairly confident in our findings.

However, as a cautionary note, our simulation results also imply that one in ten positive results might be spurious. There are at least two obvious ways to control for false positive findings. One is to use a more stringent test for significance. Since the PEWMA method we used relies on comparing AICs to determine when a significant relationship has been identified, we could change the baseline for significance from identifying AICs that are strictly lower than a benchmark AIC to a baseline that required AICs to be lower by some predetermined amount. The other way to control for false positives would be to conduct replication studies. So, for the hypothetical blind analysis we would have to gather multiple archaeological and palaeoenvironmental time-series containing observations of the same underlying phenomena—e.g., multiple proxies for Classic Maya socio-political complexity and multiple proxies for past rainfall. Then, we would re-run the PEWMA analysis and make a decision about our hypothesis on the basis of multiple results taken together, instead of relying on a single comparison. Overall, though, a false positive error rate of 1 in 10 seems acceptable for archaeological research. So, while we ought to make attempts to control for the false positive findings, our simulation results suggest that the PEWMA method is adequate for archaeological purposes. It has a 90% chance of correctly determining that no relationship exists—i.e., a high specificity—if there is no underlying relationship and only a 10% chance of spuriously identifying one.

Overall, our simulation results indicate that the PEWMA method is a promising time-series analysis tool for archaeological and palaeoenvironmental research. The method is suitable for analysing any archaeological count time-series, which potentially includes a wide range of archaeological proxies for past human behaviour and conditions. It performs well even with relatively few radiocarbon dates—only five dates for a time-series 1000 years long. Therefore, we can make use of many of the published palaeoenvironmental time-series readily available online and maintain low chronometric

costs when gathering new data. The method can also reliably find moderate to strong correlations between archaeological and palaeoenvironmental time-series when the latter have a strong signal. Thus, we think that the PEWMA method can contribute substantially to research on past human-environment interaction.

There are at least two important avenues for future research to explore. One involves looking at the effect of calibrated radiocarbon date uncertainty on the dependent—i.e., response—variable. We chose to focus on chronological uncertainty in the palaeoenvironmental data in order to limit the sources of error in the simulation and see the effects of chronological uncertainty as clearly as possible. However, most archaeological time-series will likely contain chronological uncertainty, usually from radiocarbon dating. While we suspect the effect of additional radiocarbon dating uncertainty in the response time-series to be small—since the overall effect of chronological uncertainty appears to be small—it would still be prudent to investigate it further. So, future research should involve simulations that look at how the PEWMA method performs when both the response and predictor time-series are dated with radiocarbon.

The other important avenue for future research involves exploring the impact of changing temporal scales on the PEWMA method. In this study, we effectively used an annual resolution for the time-series, but very often archaeological and palaeoenvironmental data have different resolutions. Many modern palaeoenvironmental records boast annual resolutions, for example, while most archaeological time-series will have much coarser resolutions. Consequently, we have to change the resolution of one or both time-series in order to perform analyses. Future research, therefore, should explore the effect of changing the resolutions of the independent and dependent time-series to match each other. Exploring these two potential research avenues would help us to determine the limits of the PEWMA method, a method with considerable potential to deepen our insights into past human-environment interaction.

Chapter 7. Discussion and Conclusions

The archaeological and palaeoenvironmental records are the primary sources of information about long-term human-environment interaction. With the potentially dire impacts of modern climate change expected to continue for centuries (Collins et al. 2013), we need to use these records to better understand the potential long-term impacts of climate change on human societies. Improving our understanding of past human-environment interaction could help us plan for the future and impel political leaders and the public into action, as numerous scholars have pointed out (e.g., Bryson 1994, Butzer 2012, Caseldine & Turney 2010, Costanza et al. 2007b, de Menocal 2001, Kirch 2005, Mitchell 2008, O'Sullivan 2008, Van de Noort 2011, van der Leeuw et al. 2011). However, the idiosyncrasies of archaeological and palaeoenvironmental data pose special challenges that currently limit our ability to see quantitative evidence of long-term human-environment dynamics. Most importantly, they contain temporal autocorrelation and chronological uncertainty, both of which have the potential to greatly complicate quantitative analyses. In the course of my PhD, I carried out four studies to better understand how these idiosyncrasies affect a key analytical approach—time-series analysis. Each study produced several findings with implications for the methods used in each case. In addition, the first two studies have important implications for our understanding of the history of human-environment interaction among the Classic Maya—a well-known case study on socioeconomic and political collapse potentially driven by human-environment interaction. In this chapter I consider the main conclusions of the four studies and suggest ways to mitigate the effects of temporal autocorrelation and chronological uncertainty in future studies.

7.1. Temporal Autocorrelation

Temporal autocorrelation is prevalent in both the archaeological and palaeoenvironmental records. As explained in Chapter 1, the term "temporal

autocorrelation" denotes a correlation between a given time-series and itself at one or more time lags (Chatfield 2009). In terms of the process that generates a time-series—often called the *data generating process* (Pickup 2014)—each new data point is affected by the previous values in the series. Thus, an autocorrelated time-series follows a pattern whereby observations close together in time are more similar than those further apart. Consequently, individual observations in a time-series with temporal autocorrelation are not statistically independent (Chatfield 2009, Pickup 2014). Knowing the value of the series at one time gives us information about the nearby values. Failing to account for the lack of independence can lead to two major problems: 1) over-estimation of the statistical significance of a given result, and 2) inefficient parameter estimation in regression models.

In Chapter 3, I tested the prominent hypothesis that cyclical droughts from approximately 600-1100 CE caused periodic social upheavals among the Classic Maya. The hypothesis was developed on the basis of a time-series analysis of a palaeoenvironmental drought record from Lake Chichancanab near the middle of the Yucatan Peninsula (Carleton et al. 2014; Hodell et al. 2001, 2005a). In the original analyses, the authors identified one primary cycle in the drought record with a periodicity of around 208 years. This cycle was identified as statistically significant using a standard approach called the Blackman-Tukey (B-T) method (Blackman & Tukey 1958). However, the authors did not account for two sources of autocorrelation, leading me to hypothesize that they may have overestimated the statistical significance of their findings.

The first neglected source of autocorrelation was natural. Since the rainfall amount at one time is generally more similar to the amount of rainfall at nearby times than distant times, the drought record contained natural autocorrelation, which shows up as low frequency peaks in the power spectra produced using the B-T method. The usual way to account for this is to use a null hypothesis that includes the expectation that the low-frequency end of the spectrum is going to be higher because of autocorrelation irrespective of the presence of real cycles (Ghil et al. 2002, Mann & Lees 1996, Schulz & Mudelsee 2002). The null hypothesis they used, however, did not include this expectation, increasing the likelihood of finding false low-frequency cycles.

The second neglected source of autocorrelation was their use of data interpolation. Since the B-T method cannot handle missing or irregularly spaced data in a time-series, the authors had to interpolate between the observed data points in the drought record. By its nature, interpolation increases the correlation between adjacent observations—the newly interpolated points are functions of their neighbours—making the whole series more autocorrelated (Rehfeld et al. 2011). This can be expected to have added autocorrelation to the natural autocorrelation of the drought record, inflating the low frequency peaks in the spectrum of the Chichancanab drought series. Together, the two unrecognized sources of autocorrelation might have undermined their analyses.

When I reanalysed the record, accounting for the autocorrelation, the 208-year cycle vanished. The frequency-based method I used is called Least Squares Spectral Analysis (LSSA) (Vanícek 1971). One of its advantages is that it can handle irregularly spaced data. So, there was no need to interpolate the series, meaning there was no artificial inflation of the autocorrelation. I also used a null hypothesis that reflected the expectation that some low-frequency peaks would be present because of natural autocorrelation—i.e., the *red-noise* null hypothesis (Schulz & Mudelsee 2002). Together these analytical decisions showed that autocorrelation was a significant confounding factor because the purported 208-year cycle in the drought record was not identified in the re-analyses. This highlights the need to account for autocorrelation in frequency-based studies. Failing to account for it will likely lead to finding spurious cycles in analyses of archaeological and palaeoenvironmental data.

As mentioned above, temporal autocorrelation also affects parameter estimation in regression models (Pickup 2014). When temporal autocorrelation is present but unaccounted for, the residuals in a regression—i.e., the difference between the observed data and the predictions of the statistical model—are likely to be autocorrelated too. Autocorrelation in the residuals usually leads to biased or *inefficient* parameter estimates. Inefficiency means that the error of the parameter estimate is large. So, rerunning the same analysis with the same set of predictor variables but different data could lead to different parameter estimates. To compensate, a large amount of data is required to estimate the parameters of the model accurately—i.e., the parameter estimation procedure makes an “inefficient” use of data.

Because the effect of autocorrelation on parameter efficiency in time-series models is generally well understood, there are regression methods designed to account for it (Chatfield 2009, Pickup 2014). One such method with many potential applications in archaeology and palaeocology involves the Poisson Exponentially Weighted Moving Average (PEWMA) algorithm. The Poisson distribution, as I explained in Chapter 4, is ideal for modelling count data because it allows only integer valued outcomes (Kingman 1993). Integer count time-series are ubiquitous in archaeology—e.g., numbers of sites or artifacts, or conflict events as was the case in Chapter 4. Thus, Poisson regression is very useful for archaeological and palaeoenvironmental analyses. But, archaeological and palaeoenvironmental time-series usually contain temporal autocorrelation, which violates the assumptions of classic Poisson regression. To account for this, the PEWMA algorithm employs a two-stage time-series filter and an exponential weighting scheme that represents the diminishing effect of a given observation on nearby observations. The resulting PEWMA model includes an estimate of the coefficients for any covariates, the amount of exponential weighting used to describe the autocorrelation, and Akaike's Information Criterion (AIC). The AIC is used to estimate the fit of the model, with lower AICs indicating a better overall fit (Akaike 2011).

With the PEWMA algorithm, I tested the prominent but contentious hypothesis that climate change increases conflict levels (see Buhaug et al. 2014, Gleditsch 2012, Hsiang & Burke 2014, Hsiang et al. 2013). Using the Classic Maya as a case study, I tested the hypothesis by comparing PEWMA models. As explained in Chapter 4, I created several models. The dependent time-series in each model was the number of conflicts mentioned in the Classic Maya epigraphic record every 25 years from approximately 300–900 CE. The covariate in each model was one of five palaeoenvironmental proxies—two for past temperatures and three for past rainfall. I then compared the models to a set of benchmark models in which no climate covariate was used in the PEWMA predictions. I reasoned that if climate change caused an increase in Classic Maya conflict, then the models with climatic covariates should have lower AICs, meaning they fit the conflict time-series better than the models with no climate covariates. To reiterate, the results indicate temperature increases corresponded to increases in conflict levels, but there was no evidence that changes in rainfall were a factor. Thus, I concluded that climate change—specifically temperature increases—had

a significant impact on Classic Maya conflict, supporting the more general hypothesis that climate change exacerbates human conflict. Still, the benchmark models involving no climate variables did, in fact, produce predictions that were reasonably close to the empirical conflict time-series. So, I also noted that past conflict levels appear to be a useful predictor for Classic Maya conflict—i.e., autocorrelation appears to be an important source of information in the conflict record. These findings underscore the importance of accounting for temporal autocorrelation and using appropriate statistical methods. They also demonstrate that the PEWMA method is well suited to handling archaeological integer count time-series that contain autocorrelation.

In sum, the first two studies (Chapters 3 and 4) identified the effect of temporal autocorrelation on archaeological and palaeoenvironmental time-series analysis. Temporal autocorrelation can impact our ability to correctly identify statistically significant results and create patterns in time-series that complicate regression models leading to inefficient parameter estimates with high degrees of error. Fortunately, there are methods available to handle it. We can adjust null hypotheses to account for temporal autocorrelation and avoid methods like interpolation that artificially inflate it. We can also use regression models that not only account for temporal autocorrelation, avoiding inefficient parameter estimates, but also help us understand its relevance for our interpretations—e.g., the PEWMA models we created shows that past conflict levels can drive future conflict levels. Thus, while temporal autocorrelation can create challenges for time-series analysis in archaeological and palaeoenvironmental research, it can be managed because appropriate methods already exist.

7.2. Chronological Uncertainty

The other major idiosyncrasy of archaeological and palaeoenvironmental data--chronological uncertainty--is more problematic. Chronological uncertainty has always been challenging for archaeological and palaeoenvironmental research. Regardless of the chronometric methods used, our estimates for the dates of past events are always uncertain (Aitken 1990, Bradley 2013, Bronk Ramsey 2008, Taylor & Aitken 1997). As the four papers of this dissertation demonstrate, chronological uncertainty creates special challenges for time-series methods. Yet, the impact of chronological uncertainty

of time-series analysis has been given little attention in the academic literature until very recently (e.g., Mudelsee 2014). It seems likely that this is because most time-series methods were developed to analyse modern time-series data, which contain no significant chronological uncertainty. Unlike the chronometric methods we use to date past events, we can "time-stamp" modern time-series data with accurate clocks, so the effects of chronological uncertainty are negligible. As a result, the scholars developing time-series methods have not needed to take it into account. However, given the importance of understanding long-term human-environment interaction and the ubiquitous presence of chronological uncertainty in the archaeological and palaeoenvironmental records, it is crucial that we investigate how that uncertainty affects our analyses and look for ways to mitigate those effects.

Different chronometric methods come with different kinds and degrees of uncertainty (Aitken 1990). The most common method in archaeological and palaeoenvironmental research is radiocarbon dating. It has been an essential tool for research into past people and environments since its development in the mid-20th century. Unfortunately, it has highly irregular uncertainties that can complicate time-series analysis Since its use is so widespread, three of the four studies in this dissertation were aimed specifically at investigating its impact on time-series methods.

As mentioned in Chapter 1 and explained briefly in Chapters 3 and 4, radiocarbon dating uncertainties are highly irregular because of the calibration process (Bronk Ramsey 2008, Buck et al. 1996, Parnell et al. 2011, Ramsey 2009, Ramsey et al. 2006, Taylor & Bar-Yosef 2016). To reiterate, every radiocarbon date comes with an estimate of instrument uncertainty—i.e., measurement error. These uncertainties are distributed normally and are often quite small because of the high precision of modern chronometric methods. But, "raw" radiocarbon dates have to be calibrated to account for changes in the ratio of the relevant carbon isotopes in the atmosphere through time. The calibration curve—a proxy for historic levels of carbon isotopes—has two important features that create irregular calibrated date distributions. One is that it has a variable slope with some regions of very shallow and others steep. Shallow sloped areas tend to produce dates with wider uncertainties while steeply sloped areas produce dates with narrower uncertainties (Buck et al. 1994). Moreover, the slope varies continuously,

skewing the calibrated date distributions in irregular ways. The other feature is that the curve contains many rapid short-term fluctuations. These “wiggles” are short-term reversals in isotope levels that are primarily responsible for the creating multiple modes in the calibrated date distributions (Bronk-Ramsey et al. 2006, Buck et al. 1996). Together, the variable slope and wiggles in the calibration curve produce calibrated date distributions that are highly irregular—i.e., skewed and multimodal.

The highly irregular distributions of calibrated radiocarbon dates create two major problems for statistical analysis. One is that point estimates cannot be used (Carleton et al. 2014; Telford et al. 2004a,b). Because they are so irregular, standard statistical measures of central tendency cannot be used to describe the underlying date estimated by the calibrated radiocarbon date distribution. Point estimates, like the mean, often do not describe the location—i.e., highest probability date—of the distribution well enough. The highest probability regions of a given calibrated radiocarbon date distribution might not contain the mean of the date range covered by that distribution. In which case, the highest probability date is something other than the mean date. Even a weighted average that takes the relative differences in the probability of various dates into account will often not represent the actual underlying date of a given event. Moreover, using a point estimate, like a mean, ignores the chronological uncertainty expressed by the calibrated date distribution. That there is a distribution indicates uncertainty and, so, using a single value is effectively pretending otherwise. Thus, point estimates are deficient descriptors for calibrated radiocarbon dates. Any statistical calculations that rely on the point estimates like the mean are, therefore, very challenging to implement and their results are difficult to interpret.

The other problem is that the irregularities of calibrated date distributions also undermine the Central Limit Theorem (CLT), a foundation of many statistical methods (Heyde 2006). The CLT basically states that under certain conditions the mean of a large number of samples will be approximately distributed normally (Moore et al. 2015). The theorem, when it holds, indicates that the averages of multiple samples drawn from a given parent population will be normally distributed. Moreover, the mean of that normal distribution will converge to the population mean as more samples are included. This tendency allows for assumptions to be made about the properties of samples and

populations that enable a host of statistical procedures and interpretations—e.g., standard ordinary least squares regression. In the case of calibrated radiocarbon dates, however, the theorem does not hold. Resampling a calibrated radiocarbon date distribution many times does not result in convergence to a single value because, as I stated, the mean cannot be used as a measure of central tendency and neither can any other point estimate. As a result, many methods that rely on the CLT for calculating statistics and parameter estimates cannot be used. Together, these problems complicate age-depth models, which are integral to time-series analysis.

Age-depth models establish the chronological relationships among archaeological and palaeoenvironmental observations contained in different layers of sediment (Bradley 2013). Since the data do not come out of the ground conveniently time-stamped, we have to use the relationship between sediment depth and a set of chronological anchors to estimate the dates associated with each sediment layer. Often there are only a few chronological anchors sprinkled irregularly throughout a given sediment profile, spaced irregularly in time, and separated sometimes by thousands of years. To date every sediment layer, we have to estimate the relationship between the age of the anchors and their depth in the sediment. Then we have to interpolate between the anchors to assign a date to every layer. The interpolated dates for each layer are then associated with the archaeological and palaeoenvironmental data contained in those layers, providing us with a time-stamp for each observation. But, when the chronological anchors contain chronological uncertainty, that uncertainty propagates into the age-depth model.

Calibrated radiocarbon dates are particularly problematic because we cannot rely on point estimates or the CLT. Thus, there is no reason to expect that the means of the radiocarbon date distributions are reliable estimates of the true ages of the chronological anchors in a given sediment profile. So, the interpolation created by an age-depth model based on means—or any point estimate—does not necessarily represent the most likely chronological arrangement of the observations in a given time-series. Assuming it does, creates a *chronological bias*. Other probable models could be as accurate or more accurate than the one built with point estimates of the calibrated radiocarbon dates. To reduce the bias, we have to look as these other models to determine if the patterns in a

given radiocarbon-dated time-series might be the product of uncertainty instead of reflecting real patterns in past human and environmental conditions (Carleton et al. 2014, Mudelsee 2014).

In the first, third, and fourth studies, I used bootstrap simulations to explore the effect of radiocarbon dating uncertainty on time-series methods (Efron 1979, Mudelsee 2014). In the first paper (Chapter 3) the calibrated radiocarbon dates and palaeoenvironmental observations comprised real data about past rainfall levels from Lake Chichancanab while the last two studies (Chapters 5 and 6) involved synthetic dates and data. The bootstrap experiments allowed me to explore how chronological uncertainty affected the time-series methods used in each study, namely the LSSA (Chapters 3 and 5) and the PEWMA algorithm (Chapter 6).

The bootstraps involved randomly sampling calibrated radiocarbon date distributions thousands of times for a given analysis. Each time the distributions were sampled, a new age-depth model was created by interpolating between the sampled dates. The age-depth model was then used to date the observations in its corresponding time-series. Repeating this process yielded thousands of time-series. These ensembles contained the most probable chronological arrangements of the observations in a given series—i.e., the rainfall proxy in the first study and synthetic data in the last two. By re-running a given analysis thousands of times, we were able to reduce the chronological bias. Reducing the bias increased the variance of the statistical results—i.e., each analysis in the bootstrap yielded slightly different results because a different probable age-depth model was used. I then used that variability to express the results with windows of error that accounted for the chronological uncertainty of calibrated radiocarbon dating.

In the first study (Chapter 3), I used the LSSA and a bootstrap simulation to determine whether calibrated radiocarbon dating uncertainty could be responsible for the cycles identified in previous studies of drought records from Lake Chichancanab. In addition to the 208-year cycle that was likely produced by autocorrelation, as I described earlier, Hodell et al. (2005a) also identified a 50-year cycle. This cycle, they averred, had a significant impact on the tempo of the collapse of the Classic Maya, a significant

decline in socio-political complexity between 900 and 1100 CE. According to their interpretation, the 50-year drought helps to explain an apparent pause between the beginning of the collapse at 900 CE in the southern Maya Lowlands and the start of the collapse in the northern Maya Lowlands 50 years later. The pause, they argued, occurred because the 50-year drought cycle entered a lull, alleviating the water stress that contributed to the turmoil of the collapse. The bootstrap simulation, however, determined that the 50-year cycle was likely a phantom of chronological uncertainty.

In their study, Hodell et al. (2005a) dated the Chichancanab drought record with calibrated radiocarbon dates and an age-depth model. The age-depth model was based on the means of the calibrated date distributions, which led to a chronological bias. The bootstrap simulation described in Chapter 3 reduced the bias by exploring the other probable age-depth models. I reasoned that if the 50-year cycle was a real pattern in the drought record, it should be evident in the LSSA results for a high proportion of the probable age-depth models. If instead the 50-year cycle only appeared to be statistically significant for a small proportion of the probable age-depth models, then I could not exclude the possibility that it was an artefact of chronological bias. My results showed that the 50-year cycle occurred in less than 10% of the LSSA analyses, meaning that less than 10% of the probable time-series we explored exhibited a 50-year cycle. So, I concluded that the 50-year cycle likely was an artefact of chronological bias. Thus, the original hypothesis that the 50-year drought cycle contributed to the pattern of collapse at the end of the Classic period could not be supported.

My findings demonstrate that chronological uncertainty can produce spurious cycles when using frequency-based time-series methods. More specifically, calibrated radiocarbon dating can lead to a chronological bias by invalidating the use of point estimates for age-depth modelling. Sometimes that bias can be responsible for peaks in frequency spectra that can mistakenly be considered significant. The bias, I determined, can be alleviated by running a bootstrap simulation, which can expose spurious results, a solution also recommended by other scholars (e.g., Mudelsee 2014).

In the third study (Chapter 5), I further explored the effect of chronological uncertainty on the LSSA with simulated data and more bootstrap analyses. I wanted to

determine whether the degree of chronological uncertainty affected the likelihood that the LSSA would yield spurious findings. Using simulated radiocarbon dates and synthetic palaeoenvironmental data containing a known cycle, I evaluated the impact of several parameters. They included the impact of the number of radiocarbon dates used to date the synthetic series, the slope of calibration curve at those dates, the length of the known cycle in the synthetic series, and the signal-to-noise ratio of the synthetic data. My main finding was that in the presence of chronological uncertainty the false-positive rate of the LSSA is very high, around 90%. Increasing the number of radiocarbon dates—i.e., adding temporal markers to the age-depth models to reduce chronological uncertainty—improved these results up to a point. Specifically, simulations involving 15 radiocarbon dates had a lower false positive rate than simulations involving only 5 dates. But, increasing that number to 25 dates made little difference. So, the optimal number of dates was around 15. Since the synthetic series were 1000 years long, that translates into a dating density of 0.015 dates per year. Furthermore, the simulations that involved a high-slope portion of the calibration curve—which usually produces calibrated dates with lower variance—had a lower false-positive rate than simulations involving a low-slope portion of the curve. Thus, my simulations revealed that chronological uncertainty not only leads to spurious cycles, but also that the likelihood of identifying a spurious cycle corresponds to the degree of chronological uncertainty in the data. Unfortunately, though, the false positive rate was so high that decreasing the chronological uncertainty did not amount to meaningful gains in accuracy.

Together, the two studies reported in Chapters 3 and 5 demonstrate that calibrated radiocarbon date uncertainty can severely undermine frequency-based analyses. It does this by introducing uncertainty into age-depth models where calibrated radiocarbon dates act as chronological anchors. Interpolating between the anchors with an age-depth model produces an estimate of the dates for observations that lie between them. But, if the temporal positions of those anchors are uncertain, the anchors can float in time. Choosing one set of temporal positions over another probable set introduces a chronological bias. To alleviate the effects of that bias, we have to explore different combinations of probable temporal positions for the anchors. As different combinations are explored, the time-series will be compressed and expanded in irregular ways along

its length. Consequently, cycles of various lengths will fit the series better or worse depending on which dates are chosen for the chronological anchors. My simulation study suggests that this uncertainty leads to a very high false positive rate for frequency-based time-series methods, meaning that spurious cycles are likely to arise. As corollary, my study also suggests that the probability of finding a true underlying cycle in a time-series dated with radiocarbon is very low.

In the last study (Chapter 6), I ran another set of simulations to explore the effect of calibrated radiocarbon date uncertainty on the PEWMA algorithm. The PEWMA algorithm, as I explained earlier, is used to perform a Poisson regression that accounts for autocorrelation in the dependent variable (Brandt et al. 2000). I explored the effect of radiocarbon dating uncertainty on the algorithm by running a series of experiments. For each experiment, I created a pair of synthetic time-series, including one autocorrelated integer count dependent series and one series of rational numbers that served as an independent palaeoenvironmental variable. The study involved dozens of experiments in which I evaluated the impact of several parameters on the PEWMA regression method. The parameters included the number of radiocarbon dates used to date the synthetic palaeoenvironmental series, the signal-to-noise ratio of the synthetic palaeoenvironmental series, and the strength of correlation between the independent and dependent time-series. The primary objective was to determine whether the PEWMA algorithm was capable of identifying a known correlation in the presence of chronological uncertainty in the synthetic palaeoenvironmental time-series.

My main finding was that the number of radiocarbon dates used for age-depth modelling had little effect on whether the PEWMA algorithm correctly identified the known correlation. Each experiment involved 5, 15, or 25 synthetic radiocarbon dates evenly spaced over 1000-year period. They were used to date the synthetic palaeoenvironmental time-series that acted as independent variables in the PEWMA regressions. Surprisingly, increasing the number of dates from 5 to 15 or 25 had no substantial impact on whether the regression identified a correlation between the synthetic time-series. The hit rates—i.e., the percentage of regressions in each experiment that identified a correlation—increased slightly in variability, but the means of the hit rate distributions showed little movement in response to changing the number of

dates. So, I concluded that radiocarbon dating uncertainty in the independent variable does not appear to substantially affect PEWMA regressions. This finding contrasts with my results involving the LSSA, raising an important question: *why did chronological uncertainty adversely affect the frequency-based method but not the regression*?

I suspect radiocarbon dating uncertainty affected the frequency-based method differently than regression because the former attempted to identify a specific parameter value while the latter did not. The LSSA, like all frequency-based methods, aims to estimate the frequency of one or more waveforms that could constitute a given time-series. As I explained, calibrated radiocarbon dating uncertainty leads to temporal compressions and expansions in time-series data that results in a given waveform fitting better or worse depending on which dates are used as chronological anchors for the time-series. This *accordion effect* results in uncertainty in the frequencies of the waveforms that fit a given radiocarbon dated time-series. So, the parameter of interest—i.e., the frequency of some cyclical pattern—is sensitive to changes in the chronological anchors used to date the time-series.

In contrast, during our PEWMA simulation, I was interested in whether the method could identify an existing correlation, not in estimating the exact values of the correlation parameters. In each experiment, I recorded the AICs of the PEWMA models with and without the synthetic palaeoenvironmental series used as a covariate. The AIC of the model without a covariate served as a benchmark, replicating the procedure we used for assessing the impact of climate change on Classic Maya conflict (Chapter 4). If the AIC of the model with the covariate was lower—i.e., involved less information loss—then we concluded that using the covariate improved the fit of the PEWMA regression, indicating that a correlation existed. According to my simulation results, stronger underlying correlations were easier to identify, but the degree of chronological uncertainty as indicated by the number of synthetic radiocarbon dates involved made little difference. Had I been attempting to find a specific correlation coefficient, though, it seems likely that there would have been greater variability in my results. The temporal compressions and expansions created by radiocarbon dating uncertainty would have affected the temporal distance between observations in the synthetic palaeoenvironmental time-series. Since the distance between observations would have

affected the estimates of slope values in the palaeoenvironmental covariates, the slope of the PEWMA regression would likely have also been affected. This would have resulted in slightly different estimates of the correlation parameters in each regression. Consequently, each experiment—which involved the creation and analysis of thousands of artificial bootstrapped time-series—would have yielded a distribution of parameter estimates for the strength of the underlying correlation. Thus, it seems likely that the apparent difference in robustness to radiocarbon dating uncertainty of frequency-based and regression methods has to do with the specific question being asked—i.e., whether a parameter is being estimated or not.

Together, the simulation studies indicate that radiocarbon dating uncertainty will impact statistical procedures where a single parameter is being estimated but have less impact on procedures where a benchmark is being tested. Estimating a specific parameter, it seems, is much harder in the presence of radiocarbon dating uncertainty. That said, there appears to have been a small increase in the variability of the hit rate distributions for the PEWMA simulations as well when fewer radiocarbon dates were involved, indicating that chronological uncertainty has a small effect on our ability to identify an underlying correlation. So, the PEWMA method is more robust to radiocarbon dating uncertainty when its used to determine whether a correlation exists, but it is not completely immune to the uncertainty. Thus, if used to identify a specific correlation coefficient or other regression parameter, it seems likely that radiocarbon dating uncertainty would increase the variability of such an estimate.

Still, it may be possible to use both frequency and regression methods to estimate specific parameters, even in the presence of radiocarbon dating uncertainty. The LSSA simulation results in Chapter 3 are mostly unimodal. This means that distribution of frequencies identified as significant by the bootstrap simulations converged to a specific value—i.e., the mode of the distribution. That value, in many cases, was close to the known underlying frequency of the waveform I used to create the synthetic time-series. Thus, with a large enough sample of time-series for a given climate variable we could run the LSSA with a bootstrap to account for radiocarbon dating uncertainty and determine the frequency of the most likely underlying wave pattern. It should correspond to the mode of the distribution of significant frequencies

identified in all analyses. So, if we treat individual time-series as samples of an underlying population, we could in theory build up a distribution that indicates the true cyclical patterns in a given palaeoenvironmental process. Similarly, despite some small secondary modes in a few experiments, most of the results of the PEWMA simulations were also unimodal. Therefore, as the sample size of time-series increases, the distribution of results will correctly indicate whether an underlying correlation is likely to exist. Furthermore, despite not examining the correlation parameters specifically, it is reasonable to extend this principle to parameter estimation. Increasing the number of time-series analyzed would lead to a more accurate estimate of those parameters as well.

In sum, chronological uncertainty can substantially affect existing time-series methods. It has a more substantial impact on frequency-based analyses than regression methods when the latter are focused only on identifying a relationship instead of estimating regression parameters. The difference in the effect seems to be related to the difference in analytical objectives. Frequency-based methods are intended to identify specific parameters, namely frequencies of cyclical patterns. That parameter is evidently sensitive to changes in the temporal positions of chronological anchors in a given age-depth model. As a result, radiocarbon dating uncertainty can severely undermine frequency-based methods. In contrast, regression methods can be confidently used to test hypotheses about whether a significant relationship exists between two variables because radiocarbon dating uncertainty evidently has little impact on these kinds of analyses—at least where the PEWMA method is concerned. That said, I expect that analyses aimed at identifying specific parameters, like regression coefficients, will be negatively affected by chronological uncertainty just like the frequency-based methods are. This is because accounting for chronological uncertainty involves testing different probable age-depth models, which will result in variable parameter estimates. Fortunately, the simulation studies (Ch. 5 and 6) indicate that we can account for the instability of the parameter estimates by increasing the sample size of a given analysis—the sample size in this case is the number of time-series used to indicate a given underlying archaeological or palaeoenvironmental process. Repeating a frequency-based or regression analysis many times using additional time-series samples will

produce a distribution of parameter estimates, the mode of which may indicate the true value of the target parameter.

7.3. Implications

The four studies that comprise this dissertation have several implications, one specific to the Classic Maya and the others more general. The main implication for Classic Maya research is that drought might not have been as important for shaping Classic Maya history as previously thought. Scholars have long been interested in understanding the role of climate change in Classic Maya history, especially the impact of drought on the demise of Classic Maya civilization (e.g., Aimers & Hodell 2011, Dunning et al. 2012, Gill 2000, Hodell et al. 2005a, Iannone et al. 2013, Moyes et al. 2009, Turner & Sabloff 2012) While rainfall was surely important, since the Maya relied heavily on rain fed agriculture and the region today sometimes suffers from severe drought, the results of my research suggest that the long-term importance of drought may have been overstated in the academic literature. After quantitatively evaluating two major hypotheses about the role of drought in Classic Maya history I found that the rainfall variation might have less explanatory power than previously supposed. Future studies involving new drought records or archaeological time-series might support the idea that long-term trends in rainfall did, in fact, affect the course of Classic Maya civilization. However, the studies presented in this dissertation indicate that other climate factors like temperature should be examined and drought-based hypotheses should be viewed rather skeptically until they are quantitatively tested.

One of the more general implications of the research reported in this dissertation is that most of the published human-environment research in archaeology is likely biased. In a sense, this is no surprise since most of that research is based on subjective analyses. In Chapter 2, I explained that much of the research has involved visual wiggle matching between palaeoenvironmental and archaeological time-series. So, even before I undertook the studies that comprise this dissertation I suspected that bias was a problem. But, we now know specifically that chronological uncertainty can severely bias analyses. Consequently, it appears that the qualitative assessments are likely even less reliable than they seem and the body of research, as a whole, is problematic.

Similarly, the few quantitative studies involving correlations and regression models of palaeoenvironmental and archaeological data might be unreliable. None of them has accounted for both temporal autocorrelation and chronological uncertainty. So the primary results of this dissertation imply that the biases created by the idiosyncrasies of palaeoenvironmental and archaeological data might have led to spurious findings. Thus, it is prudent to assume the published research has been affected by temporal autocorrelation and chronological uncertainty and perhaps needs to be re-examined.

A third general implication is that many of the published palaeoenvironmental studies looking into cycles are flawed, perhaps fatally. As I have explained several times, by far the most common chronometric method for palaeoenvironmental time-series is calibrated radiocarbon dating and the uncertainties associated with it undercut most established statistical methods. But only very few of the recent studies even mention chronological uncertainty, let alone account for it with a bootstrap simulation. Since the studies in this dissertation suggest that the false-positive error rate for frequency-based analyses could be as high as 90% when calibrated radiocarbon dates are involved, it seems likely that a large percentage of the published palaeoenvironmental research contains spurious findings. Thus, many of the claims about cycles, such as Bond events (Bond et al. 1997, 2001), need to be re-examined.

Lastly, the results of the present study suggest that evoking archaeological research in discussions about modern climate change is not straightforward. As I stated in the Introduction and Background (Chapters 1 and 2), recently there has been a surge of interest in using archaeology as a basis for discussing the potential impact of modern climate change on human societies. Archaeological case studies have even been used in the reports by the UN's Intergovernmental Panel on Climate Change to assess risk. Despite this upswing in interest, however, there is substantial room for improvement. Most of the published research could be biased, either because it involves overly subjective assessments or because scholars have not accounted for the idiosyncrasies of the data. So, even though there has been a lot of scholarly and public interest in gleaning information about human-environment dynamics from the past, we need to carefully avoid drawing broad conclusions about past human responses to climate change. Archaeological research cannot, as yet, be used as a solid basis for making

predictions about the impact of modern climate change on human societies—though, with a concerted effort to improve our tools, it could be in the near future.

7.4. Solutions

While the major findings in this dissertation are in some respects rather gloomy, there are potential solutions to the problems created by temporal autocorrelation and chronological uncertainty. One involves thinking more about data sampling and probability; the other involves thinking more carefully about time.

While it seems obvious, the idea that we should increase our sample sizes is a departure from the standard practice in archaeology. In Chapter 2, I discussed the history of archaeological research on human-environment interaction and I reviewed the current state of the field by referring to a sample from the literature (see Appendix A). None of the studies I looked at involved analyzing multiple time-series. Instead, scholars presented a single palaeoenvironmental time-series and interpreted it as a proxy for an underlying environmental process, like rainfall, temperature, storminess, and so on. They then compared changes in the environmental proxy to changes in the archaeological record. None of the papers included a discussion of the fact that these time-series are all samples, each one representing a single *realization* of an underlying process.

A realization is the value—or for present purposes time-series of values—that is actually observed and the process is the mechanism that generated it (Pickup 2014). For palaeoenvironmental time-series, a realization is a single series of observations of a past environmental process. Take for example the sediment core from Lake Chichancanab (Chapter 3). The palaeoenvironmental proxy for past rainfall in that core, namely sediment density, is a sample from a parent population. Sediment density varies more or less continuously in three dimensions in the bottom of the lake. The density varies in vertical space, in part because of changes in past rainfall (Hodell et al. 2001, 2005a). It also varies in horizontal space because of differences in the conditions from place to place in the lake combined with random effects like turbulence and local weather. All of the potential effects that contribute to the variation are difficult to predict and some are

likely the product of chaotic systems. So every sediment core contains only a sample time-series of the rainfall proxy—i.e., a single realization of a statistical process. Thus, the process that ultimately led to the time-series of sediment density observed in the cores from Lake Chichancanab would produce different realizations in cores from different parts of the lake. Similarly, the radiocarbon samples used to date the time-series are also from a random process with spatial variation. Consequently, using only a single time-series of sediment density as a proxy for past rainfall levels in the lake catchment gives us a biased view of past rainfall. It is difficult to know, for instance, whether a blip in the proxy time-series indicates a real change in past rainfall levels or a random fluctuation representative of the three-dimensional chaotic variation in the proxy itself. Instead of looking at only one series then, we need to look at multiple realizations. In other words, we need multiple time-series containing the same proxy for a given underlying palaeoenvironmental process. The same is likely true of archaeological time-series.

Analyzing multiple realizations of a given past process could help us overcome the problems caused by chronological uncertainty (Carleton et al. 2014, Mudelsee 2014). In addition to giving us a more complete view of the variation in past rainfall levels, for example, having multiple independently dated time-series would reduce chronological bias. As I explained earlier, the simulation results for both the LSSA and PEWMA methods were mostly unimodal distributions. In the LSSA the modes were related to frequencies of waveforms in the time-series while the modes in the PEWMA simulations were related to hit-rates—i.e., the percentage of bootstrap iterations that identified a significant correlation. Because the distributions were mostly unimodal, repeating a given analysis with additional realizations of a past processes increases the likelihood that some of the results reflect these modes. This is true because most of the values drawn from a unimodal probability distribution are expected to be close to the mode (Moore et al. 2015). Thus, as we increase the number of repeated analyses, it becomes less likely that our results represent some value in the tails of the underlying distribution. Going back to the LSSA, for instance, increasing the number of repeated analyses involving additional palaeoenvironmental time-series increases the likelihood that we would discover the true underlying frequency of a strong cyclical signal in a given palaeoenvironmental process. This logic extends to archaeological time-series as

well. Thus, an important part of the solution to the problem of chronological uncertainty is for archaeologists to treat palaeoenvironmental and archaeological time-series as realizations of an unknown, underlying process and then improve our sampling strategies to reduce sampling bias.

Archaeologists have long been aware that our chronometric estimates contain error (Aitken 1990). Most introductory textbooks discuss the fact chronometric techniques are uncertain to some extent (e.g., Renfrew & Bahn 2013), and most authors include an error when reporting a date—the familiar, commonly favoured one-sigma standard reporting of radiocarbon date error, for instance. But, there is a difference between acknowledging the error and considering its impact. The former involves noting the error exists and perhaps even discussing it. The latter involves considering what might happen to a given result *if* different chronometric estimates were used to date a given event or set of events. Evaluating *what-if* scenarios is precisely what the bootstrap approach in Chapters 3, 5, and 6 focused on. In contrast, after acknowledging that an error exists, and dutifully reporting it, most researchers simply proceed with their interpretations without looking at alternative chronologies—though there have been a few notable exceptions (e.g., Baxter & Cool 2016, Blaauw et al. 2007, Blockley et al. 2007, Dye & Buck 2015, Maher et al. 2011).

Acknowledging chronological uncertainty without accounting for its impact is, in effect, *marginalizing* time. Marginalizing is treating a given variable *as if* it has no impact. It tends to result in biases that can lead to faulty conclusions, as my co-authors and I pointed out in our re-evaluation of the drought cycle hypothesis (Carleton et al. 2014). So, the second solution to the problems created by temporal autocorrelation and chronological uncertainty is to stop marginalizing time. It needs to be treated as an important variable in and of itself. In part, this solution just involves bootstrap simulations in the short term, but in the long term it involves devising better sampling strategies, gathering more chronometric data, and exploring the impact of chronological uncertainty further. Time is a crucial variable affecting our understanding of the past and archaeologists need to start thinking more carefully about it.

7.5. Directions for Future Research

This dissertation lays some of the groundwork for developing a better toolkit for analyzing archaeological and palaeoenvironmental time-series, but there is much more work to do. One area of future research involves exploring whether increasing the sample size of time-series will allow us to use established frequency-based methods, like the LSSA. The simulation in the third study (Chapter 6) showed that the distributions of significant frequencies were unimodal, meaning that increasing the number time-series analyzed should allow us to converge on the true frequency of an underlying wave pattern. This can be explored empirically by gathering several palaeoenvironmental time-series of a proxy that we know must contain an underlying cyclical pattern—such as isotopes affected periodic variation in solar activity. Or, it could involve another simulation study. Either way, we need to know whether increasing the sample size of time-series actually improves the ability of established methods to identify wave patterns in radiocarbon dated time-series. We also need to know how large the sample needs to be given various experimental conditions or simulation parameters. I suspect that a simulation approach is the best way to proceed at first, but eventually the method needs to be tested with real data.

A second area for future research involves exploring the effect of chronological uncertainty on time-series methods other than the LSSA and PEWMA. In particular, while the first and third studies (Chapters 3 and 5) show that radiocarbon dating uncertainty severely undermines frequency-based analyses, the results pertain to one specific type of frequency method, one that looks for *static* waveforms. The LSSA, like most frequency-based methods, searches a given time-series for sine and cosine waves with constant—i.e., static—parameters throughout the length of the series. The frequency, phase, and amplitude of the waves are the same at all points. However, some frequency-based methods employ *wavelets*, which are small consecutive waves lined up in time whose parameters can be different along different sections of a given time-series. So, while the LSSA tries, for example, to fit a wave with a certain frequency to a given series, using wavelets we can fit a succession of waves with various frequencies, accounting for the possibility that the underlying wave pattern changes through time. It is possible that wavelets are more robust to chronological uncertainty

and, therefore, should be explored using the same simulation method we used to evaluate the LSSA method.

A third area for future research involves investigating the impact of chronological uncertainty on the dependent time-series in PEWMA models. In the second and fourth studies, my co-authors and I were looking at dependent time-series with no significant chronological error. In the second study, that series was a historical record of Classic Maya conflicts, while in the fourth study it was a synthetic time-series of integer counts with no temporal uncertainty. The simulation study was designed with the Classic Maya conflict record in mind, meaning we intentionally focussed only on the chronological uncertainty in the synthetic palaeoenvironmental data. In many archaeological applications, however, there will be chronological uncertainty in both the dependent and independent time-series. So, future research should examine the impact of that uncertainty on the PEWMA algorithm using the same simulation methods we used in the fourth study.

Lastly, future research should also look at chronological uncertainty in chronometric methods other than calibrated radiocarbon dating. It made sense to begin with calibrated radiocarbon dating for two reasons. Firstly, it is the most common dating method used for archaeological and palaeoenvironmental research. Secondly, we already knew that the distributions were highly irregular and likely to pose special challenges for time-series methods. But, there are many other chronometric methods available, some even fairly common like Uranium Thorium dating or dendrochronology. These dating methods come with their own challenges and uncertainties that need to be explored. It is possible that using one of these other methods alleviates some of the problems otherwise caused by calibrated radiocarbon dating, but we cannot know for certain until they are properly explored.

7.6. Conclusions

The current bout of anthropogenic climate change is surely one of the greatest threats facing humans today. Unfortunately, though, there is substantial uncertainty about what the future will bring. In part, our predictions are limited by the short-term view

of modern data—climate change has only been going on for two centuries, and it has only just started to have significant, obvious effects. Since those effects are expected to continue for centuries, we need a long-term view of human-environment dynamics to form a solid basis for our predictions about the long-term impact of climate change.

The archaeological record could provide us with the long-term view we need. Over the last two million years, hominins living in various places and times have endured a variety of climate changes from the slight to the severe, spanning decades to millennia. So, looking to the past can alleviate some of our uncertainty about the impact of climate change on human societies of all kinds, ranging from small-hunter gatherer groups to complex urban civilizations. Thus, archaeologists need to participate to a greater extent in modern debates about the impact of climate change on human societies. However, we currently lack the necessary quantitative tools for analysing past human-environment interaction. So, right now we actually know very little about how past societies responded to climate change, limiting our ability to use the archaeological record as a basis for our predictions about the future impacts of climate change.

The primary aim of this dissertation was to begin laying the groundwork for creating a new quantitative toolkit for analyzing past human-environment interaction. To accomplish this aim, I conducted four studies that together comprise the bulk of this dissertation, focusing my efforts on statistical time-series methods. The studies were intended to identify some of the challenges archaeological and palaeoenvironmental data pose for quantitative analyses and perhaps identify solutions. I explored two time-series methods, one called Least-Squares Spectral Analysis (LSSA) that was designed to find cycles in time-series data, and another called the Poisson Exponentially-Weighted Moving Average (PEWMA), an algorithm for creating regression models of integer count time-series. The first two studies involved the Classic Maya civilization and the last two involved simulations designed to explore the limits of the LSSA and PEWMA methods.

I identified two idiosyncrasies of archaeological and palaeoenvironmental data and demonstrated that they can greatly complicate time-series analyses. The first is temporal autocorrelation, which can lead to spurious results and inaccurate statistical

models. I found that it can be managed as long as researchers are aware of its effects, but it seems that few archaeologists have accounted for it in the recent literature, raising questions about the reliability of previous research. The other idiosyncrasy is chronological uncertainty, a significant source of problems for time-series methods that has received very little attention in the academic literature. I found that it can severely undermine standard methods designed to find cycles in time-series data. It also has the potential to affect regression models and correlations, though to a lesser extent according to my findings so far.

I drew two major conclusions from my findings. The first is that most of the recent archaeological literature on past human-environment interaction is probably biased. On the one hand this is simply because most of the studies have been qualitative. But even the few quantitative studies published so far have failed to account properly for temporal autocorrelation and chronological uncertainty—two important idiosyncrasies of archaeological and palaeoenvironmental records. Thus, it is highly likely that the results they report are dubious. This is particularly true of studies involving palaeoenvironmental cycles, which I found can yield spurious results upwards of 90% of the time. Fortunately, the situation is less dire for regression models. If the underlying correlation between archaeological and palaeoenvironmental time-series is strong and the data have low noise, our analyses determined it is possible to identify that the correlation exists with established methods like the PEWMA algorithm. But estimating exact regression coefficients is likely to be more difficult, suggesting that arguments that depend heavily on a specific parameter—like a given correlation coefficient—could be biased. The bias, I found, could be alleviated somewhat by performing sensitivity analyses and perhaps bootstrapping the calibrated radiocarbon dates used to date the time-series, but more research is required to fully explore this possibility.

The second conclusion is that, despite the difficulties, it may be possible to overcome the problems caused by temporal autocorrelation and chronological uncertainty. As I mentioned, being aware of the potential effects of temporal autocorrelation is critical. Knowing that it can bias findings should lead scholars to seek out methods capable of handling it—they exist, and should be employed. Chronological uncertainty, while more challenging, can be overcome by increasing the sample size of

archaeological and palaeoenvironmental time-series and by bootstrapping calibrated radiocarbon dates—i.e., exploring alternative chronological arrangements in a given time-series to reduce the bias otherwise created by using point estimates like the mean of a calibrated date distribution.

These conclusions should give us some hope. Even though most of the recent research on past human-environment interaction using archaeological and palaeoenvironmental data is likely unreliable, improving our methods will ultimately rectify this situation. Now that the key analytical challenges posed by temporal autocorrelation and chronological uncertainty have been identified, we can explore more ways to overcome them. In fact, it is imperative that we do so. The archaeological record is the best source of information about long-term human-environment interaction, which is essential for predicting the long-term impacts of modern climate change. Improving our predictions is important for planning to mitigate those impacts. Thus, we need to continue the research into time-series methods and explore more ways to overcome the challenges posed by the idiosyncrasies of the archaeological and palaeoenvironmental records. Doing so will improve our understanding of human-environment dynamics and allow archaeology to contribute meaningfully to discussions about modern climate change. It will also help us to understand our collective human past.

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Appendix A. Archaeological Human-Environment Literature Sample

Reference	Causal Implication	Category of Study
Amesbury et al. (2008)	climate deterioration>abandonment	Narrative
An et al. (2005)	increased aridity > shift away from rain-fed agriculture to pastoralism and a decrease in population levels (because, presumably, pastorialism cannot support levels of population that are as high as those that agriculture can support, and this presupposes in this specific case that population levels will stabilize near the carrying capacity of the environment, given the type of subsistence)	Narrative
Anchukaitis and Horn (2005)	human subsistence patterns > lansdscape/ecosystem alteration	Narrative
Araujo et al. (2005)	dryness events (droughts?) > population decline twice in northern Brazil	Narrative
Ashley et al. (2009)	climate variability>changes in hominin landscape use	Narrative
Asmussen & McInnes (2013)	ENSO climate change>resource uncertainty and reduced productivity>lower-rank resource exploitation	Narrative; Statistical (with respect to correlation identification)
Backwell et al. (2014)	transition to wetter, warmer Holocene from Pliestocene !> abandonment of Wanderkrater open-air site	Narrative
Barton et al. (2010)	agro-pastrol activity>vegetation change>soil erosion>landscape changes (further eroision and vegetation change)	Narrative; Simulation
Berbarian et al. (2012)	climate change and earthquakes>site abaondoment/relocation and architectural	Narrative

	innovation	
Berger et al. (2013)	climate change>sea level change>coastline reformation>mangrove formation>neolithic cite relocation and exploitation of new mangrove faunal resources	Narrative
Berglund (2003)	human ecosystem alteration > changes in pollen assemblages	Narrative
Black et al. (2008)	climate variability (Pleistocene–Holocene transition, increase in ENSO variability in the Late Holocene) > changes in fire frequency in Australia; human activity intensity !> variation in fire frequency	Narrative
Bocquet-Appel et al. (2005)	climate change>changes in NPP, ET, and territory size>changes in population density	Narrative
Boulanger and Lyman (2014)	Palaeoindian overhunting !> megafaunal extinction in North America	Narrative
Boyd (2008)	long-term climate variability (increasing aridity) > ecosystem engineering and eventually collapse (abandonment) when the adaptations were no longer sufficient	Narrative
Branch et al. (2007)	anthropogenic landscape modification and possibly climate change>changes in mire/terrace pedological, geochemical, and pollen signatures	Narrative
Brooks (2006)	long-term climate variability (increasing aridity) and short-term climate shocks (intense drought) > population agglomeration, intensified agriculture, and a variety of large-scale changes to socio-political and economic organization,	Narrative

	depending on the pre-existing social–environmental context	
Buckley et al. (2014)	monsoon varibility (driven by ENSO/ITCZ) causing severe flooding and intermittent drought > collpase of Ankgor (flooding destroyed infrastructure for water management, killed crops; then droughts reduced later yields)	Narrative
Chepstow-Lusty et al. (2007)	development of the Inca Empire>increased abundance of livestock>increased prevelance of manure in soil>increases in abundance of detritivorous mites (positive correlation between mite abundance and manure, so the opposite effect is noted at the collapse of the Inca)	Narrative
Cooper & Perros (2010)	sea level changes, post-precipitation flooding>settlement patterns, resource exploitation, architectural innovation	Narrative
Cosgrove et al. (2007)	onset of ENSO > intensified use of rainforest in Australia and consequently increased human occupation intensity	Narrative
Dark (2006)	cliamte change!>land-use changes (abandonment)	Narrative
Denbow et al. (2008)	localized ecological degredation (through grazing livestock)>dispersed livestock grazing strategies>socio-political changes	Narrative

Dillehay et al. (2004)	ENSO variability (alternating flooding and drought) > settlement relocation, architectural innovation (construction of aqueducts, dams, dykes), rebuilding of damaged architecture and hydrological engineering projects, shift away from reliance on agriculture to more varied subsistence, increased conflict and political complexity	Narrative
Dong (2013)	pre-4000 BP climate change>changes in settlement density; post-4000 BP climate change and tecnological change>changes in settlement density and patterns	Narrative
Dong et al. (2012b)	nebulous climatic deterioration > social, economic, political, demographic, and land-use changes at various times throughout Chinese history	Narrative
Dong et al. (2012a)	moisture availability > cultural flouresence; drought > cultural decline; increases in moisture > replacement of the declined group by a new group with a different subsistence base	Narrative
Doyen et al. (2013)	human ecosystem alteration (agriculture, deforestation) > changes in sedminentation, plant ecology (vegetation types and 'openness'), and charcoal abundance (fire history)	Narrative
Drake (2012)	long-term climate change>economic instability and conflict>increased nomadism and urban centre destruction/abandonment	Narrative; Statistical (with respect to regime shifts in climate proxy data, only narrative regarding climate-culture causality)

Dreslerova et al. (2013)	climate changes !> changes in assortment and distribution of cereal types through time or space [instead: local ecological variation>variation in frequency distributions of the occurrence of different types of cereal grains in a site assemblage]	Narrative (though there is a statistical assessment of ecological conditions>frequency distribution of cereal types)
Erlandson et al. (2011)	predation of shellfish by humans>shrinking mean and median size of those shellfish populations	Narrative
Fabregas Valcarce et al. (2003)	climate change<>human responses (migration, agricutlure, conflict, social complexity)	Narrative
Faith (2007)	climate change>increased abundance of reindeer>shortened travel times and increased encounter rates (OFT)>less discrimination in parts hauled back to a site (increased evenness across skeletal remain types)	Narrative; Statistical (with respect to determining whether trends existed in the data, but not for comparing climate change to archaeology)
Farooqui et al. (2013)	climate change and anthropogenic deforestation for agriculture>local aridity>changes in agricultural strategies (different, drought resistant crops compared to earlier periods)	Narrative
Favier Dubois (2003)	cliamte change>mollisol pedogenesis>reduction in littoral archaeological site visibility	Narrative
Finne et al. (2011)	climate change>cultural changes (broadly, but doesn't really test a causal relationship per se—only suggests that no panregional climate event ocurred at 4.2 ky)	Narrative; Statistical (again, not actually testing causal relationships—only synchronology)
Florentino et al. (2013)	climate drying > decreases in population and adaptive changes in agricultural	Narrative

	practices	
Frahm & Feinberg (2013)	climate change>social crisis and/or landscape changes>changes in the diversity of obsidian sources that are represented in site assemblages	Narrative
Frisia et al. (2006)	rainfall variability > changes in subsitence patterns (hunter-gathering to agriculture in times of lower rainfall and greater rainfall variability)	Narrative
Genever et al. (2003)	increases in human land-use intensity !> changes in forest cover or fire regime indicated by swamp core proxy data	Narrative
Gilliland et al. (2013)	climate change>aridity with intense winter rains>water management by landscape alteration (reservoirs and dams) & Anuradhapura was sacked in 1100 CE and another climate change occurred>collapse of water management and depopulation of the landscape	Narrative
Gronenborn et al. (2014)	climate change>changes in precipitation>changes in societal resilience>changes in population levels	Narrative
Guérin et al. (2012)	climate change>changes in stone tool assemblages (mousterian types)	Narrative
Gulyás & Sumegi (2011a)	climate change>changes in landscape hydrology>changes in settlement patterns and resource exploitation	Narrative; Statistical (correlational with respect to proxies, statistical assessments of significant changes between periods)

Gulyás & Sumegi (2011b)	climate change>changes in landscape hydrology>changes in settlement patterns, resource exploitation, socioeconomic variables (measured by proxy using ceramic variation)	Narrative; Statistical (correlational with respect to proxies, statistical assessments of significant changes between periods)
Haberle and David (2004)	climate variability >> local climate changes <> human land-use and subsistance (forest clearance, incipient plant management, horticulture)	Narrative
Hallmann et al. (2013)	climate variation !> changes in subsistence patterning (bivalve collection occurred year-round regardless of climate changes indicated by d18O)	Narrative
Heckmann et al. (2014)	consistent anthropogenic landscape degradation (forest clearance and agriculture) > increases in dryland/disturbance plant taxa and changes to sedimentation, which became drastic after AD 1200 when a 'tipping point' was reached	Narrative
Huang et al. (2004)	climatic amerolioration (increased rainfall and a shift in monsoonal variability) > soil development in the Chinese Loess Plateau > sedentary agriculture; climatic deterioration (decreased rainfall and a shift in monsoonal regime) > abandonment of sedentary agriculture and a transition to nomadism (or the replacement of agricultural groups by nomads)	Narrative
Huffman (2008)	drought>increased incidence of 'rainmaking' rituals and associated material remains	Narrative

Hunt et al. (2007)	climate change and anthropogenic deforestation (for agriculture and/or fire fuel related to large scale metallurgy)>landscape changes recognizable in geology, pedology, hydrology, and plant ecology	Narrative
Jia et al. (2013)	climate change & agricultural developments > expansion of neotlihic culture on the Loess Plateau in China	Narrative
Joyce and Goman (2012)	cultural changes like religiously motivated warfare > shift in settlement patterns from lowlands to highlighands in search of greater defensibility > increased reliance on terrace agriculture and increased erosion of upland soil	Narrative
Kaal et al. (2013)	human activity>changes in pollen frequencies, charcoal concentration, coprophylic fungi concentration, and various soil characteristics & climate change>aridity>changes in pollen frequencies and fire indicators (aridity)	Narrative
Kaniewski et al. (2010)	drought>Mediterranean Dark Age (LBA–IA transition), including providing the impetus for Sea People migrations/incursions; increased climate wetness > cultural resurgence	Narrative
Kennett et al. (2007)	drought > population migration > increased overland trade between southern Channel islands and Western North America & increased sedentism and reliance on marine resources	Narrative
Kerr et al. (2009)	climate change (toward wetter/cooler conditions)>shifts in the distribution of	Narrative

	rainfall>less grazing land available>economic shift from pastoral to more agricultural	
King et al. (2013)	climate change>fluctuations in rainfall>fluctuations in rice agricultural yields>shifting reliance on different foodstuffs	Narrative
Kuentz et al. (2012)	human activity > changes in vegetation cover; climate change involving a cold/dry snap !> changes in agriculture apparent in the palynological record	Narrative
Lane et al. (2014)	ITCZ southward shift (that led to the TCD) > adoption of agriculture & increases in political complexity & changes in settlement patterns & increases in population	Narrative
Leroy (2010)	climate change>lake level changes (up until the 20th century when human interference begins to swamp the climate signal) & climate change>aridification>changes in pollen records & human activity>changes in pollen records (vis preferences for different crop types)	Narrative
Li et al. (2009)	rice agriculture > increases in greenhouse gases (CH4 in particular) > global warming	Narrative
Li et al. (2013)	tectonic and gemorphologic conditions>settlement distributions & climate amelioration>spread of neolithic culture & climate deterioration>decline of late neolithic culture in the study area but not elsewhere	Narrative

Li et al. (2014)	hydrological changes (alternating peak flooding and contraction periods) > changes in settlement location and agriculture	Narrative
Liu and Feng (2012)	mdi-late holocene climatic transition (increased ENSO intensity, Bond 4.2 ky event [termed the 4.0 ky event in this paper] > demographic collapse and decreases in cultural sophistication and political complexity	Narrative
Lubos et al. (2013)	anthropogenic landscape modifcation<>localized landscape change<>changes in socioeconomic activity and technology	Narrative
Madella and Fuller (2006)	monsoonal variability over centuries led to increasing aridity > reliance on agriculture (wheat and barley) > urbanization and political complexity; then, further increases in aridity and a shift in monsoonal patterns > changes in seasaonal rainfall distribution > changes in crop packages for agriculture (to rice and millet) > shifting settlement patterns toward wetter regions and de-urbanization of the Harrapan area	Narrative
Mayewski et al. (2004)	rapid climate change > cultural collapse	Narrative
Mercuri et al. (2011)	climate variability (increased aridity) > human exploitation of new resources and changes in agriculture > anthropogenic landscape modification; further increases in aridity > decreases in carrying capacity > demographic changes, migration, decline	Narrative

Migowski et al. (2006)	dry climate phases > lower Dead Sea lake levels > significant cultural events like population decline, resettlement, technological change; climate amelioration > cultural floresence (urbanization in Egypt and Mesopotamia and copper smelting)	Narrative
Morales et al. (2009)	major climate change (MCA and LIA) > changes in settlement patterns, demography, and subsistence practices	Narrative
Morrison & Cochrane (2008)	climate change and human landscape modication>changes in size and abundence of shellfich remains at a site	Narrative; Statistical (with respect to determining whether trends/changes existed in the data, but not for comparing climate change to archaeology)
Morwood et al. (2008)	climate change and human interference>changs in local faunal variability evinced by time-varying faunal remains in cave deposit	Narrative
Neff et al. (2006)	rainfall variability > changes in settlement patterns and demography (shift toward complex agricultural urbanism) OR increases in complexity (classic period Maya) OR collapse [all depending on the context, history, and specific changes in rainfall)	Narrative
Olsson et al. (2010)	climate vairability & human-induced fires > variability in the Holocene charcoal record of fires from two peat bogs in Sweden	Narrative
Plunkett et al. (2013)	climate variability resulting in increased aridity or wetness !> expansion or contraction of peatland occupation	Narrative; Statistical (with respect to chronological uncertainty)

Prasad et al. (2014)	cliamte variability (aridification) > the emrgence of Indus valley urbanism & further aridification > collapse of Harrapan civilization	Narrative
Premathilake (2006)	climate variability & anthropogenic landscape changes (forest clearance, incipient plant management, full agriculture) > changes in pollen 'spectra' and magnetic suceptibility	Narrative
Qin et al. (2011)	temperature, rainfall, and seal level variability, along with persistent soil salinity > attneuated Neolithic development, low population levels, and continued reliance on hunting, gathering, and fishing	Narrative
Riede (2008)	environmental catastrophe (volcano)>territorial reorganization and population changes>loss of bow and arrow technology	Narrative
Riede & Edinborough (2012)	environmental catastrophe (volcano)>cultural changes	Narrative; Statistical (temporally statistical with regard to synchronology)
Riehl (2009)	increasing aridity > changes in agricultural species toward drought tolerant species & changes in cultural preferences > changes in crop species	Narrative
Riehl et al. (2008)	climate change>increased aridity>decreased carbon isotope uptake in non-irrigated crops	Narrative; Statistical (with respect to relationships between some proxies but not others and not statistical concerning the relationship between climate change and the other proxies or with respect to change in proxy values over time)

Roberts et al. (2011)	climate vairability (mostly changes in rainfall) > cultural transitions in the Bronze Age (sometimes urban development, other times collapse and abandonment)	Narrative
Robinson et al. (2013)	climate change>cultural changes	Narrative
Ropke et al. (2011)	human activity (agro-pastoralism) > changes in vegetation cover (deforestation) and increased charcoal influx, eventually soil errosion and landslides	Narrative
Schimmelmann et al. (2003)	solar activity, el nino, volcanic eruptions > 200-year periodic extreme flooding > social and cultureal disruptions	Narrative
Schmidt et al. (2011)	hydrological changes (rainfall leading to changes in flow rates, channel incision, alluvial deposition) & earthquake frequency !> significant changes in landscape use, settlement patterns or site abandonment, most of the time; increased moisture availability > intial settlement; severe aridity > settlement hiatus	Narrative
Schmölcke et al. (2006)	sea level change > changes in human land-use and subsitence patterns	Narrative
Schofield et al. (2010)	anthropogenic landscape alteration & global climate variation (increased strom activity in the north atlantic) > changes in geochemistry observed in a peat core	Narrative
Smith (2007)	rapid climate change > human evolution (lactose tolerance, sickle cell anemia and malaria resistence)	Narrative

Smith and Ross (2008)	climatic amelioration (increased rainfall, overall) > population increaseand range expansion despite increased ENSO variability	Narrative; Statistical (with respect to determining whether changes existed in the data, but not for comparing climate change to archaeology)
Smith et al. (2008)	ENSO variability > demographic variability	Narrative; Statistical (with respect to correlation identification)
Staubwasser and Weiss (2006)	drought > collapse	Narrative
Tallavaara and Seppa (2012)	annual mean temperature fluctuation > some variability in demographic trends, but not all	Narrative
Tarasov et al. (2006)	regional changes in precipitation > local changes in pollen assemblages (indicative of shifts in plant taxa towards those that reflect more arid conditions)	Narrative
Tinner et al. (2003)	climate deterioration (toward colder humid conditions) > decreases in anthropogenic flora [indicated by pollen taxa] like agricultural taxa and distriburbance taxa; cliamtic improvement > decreased arboreal taxa and increased anthropogenic taxa	Narrative
Wahl et al. (2006)	agriculture and population density > forest clearance and changes in plant ecology; abandonment > resurgence of forest and a return to pre-human plant ecology state	Narrative
Wahl et al. (2013)	human ecosystem alteration (agriculture, deforestation) > changes in lakebed sediment proxies (pollen, charcoal, d13C, CaCO3)	Narrative

Wang et al. (2014)	climate variability (increased aridity and decreased temperature) > decreases in human population levels in China over the last 50 000 years	Narrative; Statistical (with respect to correlation identification)
Wenxiang and Tungsheng (2004)	4000 BP 'event' (broadly defined over hundreds of years) > non-specific changes in the neolithic cultures of China 500+ years later	Narrative
Wescott & Cunningham (2006)	climate change and health and nutrition and workload (from changes in subsistance strategies)>changes in long bone morphology and changes in sexual dimorphism	Narrative
Winsborough et al. (2012)	alternating drought and flood events>changes in agricultural practices and landuse>changes in material culture and sometimes abandonment	Narrative
Woodbridge et al. (2012)	population increase (in the neolithic)and possibly some cliamte change>changes in land cover (from closed forest to semi-open pasture land)	Narrative
Yasuda et al. (2004)	climatic deterioration (increasing ariditiy) > water management (irrigation channels) & other misc. architectural developments & population nucleation > socioeconomic stratification & urbanisation ("megalopolis" culture) … climate deterioration (increasing aridity) > agricultural failure despite new technology and complexity > site abandonment	Narrative
Yu et al. (2010)	agriculture, pigment manufacture, and bronze metallurgy > increases in atmospheric concentrations of Pb and Zn	Narrative

	that were then deposited into peat	
Yuecong et al. (2011)	periods of cold and dry climate>greater accesiblility to nutrient rich wetland soils for agriculture>cultural prosperity & climate osscilation (toward wetter conditions)>oscilation in cultural prosperity (via changes in access to good cropland)	Narrative
Zhang et al. (2011c)	climate change>agricultural revolution	Narrative
Zhao et al. (2010)	overall climatic drying trend coupled with agricultural activity and human ecosystem alteration > transition to open steppe from forest steppe in a region of the Tibetan plateau between 2000 and 1000 years ago	Narrative

Appendix B. Supplement to Chapter 3

Chronological error is a significant problem for frequency based time-series analysis. Uncertainty about the chronological structure of a set of sequential observations must be accounted for because assuming a single structure may introduce a substantial bias into such analyses. That bias could cause some frequencies to spuriously appear to account for significant variation (or power) in the time-series when, in fact, they account for very little if other probable chronologies are assumed instead.

Chronological bias is a particularly important problem for time-series dated with radiocarbon assays. In order to estimate the age of an event using stratigraphically associated carbon samples, the measurement of the ratio of carbon isotopes in the sample must first be compared to a calibration curve. The calibration curve describes the changes in concentrations of carbon isotopes in the atmosphere over time. Calibration curves, which are updated periodically (e.g., Reimer et al. 2009), are estimates of past ratios of carbon isotopes inferred from several different independently dated geological and palaeoecological data sets. The curve has its own errors owing to the statistical nature of its construction. More importantly, it is a nonmonotonic function of fluctuating isotope concentrations. Consequently, the curve contains 'wiggles', or reversions, and plateaus. As a result, different times in the past can appear to have had very similar concentrations of isotopes associated with them. Therefore, when comparing a modern radiocarbon assay with the curve, it is often the case that more than one date in the past could correspond to the concentrations of isotopes found in the modern sample.

The following, more formal explanation of radiocarbon calibration should help clarify the purpose of the LSSA simulation presented in the manuscript. Take y_{id} to be a measurement of carbon isotopes from a sample corresponding to depth d within a sediment core. The measurement represents the mean of a normal distribution with an associated instrumentation error σ_d^2:

$$y_d \sim \mathrm{N}(y_d, \sigma_d^2)$$

The raw radiocarbon age estimate has to be converted to calendar time using the calibration curve, as described above. The calibration process lends itself well to a Bayesian framework, which is becoming increasingly popular as a way of calibrating radiocarbon dates. The distribution of probable calendar dates, t_d, associated with a radiocarbon measurement y_d sampled from depth d is given by

$$p(t_d|y_d)$$

where $p(t_d|y_d)$ is the posterior distribution in the following Bayesian relation:

$$p(t_d|y_d) \propto p(y_d|t_d)p(t_d)$$

The likelihood distribution, $p(y_d|t_d)$, describes the calibration process and accounts for errors in the calibration curve and errors in the raw measurement of radiocarbon isotopes. The details of the likelihood function are not crucial for our purposes, and they are well described elsewhere (e.g., Litton and Buck, 1995; Ramsey et al., 2006). For the sake of simplicity, the following abstract definition will suffice:

$$p(y_d|t_d) := f(r, \varepsilon)$$

where $f(r, \varepsilon)$ is the calibration function, r is the calibration curve, and ε is the combined errors of the calibration curve and the instrumentation error associated with the radiocarbon assay.

The prior term, $p(t_{id})$, is generally a constant when only a single radiocarbon assay is being calibrated, but it takes on a more important role when multiple assays are involved. The Bayesian relation can be generalized to include a set of radiocarbon assays as follows:

$$p(\boldsymbol{t}|\boldsymbol{y}) \propto p(\boldsymbol{y}|\boldsymbol{t})p(\boldsymbol{t})$$

The prior term $p(\boldsymbol{t})$ then refers to the *a priori* known relative chronological relationships among the carbon samples, $\boldsymbol{y}$. In comparatively simple cases, like the sediment core from Lake Chichancanab, the relative chronological relationships among the $\boldsymbol{t}$ radiocarbon date distributions can be described by a simple step function:

$$p(\boldsymbol{t}) = \begin{cases} 1 \; if \; t_{d_1} < t_{d_2} < t_{d_3} \ldots < t_{d_n} \\ 0 \; otherwise \end{cases}$$

The step function serves to constrain the posterior distributions—i.e., modify their shapes—to reflect the known stratigraphic relationships among the carbon samples in the sediment core. The stratigraphic relationships are given by the depth measurements, d_n, associated with each assay, assuming no stratigraphic inversions have occurred. This Bayesian approach can greatly improve the estimation of the range of calendar dates likely to be associated with a set of carbon samples, and is becoming the standard approach to radiocarbon calibration as a result. However, despite the improvements in precision, the posterior distributions are still highly irregular and multimodal. Thus, assuming a single point estimate from each distribution in a sequence of calibrated radiocarbon dates to estimate an age-depth model will yield a biased estimate of the true, unknown age-depth relationship.

Our LSSA simulation was intended to reduce the effect of this bias on the analysis of the Lake Chichancanab time-series. This was accomplished by scanning a range of probable chronological structures for significant frequencies. Those frequencies that appeared to be statistically significant, compared to a red noise null spectrum, over a substantial sample of varying chronological structures would be considered much more likely to represent actual periodicity in the series. To scan the possible chronological structures of the time-series, we had to sample from each of the posterior distributions, $p(t_d|y_d)$, a large number of times in accordance with the relative probabilities they described. The complete simulation can be described using the following pseudo code:

For 5000 iterations:

> *sample* t_d from $p(t_d|y_d)$ for all d
>
> *build* an age-depth model using a cubic polynomial
>
> *if* the model does not conform to $p(\boldsymbol{t})$, then resample

else run LSSA on the new series

build 5000 red noise time-series

for each red noise time-series *run* LSSA

calculate point-wise 95% CI from the 5000 red noise LS-spectra

return set of new series LS-spectrum peaks > red noise 95% CI

Supplementary Material: R Code

The following code is intended to clarify the simulations we ran and improve reproducibility. It is provided without warranty. Each R function is preceded by a gloss that briefly describes the function's purpose and the expected inputs. Lines that begin with ↙ have been wrapped from the previous line to fit the page. The R code utilizes parts of the following R packages and libraries:

MASS (Venables and Rippley, 2002)

pracma (Borchers, 2014)

Rmpi (Yu, 2012)

snow (Tierny et al., 2012)

rlecuyer (Sevcikova and Rossini, 2012)

The following is the main LSSA function. It expects an n-by-2 matrix where the first column is the observation times and the second is the observations.

```
lssa.fast <- function(x){
        x[,2] <- x[,2] - mean(x[,2])
        if(mod(nrow(x),2) > 0){x <- x[-nrow(x),]}
        N <- nrow(x)
        delta <- mean(diff(x[,1]))
        f.fund <- 1/(max(x[,1])-min(x[,1]))
        f.nyq <- 1/(2*delta)
        f <- seq(f.fund,f.nyq,(f.nyq-f.fund)/(N/2))
        omega <- f*(2*pi)
        s <- c()
        theta <- c()
        tau <- c()
        center <- diag(N) - (1/N) * (rep(1,N)%*%t(rep(1,N)))
        x_ <- x
```

```
        fitted_ <- c()
        for(i in 1:length(omega)){
        tau <-
↙atan(sum(sin(2*omega[i]*x_[,1]))/sum(cos(2*omega[i]*x_[,1])))/(2*omega[i])
            theta <- cbind(cos(omega[i]*(x_[,1]-tau)),sin(omega[i]*(x_[,1]-↙tau)))
                beta <- ginv(t(theta)%*%theta)%*%t(theta)
                P <- theta%*%beta
                fitted <- P%*%x_[,2]
                fitted_ <- cbind(fitted_,fitted)
                M <- diag(N) - P
        s <- c(s,1 -↙(t(x_[,2])%*%M%*%x_[,2])/((t(x_[,2])%*%center%*%x_[,2])))
                x_ <- cbind(x[,1],x[,2]-fitted)
        }
        return(s)
}
```

The following function calculates the red noise significance level with a bootstrap simulation. It expects the following inputs:

series: n by 2 matrix, where the first column is the depth measurements and the second is the (interpolated) calibrated radiocarbon date point estimates associated with each depth

nullsim: number of simulation runs

alpha: statistical significance level (usually 0.05)

```
lssa.bootsigred <- function(series,nullsim,alpha){
        simAR1set <- simAR1(series,nullsim)
        lssa_sims <- lapply(simAR1set,lssa.fast)
        lssa_sims <- do.call(cbind,lssa_sims)
        return(apply(lssa_sims,1,quantile,prob=1-alpha))
}
```

The function above calls the following functions either directly or through the call to function simAR1():

```
simAR1 <- function(series,nsim){
        simsAR1set <-
↙lapply(1:nsim,function(x)gen_ar1(series[,1],findTau(series)))
        return(simsAR1set)
}

gen_ar1 <- function(times,tau){
        d_times <- diff(c(0,times))
        rho <- exp(-(d_times)/tau)
        sigma2 <- 1- exp(-2*(d_times)/tau)
        epsilon <- rnorm(length(times)+1,0,sigma2)
        ar1 <- epsilon[1]
        for(i in 2:length(times)){
                ar1 <- c(ar1,rho[i]*ar1[i-1]+epsilon[i])
        }
        return(cbind(times,ar1))
}

findTau <- function(series,a=1/exp(1)){
        series[,2] <- detrend((series[,2]-mean(series[,2]))/sd(series[,2]))
        a_hat <-
↙optim(a,a_min,gr=NULL,series,method="Brent",lower=0,upper=1)$par
        return(-1/log(a_hat))
}

a_min <- function(a,series){
        a_hat <- c()
        for(i in 2:nrow(series)){
          a_hat <- c(a_hat,(series[i,2]-series[i-1,2]*a^(series[i,1]-↙series[i-1,1]))^2)
        }
        return(sum(a_hat))
```

```
}
```

The following function calculates the white noise significance level with a bootstrap simulation.

```
lssa.bootsigwhite <- function(series,nullsim,alpha){
        sim_wht_set <-
↙lapply(1:nullsim,function(x)cbind(series[,1],rnorm(length(series[,1]),mean↙=0,sd=sd(s
eries[,2]))))
        lssa_sims <- lapply(sim_wht_set,lssa.fast)
        lssa_sims <- do.call(cbind,lssa_sims)
        return(apply(lssa_sims,1,quantile,prob=1-alpha))
}
```

The following function runs the simulation and expects a cluster to be available. It uses the SNOW package to manage the cluster. The function takes the following arguments:

data: an n by 2 matrix where the first column is the observation times (calibrated radiocarbon point estimates for each observation) and the second column contains the observations

series: n by 2 matrix, where the first column is the depth measurements and the second is the (interpolated) calibrated radiocarbon date point estimates associated with each depth

dates: this a list of length n that contains the calibrated radiocarbon date information. Each element of the top-level list is another list with three elements. The first element is the depth associated with the carbon date; the second is the (68.2%, in our study) highest probability density region of the calibrated radiocarbon date distribution from OxCal output (really a histogram with ten-year bins that describes the relative probabilities that the carbon assay is dated to each of the ten-year bins—the data is contained in an n by 2 matrix where the first column is in years BP and the second is the probability density)

core_top: the year that dates the top of the core (generally the year the core was actually collected)

bounds: a vector of length 2, [most recent date in years BP < earliest date in years BP], that provides temporal bounds for the series so it can be truncated if desired.

nsim: the number of times the age-depth model will be sampled (exploring the effect of radiocarbon calibration on the LSSA results)

nulltest: either “white” or “red” to indicate which null hypothesis will be used

nullsim: the number of simulation runs for finding the designated confidence level of the null spectrum distribution

alpha: the level of significance (usually 0.05)

rout: PATH to location for saving the R output, which will be an R image that contains the simulation results

```
simLSSA <-
function(data,series,dates,core_top,bounds,nsim,nulltest,nullsim,alpha,rout){

        simseries <- clusterApplyLB(cl,1:nsim,function(x){cat("simseries
↙",x,"\n",file=rout);newSeries(data,series,dates,core_top)})

        simseries <- ↙clusterApplyLB(cl,1:length(simseries),function(x){cat("trim
↙",x,"\n",file=rout);simseries[[x]][which(simseries[[x]][,2]>=bounds[1]&sim↙series[[x]][,2]
<=bounds[2]),]})

        freqs_sims <-
↙clusterApplyLB(cl,1:length(simseries),function(x){cat("freqs
↙",x,"\n",file=rout);findf(simseries[[x]][,2])})

        if(nulltest=="red"){

        lsspec_sig <-
clusterApplyLB(cl,1:length(simseries),function(x){cat("rednoise
↙",x,"\n",file=rout);lssa.bootsigred(simseries[[x]][,c(2,3)],nullsim,alpha)↙})

        } else if(nulltest=="white"){

        lsspec_sig <-
↙clusterApplyLB(cl,1:length(simseries),function(x){cat("whitenoise
↙",x,"\n",file=rout);lssa.bootsigwhite(simseries[[x]][,c(2,3)],nullsim,alph↙a)})

        }

        lsspec_sims <-
↙clusterApplyLB(cl,1:length(simseries),function(x){cat("lssa_fast
↙",x,"\n",file=rout);lssa.fast(simseries[[x]][,c(2,3)])})

        maxlength <-
↙max(do.call(c,clusterApplyLB(cl,1:length(simseries),function(x){length(fre↙qs_sims[[x]]
)})))

        freqs_sims <-
↙clusterApplyLB(cl,freqs_sims,function(x){c(x,rep(NA,diff(c(length(x),maxle↙ngth))))})

        freqs_sims <- do.call(cbind,freqs_sims)

        lsspec_sig <-
↙clusterApplyLB(cl,lsspec_sig,function(x){c(x,rep(NA,diff(c(length(x),maxle↙ngth))))})

        lsspec_sig <- do.call(cbind,lsspec_sig)

        lsspec_sims <-
↙clusterApplyLB(cl,lsspec_sims,function(x){c(x,rep(NA,diff(c(length(x),maxl↙ength))))})

        lsspec_sims <- do.call(cbind,lsspec_sims)
```

```
		return(list(spec_freqs=freqs_sims,spec_sims=lsspec_sims,spec_sig=lssp
e↙c_sig))

}
```

Once the simulation has finished, the above function returns a list of matrices containing the results. To produce figures 5 and 6 in the manuscript we had to compare the matrix of empirical LSSA results (lsspec_sims) to the matrix of null LS-spectra (lsspec_sig). Where lsspec_sims > lsspec_sig, the corresponding frequencies from the matrix freqs_sims were identified, converted to periods, and binned into a histogram with bin widths of one year with the following code snippets:

```
temp_freqs <- 1/round(1/sort(sim_res_[[1]][sim_res_[[2]]>sim_res_[[3]]]),0)

fcounts <- cbind(unique(temp_freqs),table(temp_freqs))
```

The cluster based simulation function above calls the following functions as well, directly or indirectly (see the function arguments list above to determine what these functions are expecting):

The next two functions resample the highest probability density regions of the calibrated radiocarbon date distributions, $p(t_{id}|y_{id})$, and create new age-depth models using those dates.

```
newSeries <- function(data,series,dates,core_top){

		newdates <- newDateSeries(series,dates,core_top)

		max_depth_interp <- dates[[length(dates)]][1]

		data_trim <- data[which(data[,1]<max_depth_interp),]

		data_trim[,2] <- newdates[which(newdates[,1] %in% data_trim[,1]),2]

		return(data_trim)

}


newDateSeries <- function(series,dates,core_top){

		jitterdates <- lapply(dates,function(x){sampleC14(x[[2]],x[[3]])})

		while(!all(diff(unlist(jitterdates)) > 0)){

				jitterdates <- lapply(dates,function(x){sampleC14(x[[2]],x[[3]])})

		}

		for(i in 1:length(jitterdates)){

								dates[[i]][[2]] <- jitterdates[[i]]

		}
```

```
        dates_ <- matrix(unlist(dates),ncol = 4, byrow=T)

        newseries <- interpolateC14(series,dates_,core_top)

        return(newseries)

}
```

This function determines the frequencies that will be evaluated in the LSSA. The specific length and observation times for the resampled age-depth models are slightly different, of course, so each age-depth model needs to have a set of frequencies estimated for it. The specific frequencies estimated for each age-depth model are very similar between models, but not exactly identical. Rounding allows for them to be considered equivalent by the end of the simulation where equivalent means that the corresponding periods differ by less than a year.

```
findf <- function(x){

        if(mod(length(x),2) > 0){x <- x[-length(x)]}

        N <- length(x)

        delta <- mean(diff(x))

        f.fund <- 1/(max(x)-min(x))

        f.nyq <- 1/(2*delta)

        f <- seq(f.fund,f.nyq,(f.nyq-f.fund)/(N/2))

        return(f)

}
```

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Appendix C. Supplement to Chapter 4

The Data

To test the hypothesis that climate change drove Classic Maya conflict, we looked for an association between a historical record of conflict and five palaeoclimate proxies. Compiled from the published literature (Chase & Chase 2003b, Hassig 1992, Kennett et al. 2012, Schele & Mathews 1991), the conflict record contains information about 144 conflict events that occurred between approximately 350 and 900 CE. The events were recorded on monuments in more than 30 Classic Maya urban centers described in dozens of scholarly works (Table S4)—see Kennett et al.'s (2012) supplementary information for a complete bibliography of the conflicts they included. The events include general attacks, captive taking, beheadings, defacement of monuments, and "Star Wars", which were large inter-polity conflicts timed to accord with certain celestial alignments (Webster 2000). The same basic information accompanied most of the scholarly references to conflict events. Most scholars listed a Classic Maya calendar date, the Gregorian date, one or two archaeological site names, and some text describing the type of conflict (Table S5). For those that only provided the Classic Maya date, we calculated the Gregorian date using the standard translation method (Kennett et al. 2013). We also attempted to eliminate duplicate references to conflict events by comparing site names, descriptions, and Classic Maya calendar dates. When a potential duplicate was identified, we eliminated it from our list of conflicts—the eliminated events are identified in Table S5 by a "1" in the "likely duplicate" column. The conflict events were binned into 25-year intervals so that the analyses would be comparable to recent work on the impact of climate change on Maya conflict (e.g., Kennett et al. 2012).

The palaeoclimate proxy records we used include three rainfall records and two temperature records. Two of the rainfall proxies come from the Classic Maya region. One is a record of oxygen isotopes in a speleothem from Yok Balum Cave in southern Belize (Kennett et al. 2012). Speleothems are cave deposits, in this case made of limestone, that precipitate out of ground water that seeps into the cave. The ground water was, at one point, rainwater on the surface before filtering into the cave system. Thus, oxygen isotopes in the rainwater become incorporated into the speleothem as it grows in the cave. Perceptible layers of speleothem growth capture changes in the proportions of different oxygen isotopes over time. So, the speleothem can serve as a record of changing isotopic ratios, which can be compared to present conditions. The ratios of these isotopes are affected by temperature and precipitation amounts, though differently depending on the local environmental conditions (Lachniet 2009). The conditions at Yok Balum Cave suggest that precipitation amounts have long been the primary controller of the isotope ratios (Kennett et al. 2012). Therefore, once the layers of speleothem growth have been dated, the oxygen isotope record can be used as a proxy for past rainfall. The Yok Balum record is a particularly high-resolution record (it records sub-annual variation). This is due to the visibility of the speleothem layers and the high-resolution radiometric methods used to date them.

The other rainfall proxy from the Classic Maya region is a sediment density record from Lake Chichancanab, located near the center of the Yucatan Peninsula (Hodell et al.

2005a). The sediment density in the lake reflects lake levels. When levels are low, ions in the water precipitate out as Gypsum and become incorporated into the lakebed sediment, increasing the sediment density. Low lake levels at Lake Chichancanab are indicative of a high evaporation to precipitation ratio. Substantially less precipitation would lead to low lake levels and, consequently, higher sediment densities. Thus, the sediment density record can serve as a proxy for past rainfall amounts.

The third rainfall proxy we employed is a record of titanium concentration in sediment from the Cariaco Basin, ~3000 km east of the Classic Maya region (Haug et al. 2001). Titanium concentration in the Cariaco basin is indicative of surface runoff from local watersheds (Haug et al. 2001).. Increased rainfall leads to additional surface sediment input into the basin, which accumulates over time in layers along with other oceanic sediments. Titanium is carried along with surface sediments into the Cariaco basin during the rainy season, which means that titanium concentration in the basin sediments increases with greater amounts of rainfall. Thus, titanium concentration can be used as a proxy for past rainfall amounts. The titanium record we assessed has a resolution or ~4–5 years, based on ten high precision radiocarbon dates.

The two temperature proxies we utilized are records of magnesium to calcium ratios from the Cariaco Basin (Wurtzel et al. 2013). Magnesium becomes incorporated into the shells of foraminifera as they form. The ratio of Magnesium intrusions to calcium in the calcite shells is controlled by temperature—higher temperatures result in increased proportions of magnesium. As the foraminifera die, their shells become incorporated into the layers of ocean sediment, which creates a record of the time-varying magnesium to calcium ratio. Once that ratio is measured and the sedimentary layers dated, the record can serve as a proxy for past temperature. And, because of the seasonal differences in foraminifera species abundances, two separate temperature proxies can be created from a single sediment core. The record we used contains winter/spring (approx. March-May) and summer/fall (approx. September-November) temperature estimates. It also has an annual resolution based on a combination of radiometric dates, an established foraminifera biostratigraphy, and varve counting.

There is, however, upwelling which complicates SST reconstructions for the Cariaco Basin (Wurtzel et al. 2013).. Upwelling occurs when deeper, colder ocean waters are pushed upward in the water column and mixed with surface waters (Black et al. 2004, Wurtzel et al. 2013). Upwelling brings nutrients and organisms up in the water column, which is important for SST reconstructions based on foraminifera because different species prefer different depth and temperature conditions. For example, *Globigerina bulloides*, the species used for the winter SST reconstruction, prefers cooler waters at around 30 m depth. So, the reconstruction based on *G. bulloides* might not indicate winter surface conditions if upwelling intensity was too low. On the other hand, as long as upwelling intensity was sufficient, the cooler deep waters would have been mixed with the warmer upper waters and the SST reconstruction based on *G. bulloides* reflects surface conditions, although the SST will be lower than the air temperature at the surface because of the mixing. The intensity of upwelling in the Cariaco Basin is seasonal, allowing for the differentiation between summer and winter SST

reconstructions, and it is known to have changed through time (Black et al. 2004). However, according to an independent reconstruction, upwelling intensity was increasing or stable in the centuries leading up to 900 CE, the end of our study period (Black et al. 2004).. The interpretation that upwelling was intensifying is further supported by the decrease in rainfall in the Cariaco basin over the same interval, as indicated by decreasing Ti concentrations (Haug et al. 2001). Decreasing rainfall amounts could indicate a southward migration in the Inter-Tropical Convergence Zone (ITCZ), the area near the equator where northeast and southeast trade winds meet (Black et al. 2004). The ITCZ is a low-pressure region usually characterized by low wind strength. Over time, the ITCZ migrates north and south over the equator (Black et al. 2004).. When the ITCZ is farther south, Venezuela receives less rainfall, leading to lower Ti concentrations in the Cariaco Basin (Haug et al. 2001). Simultaneously, upwelling intensity increases in the basin because of the intensification of trade winds. The greater wind strength increases the intensity of so-called Ekman upwelling, a phenomenon of fluid dynamics whereby surface waters are moved by wind action generating a gyre that brings subsurface water upward. Thus, it seems likely that our winter SST reconstruction does reflect SSTs in the centuries leading up to 900 CE. The upwelling likely caused a decrease in SSTs relative to the air temperatures as colder, lower water mixed with warmer surface water, but the trend in SSTs would not have been affected.

The Temperature Trend

The temperature records we used in our models come from the Cariaco Basin, approximately 3000 km east of the Classic Maya region. Consequently, our results might be questionable if SSTs in the Cariaco Basin had idiosyncratic trends between 300 and 900 CE compared to elsewhere in the Caribbean or if SSTs in the Cariaco basin were unrelated to surface temperatures in the Maya region. To evaluate these possibilities, we carried out two analyses. In one we compared the Cariaco Basin SSTs to another set of Caribbean SST reconstructions. In the other we compared the Cariaco Basin SSTs to air temperatures in the Classic Maya region between 1900 and 2000 CE.

To make the first comparison, we searched the online database of the National Oceanic and Atmospheric Administration (www.ncdn.noaa.gov) for other circum-Caribbean SST proxies. We found only one set of records with sufficient resolution and temporal coverage to compare to the Cariaco Basin records. The records we found come from a borehole located off the south coast of Puerto Rico, approximately 800 km north of the Cariaco Basin (Nyberg et al. 2002). The temperature data are reconstructed from foraminifer species abundances and have an average resolution of approximately 70 years. The time stamps for the temperature estimates were derived from an age-depth model that involved 14 high precision radiocarbon dates. Because foraminifer species abundances can distinguish between seasons, the records include winter and summer temperature reconstructions.

As a simple test of whether the Cariaco Basin records corresponds to other circum-Caribbean records spanning 300-900 CE, we used linear regression to determine whether temperature trends in the Cariaco records were similar to those from the Puerto Rico records. We performed four linear regressions corresponding to the two seasonal estimates in each record. In all regressions, temperature was the dependent variable and time was the independent variable. We reasoned that the data from the Cariaco Basin might reflect temperatures elsewhere if the trends from 300-900 CE near Puerto Rico were similar to the trends in the Cariaco Basin from the same period.

The results of the regressions show general agreement among records (see Table S3). All regressions showed a positive correlation between time and temperature, suggesting that all four reconstructions agreed that temperature was increasing from 300-900 CE. The coefficients in the regressions of the summer and winter estimates from Puerto Rico were significant at $p \leq 0.1$ and those involving data from Cariaco were significant at $p \leq .05$. The difference is likely because there are so few observations in the records from Puerto Rico (n=9) compared to the large number in the Cariaco data (n=601). Still, the coefficients are all positive and three of them are quite similar, between 0.001 and 0.002, further suggesting that the temperature trend in the Cariaco data might have been a circum-Caribbean phenomenon.

Despite the agreement among records that temperatures were increasing from 300-900 CE, we decided to use analysis of variance (ANOVA) to test whether the slopes of the trends were similar. We reasoned that if the slopes differed, it would suggest that the amount of temperature change from 300-900 CE might have been significantly different between the Cariaco and Puerto Rico records. ANOVA is a standard way to compare regression coefficients using dummy variables and interaction terms. First, we combined the four temperature series and created a dummy variable to indicate which series each observation belonged to. Next, we ran the ANOVA using the R function aov(), setting up an interaction term between time and the dummy variable. The interaction term allowed us to determine if the regression coefficients—i.e., the slopes—differed among the four temperature series.

The ANOVA results initially indicated that the interaction term was significant, suggesting that the four temperature trends had different slopes (see Table S4). But looking at the slope values from the original regressions, it was clear that one series stood out—the summer SST series from the Cariaco Basin. To test whether it really was an outlier, we re-ran the ANOVA leaving out the Cariaco summer SST record. This test indicated that the interaction term was not statistically significant, which suggests that the Cariaco summer SST regression is indeed different. However, the trend in the summer SST series from the Cariaco Basin is still positive. Thus, all four temperature series have positive time-trends, showing an increase in temperatures from 300-900 CE, and three of the four series also have statistically indistinguishable slopes suggesting that even the rate of increase might have been consistent. This suggests that the positive temperature trend from 300-900 CE was not unique to the Cariaco Basin but rather was a region-wide phenomenon.

In the second comparison, we sought to determine whether the Cariaco SSTs correlated with air temperatures in the Classic Maya region. However, there are no historical temperature series from the Maya region dating to the Classic period in the literature. So, we decided to compare modern temperature records from the Maya region to the modern period of the Cariaco SST reconstruction. We reasoned that we could still test whether the Cariaco SST reconstruction was showing us temperature trends that plausibly reflected temperature trends in the Maya region from 350–900 CE, despite the effects of modern climate change. While global average temperatures have been increasing for the last two centuries, it seems unlikely that global warming would be causing temperatures in the Maya region to correlate with those in the Cariaco basin in a way that they had not done before. We hypothesized that if the modern records and the Cariaco Basin reconstruction did not correlate, then we could dismiss our findings as certainly spurious.

To perform the comparison, we downloaded decadal global air temperature records spanning the 20th century from the website of the Climate Research Unit of the University of East Anglia (http://www.cru.uea.ac.uk/). From those data, we extracted a time-series of Classic Maya region air temperature records located at 90.25° W, 17.25°N, a point in the northern Department of Peten, Guatemala, close to the Classic Maya centre of Tikal. We then performed a time-series regression using those decadal temperature records and the Cariaco SST reconstructions spanning the period 1900-2000 CE.

Our results showed a strong correlation between the Cariaco SST reconstruction and modern air temperatures ($R^2=0.74$, $p=0.001$). Next, we used the Portmanteau method to test the residuals of the regression for autocorrelation, so that we could see if autocorrelation in the original temperature series had biased our results. The test showed that the residuals were not autocorrelated up to the third lag ($ChiSq=0.86$, $p=0.65$, $df=2$), which accounted for 30% of the length of the series. This means that our results were probably not biased by autocorrelation. Thus, the available evidence suggest that the Cariaco Basin SST reconstruction can be used as an indication of temperature trends over the Maya region, supporting our findings.

Sensitivity Analyses

Binning potentially adds bias to analyses of the type reported here because of the arbitrary locations of bin edges and bin width. With this in mind, we performed two sensitivity tests to assess the robustness of our results (see Table S1). In the first test, we shifted the bin edges of the 25-year intervals. Shifting bin edges means that some conflict events would move from one bin into a neighbouring bin, potentially changing the binned onset of sharp changes in the number of conflicts. The edges were shifted 25 times by +1 years each time. All of the shifted analyses indicated that the model involving Cariaco winter SST was far more likely than any other model, including the benchmark model. Furthermore, in at least 85% of the analyses, our results consistently showed that no rainfall proxy substantially out-performed the benchmark. Thus, it seems that the results were robust to changing the locations of bin edges.

In the second test, we evaluated the impact of the 25-year bin size on our findings (Table S3). To do so, we re-ran the PEWMA models and AIC comparisons using 10-year intervals. The results of this analysis indicated that the Cariaco winter SST model was approximately 10 times more likely than the benchmark and the models involving rainfall proxies. According to the re-analysis, one degree of temperature increase corresponded to a 116% increase in Classic Maya conflict levels. This suggests that the direction of the relationships and relative performance of the models are robust to variations in bin size. It also indicates that short-term effects are smaller than long term ones, as expected.

Table S1.

Full PEWMA Results—attached as a separate XLSX spreadsheet. This table shows all of the PEWMA modeling results using 25-year bins. Bin edges were shifted in increments of +1 years for each analysis—the analyses are numbered 1–25. Bold text in the AIC and %Conflict columns highlight the best model results in each case. Note, however, that the CI values remain logged in this table.

Table S2.

Extended PEWMA Results— attached as a separate XLSX spreadsheet. This table shows the modeling results when monument numbers were including along with SST as a covariate. We used these to calculate the effect size presented in the main text.

Table S3. SST Trend Analysis Results. This table shows the results of regressions comparing SST reconstructions to time.

Temperature Record	Coeff.	Std. Err.	t value	Pr	R-squared	DF
Cariaco (w)	0.0013	0.0001	11.17	0	0.172	599
Cariaco (s)	0.0001	0.0001	2.43	0.015	0.01	599
Puerto Rico (w)	0.0024	0.0013	1.91	0.098	0.343	7
Puerto Rico (s)	0.0023	0.0012	1.9	0.099	0.341	7

Table S4. SST ANOVA Results. This table shows the results of the ANOVA comparing slope coefficients between the regressions from Table S3. The first section is an ANOVA involving all regression models. The second is an ANOVA excluding Cariaco summer SST.

All Four Temperature Series					
Var	Df	Sum Sq	Mean Sq	F Value	Pr(>F)
time	1	19.19	19.19	124.86	0
dummy	3	255.32	85.11	553.62	0
time:dummy	3	14.57	4.86	31.58	0
Residuals	1212	186.32	0.15		
Excluding Cariaco Summer SST					
Var	Df	Sum Sq	Mean Sq	F value	Pr(>F)
time	1	32.56	32.56 126.304	0	
dummy	2	14.97	7.48	29.028	9.02E-13
time:dummy	2	0.65	0.33	1.265	**0.283**
Residuals	613	158.03	0.26		

Table S5: Classic Maya Conflict Data.

Monument Location	Monument	Site(s) Named	Site(s) Named	Gregorian Year	Reference	Notes	Likely Duplicates
Aguateca	Stela 02			737	Kennet et al. 2012		
Bonampak	Lintel 04			603	Kennet et al. 2012		
Bonampak	Column			715	Kennet et al. 2012		
Bonampak	Lintel 03			741	Kennet et al. 2012		
Bonampak	Lintel 01			787	Kennet et al. 2012		
Bonampak	Lintel 02	Yaxchilan		792	Kennet et al. 2012		
Caracol	Altar 21	Mutal (Tikal)		556	Kennet et al. 2012		
Caracol	B16 Stucco			626	Kennet et al. 2012		
Caracol	Stela 03			627	Kennet et al. 2012		

Caracol	Stela 03	Naranjo		632	Kennet et al. 2012		
Caracol	Altar 23			801	Kennet et al. 2012		
Caracol	Altar 12	Mutal (Tikal)		820	Kennet et al. 2012		
Caracol	Altar 12	Mutal (Tikal)		820	Kennet et al. 2012		1
Caracol	Altar 12			820	Kennet et al. 2012		1
Chichen Itza	Casa Colorada frieze			870	Kennet et al. 2012		
Chichen Itza	Las Monjas			880	Kennet et al. 2012		
Chichen Itza	Las Monjas			880	Kennet et al. 2012		1
Chichen Itza	Temple of the Four Lintels Lintel 02			881	Kennet et al. 2012		
Chinikiha	Throne			574	Kennet et al. 2012		

Dos Pilas	HS 04, Step 4			648	Kennet et al. 2012		
Dos Pilas	HS 02 east, Step 04	Mutal (Tikal)		657	Kennet et al. 2012		
Dos Pilas	HS 02 east, Step 01			664	Kennet et al. 2012		
Dos Pilas	HS 02 east, Step 01			664	Kennet et al. 2012		1
Dos Pilas	HS 02 west, Step 04			678	Kennet et al. 2012		
Dos Pilas	HS 04, Step 3			678	Kennet et al. 2012		
Dos Pilas	HS 02 west, Step 03			679	Kennet et al. 2012		
Dos Pilas	HS 02 west, Step 03			679	Kennet et al. 2012		1
Dos Pilas	HS 04, Step 5			679	Kennet et al. 2012		1

Dos Pilas	Stela 02	Seibal		736	Kennet et al. 2012		
Dos Pilas	Stela 02			736	Kennet et al. 2012		
Dos Pilas	Stela 02			736	Kennet et al. 2012		1
Itzan	Stela 17			746	Kennet et al. 2012		
Itzan	Stela 17			768	Kennet et al. 2012		
Itzan	Stela 17			768	Kennet et al. 2012		1
Itzan	Stela 17			781	Kennet et al. 2012		
Ixkun	Stela 02			780	Kennet et al. 2012		
Ixkun	Stela 02	Ucanal		780	Kennet et al. 2012		
Ixlu	Altar 01			880	Kennet et al. 2012		
La Amelia	HS 01, Block 04			805	Kennet et al. 2012		

La Mar	Stela 03			794	Kennet et al. 2012		
La Mar	Stela 03			794	Kennet et al. 2012		
La Pasadita	Lintel 01			759	Kennet et al. 2012		
Laxtunich	Lintel 01			784	Kennet et al. 2012		
Laxtunich	Lintel 01			784	Kennet et al. 2012		
Laxtunich	Lintel 01			784	Kennet et al. 2012		1
Machaquila	Stela 02			801	Kennet et al. 2012		
Naranjo	Stela 28			720	Kennet et al. 2012	No event date was provided. The monument date was used instead.	
Naranjo	Stela 35			801	Kennet et al. 2012	No event date was provided. The monument date was used instead.	

Naranjo	Stela 22			693	Kennet et al. 2012		
Naranjo	Stela 22			693	Kennet et al. 2012		1
Naranjo	Stela 22			694	Kennet et al. 2012		
Naranjo	Stela 22			694	Kennet et al. 2012		
Naranjo	Stela 22			694	Kennet et al. 2012		1
Naranjo	Stela 22	Mutal (Tikal)		695	Kennet et al. 2012		
Naranjo	Stela 22	Mutal (Tikal)		695	Kennet et al. 2012		1
Naranjo	Stela 22	Ucanal		696	Kennet et al. 2012		
Naranjo	Stela 22	Mutal (Tikal)		699	Kennet et al. 2012		
Naranjo	Stela 24			699	Kennet et al. 2012		
Naranjo	Stela 23	Yaxha		710	Kennet et al. 2012		

Naranjo	Stela 23			711	Kennet et al. 2012		
Naranjo	Stela 02			713	Kennet et al. 2012		
Naranjo	Stela 30			715	Kennet et al. 2012		
Naranjo	Stela 13			775	Kennet et al. 2012		
Naranjo	Stela 12	Yaxha		799	Kennet et al. 2012		
Naranjo	Stela 12			799	Kennet et al. 2012		1
Naranjo	Stela 12			799	Kennet et al. 2012		
Naranjo	Stela 12			799	Kennet et al. 2012		
Naranjo	Stela 12	Yaxha		800	Kennet et al. 2012		
Naranjo	Stela 12			800	Kennet et al. 2012		
Naranjo	Stela 11			597	Kennet et al. 2012		

Palenque	House C HS			599	Kennet et al. 2012		
Palenque	Tablet, east panel			611	Kennet et al. 2012		
Palenque	House C HS	Mutal (Tikal)		659	Kennet et al. 2012		
Palenque	Tablet, middle panel			672	Kennet et al. 2012		
Palenque	Tablet of the Slaves			724	Kennet et al. 2012		
Palenque	Tablet of the Slaves	Kinal		725	Kennet et al. 2012		
Palenque	Tablet of the Slaves	Kol		729	Kennet et al. 2012		
Palenque	Pier A			690	Kennet et al. 2012	No event date was provided. The monument date was used instead.	
Palenque	Tablet of Temple			688	Kennet et al. 2012	No event date was provided.	

	17					The monument date was used instead.	
Piedras Negras	Lintel 04			631	Kennet et al. 2012		
Piedras Negras	Throne 01			780	Kennet et al. 2012		
Piedras Negras	Throne 01			781	Kennet et al. 2012		
Piedras Negras	Stela 12			788	Kennet et al. 2012		
Quirigua	Monument 23/Altar O'			787	Kennet et al. 2012		
Quirigua	Monument 23/Altar O'			790	Kennet et al. 2012		
Seibal	Tablet 04			746	Kennet et al. 2012		
Tamarindito	HS 02, Step 3			761	Kennet et al. 2012		
Tamarindito	HS 02, Step 3			761	Kennet et al. 2012		

Tikal	Stela 04			379	Kennet et al. 2012		
Tikal	Stela 18			396	Kennet et al. 2012		
Tikal	Seated Figure from Str. 3D-43			406	Kennet et al. 2012		
Tikal	Ballcourt marker			414	Kennet et al. 2012		
Tikal	Stela 01			451	Kennet et al. 2012		
Tikal	Temple I Lintel 03			696	Kennet et al. 2012		
Tikal	Temple I Lintel 03	Calakmul		696	Kennet et al. 2012		1
Tikal	Temple IV Lintel 03	Yaxha		744	Kennet et al. 2012		
Toninˆ	Monument 141			699	Kennet et al. 2012	No event date was provided. The monument date was used instead.	

Tortuguero	Monument 06 (T Shaped Tablet)			644	Kennet et al. 2012		
Tortuguero	Monument 06 (T Shaped Tablet)			645	Kennet et al. 2012		
Tortuguero	Monument 06 (T Shaped Tablet)	Yompi		649	Kennet et al. 2012		
Tortuguero	Monument 8 (Sarcophagus)			650	Kennet et al. 2012		
Tortuguero	Monument 8 (Sarcophagus)			650	Kennet et al. 2012		
Tortuguero	Monument 06 (T Shaped Tablet)			650	Kennet et al. 2012		

Tortuguero	Monument 8 (Sarcophagus)			650	Kennet et al. 2012		1
Tortuguero	Jade Earflare			656	Kennet et al. 2012		
Yaxchilan	HS 03, Step 1			513	Kennet et al. 2012		
Yaxchilan	HS 03, Step 1			662	Kennet et al. 2012		
Yaxchilan	Lintel 46			662	Kennet et al. 2012		1
Yaxchilan	Stela 20			662	Kennet et al. 2012		1
Yaxchilan	HS 03, Step 1			677	Kennet et al. 2012		
Yaxchilan	HS 03, Step 1			677	Kennet et al. 2012		1
Yaxchilan	Stela 19			681	Kennet et al. 2012		
Yaxchilan	HS 03, Step 3			681	Kennet et al. 2012		

Yaxchilan	HS 03, Step 3			681	Kennet et al. 2012		
Yaxchilan	Lintel 45			681	Kennet et al. 2012		1
Yaxchilan	Stela 15			681	Kennet et al. 2012		1
Yaxchilan	HS 03, Step 3			682	Kennet et al. 2012		
Yaxchilan	Lintel 44			690	Kennet et al. 2012		
Yaxchilan	HS 03, Step 6			698	Kennet et al. 2012		
Yaxchilan	HS 03, Step 2			708	Kennet et al. 2012		
Yaxchilan	Stela 18			729	Kennet et al. 2012		
Yaxchilan	HS 04, Step 3			752	Kennet et al. 2012		
Yaxchilan	HS 04, Step 3			752	Kennet et al. 2012		1
Yaxchilan	Lintel 16			752	Kennet et al. 2012		1

Yaxchilan	Lintel 08			755	Kennet et al. 2012		
Yaxchilan	HS 05			797	Kennet et al. 2012		
Yaxchilan	HS 05			799	Kennet et al. 2012		
Yaxchilan	HS 05			800	Kennet et al. 2012		
Yaxchilan	Lintel 10			808	Kennet et al. 2012		
		Tikal	Caracol	556	Chase and Chase 2003		
		Caracol	Tikal	562	Chase and Chase 2003		
		Yaxchilan	Lacanha	564	Chase and Chase 2003		
		Chinikiha		573	Hassig 1992		
		Yaxchilan		594	Hassig 1992		
		Altun Ha		596	Hassig 1992		

		Caracol	Naranjo	626	Chase and Chase 2003		
		Caracol	Naranjo	627	Chase and Chase 2003		
		Caracol	Naranjo	631	Chase and Chase 2003		
		Caracol	Naranjo	636	Chase and Chase 2003		
		Tortuguero		644	Hassig 1992		
		Tortuguero		645	Hassig 1992		
		Yaxchilan		647	Hassig 1992		
		Tortuguero		649	Hassig 1992		
		Tortuguero		649	Hassig 1992		
		Tortuguero		649	Hassig 1992		
		Tortuguero		649	Hassig 1992		

		Tortuguero		652	Hassig 1992		
		Palenque		654	Hassig 1992		
		Palenque	Site Q	654	Chase and Chase 2003		
		Palenque		659	Hassig 1992		
		Palenque	Yaxchilan	659	Chase and Chase 2003		
		Piedras Negras		662	Hassig 1992		
		Dos Pilas	Machaquila	664	Chase and Chase 2003		
		Dos Pilas		664	Hassig 1992		
		Piedras Negras		669	Hassig 1992		
		Dos Pilas		670	Hassig 1992		
		Dos Pilas	Tikal	670	Chase and Chase 2003		

		Tikal		671	Hassig 1992		
		Dos Pilas		672	Hassig 1992		
		Tikal	Dos Pilas	672	Chase and Chase 2003		
		Dos Pilas		677	Hassig 1992		
		Site Q	Tikal	677	Chase and Chase 2003		
		Naranjo	Caracol	680	Chase and Chase 2003		
		Yaxchilan		681	Hassig 1992		
		Yaxchilan		681	Hassig 1992		
		Yaxchilan		689	Hassig 1992		
		Naranjo	Ucanal	693	Chase and Chase 2003		
		Naranjo	Tikal	695	Chase and Chase 2003		

		Tikal	Site Q	695	Chase and Chase 2003		
		Tikal	El Peru	695	Schele and Matthews 1991		
		Dos Pilas		697	Hassig 1992		
		Naranjo	Ucanal	698	Schele and Matthews 1991		
		Yaxchilan		701	Hassig 1992		
		Dos Pilas	Tikal	705	Chase and Chase 2003		
		Tonina	Palenque	711	Schele and Matthews 1991		
		Tonina	Palenque	711	Chase and Chase 2003		
		Altar de Sacrificios		713	Hassig 1992		

		Yaxchilan		713	Hassig 1992		
		Palenque		723	Hassig 1992		
		Yaxchilan		727	Hassig 1992		
		Yaxchilan		729	Hassig 1992		
		Palenque		729	Hassig 1992		
		Yaxchilan	Lacanha	729	Chase and Chase 2003		
		Yaxchilan		732	Hassig 1992		
		Dos Pilas	Seibal	735	Hassig 1992		
		Dos Pilas	Seibal	735	Chase and Chase 2003		
		Quirigua	Copan	738	Hassig 1992		
		Quirigua	Copan	738	Chase and Chase 2003		

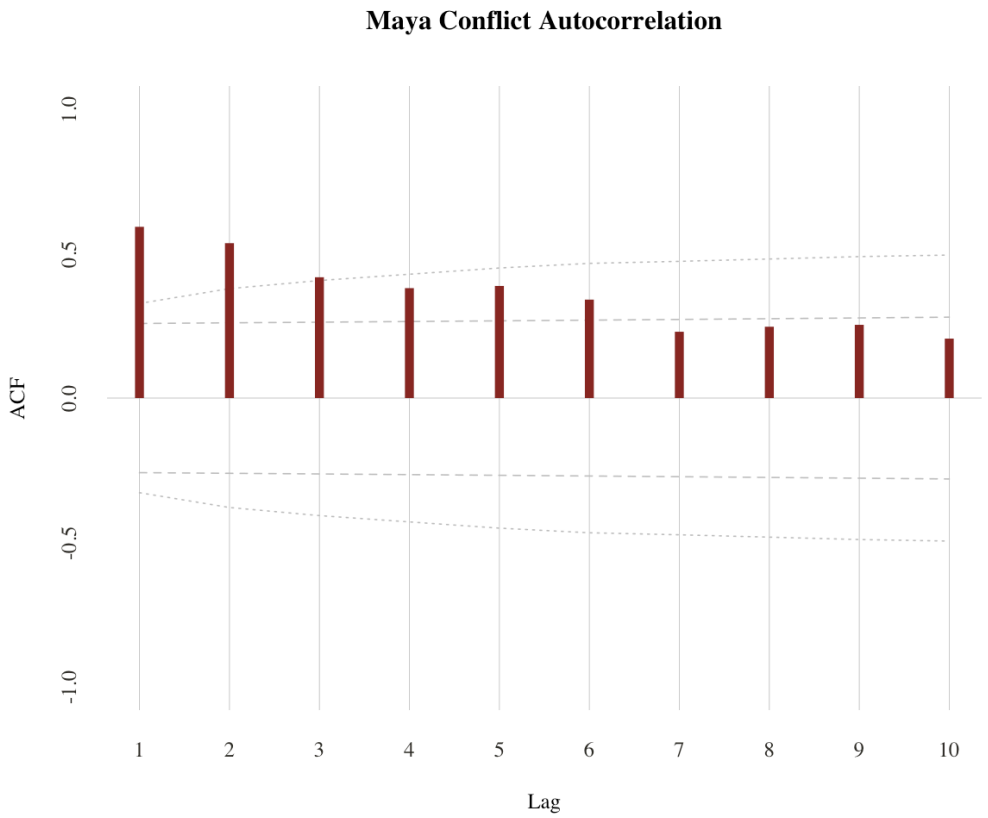
		Bonampak		740	Hassig 1992		
		Aguateca	Cancuen	741	Chase and Chase 2003		
		Machaquila	Motul de San Jose	741	Chase and Chase 2003		
		Tikal	Yaxha	743	Chase and Chase 2003		
		Tikal	Motul de San Jose	744	Chase and Chase 2003		
		Dos Pilas	Yaxchilan	751	Chase and Chase 2003		
		Yaxchilan		752	Hassig 1992		
		Yaxchilan		755	Hassig 1992		
		Aguateca	El Chorro	771	Chase and Chase 2003		
		La Mar	Pomona	774	Hassig 1992		
		La Mar	Pomona	774	Chase and Chase 2003		

		Ixkun		779	Hassig 1992		
				780	Hassig 1992		
				781	Hassig 1992		
			Yaxchilan	782	Schele and Matthews 1991		
		Yaxchilan subsidiary?		783	Hassig 1992		
		Bonampak		787	Hassig 1992		
		Bonampak		787	Hassig 1992		
		Piedras Negras		787	Hassig 1992		
		Piedras Negras	Pomona	787	Chase and Chase 2003		
		La Mar		792	Hassig 1992		

		Copan		793	Hassig 1992		
		La Mar		794	Hassig 1992		
		Piedras Negras	Pomona	794	Chase and Chase 2003		
		Yaxchilan		796	Hassig 1992		
		Yaxchilan		796	Hassig 1992		
		Yaxha captures Bat Jaguar		796	Hassig 1992		
		Yaxchilan		798	Hassig 1992		
		Yaxchilan		798	Hassig 1992		
		Yaxchilan		798	Hassig 1992		
		Yaxchilan		799	Hassig 1992		

		Yaxchilan		800	Hassig 1992		
		Yaxchilan		800	Hassig 1992		
		Yaxchilan		800	Hassig 1992		
		Caracol	Ucanal	800	Chase and Chase 2003		
		Yaxchilan		808	Hassig 1992		
		Caracol	Tikal	819	Chase and Chase 2003		

Supplementary Figure 1. Autocorrelation function plot showing the autocorrelation in the data using 10-year temporal bins.



Supplementary Figure 2. Conflict record compared only to the SST summer record and Classic Maya History.