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Global temperature changes of the last millennium

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Abstract

A review of the various global (or hemispheric) millennial temperature reconstructions was carried out. Unlike previous reviews, technical analyses presented via internet blogs were considered in addition to the conventional peer-reviewed literature.

There was a remarkable consistency between all of the reconstructions in identifying three climatically distinct periods. These consisted of two relatively warm periods - the "Medieval Warm Period" (c. 800-1200 AD) and the "Current Warm Period" (c. 1900 AD on) - and a relatively cool period - the "Little Ice Age" (c. 1500-1850 AD). Disagreement seems to centre over how the two warm periods compare to each other, and exactly how cold, and continuous the cool period was.

Unfortunately, many of the assumptions behind the reconstructions have still not been adequately justified. Also, there are substantial inconsistencies between the different proxy data sources, and between proxy-based and thermometer-based estimates. Until these issues have been satisfactorily resolved, all of the current millennial temperature reconstructions should be treated with considerable caution.

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1 Introduction

In recent decades, there has been considerable interest in trying to accurately quantify how globally-averaged surface temperatures have changed over the last millennium or so.

Some groups, e.g., the University of East Anglia's Climate Research Unit (CRU)[A1, A2] have attempted to estimate global surface temperature changes from thermometer records at various weather stations across the globe. Such analyses have suggested an almost continuous "global warming" trend since at least the late 19th century. However, these estimates only stretch back to the mid-to-late 19th century, as there are only a few longer thermometer records (mostly European).

In the absence of direct temperature measurements before the 19th century, researchers have attempted to estimate past temperatures using *"temperature proxies"*. A temperature proxy is any measurable, temperature-dependent occurrence or process which can be dated (either exactly or approximately). Many different temperature proxies have been used, from Japanese records of the dates when cherry blossom trees bloom[A3] to changes in pollen species in lake or peat bog sediments[A4].

By calibrating temperature proxies to local thermometer records in the period for which they overlap, estimates of long-term temperature trends for the region can be made. These estimates of *local* temperature trends can then be combined with other estimates from different locations around the world to construct *hemispheric* or *global* estimates for the last millennium[A5–A31].

The studies considered here predominantly rely on just a few types of proxy. The most frequently used have been:

1. Tree-rings (either widths or maximum density thicknesses)
2. Isotopic analyses of various depositional substances, e.g., speleothems (i.e., stalactites/stalagmites/etc.), ice cores and lake

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sediments

Some proxies have an annual resolution, e.g., tree rings, while others are less precise and can only be used for studying changes on time-scales of tens (or even hundreds) of years, e.g., sea sediment cores. The higher resolution proxies are usually favoured. But, as the proxies are generally used for estimating long-term trends, the low resolution proxies are also useful[A17, A24, A32–A34].

Early proxy studies, e.g., Lamb, 1965[A5] suggested that over the last millennium, global temperatures varied substantially on ten to hundred year time-scales. It was thought that sometime between c. 800-1200 A.D., there was a "Medieval Warm Period"[A5], while sometime between 1500-1850 A.D., there was a cold period known as the "Little Ice Age"[A35]. In this view, we have recently entered another warm period[A36], which we will call the "Current Warm Period".

In the late 1990s, a few studies suggested that the Current Warm Period was substantially warmer than the Medieval Warm Period, and that recent temperatures were unprecedented in the last millennium[A9–A12]. A 1999 study by Mann, Bradley & Hughes, which extended a 1998 study (sometimes called "MBH99"[A11] and "MBH98"[A10] respectively, after the author initials and year of the studies) was particularly striking.

The Mann et al. studies (Figure 1) suggested that global temperatures had shown little variability over most of the last millennium, other than a gradual cooling from the Medieval Warm Period to the Little Ice Age, but that at the start of the 20th century, temperatures had begun to rise dramatically[A11]. The study's graph of northern hemisphere temperatures of the last millennium became known as the "hockey stick graph", due to its similarity in shape to an ice hockey stick[B1], and henceforth we will refer to the Mann et al., 1998 and Mann et al., 1999 studies collectively as *"the hockey stick study"*.

This iconic image had a very powerful political and social impact as it appeared to vindicate the theory that much of the 20th century global warming suggested by the thermometer-based estimates was due to "man-made global warming". This is a theory which suggests that increasing atmospheric carbon dioxide (CO_2) concentrations from fossil fuel usage is leading to unnatural global warming.

Before the *hockey stick study*, critics of the man-made global warming theory argued that if the Medieval Warm Period had occurred naturally then

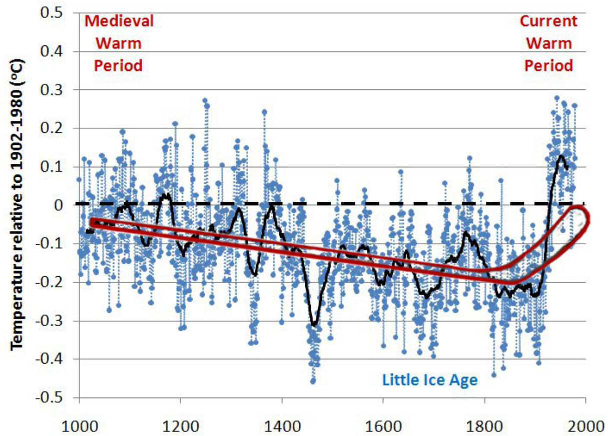


Figure 1: *The Mann et al., 1999 proxy-based estimates of the temperature trends of the last millennium, relative to the 1902-1980 mean, commonly referred to as "the hockey stick graph". Data taken from World Data Center for Paleoclimatology. Solid black line is the 31 year running mean. Red lines show a schematic outline of an ice hockey stick.*

there was no reason to assume the recent global warming was related to CO_2[A37, A38]. Meanwhile, many supporters of the theory agreed that much of the global warming of the Current Warm Period was "natural global warming" but argued that man-made global warming would dominate over natural trends *in the future*, if CO_2 concentrations continued to increase[A39].

The *hockey stick study* initially appeared to discredit both arguments as it implied that the recent global warming was unprecedented in the last millennium, and seemed to be correlated with the increases in CO_2 since the Industrial Revolution. The hockey stick graph featured prominently in both scientific reports[A40] and popular public presentations[B2], and generated considerable scientific and public concern over atmospheric CO_2 concentrations.

However, since then, a number of flaws in the *hockey stick study* have been highlighted[A37–A39, A41–A48]. In addition, many subsequent studies have suggested considerably more temperature variability over the last millennium[A13, A14, A17, A21, A24], even from the authors of the *hockey stick study*[A22].

This topic has become highly contentious. On one side of the debate, some contend that the *hockey stick study* is non-scientific and politically motivated[B3, B4], while on the other side, some contend that *crit-*

icism of the *hockey stick study* is non-scientific and politically motivated[B2, B5]. In this review, we will try to present the arguments from both sides.

A considerable amount of relevant analysis has occurred on "non peer-reviewed" internet weblogs (or "blogs"). However, this analysis has been overlooked in the "peer-reviewed" forums, including recent literature reviews[A34, A49, A50]. For many researchers, this may be due to an unawareness of the analyses, but in some cases it appears to be due to a belief that "non peer-reviewed" analyses have no relevance. This is unfortunate as the merit of an idea or argument does not depend on its source. Hence, we will consider analysis from both forums - references from peer-reviewed sources are denoted with the prefix "A", and non peer-reviewed sources with the prefix "B".

Some blogs have been critical of the *hockey stick study*, e.g., Climate Audit, The Air Vent, Bishop Hill, or Watts Up With That?. Some have defended the *hockey stick study*, e.g., Real Climate, Skeptical Science, or Open Mind. Others have tried to avoid a partisan approach, e.g., Climate Etc., Die Klimazwiebel, The Blackboard, or Collide-a-Scape.

In Section 2, the proxy-based temperature estimates are compared to the thermometer-based estimates which are used for calibrating the proxies. In Section 3, we will discuss some of the problems involved with the proxies used in the studies. In Section 4, the specific criticisms of Mann et al.'s *hockey stick study* are reviewed. In Section 5, the different proxy-based temperature estimates are compared to each other. Finally, in Section 6, conclusions are offered on what the current scientific information tells us and does not tell us, and how future investigation should be approached.

2 Comparison between proxies and thermometer records

2.1 Proxy assumptions

Unlike thermometer measurements, temperature proxies only give indirect estimates of temperature, at best. Palaeoclimatologists hope that, by calibrating the proxies with actual thermometer records, the proxies can provide a reasonable approximation of temperature trends. However, as the thermometer records are not available outside of the calibration period, their accuracy cannot be directly tested. Furthermore, in calibrating (or training) the proxies, some of the following problematic assumptions are often made:

1. The thermometer-based data used for calibration is assumed to be accurate and reliable.
2. Proxy records which have been identified by a researcher as a "temperature proxy" are assumed to contain a strong temperature signal.
3. Assumption of "uniformitarianism", i.e., the current relationship between local temperatures and proxy values existed for the entire proxy record.
4. The relationship between local temperatures and proxy values is assumed to be linear.

Unfortunately, all of the above assumptions are problematic:

- In a series of companion papers, we show that there are a number of serious biases which have not been adequately handled in current thermometer-based estimates[B6–B9].
- Proxy records are *supposed* to be chosen on the basis that they contain a temperature signal. However, the rationale and justification for this basis is not always given. In some cases, the researcher may merely have selected proxies which they believe are *likely* to contain *some* temperature signal. Therefore, some records which are nominally "temperature proxies" might not have **any** actual temperature signal.
- Many temperature proxies could have non-linear temperature responses[A51]. This is particularly problematic if temperatures before the calibration period are believed to have been substantially cooler or warmer than temperatures in the calibration period, as the proxy might not be adequately "trained"[A46–A48].
- If temperature is considered the "limiting factor" for a given proxy, e.g., tree ring growth, then if another factor (precipitation, sunlight, nutrients, etc.) became the limiting factor at some stage over the proxy record, the temperature relationship would have ceased.
- It is quite likely that the "noise" in the proxy record varies over time[A52], therefore the signal-to-noise ratio would similarly vary. This is of particular concern if the noise in the calibration period is substantially smaller or larger than at other stages[A53].

Given the problems with the above assumptions, it is difficult to assess the reliability of the individual proxy records, let alone that of the *global* temperature estimates which use them. A number of different methods have been used to make such assessments.

Some groups favour assessing and selecting proxies on theoretical grounds *before* the records are compared to the calibration data[A34].

Some groups assume that proxies which show the best correlation to the local thermometer records during the calibration period are the most reliable. While this might naïvely appear sensible, it is actually an unjustified assumption, since it is quite likely that the temperature signal of each proxy varied substantially over the duration of the record.

Groups taking this approach then discard[A18, A22] or underweight[A19] proxy series which show a poor match. This may lead to over-fitting problems[A54].

Some groups withhold some of their calibration data for verification purposes[A10, A22]. E.g., for *the hockey stick study*, Mann et al. only used the thermometer-based data for 1902-1980 for calibration, and withheld the 1854-1901 data to test the reliability of their estimates. They did this by comparing their proxy-based estimates of temperatures for 1854-1901 to the thermometer-based estimates[1]. If a reasonable correlation existed, then it might give them some confidence that their temperature estimate had some reliability. However, as we will discuss in Section 4.4, McIntyre & McKitrick[A44, A45] argue that the *hockey stick study* failed these tests.

None of the above methods are entirely satisfactory, although the first method offers the advantage of not relying on the proxy behaviour in the calibration period being constant for the entire proxy record. Perhaps inter-proxy comparisons would offer a useful additional method. However, as will be discussed in Section 3.4, current proxy series often differ substantially in their trends and variability[A32], so this might not be a simple matter.

2.2 Problems with the overlap period

A common mistake made when considering the current proxy-based temperature studies is to directly compare the *proxy-based* temperature estimates in the pre-instrumental period to the *thermometer-based* temperature estimates for the instrumental period. This can lead an unwary researcher into drawing conclusions which are unwarranted by the proxy-based estimates.

There are at least three major reasons why such comparisons should be avoided:

1. If the thermometer-based estimates are used for calibrating the proxies to temperature, then the calibration data can only be used for "training" the proxy-based estimates. If there are features of the calibration data that the estimates are unable to reproduce, then there is no reason to assume it would perform better outside of the training period. A serious danger with this training process which is often overlooked is that the proxy-based temperature estimates may then be affected by the "over-fitting" problem[A54]. This can be particularly problematic for estimates which rely on the apparent fit of a proxy to the training data for determining its "reliability"[B10–B12]. For example, the Mann et al., 2008[A22] approach yields different estimates depending on which calibration period is used - see Figure S10 of the supplementary information on Mann's website[B13].

2. Thermometer measurements are typically both physically and statistically different from the proxy measurements (e.g., tree ring widths, isotopic variations of an ice core), so a direct comparison between them should not be treated with any more seriousness than Sandford, 1995[B14]'s satirical "comparison" between apples and oranges. One approach that is sometimes taken to improve the statistical similarity of the proxies and the thermometer readings is to scale the proxy measurements so that both data sets have a similar variance (i.e., the inter-annual fluctuations are of a similar magnitude). However, it should be remembered that this does not improve the signal-to-noise ratio of the proxy measurements[2].

3. In general, the various proxy-based temperature estimates are only partially able to reproduce the trends and/or inter-annual variability of the

[1] They actually constructed their estimate in a number of different "steps". For each step, they constructed a separate estimate which only used proxies with data in that step - see Section 3.2. All estimates were tested for verification using the 1854-1901 data, but only the data for that estimate's step (e.g., 1450-1400) was included in the final hockey stick graph.

[2] A somewhat outdated, but still useful, analogy is that of increasing the volume on a gramophone when listening to an old scratchy record. The volume of the signal may increase, but so does the volume of the noise.

thermometer-based estimates. In particular, depending on both the proxies used and the period to which the estimates are normalised, two fitting "problems" have been identified as causes for concern. In recent decades, many of the proxies (particularly tree ring widths and densities for high northern latitudes) suggest a *"divergence problem"*, in that they fail to show the strong warming suggested by the thermometer-based estimates, in some cases suggesting a cooling. For the early part of the calibration period (19th century), many of the estimates have a *"convergence problem"*, whereby they show warming trends following "the Little Ice Age", while the thermometer-based estimates show a cooling trend. These two problems will be discussed separately in Sections 2.4 and 2.5.

The thermometer-based estimates of the Current Warm Period usually suggest warmer temperatures than the proxy-based estimates. As a result, directly comparing the thermometer-based estimates of the Current Warm Period to the proxy-based estimates of the Medieval Warm Period makes the Current Warm Period appear more "unusual" than if the proxy-based estimates are considered on their own. Remarkably, this appears to be the sole basis on which the IPCC Working Group 1 made the following statement in their 2007 Summary for Policymakers:

> *"Palaeoclimatic information supports the interpretation that the warmth of the last half century is unusual in at least the previous 1,300 years."* - IPCC, 2007[A55]

In this article, such comparisons will not be made, and as will be seen in Section 5, this leads to more equivocal interpretations. But, it is nonetheless instructive to compare the proxy-based estimates to the thermometer-based estimates.

In Sections 2.4 and 2.5, the main discrepancies often found between the two sets of estimates will be discussed. However, as different proxy-based and thermometer-based estimates are constructed using different techniques, they need to be rescaled before they can be directly compared. In Section 2.3, we describe the rescaling approach we adopted for this article.

2.3 Techniques used in this article

Table 1 lists the 19 proxy-based global or hemispheric temperature estimates for the last millennium which had been published at the time of writing. However, as archived, many of these estimates are not directly comparable. For this reason, we have applied various analytical techniques to the data before comparison.

There are a number of reasons why the original estimates are not directly comparable, e.g., the different estimates (i) cover different regions of the globe, (ii) have different standard deviations, (iii) have been scaled to different mean values and (iv) cover different time periods - see Table 1. To deal with these problems, we will take the following crude approaches, but offer some justifications and caveats:

- We will consider the various "northern hemisphere" and "extra-tropical northern hemisphere" ("NH" and "ext-NH" respectively in Table 1) and "global" estimates to all be equivalent. "Arctic" estimates, such as Kaufman et al., 2009[A23] or Hanhijärvi et al., 2013[A30] are not considered here, although they are similar.

At first glance, a "global temperature estimate" might seem quite different from an "extra-tropical northern hemisphere temperature estimate". However, there is actually a considerable overlap between the proxies used in the various studies. *All* of the estimates have a strong contribution from the extra-tropical northern hemisphere, i.e., the region north of the tropics. In contrast, the southern hemispheric contribution is typically small, and as a result, the nomenclature is somewhat arbitrary. E.g., only 3 of the 18 proxies used in Loehle, 2007[A21]'s "global" estimate are from the southern hemisphere, while 4 of the 12 proxies used in Mann et al., 1999[A11]'s "northern hemisphere" estimate are.

- All proxy-based estimates are rescaled so that they have the same mean and standard deviation as the CRUTEM3 thermometer-based estimates in the common period of overlap (1850-1935).

We should point out that different versions of the thermometer-based estimates were used for constructing different proxy-based estimates. However, typically, a version of one of the Climate Research Unit's datasets was used, and the other thermometer-based estimates which have been used are quite similar. So, we simply used a recent version of the Climate Research Unit's estimate (CRUTEM3)[A1, A2].

Proxy-based millennial temperature estimates:

Model name †	Period covered	Region covered	Season	1850-1935 mean	1850-1935 σ
Jones et al., 1998[A9, A56]	1000-1991	NH$^{(2)}$	Summer	-0.35°C	0.23°C
Mann et al., 1999[A11]	1000-1980	NH	Annual	-0.19°C	0.19°C
Briffa, 2000[A13, A57]	1-1996	ext-NH	Summer	0.12°C	0.52°C
Crowley, 2000[A12, A58]	1000-1965	NH	Annual	-0.04°C	0.09°C
Esper et al., 2002[A14, A59, A60]	831-1992	ext-NH	Summer	1.07°C	0.07°C
Mann & Jones, 2003[A15, A16]	200-1995	NH$^{(2)}$	Annual	-0.28°C	0.07°C
Moberg et al., 2005[A17, A61]	1-1979	NH	Annual	-0.23°C	0.15°C
D'Arrigo et al., 2006[A18] "RCS"	713-1995	ext-NH	Annual	-0.41°C	0.17°C
Hegerl et al., 2007[A19] "long"	946-1960	ext-NH	Annual	-0.16°C	0.14°C
Juckes et al., 2007[A20] "union"	1000-1980	NH	Annual	-0.11°C	0.12°C
Loehle, 2007[A21, A62]	16-**1935**	Global	Annual	-0.08°C	0.07°C
Mann et al., 2008[A22] "CPS"$^{(1)}$	200-1995	NH$^{(2)}$	Annual	-0.33°C	0.13°C
Ljungqvist, 2010[A24]	1-2000	ext-NH	Annual	-0.25°C	0.11°C
McShane & Wyner, 2011[A25]	1000-1998	NH	Annual	-0.34°C	0.11°C
Christiansen & Ljungqvist, 2011[A26]	1000-1975	ext-NH	Annual	-0.46°C	0.44°C
Christiansen & Ljungqvist, 2012[A27]	0-1973	ext-NH	Annual	-0.50°C	0.48°C
Shi et al., 2013[A28] "PC10+AR2"	1000-1998	NH	Annual	-0.32°C	0.09°C
Shi et al., 2013[A28] "CPS"	1000-1998	NH	Annual	-0.37°C	0.18°C
Shi et al., 2013[A28] "EIV"	1000-1998	NH	Annual	-0.34°C	0.13°C
Thermometer-based estimate:					
CRUTEM3[A1, A2]	**1850**-now	NH$^{(2)}$	Monthly	-0.33°C	0.22°C
Central Europe[A52, A63]	1760-2007	C. Eur.	Monthly	-0.71°C	0.65°C

Table 1: *Means and standard deviations (σ) over the common period 1850-1935 of the various proxy-based millennial temperature estimates and two thermometer-based estimates. Data taken from World Data Center for Paleoclimatology, except Briffa, 2000 (Climate Research Unit); Juckes et al., 2007 (Climate Audit website); Loehle, 2007 (National Council for Air and Stream Improvement); McShane & Wyner, 2011 (Article supplementary materials) and CRUTEM3 weather station-based temperature estimate taken from Climate Research Unit.*

† *The updated versions of Mann & Jones, 2003[A15] (Jones & Mann, 2004[A16]) and Loehle, 2007[A21] (Loehle & McCullach, 2008[A62]) were used. But, the original version of Esper et al., 2002[A14] (as the Frank et al., 2007[A59, A60] estimates were unarchived) was used.*

(1) *Mann et al., 2008[A22] did not archive their post-1850 "EIV" estimate.*

(2) *Global and/or southern hemisphere estimates were also available.*

Rescaling the proxy-based estimates to the same mean and variance allows us to directly compare them to each other. However, it also introduces statistical artefacts which can be misleading.

For instance, rescaling different estimates to have the same mean over a specific period, misleadingly implies greater agreement during that period (1850-1935 in our case) and disagreement outside that period[A64].

Also, rescaling different estimates to have the same variance (standard deviation) over a specific period, can be particularly problematic if that period was one with unusually high or low variability. For example, in Figure 2 of Briffa et al., 2000[A13], it can be seen that the period which was chosen for normalisation (1601-1974) was one with unusually low variability for the Tasmania chronology.

It should also be noted that some estimates were constructed with methods which were not *"scale-invariant"*[B15]. As a result, they may lose some of their meaning by rescaling.

- For comparison purposes, estimates will be "smoothed" before plotting by using a 31-year running mean.

This is merely for visual clarity. It should be remembered that all "smoothing" processes remove information, and there is no guarantee that this information is all "noise". Sometimes, unwary researchers may be misled by the apparent clarity of smoothed data into thinking that it has a higher "signal-to-noise" ratio. This is not necessarily the case[B16]. Running means can artificially introduce apparent "trends" which may not exist.

The various proxy-based and thermometer-based estimates (the archived, the rescaled and the smoothed versions) used in this article are included in the Supplementary Information.

2.4 The "divergence problem"

Instead of showing the almost continuous warming trends of the thermometer-based global temperature estimates[A1], many tree ring proxies suggest there was cooling in the second half of the 20th century (at least until the 1980s, when most of the tree rings were collected). This divergence between the thermometer-based estimates and the tree ring-based estimates has come to be known as the *"divergence problem"*[A65–A67][B17].

Jacoby & D'Arrigo, 1995[A68] first noted this for several samples of trees in Alaska, but Briffa et al., 1998a & b[A69, A70] found the same phenomenon across much of the Northern Hemisphere. They suggested that it was mostly confined to the more northerly regions.

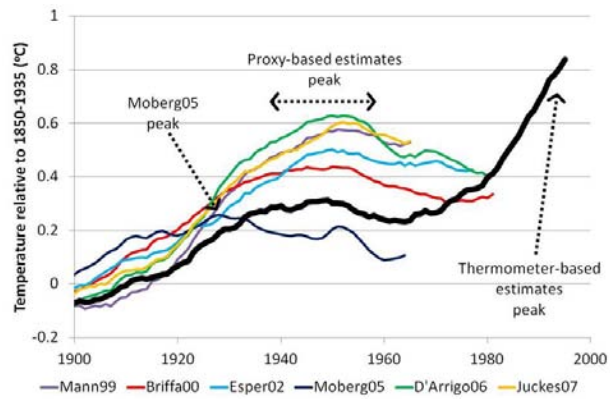


Figure 2: *Comparison of several proxy-based millennial temperature estimates to the thermometer-based CRUTEM3 estimate for the 20th century[A1, A2]. All plots are the 31 year running means of the archived data, rescaled following the procedure described in Section 2.3.*

From Figure 2, it can be seen that this divergence also exists between many of the proxy-based estimates and the thermometer-based estimates, although only 11 of the 19 proxy-based estimates actually consider temperatures after 1980 (see Table 1), and the Loehle, 2007 estimate finishes in 1935[A21, A62]. Many of the proxy-based estimates[3] reach a 20th century peak in the 1940s or 50s, and then show cooling until they finish. Indeed, the Moberg et al., 2005[A17] estimate actually has its peak 20th century temperatures in the 1920s, although temperatures in the 1940s and 1950s are still relatively warm. In contrast, although the CRUTEM3 thermometer-based estimates show a slight cooling in the 1950s and 1960s, they suggest an almost continuous warming from the start of the 20th century.

As it is the post-1950s warming suggested by the thermometer-based estimates which is alleged to be due to "man-made global warming"[A55], the fact that it is not replicated by the proxy-based estimates is significant[4]. It raises the possibility that either (i) there are problems with the thermometer-based estimates, or (ii) proxy-based estimates are somehow

[3] See Supplementary Information for a graph of all 19 (smoothed) estimates in the period 1750-2000.

[4] Although, a few of the estimates do show some late-20th century warming - see Supplementary Information.

unable to detect the recent warming. If the latter applies, then it is possible that the same could have happened during previous warming periods, e.g., during the Medieval Warm Period.

We argue elsewhere[B6–B9] that the apparent strong warming trends in the thermometer-based estimates of recent decades were mistakenly biased warm. Instead, we suggest that, since the late 19th century, there have been two relatively cool periods and two relatively warm periods, with "global warming" *and* "global cooling" between them, i.e., global cooling from the 1950s-1970s has been underestimated, while global warming since the 1980s has been overestimated.

If this is accurate, then the so-called divergence problem is not necessarily a proxy "problem". However, as mentioned in Section 2.1, most researchers constructing global or regional temperature proxy constructions have assumed (either implicitly or explicitly) that the thermometer-based temperature estimates are completely reliable. Therefore, they assume that the apparent divergence is a problem exclusively with the proxies.

On this basis, several researchers have even removed the "diverging" data or replaced it with thermometer-based estimates[B18]. One popular justification for doing that is that the divergence *might* be just a recent phenomenon, due to some kind of human activity[A69, A70] - see D'Arrigo et al, 2008[A65] for a brief summary of such theories. We find it difficult to see why such a speculative, untested (possibly untestable) hypothesis should be used as the sole basis for discarding a critical portion of the proxy-based temperature estimates[B19–B21].

Regardless, later research has suggested that the divergence problem is not as well-defined as originally proposed. A number of studies have found that even in regions where some trees show divergence, others can be found which do not show divergence[A65, A67, A71–A73][B21, B22]. These findings have been quite divisive in dendroclimatological circles, as can be seen by reading the review comments of Wilmking et al., 2008[B22].

From a reanalysis of their earlier work in Esper et al., 2002[A14], Cook et al., 2004[A59] agreed with Briffa et al.'s suggestion[A69] that the problem was real but limited to the more northerly regions. They came to this conclusion by dividing the sites used in the Esper reconstruction into two halves - north and south. The northern half showed a divergence, while the southern half did not. However, McIntyre pointed out[B20] that the southern half consisted of only 5 sites, and two of those sites were foxtail sites (a problem discussed in Section 3.5.1). Moreover, the chronologies constructed from the two subsets disagreed radically over the strength of the Medieval Warm Period, and the two subsets were poorly correlated to each other, although it seems that they did give reasonable correlation statistics over the specific 1200-1950 period Cook et al. chose.

More recently, Esper et al., 2010[A74] found that temperature-sensitive trees in Siberia were following local weather station temperature trends after all. They suggested that the divergence problem may simply have arisen from inappropriate tree ring standardisation approaches, and by not considering the possibility of errors/biases in the weather station records. Interestingly, the local weather station temperature trends in Siberia appear to have been quite modest compared to the warming trends of the global weather station-based temperature estimates.

2.5 The "convergence problem"

Another noteworthy discrepancy between the thermometer-based and proxy-based estimates occurs near the start of the thermometer-based estimates. From Figure 3, it can be seen that most of the proxy-based temperature estimates suggest strong global warming following "the Little Ice Age" during the 19th century. However, the first few years of the global thermometer-based estimates (solid black line) suggest global cooling.

While the early portion of the global thermometer-based estimate is too short a period to assess if this is significant or not, there are a few long instrumental records which cover a longer period, e.g., England[A75, A76], Sweden[A77, A78], Iceland[A35]. A few groups have combined some of these long records together to construct long European temperature estimates, e.g., Dobrovolný et al., 2010[A52, A63]. These longer records (thick red line in Figure 3) suggest the apparent *"convergence problem"* is significant.

For some of the proxy-based estimates, the convergence is less pronounced (see Supplementary Information), e.g., the *hockey stick study*[A10, A11] or Loehle, 2007[A21, A62]. However, it should be noted that some of the proxy-based estimates actually included long European thermometer records as "temperature proxies"[A10, A79], so this may have partially hidden the convergence problem for some of the studies.

Some researchers have suggested that inadequate

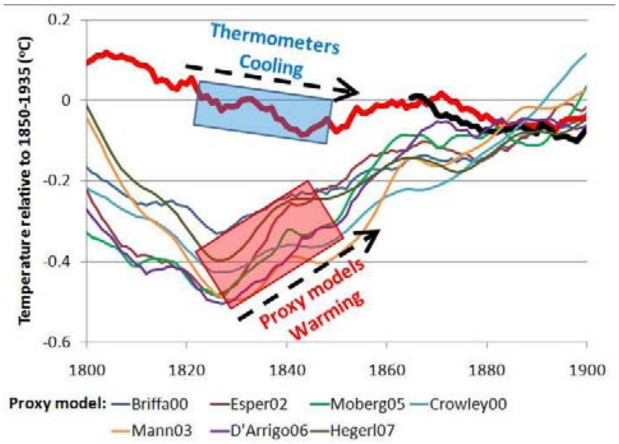


Figure 3: *Comparison of several proxy-based millennial temperature estimates to the thermometer-based global CRUTEM3[A1, A2] (thick black line) and Dobrovolný et al., 2010[A52, A63] "Central Europe" (thick red line) estimates for the 19th century. All plots are the 31 year running means of the archived data, rescaled following the procedure described in Section 2.3.*

thermometer exposure of the weather stations in the early part of their records led to the recorded measurements of the long records being too warm[A63, A80–A82]. For instance, early measurements were often recorded indoors in well-ventilated rooms, while later measurements were recorded outdoors in specially-designed instrument shelters[A75]. However, Dobrovolný et al., 2010[A63] believed that the station records from which they constructed their Central Europe estimates (the thick red line in Figure 3) had been adequately homogenised to account for the early exposure bias[A82][5].

We argue elsewhere[B6–B9] that there are a number of serious biases in the thermometer-based estimates for recent decades, including issues with station exposure[B9]. So, it is plausible that there are also biases for the earlier periods. However, as for the recent biases, it is a challenging problem to resolve. Hence, we should also consider the possibility that the proxy-based estimates may have exaggerated the apparent coldness of the Little Ice Age.

As an aside, it seems odd that researchers considering the divergence problem (Section 2.4) seem inclined to assume the proxies are at fault, while researchers considering the convergence problem seem inclined to assume the thermometer records are at fault. Perhaps we should be more prepared to recognise that there may be problems with either or even both of the data sets[A52] (a possibility Frank et al., 2007a[A81] do admittedly acknowledge).

[5]They also believed the stations had been homogenised to account for urbanisation, but the *step* bias homogenisation that was used[A83] is often inadequate for dealing with *trend* biases, such as urbanisation[B8], so it is likely urbanisation bias still remains

3 Problems with the proxies

In order to properly assess the reliability of the various proxy-based global temperature estimates, an assessment of the individual proxies from which the estimates are constructed is necessary. Unfortunately, at present, there are a number of problems with many of these proxies. In this section, we will review some of these problems.

For example, many of the proxy series are out-of-date (finishing in the early 1980s or earlier), and most of them are too short to allow direct comparisons between the Current Warm Period and the Medieval Warm Period (Section 3.2), one of the main goals of millennial temperature estimates. A major problem in assessing the proxies is that relevant information on the series is often withheld from the scientific community (Section 3.3). Another difficulty is that there is often substantial disagreement between the temperature trends suggested by different proxy series, even within the same area (Section 3.4). As a result, the trends of different millennial estimates are often highly sensitive to the choice of proxy series used. For this reason, the common use of two particularly problematic groups of proxies by most of the estimates is of particular concern (Section 3.5).

To appreciate the challenges involved in using temperature proxies, the reader may find it helpful to first consider how individual proxy series are compiled. To illustrate this process, we will briefly discuss one of the most commonly-used types of proxies as an example - tree rings.

3.1 Case study: Tree rings as proxies

Like most plants, the growth of a tree depends on a number of factors: e.g., the age and species of tree; the amount of rain the area receives, i.e., soil moisture; nutrient availability; the amount of sunlight during the growing season; the amount of competition from neighbouring trees (for sunlight and/or nutrients and/or water); temperature during the growing season. Insect infestations and fires can lead to scars in tree rings.

If one of these factors is exclusively limiting the growth of a particular tree at a particular time, then it is plausible that changes in the tree rings from year to year can be used as a proxy for changes in that factor. This is the reasoning behind their use as temperature (or similarly precipitation) proxies. However, it is important to remember that, just because a tree's growth might at one point be temperature-limited, it might not always have been.

Dendroclimatologists try to maximise the temperature (or precipitation) signal by selecting trees which should be on average predominantly temperature-limited. For instance, trees at high latitudes (subarctic or "boreal") or at high altitudes near the tree-line ("alpine" after the European Alps), which receive adequate precipitation, and are sparsely populated may be predominantly temperature dependent[A66, A84, A85][B23]. On the other hand, trees growing in a drought-sensitive region may be precipitation dependent, while other trees may be limited by competition for soil nutrients.

In order to construct a useful proxy series from tree rings, dendroclimatologists extract cores from as large a selection of trees (living and/or sub-fossil) from a given area as possible. Ideally, more than one core is taken per tree. Different cores are then lined-up with each other ("cross-dating") and averaged together to construct a regional tree-ring time-line ("chronology"), which can then be used as a proxy series.

However, a major difficulty in the construction of a chronology is the problem of "standardization". As a tree ages, its growth rate may change. But, since it is changes in growth rate which are being used as the temperature proxy, it is important to remove those age-related trends.

A number of standardization techniques have been developed in an attempt to resolve this problem, but removing age-related trends, *without also* removing temperature-related trends, is a difficult challenge. So, each technique has its critics and supporters[A18, A41, A86–A90][B24–B27].

Once a chronology is constructed, it can then be used to generate the desired proxy series, whether it is a proxy for temperature, precipitation or for some other factor. Ideally, to create a temperature proxy, the tree ring growth is calibrated against the local temperature records. But, sometimes, they are calibrated directly against regional (or even global) thermometer-based temperature estimates.

There are different possible approaches for calibrating the proxies. A typical engineering approach might be to compile a table of annual ring widths and the mean local temperature for the corresponding year, or perhaps just for the growing season. A simple model (e.g., linear or a polynomial) could then be fitted to the data for the calibration period ("training data"), and the annual ring width values of the chronology could then be converted into modelled temperatures.

However, such approaches do not seem to be common amongst the dendroclimatology community. Instead, for most of the proxy-based estimates reviewed here, proxy records were calibrated by simply rescaling the record so that they had the same mean value and standard deviation as the thermometer records over the calibration period, e.g., Briffa et al., 2000[A13].

Some groups then calculated the correlation between the calibrated proxy and the thermometer records, and removing (or applying a reduced weighting to) the proxies with the lowest correlations, e.g., Mann et al., 2008[A22]. However, this leads to the danger of "over-fitting" - see Babyak, 2004 for an overview of the problem[A54].

The above introduction should provide the reader with sufficient background to appreciate the basic logic behind using tree rings as temperature proxies. Other types of temperature proxies also have their own issues that need to be similarly considered.

3.2 Lack of long, up-to-date, proxies

One problem which is often unappreciated outside the palaeoclimatology community is the shortage of long temperature proxy series which are available. As a result, many of the same proxy series are re-used in different proxy-based temperature estimates[A41]. This means that different "independent" studies are not as independent as might be first thought. This was particularly problematic for early studies, e.g., Jones et al., 1998 only had 3 millennial proxies for their northern hemisphere estimate and 3 for their southern hemisphere estimate[A9]. Even today, there are still only a few long records [A32].

The *hockey stick study*[A10] and more recently, Mann et al., 2008[A22] attempted to reduce this problem by constructing their estimates in a step-wise manner, and thereby including large numbers of shorter proxy series. The Mann et al., 1998 part of the *hockey stick study* divided up their analysis into several steps: 1980-1820, 1820-1800, 1800-1780, 1780-1760, 1760-1750, 1750-1730, 1730-1700, 1700-1600, 1600-1500, 1500-1450 and 1450-1400[A91]. The Mann et al., 1999 part of the *hockey stick study* then extended the 1980-1400 estimates with an additional 1400-1000 step[A11]. When estimating temperatures for each step, *all* the series that had data for that step were included. Hence, Mann et al., 1998 estimated 1980-1820 temperatures using the complete selection of series (159), but only 22 series for estimating 1450-1400 temperatures[A91]. For the 1400-1000 extension[A11], they only used 12.

In effect, Mann et al. created several different "mini-reconstructions", each only spanning a few decades or centuries (depending on the step). These mini-reconstructions were then stitched together to create a much longer reconstruction of 600 years[A10], 1000 years[A11] or longer[A22]. Although this might initially appear a useful way of incorporating more information into the estimates, it actually leads to a *less informative* estimate. This is because *direct* comparisons are only meaningful for temperatures *within* a given step. For instance, while the temperature estimate for 1400 could be directly compared to the one for in 1450, it could not be directly compared to the one for 1460, since the 1460 temperature was estimated from a statistically different proxy network.

This approach also led to problems when proxy series in one step were replaced with different series. For example, Mann et al., 1998 had used bristlecone/foxtail tree ring proxies which were believed to be affected by non-climatic effects (Section 3.5.1). To address this concern, Mann et al., 1999 decided to apply an *ad hoc* adjustment (Section 4.1) to a series based on those proxies in their 1000-1400 extension[A11]. However, since they only applied the adjustment to the new step, the series was different when used for the 1000-1400 step than for the 1400-1980 steps. This substantially altered the apparent millennial temperature trends[B28].

By relaxing their requirements over what constitutes a "temperature proxy", Mann et al., 2008[A22] dramatically increased the number of proxies considered to 1209 (although only 59 extended back to 1000 A.D.). After this relaxation, they discarded those proxies ($\sim 40\%$) which showed very poor correlation to the thermometer-based data. Naïvely, this might have seemed reasonable. However, it resulted in severe over-fitting problems, meaning that their estimates were only of limited value for describing temperatures outside of their "training period", i.e., before 1850 A.D.

Another problem with the currently-used proxies is that many of them are quite out of date, e.g., finishing in the 1970s or 1980s[B29]. As it is since the 1980s that temperatures are alleged to have become unprecedented due to man-made global warming[A10, A11], this is quite a serious short-coming. Mann has argued that this is because updating proxies is a costly, and labour-intensive activity[B30]. However, this seems to be an exaggeration[B29], as, in response, McIntyre was able to update one of the controversial Graybill bristlecone chronologies (discussed in Sections 3.5.1 and 4) critical for the *hockey stick study* while on a holiday visit to his sister[B31].

3.3 Lack of information

Often the authors of proxy-based studies provide little or no discussion of why they used certain proxies, why they discarded others, why they might have chosen an old version of a series in preference to more recent updates, or the basis for any adjustments/standardisations they may have applied to particular series[B32]. This means other researchers often have to do their own analysis with limited information[B33].

Also, researchers often appear surprisingly reluctant to archive the proxies and/or code they used[B34]. In the case of proxies, this is not always entirely the fault of the researchers. Unfortunately, within the dendrochronology/dendroclimatology community, there is a significant amount of "grey data"[B35]. The owners of this data do not want to make it public, but often allow researchers to *use* it, on the provision they do not pass it on, e.g., some of the proxies used by Esper et al., 2002[B36] or Moberg et al., 2005[B37]. This obviously hampers the abilities of those researchers to archive all of their data.

Without having access to the data from which a study was derived, it can be very difficult to conclusively assess the study. Hence, unresolved discrepancies between different studies cannot be satisfactorily resolved[B38]. For this reason, perhaps it would be best if future proxy-based studies were only carried out using proxy series that the study authors are allowed to archive, i.e., no "grey data".

It is understandable that *in the past* open access to data was often unrealistic. However, with modern internet archives such as the World Data Center for Paleoclimatology, most of those arguments no longer apply. Indeed, it seems that when scientists make their data freely available, it not only helps alleviate suspicions over the validity of their research, but also encourages a better appreciation of their work[B39, B40]. Admittedly, where there are commercial applications for the data, or the research was privately funded, exceptions may be justifiable. But, this does not seem to be an issue for most of the palaeoclimate studies discussed here.

3.4 Lack of consistency

As mentioned in Section 1, a common palaeoclimatic view maintains that there have been three main climatically distinct periods over the last millennium - the Medieval Warm Period[A5], the Little Ice Age[A35] and the Current Warm Period. However, since the 1990s, a few groups have questioned this view. Bradley & Jones, 1992[A92] pointed out that researchers often disagreed over exactly when and where the Little Ice Age occurred, as well as how long it lasted and how severe it was. This raised the question that researchers may have been using confirmation bias[A93] to "identify" a global Little Ice Age in their studies.

Hughes & Diaz, 1994[A94] noted similar problems for the Medieval Warm Period. They also noted a few proxy studies which did not show Medieval Warm Periods. They suggested that the Medieval Warm Period was a "regional" phenomenon confined to areas such as Europe and Greenland. Several studies have since argued that point[A95–A99]. However, a number of other studies have found evidence of a strong Medieval Warm Period in many locations across the world, suggesting that it was a global phenomenon[A21, A33, A37–A39, A62, A100].

One part of the controversy seems to arise out of the inconsistencies between different proxies. Sometimes inconsistencies even exist between different versions of the same proxy series.

For example, Briffa et al., 1995[A101] developed a Polar Urals chronology which was used in several of the early proxy-based temperature estimates[B41]. But, another version[A102] has been used by Esper et al., 2002[A14]. Both chronologies provide considerably different contexts for the Current Warm Period[B42]. The differences between the two Polar Urals chronologies are immediately apparent in Figure 4.

The Briffa chronology implies a cold Medieval Warm Period and even suggests that 1032 A.D. was the coldest year of the millennium. In contrast, the Esper chronology suggests that the Medieval Warm Period was considerably warmer than the Current Warm Period. In addition, it suggests there was a second warm period from about 1400-1600 which was also warmer than the Current Warm Period.

Both of these versions show similar trends since the mid-19th century when the weather station-based estimates begin, so it is difficult to distinguish between them on this basis (Figure 4b). There do appear to be problems with how the Briffa chronology was constructed[B41, B43, B44]. However, some have argued that the Esper chronology also has problems[B45]. More recently, a third chronology from the area (the Yamal chronology) has become popular in proxy-based temperature estimates. But, as we will discuss in Section 3.5.2, this chronology suggests a different context still.

If the Briffa Polar Urals chronology is accurate, then perhaps there was no Medieval Warm Period in that area[A101]. But, if the Esper chronology is accurate, then the Medieval Warm Period was considerably warmer than the Current Warm Period in that area. Perhaps neither is accurate.

While it is true that some proxies fail to show a Medieval Warm Period, the same could be said of the Current Warm Period. If researchers preferentially select[A93] proxies which show strong correlations with the thermometer-based data, i.e., show a warm 20th century, then this would introduce an artificial bias towards an apparently more "homogeneous" Current Warm Period, but not the Medieval Warm Period[A103]. Even if the selection is not carried out by the compiler[A97], it may exist with the researchers who constructed the individual series.

The objection that the dates of the Medieval Warm Period are not always consistent[A94, A95] may well be due to dating errors involved with the proxies[A21]. But, there are other possible explanations, e.g., the temperature "signal" of the proxies may vary over time[A51], or the proxies may show considerable "noise" due to non-temperature related changes.

The inconsistency problem is often accentuated by the compositing of short proxies which do not extend back to the Medieval Warm Period with those which do.

McIntyre pointed out that one of the three northern hemisphere millennial proxies used by Jones et al., 1998[A9] showed little variability over its entire record, i.e., it did not suggest either a Medieval or Current Warm Period, or for that matter a Little Ice Age. This was the Greenland ^{18}O isotope "Crete" record. Perhaps the reason for this is that the proxy

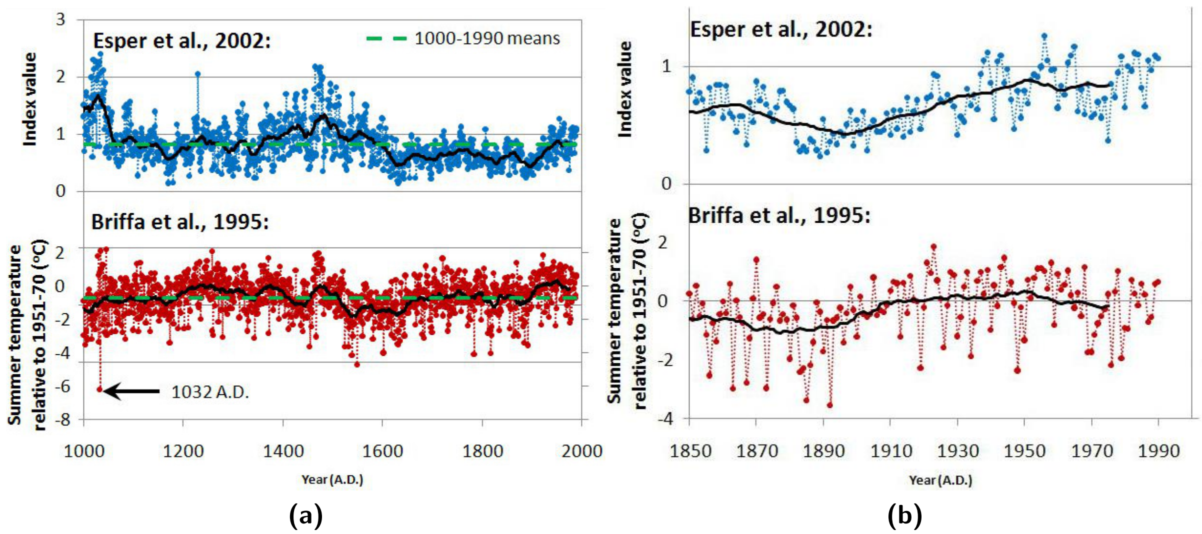


Figure 4: *Two conflicting Polar Urals chronologies. Solid black lines correspond to 31-year running means. (a) 1000-1990. (b) 1850-1990. Data for Briffa chronology taken from* http://www.climateaudit.info/data/briffa/briffa.raw.txt*. Data for Esper chronology taken from* http://www.climateaudit.info/data/esper/*.*

was not particularly temperature sensitive, or maybe these hypothesised climate changes were not as pronounced in that part of Greenland as the rest of the world. Whatever the case, one might initially suppose that the use of the "Crete" proxy series should not alter the relative ratio of the Medieval and Current Warm Periods (for instance) in the global estimates, since they were both similarly dampened.

However, Jones et al. also used several short proxies, e.g., the Jacoby North American tree-line, which showed considerable variability, with a cold Little Ice Age and warm Current Warm Period. When such proxies are averaged with a proxy such as the "Crete" ice core, the Medieval Warm Period would be unaltered by the short series, but the Current Warm Period would be made warmer, and the Little Ice Age made cooler. McIntyre pointed out that this would artificially make the Current Warm Period *appear* to be warmer than the Medieval Warm Period[B46].

As an aside, if McIntyre replaced the other two millennial proxies (Polar Urals and Torneträsk) with other published versions, it substantially altered the Jones et al., 1998 temperature estimates - suggesting a Medieval Warm Period considerably warmer than the Current Warm Period[B47] - the opposite of Jones et al., 1998's original conclusions[A9]. This all suggests that the inconsistencies between different proxies remain a serious problem for constructing reliable millennial proxy-based temperature estimates.

3.5 Problematic proxies

As mentioned in Section 3.2, a surprisingly small number of long temperature proxy records have been used in multi-proxy temperature estimates. This problem is made more serious by the fact that there are known problems with some of the most widely-used proxies and composites, e.g., the Dunde ice cores[B48] and Yang's Chinese proxy composite[B49].

As described in Section 3.4, there are also substantial inconsistencies between individual proxies, even within different versions of the same proxy. We saw in Figure 4 the substantial differences between two different versions of the Polar Urals chronologies. Another example is that of the Torneträsk tree ring chronology. While the original chronology used in a number of the estimates suggested a very warm Current Warm Period[B50], Grudd, 2008's updated chronology[A104] does not[B51].

With such inconsistency, it is difficult to draw definitive conclusions from studies which rely heavily on any one series[A32]. So, it is a serious concern that almost all of the proxy-based temperature estimates rely heavily on at least one of two groups

Millennial temperature estimate	# series	Bristlecones/foxtails		Yamal chronology
		Individual	"MBH PCs"	
Jones et al., 1998[A9, A56]	17			
Mann et al., 1999[A11]	12		√	
Briffa, 2000[A13, A57]	7			√
Crowley, 2000[A12, A58]	15	√		
Esper et al., 2002[A14, A59, A60]	14	√		
Mann & Jones, 2003[A15, A16]	13		√	√
Moberg et al., 2005[A17, A61, A105]	18	√		√
D'Arrigo et al., 2006[A18]	19			√(1)
Hegerl et al., 2007[A19]	14	√	√	√
Juckes et al., 2007[A20]	13	√		
Loehle, 2007[A21, A62]	18			
Mann et al., 2008[A22, A106, A107]	1209(2)	√(3)		
Ljungqvist, 2010[A24]	30			
McShane & Wyner, 2011[A25]	1209(4)	√		
Christiansen & Ljungqvist, 2011[A26]	40	√		√
Christiansen & Ljungqvist, 2012[A27]	32	√		√
Shi et al., 2013[A28]	45	√		√

Table 2: *The use of bristlecone/foxtail pines and/or the Yamal chronology in the various proxy-based millennial temperature estimates (listed chronologically). Bristlecone/foxtail series were either used as individual series, or indirectly through the use of Mann et al., 1999's 1st principal component of the North American tree ring database ("MBH PCs"), which was heavily weighted by bristlecone/foxtail pines (see Section 4.3). (1) D'Arrigo et al., 2006 used Yamal, but in the text they used the name and core counts of a neighbouring chronology - "Polar Urals (POL)"[B45]. (2) Mann et al., 2008 considered a large number of series, but only 59 of them extended back to 1000 A.D. Many of those series were discarded for showing poor correlation to the calibration data. (3) Mann et al., 2008 also carried out a sensitivity test where they excluded bristlecone/foxtails as well as other tree rings, but included the problematic Tiljander lake sediments - see Section 3.5.3. (4) McShane & Wyner, 2011 used the same dataset as Mann et al., 2008.*

of problematic tree rings - bristlecone/foxtail pines (Section 3.5.1) or Briffa et al.'s Yamal chronology (Section 3.5.2) - see Table 2. If these groups are removed or even replaced with plausible alternatives, the relative ratio between the Medieval Warm Period and the Current Warm Period is often altered - specifically, the Medieval Warm Period becomes "warmer" and the Current Warm Period becomes "cooler"[A45][B26]. For instance, for the Shi et al., 2013 "PC10+AR2" and "CPS" estimates, their so-called "dendro" subset which includes both bristlecones and Yamal shows a much colder Medieval Warm Period than their "no-dendro" subset (see Figures 2 and 3 in Shi et al., 2013)[A28].

The use of these proxies does not in itself automatically alter the ratio. For example, although Moberg et al., 2005[A17] used both the Yamal chronology and two foxtail series, they only used the high-frequency components of those series, i.e., they removed their long-term trends, but just kept the inter-annual variability. In addition, although the bristlecone/foxtail pines are all from a similar area (south-western North America), there are significant differences between different chronologies which have been constructed from them, e.g., Lloyd & Graumlich, 1997's foxtail chronologies[A108] suggest a warmer Medieval Warm Period than the Graybill & Idso, 1993[A109] bristlecone/foxtail chronologies.

Nonetheless, as we will discuss in Sections 3.5.1 and 3.5.2, both of these proxy groups have been controversial, so it is surprising that they have not been used with more caution. More importantly, if these specific proxies are critical in establishing the ratio of the two warm periods, then this has serious consequences for the robustness of the studies. Hence, it is worth briefly reviewing the controversy over these two specific proxy groups in Sections 3.5.1 and 3.5.2.

Mann et al., 2008[A22, A106, A107] claimed to have carried out *"sensitivity studies"*, and shown that their estimates were not unduly affected by any individual problematic proxy. For proxy-based studies, sensitivity experiments typically involve constructing several different estimates, each using a different subset of the available proxy series. If the estimates from the subsets are all similar to the estimates from the complete set, this suggests that the estimates are not overly reliant ("sensitive") on any particular proxy series. However, as will be discussed in Section 3.5.3, the Mann et al., 2008 sensitivity studies were very poorly devised. It later transpired that the Mann et al., 2008 estimates relied heavily on including *either* bristlecone/foxtails *or* another proxy series, known as the "Tiljander lake sediments", which were known to be problematic for the Current Warm Period.

Condon illustrated the weakness of the Mann et al., 2008 sensitivity studies by carrying out sensitivity studies on the Ljungvist, 2010a estimate at the blog The Air Vent[B52]. As can be seen from Table 2, Ljungvist, 2010a was one of the three estimates which includes neither bristlecone/foxtails nor Yamal. Condon created a large number of different proxy-based estimates by randomly selecting different subsets of Ljungqvist's proxy network. All of the subsets were relatively similar to the original Ljungvist, 2010a estimate, suggesting that none of the proxy series unduly influenced the estimate, unlike the Mann et al., 2008 estimates.

Since Mann et al., 2008[A22] argue that they obtain similar temperature estimates if they exclude those groups, provided they include the Tiljander lake sediment proxies, we will also briefly assess the Tiljander proxies in Section 3.5.3.

3.5.1 Bristlecone/foxtail pine proxies

One family of trees which has been of considerable interest to climatologists is the bristlecone pine family. This consists of three closely-related five-needled pine species found at high altitudes in the California, Nevada and Colorado mountain ranges - the Rocky Mountains bristlecone pine (*Pinus aristata*); the Great Basin bristlecone pine (*Pinus longaeva*); and the foxtail pine (*Pinus balfouriana*).

The bristlecone pine trees are very long-lived - in some cases being several millennia old. It has been supposed that the highest altitude trees of these species are temperature-sensitive. These two factors initially suggest that they would make promising temperature proxies. However, LaMarche et al., 1984[A110] had noted unusual tree ring growth in bristlecone pines in recent decades, which had no relation to regional climatic trends.

LaMarche et al., 1984 suggested that the unusual growth was due to fertilisation from increasing concentrations of atmospheric CO_2, although this theory was controversial[A111]. In order to investigate this theory, Graybill & Idso, 1993[A109] sampled various bristlecone and foxtail pines. As well as the regular (*"full-bark"*) trees, *"strip-bark"* trees were also sampled. Strip-bark trees are pine trees where a lot of the bark has peeled off, leaving only strips of bark.

Graybill & Idso believed that the strip-bark trees would be more influenced by changes in CO_2. Indeed, they found a rapid increase in growth rate after the mid-19th century in the strip-bark trees, but not the full-bark. They agreed with LaMarche et al., 1984 that this dramatic growth was *not* related to local temperature changes, but was merely a consequence of CO_2 fertilisation.

Despite Graybill & Idso's explicit statement that the unusual growth rate of their strip-bark pines was non-climatic, the *hockey stick study* used the Graybill strip-bark chronologies as temperature proxies, contributing strongly to its "hockey stick" shape[A44, A45]. Before the *hockey stick study*, none of the proxy-based estimates used these proxies[B53] as it was generally agreed that their rapid 20th century growth was *not* due to temperature[B53–B55]. But, it can be seen from Table 2 that they have been heavily used since.

LaMarche et al.'s theory of CO_2 fertilisation was criticised because it had not been detected in other tree species[A112] or in the full-bark pines[A108], which appears a valid point. But, various other non-climatic explanations have been suggested for the unusual growth[A42, A108]. So, to justify the widespread use of bristlecone/foxtails in proxy-based temperature estimates, it is important to provide some evidence that its anomalous growth is related to local temperatures.

McIntyre specifically compared several of the Graybill pines to local temperatures, and found they were very poorly related[B56]. In addition, other tree ring studies in the area found the Current Warm Period to be comparable to the Medieval Warm Period[A108, A113, A114]. Indeed, when an update was carried out on a Graybill chronology which had originally shown particularly strong 20th century growth, the 20th century growth no longer appeared unusual[B57]. A recent isotopic analysis of several bristlecone trees also failed to identify anomalous 20th century climate change[A115]. After carrying out an update of another Graybill chronology, McIntyre noted that the recent sharp growth in strip-bark cores was often countered by reduced growth in other cores from the same tree. He suggested that the unusual growth may be related to the elliptical growth of strip-barked trees, rather than a climatic effect or CO_2 fertilisation effect[B58].

Bunn et al., 2005[A116] claimed that the unusual growth of the bristlecones in the 20th century was temperature-related. However, their entire basis for this claim was that the bristlecone growth was similar to the *hockey stick study*. As McIntyre pointed out[B59], this was effectively circular logic since the *hockey stick study* was itself heavily dominated by the Graybill pines.

Later, Salzer et al., 2009[A117] claimed to have vindicated the use of strip-bark bristlecones as temperature proxies. They had updated several of the Graybill proxies on Sheep Mountain. They then compared the bristlecone growth rates to those of other tree ring measurements in a similar area - the "MXD" measurements of Rutherford et al., 2005[A118]. They found a reasonable match during the period 1630-1950, and therefore concluded that if the Rutherford MXD measurements were reliable, then so were their updated bristlecones. However, the Rutherford MXD measurements do *not* show the post-1900 "hockey stick" shape of the updated bristlecones (see Figure 5 of Salzer et al., 2009[A117]). Hence, that argument of Salzer et al. is limited to suggesting the bristlecones may have some merit *before* the contentious "hockey stick" rise.

Salzer et al., 2009 also argued that the characteristic "hockey stick" trend occurred in both the whole bark and the strip-bark pines - contradicting Graybill & Idso, 1993[A109]'s findings. They suggested that the contradiction was due to an inappropriate standardization used by Graybill & Idso. Hence, they compared the non-standardized chronologies of the whole-bark and strip-bark pines. They found no substantial difference between the two chronologies in the modern period[A117]. On this basis, they concluded that there was no divergence between the strip-bark and whole-bark. However, in Figure S4 of their Supplementary Information[A117], it is apparent that when they took this approach, there was a divergence *before* the 20th century. Hence, that particular argument appears very weak[B60].

Recently, Salzer et al., 2013 have put up another argument[A119]. When they compared their bristlecone chronology to three different Global Climate Model (GCM) simulations of the last 1000 years, one of the simulations ("ECHO-G2") showed a similar trend to their bristlecone chronology. However, we note that neither of the other two simulations ("MPI" or "CSM") showed this trend (see their Figure 4). So, we do not consider this a particularly compelling argument.

Bearing all of this in mind, there should be serious concern over the estimates which used bristlecone/foxtail pines. As can be seen from Table 2 this includes most of the millennial estimates. Even if part of the sharp 20th century up-tick in some of the bristlecone/foxtail pines is found to be due to temperature change[B61], considering the controversy over them, it is surprising they are so widely used.

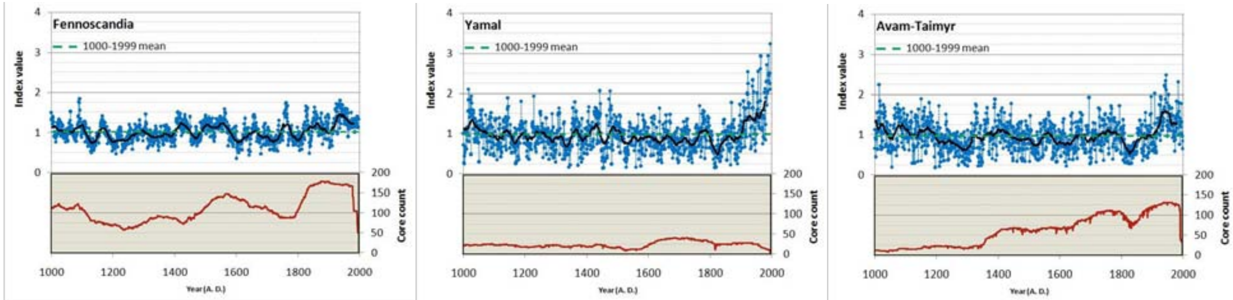
3.5.2 The Yamal chronology

Briffa, 2000[A13] introduced the Yamal chronology, which showed dramatic growth in the 20th century. As can be seen from Table 2, it has been extensively used since. However, for such a widely used proxy, it has a number of problems.

Briffa et al., 2008[A120] revisited this Yamal chronology and created two other northern Eurasian chronologies - Fennoscandia and Avam-Taimyr. All three of these chronologies were located at around 62.5^oN, at different locations on the Eurasian continent. However, they each present rather different estimates for temperatures of the last millennium (Figure 5). If the strong 20th century growth rate of the Yamal chronology is genuinely representative of global temperatures, then it is hard to see why it is largely absent from the other two chronologies (from the same latitude and continent). Indeed, on the basis of the number of cores used for the construction of the chronologies (bottom panels of Figure 5), Yamal would appear the least reliable of the three.

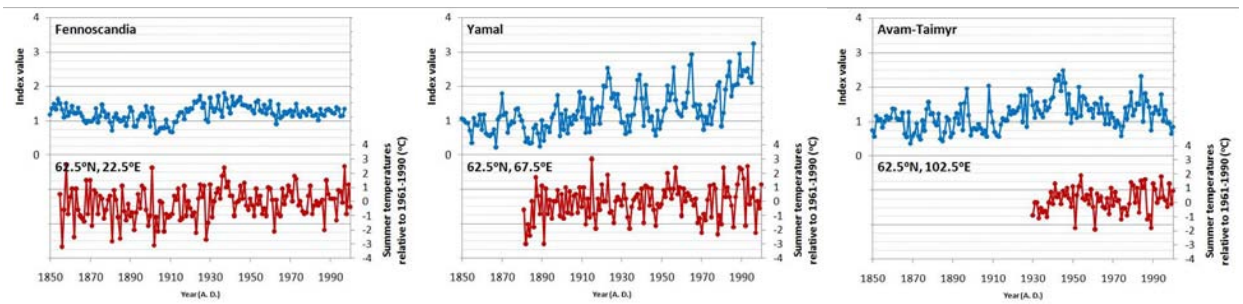
Briffa et al. implied that all three chronologies showed a reasonable correlation with local summer temperatures (e.g., see Figure 1 of Ref. [A120]). However, from Figure 6, this is not immediately obvious. Certainly, the distinctive 20th century growth implied by the Yamal chronology appears to be absent from the corresponding local gridded temperatures (Figure 6b).

Following the publication of Briffa et al., 2008, Briffa finally archived the data for the Yamal chronology after several years of requests from McIntyre[B62]. McIntyre noted that only a few trees (17) were used for constructing the recent portion of the



(a) **(b)** **(c)**

Figure 5: *The three northern Eurasian chronologies given in Briffa et al., 2008[B24], and the numbers of cores used for their construction. Data taken from* `http://www.cru.uea.ac.uk/cru/people/melvin/PhilTrans2008/`*. Thick black lines correspond to 31-year running means.*



(a) **(b)** **(c)**

Figure 6: *Comparison of local gridded (weather station-based) summer temperatures (June-August) to the three chronologies described in Figure 5. Data taken from* `http://www.cru.uea.ac.uk/cru/people/melvin/PhilTrans2008/`*.*

Yamal chronology[B63], i.e., the living samples. In addition, one of the trees, YAD061, showed 8 standard deviations of growth in the 20th century - a remarkable growth rate, which was not matched by any of the others. This had noticeably increased the 20th century average of the chronology[B64].

McIntyre carried out two sensitivity experiments for the Yamal chronology. In one experiment, he removed 12 cores and replaced them with 34 archived cores from the Khadyta River (which was in the Yamal area). In the other experiment, he added the 34 cores to the complete Yamal chronology. In the first experiment, the unusual 20th century growth was replaced with a decline. In the second experiment, the 20th century growth was higher than in the centuries immediately preceding it, but comparable to growth at various stages over the last two millennia, including the 11th and 15th centuries[B26].

Briffa et al. criticised these experiments[B65, B66] and suggested that the cores McIntyre had selected were anomalous and arbitrarily chosen. However, McIntyre argued that he had done a better job of justifying his selection than Briffa had for his selections[B67]. He also argued that the 17 living cores in Briffa's original chronology were inhomogeneous[B68], i.e., there was little consistency from core to core and between them and the sub-fossil cores, and that the Khadyta River cores showed better homogeneity.

Condon argued that the "hockey stick" shape of Yamal was due to Briffa's age-related tree ring standardisation (see Section 3.1), and argued that other plausible standardisations yielded 20th century growth rates that were fairly average[B69].

Recently Briffa et al., 2013 has revised the Yamal chronology[A121], apparently reducing the magnitude of the "hockey stick" up-tick in the process[B70]. At any rate, whether the Yamal chronology has any merit as a temperature proxy[B45] or not[B71], it is striking that its distinctively sharp 20th century growth is absent from the other Briffa et al., 2008 chronologies (Figure 5) as well as the two versions of the nearby Polar Urals chronology (Figure 4). It also fails to detect the strong Medieval Warm Period others have reported in the area[A122]. This suggests that it should only be used cautiously in proxy-based temperature estimates, if at all.

3.5.3 The Tiljander lake sediments

Following criticism[A38, A41, A42, A44, A45, A123, A124] of Mann et al.'s *hockey stick study*[A10, A11, A91, A125] for being highly dependent on the Graybill strip-bark pines described in Section 3.5.1, Mann et al., 2008[A22, A106, A107] boasted of being robust to the exclusion of either (a) tree rings or (b) a new set of 7 other potentially problematic proxies.

Four[B72] of these 7 non-tree ring problematic proxies were Tiljander et al., 2003[A126]'s Lake Korttajärvi sediment cores from Finland. Tiljander et al. had constructed a 3,000 year long chronology from lake sediments which suggested a strong Medieval Warm Period around 980-1250AD with several cool periods during the 16th, 17th and 18th centuries, possibly corresponding to the Little Ice Age.

However, after about 1720AD, the sediments appeared to have become increasingly contaminated by local human activity, e.g., wastewater run-off, bridge construction, leading to anomalously low apparent "temperatures". Therefore, Tiljander et al. had stressed that much of the post-1720 variability was strongly non-climatic.

Recognising that there was a problem with the post-1720 portion of the proxies, Mann et al., 2008 treated the Tiljander proxies as having *"potentially spurious features"*[A22]. However, Mann et al. still decided to use the complete proxies including the contaminated sections, anyway.

As an additional problem, Mann et al. effectively used two of the proxies in the opposite manner to that intended by Tiljander et al. suggesting a cold "Medieval Warm Period", mild "Little Ice Age" and a "hockey-stick" like warming for the Current Warm Period[B73].

Mann et al., 2008 created two separate sets of estimates - one using a composite-plus-scale approach ("CPS") and one using a climate field reconstruction ("EIV"). For the CPS estimates, the inversion of the Tiljander proxies from their intended interpretation appears to have been manually done.

For the EIV estimates, the inversion was an implicit feature of the algorithm which altered the sign of the proxy to yield the highest correlation with the weather station-based calibration temperatures of the Current Warm Period. Since the post-1720 portion of the proxy was non-climatic, any apparent correlation between temperature and the proxy in this period would be just a coincidence. However, the EIV algorithm does not consider this possibility. Hence, the sign of the proxies were adjusted by the algorithm so that the non-climatic portions appeared to show "warming" in the Current Warm Period[B74].

This second approach was also carried out in Mann et al., 2009[A98]. Kaufman et al., 2009[A23] also used these Tiljander proxies inverted in their Arctic analysis, in the same way Mann et al. had used them in the CPS estimate. However, when Kaufman et al. discovered that this was not how Tiljander et al. had intended them, they issued a correction to revert the sign back to the original interpretation[A127].

Mann et al., 2008 relied on the Tiljander proxies for their claim that their estimates were not dependent on the use of the bristlecone/foxtail pines[A22]. If they carried out a sensitivity analysis by removing all tree-ring proxies (including the bristlecone/foxtail pines), they obtained a similar estimate to their complete analysis. However, that "no-dendro" estimate included the four Tiljander proxies (with the contaminated portions) as well as another three proxies they had identified as potentially problematic. To test if they were a problem, they carried out a second sensitivity analysis by removing the 7 non-tree ring potentially problematic proxies, but leaving all the others (including the bristlecone/foxtail pines) in. This also yielded a similar estimate. On this basis, they concluded that their estimate was not biased by any particular proxy.

Strangely[B75], they did not carry out the simple test of *just* removing the 7 non-tree ring proxies they had identified as potentially problematic *and* the bristlecone/foxtail pines that the *hockey stick study* had specifically been criticised for using[A38, A41, A42, A44, A45, A123, A124]. Nonetheless, after much debate on the blogs over the reliability of the Tiljander proxies (see links at Ref. [B76]), Mann et al., 2009 included in Figure S8 of their S.I.[B77], results of an additional sensitivity analysis carried out for Mann et al., 2009[A98], which was equivalent to the EIV estimate of Mann et al., 2008. If both the tree ring proxies and the Tiljander proxies were excluded, then estimated temperatures for the period 1000-1850 were substantially increased[B78–B80]. However, the estimates failed verification before 1500 (possibly because they had excluded so many proxies).

At a later stage, Mann posted on his website[B13], a similar test for the CPS estimate. Again, this had significant effects, e.g., temperatures in the Medieval Warm Period reached higher values than in the 20th century. This suggests that the Mann et al., 2008 estimates were *not* robust to the proxies used, as had been claimed. Indeed, it again highlights the danger in relying heavily on questionable proxies, such as the bristlecone/foxtail pines discussed in Section 3.5.1, the Yamal chronology discussed in Section 3.5.2, or even the Tiljander lake sediments which were known to be problematic after 1720.

4 Criticism of the *hockey stick study*

As discussed in Section 1, the *hockey stick study* by Mann et al.[A10, A11, A125] was very influential, both politically and socially, due to its prominence in both scientific[A40] and popular presentations[B2][6]. Perhaps for this reason, despite a number of flaws having been identified with it[A37–A39, A41–A46], its dramatic claims that (i) global temperature change since the late 19th century have been strongly dominated by man-made global warming[A10], and (ii) current temperatures are unprecedented in the last millennium[A11] appear to be widely believed by the general public.

Part of this seems to be due to Mann and his supporters continuing to imply that it was a reasonably accurate study, e.g., see Ref. [B82] for a 2011 TEDxTalk by Mann.

Another factor also appears to be that many supporters of man-made global warming theory are reluctant to acknowledge that there may have been flaws with the iconic *hockey stick graph*. This appears to be due to a fear that if the public becomes aware of those flaws, they may become suspicious of other aspects of climate science. For example the Anonymous Reviewer #1 for Ljungqvist et al., 2011[B83] believes that there is a *"stubbornness by the sceptical community to accept very real environmental and climatic changes that more and more appear to be exceptional over the last 1000 years"* and worries that criticising previous palaeoclimatology studies might *"muddy the message"*.

This should be irrelevant for the reader who is trying to genuinely understand how climate has changed over the last millennium or so. However, the *hockey stick study* still seems to hold a strong influence on public thought. Hence, in this section, we will review the contentious debate over this one particular study. The reader who is uninterested in this outdated study may prefer to skip to Section 5.

4.1 Initial criticism and defence

As discussed in Section 3.2, while the *hockey stick study* used 159 proxies in total, for the critical 1000-1400 step they only used 12 proxies. With this in mind, the first point to note is how few of these 12 proxies (Figure 7) bear any resemblance to the final *hockey stick graph* (Figure 1).

Jones, 1998[A79] criticised Mann et al., 1998 for using long thermometer records as some of their temperature "proxies" and for failing to adequately discuss the problems associated with the various proxies they used. He also noted that other temperature estimates for 1400-1900s suggested different temperature histories. However, he later clarified[A128] that he agreed that those estimates concurred with Mann et al., 1998 that the 20th century was warmer than the 15th-19th centuries.

Briffa & Osborn, 1999[A41] cautioned that the apparent agreement of the *hockey stick study* with others, was at least partially due to a substantial overlap in the proxies used by those studies (Section 3.2). They worried that Mann et al. had not paid enough attention to the problem of tree ring standardisation (Section 3.1). They also expressed concern over an adjustment Mann et al., 1999 had applied to one of their series which they relied heavily on - the first principal component (PC1) of a network of tree rings which was dominated by bristlecone/foxtail pines from western USA.

As discussed in Section 3.5.1, some researchers had argued that these trees were showing unusual 20th century growth due to CO_2 fertilisation[A109, A110]. To counter this concern, in their extension to Mann et al., 1998, Mann et al., 1999 had applied an *ad hoc* adjustment to this series (PC1 in Figure 7). However, there were a number of problems with this. First, the actual adjustment seems somewhat arbitrary, and

[6] The *hockey stick study* appears to have been mistakenly labelled as "Dr. Thompson's thermometer" in Ref. [B2] - see Ref. [B81].

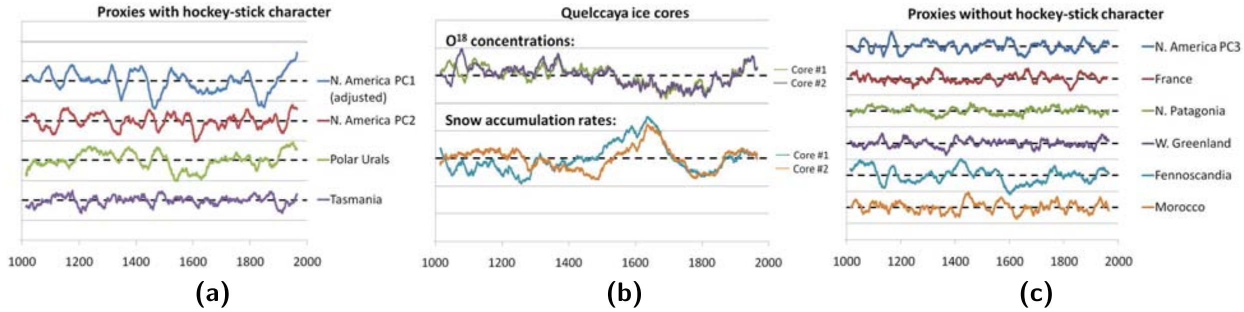


Figure 7: *All of the 12 proxy series used in for the 1000-1400 step of the* hockey stick study. *All data has been smoothed with a 31 year running mean, for clarity. (a) includes the only series which show any sort of "hockey stick"-like uptick for the Current Warm Period. (b) There were two nearby ice cores taken from Quelccaya, and so the two series from there were each averages of two cores. (c) shows the other series. Proxy data taken from World Data Center for Paleoclimatology, except for the Morocco data which was taken from the Climate Audit website. Units were not provided with the archived data, and vary from proxy to proxy, so they are all plotted here in generic "proxy units". Each proxy was rescaled by dividing by its standard deviation and subtracting its 1000-1980 mean.*

not as simple as Mann et al., 1999 implies[B28, B57, B84–B87]. Second, from Figure 7, it does not appear to have worked as, even after applying this adjustment, the 20th century up-tick of the "PC1" series is still far sharper than the other series. Finally, they only applied the adjustment to the 1000-1400 step, so even if the adjustment did correctly remove a non-climatic bias, the bias still remained in the 1400-1980 steps[B28].

Broecker, 2001[A39] expressed concern over the apparent absence of a strong Medieval Warm Period in the *hockey stick study*. He argued that there was considerable evidence to counter this finding. Although Bradley et al., 2001[A99] pointed out that some studies failed to find a Medieval Warm Period, Soon et al.[A37, A38] pointed out a large selection of studies which did (see Section 3.4).

More specifically, Soon et al. only found a few studies (including the *hockey stick study*) which showed the Current Warm Period to be climatically anomalous in the last millennium (either in terms of temperature or precipitation). Indeed, they claimed the opposite, i.e., that *"the 20th century is probably not the warmest nor a uniquely extreme climatic period of the last millennium."*[A37], although this particular claim was strongly criticised by von Storch for being inadequately justified[B88].

Mann et al., 2003a[A129] criticised Soon et al. However, they do not appear to have carefully considered Soon et al.'s arguments since most of their criticisms had already been addressed in the Soon et al. articles[A37, A38]. Nonetheless, a brief debate was attempted[A130, A131].

McIntyre & McKitrick, 2003[A42] re-analysed Mann et al., 1998 using the data and methods provided to them by Mann et al.[B89]. They found that the data set Mann et al., 1998 had used was poorly organised and also contained a large number of errors. After accounting for those errors, and replacing some proxies with more up-to-date versions or comparable substitutes, their reanalysis suggested the 15th century was warmer than the 20th century. This contradicted the *hockey stick study*'s conclusion that 20th century temperatures were unusually warm, suggesting that the *hockey stick study* was not robust.

Rutherford et al., 2005[A118] suggested that some of the errors McIntyre & McKitrick, 2003 had noticed were due to them using an incorrect dataset. When McIntyre had asked Mann for the Mann et al., 1998 data, Mann had put him in contact with Rutherford who apparently gave McIntyre a slightly incorrect version. Ironically, this apparently incorrect version appears to have been the one used by Rutherford et al., 2005 and also later archived in Mann et al., 1998's 2004 corrigendum[B90]. The fact that even the authors of Mann et al., 1998 (who also co-authored Rutherford et al., 2005) were unclear over which dataset to use seems to have vindicated McIntyre & McKitrick's criticisms of the disorganised nature of the Mann et al., 1998 study.

However, Rutherford et al. also argued that McIntyre & McKitrick had taken a traditional approach to calculating the principal components of Mann et al., 1998's high density tree ring networks (see Section 4.3), rather than the undisclosed approach which it transpired Mann et al., 1998 had actually used. This apparently led to too strong an increase in the 15th century temperatures. McIntyre & McKitrick, 2005b[A45] applied the now-disclosed approach and the 15th century temperatures were indeed a bit lower than for McIntyre & McKitrick, 2003. Nonetheless, they were still comparable to the 20th century temperatures, and so the contradiction with the *hockey stick study's* conclusions remained.

4.2 "Pseudoproxy" analysis

A problem with all proxy-based temperature estimates is that we do not know if the method of reconstruction actually works. The purpose of developing such estimates is to estimate what past temperatures were. But, since we do not know what the past temperatures actually were, we cannot check how accurate our estimates are.

One approach to overcoming this problem has been to use computer simulations of temperature changes of the last millennium (for instance). Of course, we do not know if the simulated temperature changes are at all representative of the real temperature changes which occurred over the last millennium. But, unlike the real world, in our simulated world, we can check *with 100% accuracy* the exact *simulated* temperatures at *any* time or place during the simulation. So, if we can construct realistic mimics of our real proxies from our simulation results ("pseudoproxies"), we can at least test how reliable our reconstruction *method* is.

We can do this by withholding the "true" *simulated* temperature changes and then directly comparing them to our pseudoproxy reconstructed estimate. "True" is in quotes because, although we know the exact values *of the simulation*, we do not know how closely the simulation reproduces the actual temperatures.

Nonetheless, if our reconstruction method is unable to accurately approximate the mean temperature trends of the simulation (which we know exactly), then we at least know that it will not do any better for describing the temperature trends of the real world.

In other words, pseudoproxy simulations can give us a simple "validation test" for our reconstruction method. If our reconstruction method passes the test, this does *not* prove that the reconstructions are accurate. However, if the reconstruction method *fails* the test, then we know for certain that any reconstructions generated by this method are unreliable.

Due to the high profile nature of the *hockey stick study*, a number of such studies have been carried out[A43, A46–A48, A132–A146] to investigate the reliability of its particular reconstruction method, henceforth referred to as the "MBH" method (after the initials of Mann, Bradley and Hughes, i.e., the authors of the *hockey stick study*).

A difficult challenge in this approach is in deciding how to construct realistic pseudoproxies. It is relatively easy to generate pseudoproxies for the same locations of an actual proxy network (such as that used by the *hockey stick study*) by selecting the gridded simulated temperatures for those locations. However, as we discussed in Sections 2.1 and 3.1, real proxy series contain a lot of "noise" from non-temperature factors as well as variability in the temperature response.

A simple first approximation in the construction of a pseudoproxy network is to introduce different amounts of random noise. In this way, pseudoproxies with different "signal-to-noise" ratios can be generated. However, real proxy "noise" is probably more complex than that. For this reason, more realistic studies involve the use of different types of noise.

Ordinary random noise is considered "white". However, often noise has non-random properties. "Red" noise is noise whose value for one point has some dependence on the previous point, i.e., it is possible to have randomly occurring trends. Many temperature proxies, such as tree rings are thought to have more similarity to red noise than the trendless white noise. In the case of tree rings this is because a previous years' growth can influence a given year's growth[A52, A53]. For example, a year of good growth could make the tree healthier, improving its growth for the next year.

As a first step, von Storch et al., 2004[A43] tested the *hockey stick study* reconstruction method on a pseudoproxy network constructed by applying varying amounts of white noise to the "Erik" simulation of the last millennium. They found that, even just using white noise, the MBH method substantially underestimated the actual temperature variability of the simulation. This suggested that much of the "flatness" of the "hockey stick handle" was merely an artifact of their reconstruction method.

However, their study was quite controversial and led to considerable debate[A46–A48, A133–A135, A146, A147]. Much of this debate was over the fact that they had used so-called “detrended” pseudoproxies[A132]. Before carrying out their analysis, they had temporarily removed the long-term trends of all their pseudoproxies and calibration data, so that they would achieve a better inter-annual calibration, and thereby a more realistic estimate overall. However, Wahl et al., 2006[A134] argued that this detrending should not be carried out. If non-detrended pseudoproxies were used, the underestimation of the MBH method was reduced[A134], although it was still substantial[A132, A135].

Another criticism was that there were problems with the “Erik” simulation that they had used[A147]. In particular, the simulation had been insufficiently equilibrated, and so it had suggested a warmer Medieval Warm Period than other simulations. However, for the purposes of pseudoproxy tests, this was irrelevant, as they were merely assessing how successful the MBH method was at reconstructing the simulated temperatures, not how accurate the simulated temperatures were[A136]. Indeed, similar results were found for the MBH method when an improved simulation (“Erik II”) were used[A146].

Rutherford et al., 2005[A118] applied a new method, called “RegEM”, to the same proxy network as the *hockey stick study* and achieved a similar result. When Mann et al., 2005[A133] carried out their own pseudoproxy analysis on this new method, the RegEM method appeared to be very successful at reconstructing simulated temperatures.

Initially, this seemed to suggest that the conclusions of von Storch et al. were invalid, leading to some debate[A133, A136, A137]. However, it later transpired that Mann et al., 2005 had made a serious error in their analysis. Before applying the RegEM method, they had standardised all their pseudoproxies over the entire simulation period, rather than just over the calibration period[A138, A139, A148, A149]. This meant that all of their pseudoproxies already roughly approximated the simulated temperature over the entire simulation. In the real world, the pre-instrumental temperatures are unknown - after all, that is why proxy-based studies are being carried out. After correcting for this, the RegEM method also significantly underestimated the actual simulated temperatures[A138].

Mann et al., 2007c[A139] tested a new version of RegEM, called “RegEM TTLS” (the older version is now known as “RegEM Ridge”). This method did not show as much underestimation as the older version (or the original MBH method), and when this method was applied to the *hockey stick study's* proxy network, it again yielded a similar reconstruction to the original *hockey stick study.*

This initially appears puzzling[A142, A150]. Although Smerdon et al., 2008b[A140] noted that Mann et al. had been using a badly corrupted version of a computer simulation for their 2005 and 2007 analyses, this did not affect Mann et al., 2007c's essential conclusion[A140, A141, A150]. Even though the RegEM methods still showed underestimation[A149, A151, A152], they did appear to give more realistic results than the original MBH method[A149]. However, when applied to the *hockey stick study's* proxy network, they all yielded essentially the same result[A139, A140, A150].

A likely explanation is that while there were problems with the MBH method, coincidentally, there were also serious problems with the proxy network itself. As we will see in Sections 4.3 and 4.4, this is the case. Hence, while the problems with the original MBH method are serious, a more reliable reconstruction method, while important, would not have been sufficient.

Nonetheless, von Storch et al., 2004[A43]'s study has been very useful in that it has led to a recognition of the value of pseudoproxy studies and the development of more robust reconstruction methods. This should be of benefit if applied to more reliable proxy networks.

4.3 Principal Component Analysis (“PCA”) problems

One problem with the *hockey stick study's* proxy network was that most of the proxies were from similar areas. In particular, 70 of the 95 series used for the 1400-1450 step[7] were U.S. tree ring series. If the *hockey stick study* had given all the series a similar weighting then their entire “Northern Hemisphere” estimate would be strongly dominated by those proxies, and would be little more than a “U.S.” temperature estimate.

In an attempt to overcome this weighting problem, Mann et al., 1998 tried to estimate the main “climatic signals” of these high density networks through principal component analysis[8] (“PCA”). They then

[7] 70 out of the 110 series they considered[A91].

[8] Not to be confused with their principal component analysis of the calibration data they used for their reconstruction, i.e.,

treated the top few principal components for those networks as replacement "proxies". For the 1400-1450 step, 3 out of the 22 series used were principal components, while for Mann et al., 1999's 1000-1400 step, they comprised 3 out of 12 series ("PC1-3" in Figure 7).

McIntyre & McKitrick, 2005a noticed that the Mann et al., 1998 algorithm for calculating principal components for those high density networks was non-standard, in that Mann et al., 1998 normalised all of the individual proxies to their 1902-1980 means instead of their means over the entire period being considered, e.g., 1400-1980 in the 1400 step[A44, A45, A64, A153–B155, B91].

This was significant because it gave very high weights to proxies whose 1902-1980 mean was substantially different from the mean over the entire period. This meant that the tree ring series which did not show unusual 20th century growth (i.e., proxies without "hockey stick" shapes) received negligible weighting, while the series with the greatest "hockey stick" shape received the greatest weighting.

For the North American network, Sheep Mountain (the proxy with the strongest "hockey stick" - and also one of the bristlecone pine proxies mentioned in Section 3.5.1) received 390 times the weight of Mayberry Slough (the proxy with the weakest "hockey stick")[A44, A45] in the 1400-1450 step - see Figure 8.

McIntyre & McKitrick demonstrated the problem this introduced, by generating a large number of random, red noise simulations with no overall trend. When they applied the standard principal component analysis to these simulations, the 1st principal components (PC1) showed no trend. But, when they applied the Mann et al., 1998 version, the 1st principal components tended to have "hockey stick" shapes, even though they had no intrinsic trend.

As von Storch & Zorita, 2005 noted[A153], the magnitude of McIntyre & McKitrick's red noise "hockey sticks" was small compared to the Mann et al., 1998 global temperature estimate. But, McIntyre & McKitrick were not suggesting that this artefact in itself led to the hockey stick shape of the *hockey stick study*[A154] (although Mann mistakenly seems to have thought they were[B92]). Rather, the significance was that the Mann et al., 1998 version effectively "mined" the high density networks for "hockey sticks". As a result, the 1st principal component for the North American network was excessively dominated by the problematic Graybill bristlecone/foxtail strip-bark pines discussed in Section 3.5.1.

the "MBH method" discussed in Section 4.2.

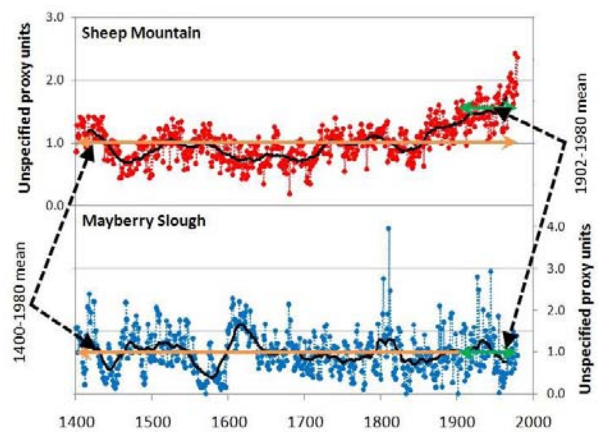


Figure 8: *The highest weighted (top panel) and lowest weighted (bottom panel) series in Mann et al., 1998's 1400-1450 1st principal component of the North American ITRDB tree ring proxies. Black curves correspond to 31-year running means. Proxy data taken from the Supplementary Information to the 2004 corrigendum to Mann et al., 1998[A91]. Proxy names taken from ClimateAudit.*

Huybers, 2005[A155] agreed that the Mann et al., 1998 version was flawed, and underestimated pre-20th century temperatures. However, he argued that McIntyre & McKitrick should have scaled their proxies to unit variance before their analysis, since some of the proxies showed less variability than others. When Huybers did this, he obtained an intermediate result between Mann et al., 1998 and McIntyre & McKitrick, 2005a.

McIntyre & McKitrick responded that this was only really of relevance to accommodate two of the 70 tree rings in the 1400-1450 North American network[A64]. They argued that it also underestimated the variance of proxies which showed strong trends, i.e., the strip-bark pines. However, they noted that when the three different 1st principal components were plotted to the 1400-1980 mean, instead of the 1902-1980 mean as Huybers had done, both Huybers' and McIntyre & McKitrick's versions were actually quite similar, while the Mann et al., 1998 version was a clear outlier.

The effect of the *hockey stick study's* non-standard principal component analysis was most pronounced in the earliest step (1400-1450). In Mann et al., 1999's 1000-1400 step, the North American 1st principal component ("PC1" in Figure 7) was also dominated by the Graybill strip-bark pines, but this was mainly due to the fact that these were the trees with the longest chronologies in that network.

McIntyre & McKitrick noted that, using the standard approach, the strong "hockey stick" shape of the bristlecones were instead relegated to the 4th principal component (PC4). If they then carried out the rest of the Mann et al., 1998 algorithm (i.e., including the top two principal components), this made the 15th century appear comparable to the 20th century, i.e., the "hockey stick" disappeared[A45].

Mann and his colleagues attempted to counter this criticism in a few ways, although their arguments seem to have been based on a misunderstanding of the criticism and/or the reasons for using principal component analysis. Mann claimed that the Mann et al., 1998 approach was in fact a well-established form of principal component analysis, which had been recommended by Jolliffe for certain applications[B92]. Jolliffe denied this and strongly criticised its use in Mann et al., 1998 when he became aware of this[B93].

Mann also claimed on his Real Climate blog that, if they had used the standard approach McIntyre & McKitrick favoured, then the top *five* principal components should be used, rather than the top two used with the *hockey stick study* approach, stating that Mann et al., 1998 had used "Preisendorfer's Rule N"[B30, B94, B95]. Hence, he argued they could still include the hockey stick shape of the Graybill pines. This argument was later repeated by Ammann & Wahl[A156, A157]. However, McIntyre noted that:

- There was no evidence that Mann et al., 1998 had actually used Preisendorfer's Rule N[B96].
- There were many other selection rules which could have been used[B97]
- It was unclear if Preisendorfer's Rule N was appropriate[B98]

Wahl & Ammann argued that it was important to include the bristlecone/foxtails, otherwise the *hockey stick study* failed its verification tests[A157]. But, this had been McIntyre & McKitrick's essential criticism - if the *hockey stick study* was supposed to be genuinely representative of northern hemispheric temperatures, then it should not have to rely on a small subset of trees in western U.S.[A44, A45]. This point had also been made earlier by Soon et al., 2003b[A38].

Mann et al. argued that Mann et al., 1998's hockey stick shape could also be obtained without using any principal component analysis[A139, A156, A157][B30, B94, B95]. However, that was merely because the entire proxy network was then dominated by the U.S. tree ring network - the problem the principal component analysis was supposed to reduce. In that case, the *hockey stick study* was again biased by the problematic Graybill pines, due to them comprising 20 of the 95 series[B99]. This was easily confirmed by removing the Graybill pines from the network, since the 15th century temperatures then appeared comparable to those of the 20th century[A45].

Finally, Rutherford et al., 2005[A118] had repeated the Mann et al., 1998 estimate using a slightly different approach (the "RegEM" method described in Section 4.2), and obtained a similar result. Mann et al. claimed that this vindicated the approach of the original *hockey stick study*[B30, B94, B95]. However, Rutherford et al., 2005 had used the same proxy network and principal component analysis as Mann et al., 1998[9], so the criticisms still held[B100].

4.4 Lack of statistical robustness

It is often assumed that the temperature proxies used for proxy-based temperature estimates are at least moderately correlated to actual local temperature measurements[A16]. Indeed, most readers would probably consider this an essential requirement. However, McIntyre & McKitrick noted that many of the proxies used by Mann et al., 1998 were very poorly correlated to local temperatures[A154]. Most of the U.S. tree ring proxies they used appeared to be better correlated to other factors, such as precipitation or CO_2 concentrations[A154].

Mann et al., 1998 were not overly concerned with how well individual proxies were correlated to local temperatures, and in fact several of the Mann et al., 1998 proxy series were actually precipitation weather records[A10][10]. Instead, they believed that their climate field reconstruction method ("MBH" in Section 4.2) would be able to detect *global* changes in climate patterns from their proxies. They pointed out that changes in local climate could sometimes also reflect more widespread climate change, via climate teleconnections, e.g., El Niño-Southern Oscillation (ENSO) variations[A10, A156, A157]. However, they did not

[9] They also considered the case without any principal component analysis as described above.

[10] Some of the precipitation records were seriously mislocated, e.g., Mann et al., 1998's "Boston" (U.S.A.) precipitation series actually appeared to be the series for Paris (France)[A42].

offer a mechanism by which a proxy would be affected by global climate signals, but not by local climate signals, and this assumption seems to be at best unrealistic[B101, B102].

With this in mind, McIntyre & McKitrick decided to investigate Mann et al., 1998's claim that their hemispheric reconstruction had a "high level of skill' back to their earliest step (1400-1450). First, they considered standard statistical variables, such as R^2, the correlation coefficient of determination (also known as r^2), which varies from 0 (non-correlated) to 1(perfectly correlated). They found that the reconstructed temperatures showed a negligible correlation ($R^2 = 0.02$) to instrumental temperatures in the verification period[A44, A45] for that step.

It is true that a high R^2 value would not in itself indicate robustness. For instance, the verification data Mann et al., 1998 used (thermometer-based data for 1854-1901) consisted of an almost continuous global warming trend from start to finish. As a result the data was highly "autocorrelated", i.e., values for an early section of the data are highly correlated to later sections of the data, because the trend was similar (i.e., warming) over the entire period. In such cases, high R^2 values often occur spuriously[A158]. Hence, it is important to also consider other statistics. However, a negligible (or even low) R^2 value should have been a serious concern[B103].

Wahl & Ammann[A156, A157] claimed that the *hockey stick study* was more concerned with the long-term trends of the estimates being similar to the verification data, than in the annual temperatures being accurate. For this reason, the *hockey stick study* favoured a different statistic[A118, A139] - the *"reduction of error"* (RE, called "β" in Mann et al., 1998).

McIntyre & McKitrick were also concerned with the RE results of the *hockey stick study*. Mann et al., 1998[A10] had arbitrarily decided that a non-zero value of RE indicated statistical significance. Hence, they believed that the $RE = 0.51$ value of the 1400-1450 step was statistically significant. However, McIntyre & McKitrick, 2005a[A44] found that red noise series (the ones they used in their PC1 simulations - see Section 4.3) which had no intrinsic climatic signal actually yielded higher RE values. By assuming that the RE of a genuinely climatic series would have to be higher than 99% of the red noise series, they obtained a benchmark value of statistical significance of $RE = 0.59$. On that basis, the *hockey stick study's* 1400-1450 step was *not* statistically significant. It also failed other cross-validation statistical tests.

Huybers, 2005 criticised this benchmarking process, by pointing out that McIntyre & McKitrick had not scaled their red noise simulations to have the same variance as the calibration/verification data. When Huybers did this, he calculated a benchmark of 0.0, i.e., the same as Mann et al., 1998 had assumed[A155].

McIntyre & McKitrick accepted this criticism, but noted that they also should have carried out a more complete emulation of the Mann et al., 1998 reconstruction in their benchmarking. They had only simulated one of the proxy series Mann et al., 1998 had used, i.e., the "PC1" series, while the actual 1400-1450 step used 22 series. Hence, they increased the variance of their red noise series by combining them with another 21 white noise pseudoproxies[B36]. This yielded a benchmark of $RE = 0.54$[A64], lower than their original analysis, but still higher than the 1400-1450 step.

Wahl & Ammann, 2007[A156, A157] initially claimed that they had also obtained a benchmark of 0.0[A156, A157]. However, when they published their Supplementary Information, it transpired that they had actually calculated a benchmark of $RE = 0.52$[B104, B105] - only slightly lower than McIntyre & McKitrick's $RE = 0.54$. Moreover, there were also statistical problems with Wahl Ammann's lower value[B105, B106]. In any case, the more serious issue was still that it had a negligible R^2 statistic, while a robust estimate should have passed both tests[B105, B107].

4.5 Summary of the criticisms of the *hockey stick study*

To summarise, although the *hockey stick study* and its conclusions had a powerful impact on the scientific community[A40] and general public[B2], a number of serious flaws have since been found with it. In particular:

- Its characteristic "hockey stick" description of millennial temperature changes was highly dependent on the inclusion of a small set of problematic bristlecone/foxtail pine proxies (Section 3.5.1).
- By using a flawed approach to principal component analysis, the influence of these problematic proxies was dramatically increased (Section 4.3).

- Attempts to adjust these proxies to account for their non-climatic trends were themselves problematic (Section 4.1).
- The dataset used by Mann et al., 1998 was error-ridden and badly organised, reducing the study's reliability (Section 4.1).
- Its reconstruction method substantially underestimated actual temperature variability, making the "handle" of the "hockey stick" seem unrealistically flat (Section 4.2).
- It failed to pass basic tests of statistical significance (Section 4.4).

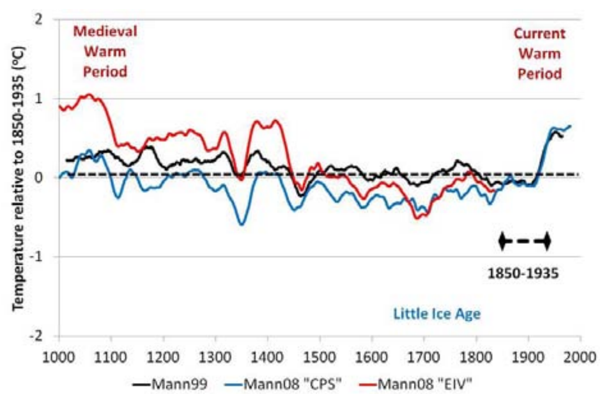


Figure 9: *Millennial temperature estimates of the* hockey stick study*[A11] compared to Mann et al.'s more recent 2008 "CPS" and "EIV" estimates[A22]. The Mann et al., 1999 and CPS estimates were rescaled and smoothed as described in Section 2.3, but the EIV estimate was only archived up to 1850, so was rescaled by assuming the estimate would have the same 1850-1935 mean and standard deviation as its calibration/verification data.*

Following the controversy over the *hockey stick study*, two independent reviews were carried out in the U.S. - one for the National Academy of Sciences ("the NAS Report")[A123] and one headed by a team of statisticians ("the Wegman Report")[A124]. The NAS Report partially agreed with some of the conclusions of the *hockey stick study*[A123][B108], i.e., that the Current Warm Period was warmer than the Little Ice Age. It also noted several studies which agreed with the *hockey stick study's* conclusion that the Current Warm Period is warmer than the Medieval Warm Period. However, both of the reports agreed with much of the criticism of the *hockey stick study*[A123, A124][B109].

The authors of the *hockey stick study* have been quite vocal in their insistence that the criticisms of Mann et al., 1998 and Mann et al., 1999 have all been countered or shown to be irrelevant[A15, A16, A99, A118, A129, A131, A133, A137, A139, A141, A144, A148][B30, B94, B95, B110]. However, their most recent millennial reconstructions[A22] actually show considerably more variability and uncertainty over the millennium than their *hockey stick study* - see Figure 9. This suggests that even Mann et al. probably now agree that the original *hockey stick study* was unreliable. Hence, in the next section, we will discuss the other millennial reconstructions.

5 How did the Medieval Warm Period compare to the Current Warm Period?

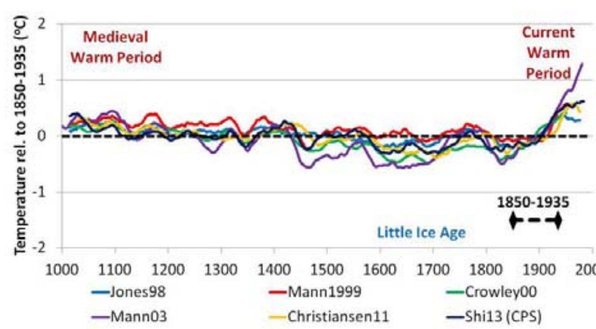


Figure 10: *The "hockey stick-like" proxy-based temperature estimates for the period 1000-2000, which suggest the Current Warm Period is unusually warm: Jones et al., 1998[A9]; "MBH99", i.e., the original* hockey stick study*[A11]; Crowley, 2000[A12, A58]; Mann & Jones, 2003[A15, A16]; Christiansen & Ljungqvist, 2011[A26]; and Shi et al., 2013 ("CPS")[A28]. All plots are rescaled and smoothed as described in Section 2.3.*

In Figures 10, 11 and 12, all of the millennial proxy-based temperature estimates discussed in this article are plotted - rescaled and smoothed following the description in Section 2.3. One noteworthy difference between the plots in Figures 10-12 and other presentations of the data, e.g., that in the 2007 IPCC report[A49], is that thermometer-based estimates are not superimposed over the plots. This is for the reasons discussed in Section 2.

There appear to be three main groups of estimates, so they have been separately plotted for clarity (Fig-

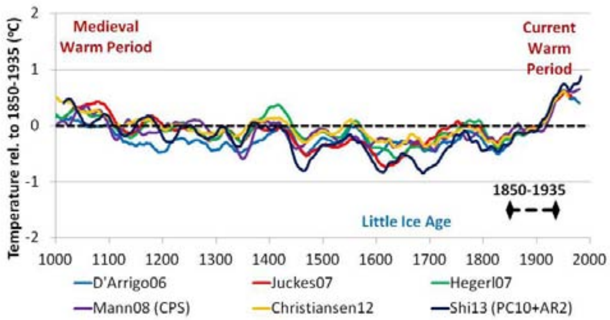


Figure 11: *The proxy-based temperature estimates for the period 1000-2000, which suggest the Current Warm Period is warmer than the Medieval Warm Period: D'Arrigo et al., 2006[A18]; Juckes et al., 2007[A20]; Hegerl et al., 2007[A19]; Mann et al., 2008 ("CPS" northern hemisphere estimate)[A22]; Christiansen & Ljungqvist, 2012[A27]; and Shi et al., 2013 ("PC10+AR2")[A28]. All plots are rescaled and smoothed as described in Section 2.3.*

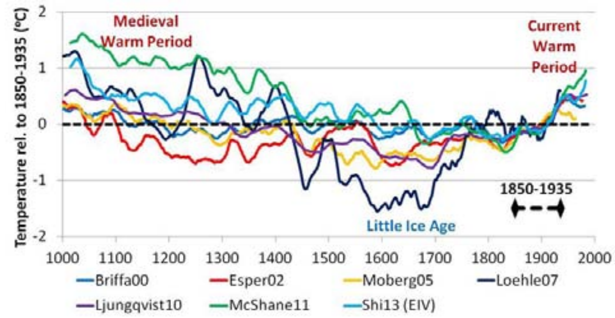


Figure 12: *The proxy-based temperature estimates for the period 1000-2000, which suggest the Medieval Warm Period was similar to or warmer than the Current Warm Period: Briffa, 2000[A13]; Esper et al., 2002[A14]; Moberg et al., 2005[A17]; Loehle, 2007[A21, A62]; Ljungqvist, 2010a[A24]; McShane & Wyner, 2011[A25]; and Shi et al., 2013 ("EIV")[A28]. All plots are rescaled and smoothed as described in Section 2.3.*

ures 10-12).

The first group comprise those suggesting a "hockey stick"-type description of the last millennium[A9, A11, A12, A15, A16, A26, A28, A58] (Figure 10). These estimates suggest that the recent global warming of the Current Warm Period is highly unusual in the context of the last millennium.

None of the other estimates are quite as dramatic, however. They all suggest that there was a substantial Medieval Warm Period about a thousand years ago. Several of them suggest that the Current Warm Period is warmer[A18–A20, A22, A27, A28] (Figure 11).

You might argue that Figure 11 supports the argument that at least some of the recent warming is "unusual", however we do not see how you can claim that one of two warm periods is "unusual" and the other is "usual", merely because one is warmer than the other. Indeed, some of the estimates which cover more than the thousand years shown in Figures 10, 11 and 12, also suggest earlier warm periods, such as the so-called "Roman Warm Period" around two thousand years ago[A24].

The Mann et al., 2008[A22] estimates came under particular criticism, as they had specifically claimed their estimates were robust to the exclusion of problematic proxy series or the use of different reconstruction methods, but this claim later transpired to be wholly inaccurate - see Section 3.5.3. In addition, their estimates appeared to be strongly affected by the over-fitting problem[A54][B10, B11] - see Figure S10 of Ref. [B13].

In 2011, two statisticians with no prior experience in palaeoclimate, McShane & Wyner, constructed their own estimates using Mann et al., 2008's dataset[A25]. Their analysis suggested that the wide variability of the proxy data (Section 3.4) meant that the necessary error bars were too great to definitively resolve the question of whether the Current Warm Period was warmer, colder or similar to the Medieval Warm Period. However, both periods did appear to be warmer than the Little Ice Age, and the mean values of their estimate *suggested* that the Medieval Warm Period was the warmer of the two.

The McShane & Wyner, 2011 study was published in a statistical journal as a discussion essay, and responses were sought from both statisticians and climate scientists (see links in Ref. [A25]). It also generated considerable discussion on various blogs (e.g., see Refs. [B111–B115] for some of the more considered discussion). There was a general impression that their analysis was weakened by a number of mistakes, misunderstandings and errors which could have been averted if they had collaborated with palaeoclimatologists. But, it was still considered useful, and there was considerable agreement with McShane & Wyner's recommendation that palaeoclimatologists should seek more advice from statisticians for future studies.

We included McShane & Wyner, 2011's estimate in the third group of estimates[A13, A14, A17, A21, A24, A25, A28, A61, A62] which suggest that the Medieval Warm Period was comparable to, if not warmer than, the Current Warm Period (Figure 12).

Surprisingly, these estimates are often taken to imply the opposite conclusion[A49, A55]. This seems to happen when researchers incorrectly compare the proxy-based Medieval Warm Period estimates to the thermometer-based estimates for the Current Warm Period, rather than to the proxy-based Current Warm Period. As discussed in Section 2, this is inappropriate, and Ljungqvist[A24] urged caution over such comparisons.

Mann & Hughes were critical of the Esper et al., 2002 estimate, as it disagreed with their *hockey stick study* and the other "hockey stick-like" estimates of Figure 10, leading to some debate[A159]. Esper et al. have been concerned about the robustness of the early part of their estimate, as it was based on a smaller sample of trees, so they have since revisited the study twice[A59, A60]. Each time, their reanalysis has slightly lowered their estimates of the warmth during the Medieval Warm Period. Hence, Frank et al., 2007[A60] now suggests that the Current Warm Period is a bit warmer than the Medieval Warm Period.

From pseudoproxy analysis, Mann et al., 2005[A133] suggested that Moberg et al., 2005[A17] was less reliable than the *hockey stick study*[A11]. However, other pseudoproxy analyses have suggested the opposite[A105, A132].

It is worth noting that each of the three Shi et al., 2013 estimates fits into a different one of our three groups, i.e., the "CPS" estimate is in Figure 10; the "PC10+AR2" estimate is in Figure 11; and the "EIV" estimate is in Figure 12. All three of these estimates used the same proxy dataset, but different reconstruction methods. This suggests that at least some of the differences between the various estimates are purely statistical in nature.

While there is a remarkable consistency between most of the estimates on the timing (and to a lesser extent, the magnitude) of the two warm periods, there is less agreement on the intervening periods. For instance, some estimates, e.g., Briffa, 2000[A13]; Mann et al., 2008[A22]; Moberg et al., 2005[A17] suggest there was a (possibly brief) warm period around 1400 A.D., but this is not as pronounced in other estimates. On the other hand, some estimates place the Little Ice Age at its coldest around 1600 A.D., while the "hockey stick" estimates (Figure 10) suggest a more recent trough. This has significance for those arguing the apparent recovery from the Little Ice Age was due to increases in atmospheric CO_2 since the Industrial Revolution. If the "recovery"[A36] started in the 1600s, then that would have pre-dated the Industrial Revolution by a few centuries.

6 Conclusions

In recent decades, there has been considerable interest[A5–A31] in statistically combining different temperature proxies (e.g., tree rings, ice cores, lake sediments) together to construct large-scale estimates of global (or at least hemispheric) temperature changes over the last millennium or so.

All 19 of the millennial proxy-based temperature estimates discussed in this review (Table 1) have identified at least three climatically distinct periods: two relatively warm periods - the "Current Warm Period" (c. 1900 AD on) and the "Medieval Warm Period" (c. 800-1200 AD), and a relatively cool period - the "Little Ice Age" (c.1500-1850 AD). Disagreement between estimates appears to be mainly limited to establishing exactly how much temperatures have differed between each of the periods (Section 5).

This might offer cause for optimism that we are close to reaching a reasonable understanding of temperature changes of the last millennium. However, unfortunately, much of the apparent agreement between estimates may be due to the substantial overlap in the proxy series used by the estimates (Section 3.5).

More worryingly, there seem to be a number of paradigms already accepted by many in the palaeoclimate community. Bradley & Jones, 1992[A92] and Hughes & Diaz, 1994[A94] warned of two such paradigms and their danger - the common belief that palaeoclimatologists should *expect* to find a "Little Ice Age"[A92] and "Medieval Warm Period"[A94] in their data. A third paradigm seems to have arisen in recent decades - that researchers should *expect* to find unusual recent warming due to man-made global warming.

This is not to imply that any of these paradigms are necessarily wrong - they may well be valid. However, if a researcher is expecting to find a particular result, it is quite possible that they will (in good faith) eventually "find" it, regardless of whether it actually occurred or not. This is why Konrad Lorenz (1903-1989) humorously suggested that: *"It is a good morning exercise for a research scientist to discard* *a pet hypothesis every day before breakfast. It keeps him young."*

Simmons et al., 2011[A93] have illustrated, by presenting the results of an intentionally nonsensical study, how confirmation bias can easily lead unwary researchers to reach false conclusions. As funding is rarely prioritised for attempting to reproduce earlier studies, these conclusions may then become embedded in the scientific literature.

We see enough contradictions in the current palaeoclimate data to suggest that the current paradigms should be treated cautiously, at the very least.

On the Little Ice Age paradigm: Since Bradley & Jones, 1993[A7], there seems to have been a general consensus that there was a period of several centuries before the Current Warm Period that was particularly cold. It has even been suggested that current estimates are underestimating this coldness[A24].

However, the existence of the "convergence problem" (Section 2.5) suggests that if there was a Little Ice Age, it might not have been that long or cold after all. In other words, the uncertainties over exactly how long and cold it was[A35] remain.

On the Medieval Warm Period paradigm: There is considerable inconsistency in the estimates of the "Medieval Warm Period" (in terms of time and extent) between different proxy series, even for the same area (Section 3.4). Unless the reasons for these differences can be satisfactorily resolved, and it can be established which series (if any) are most reliable, considerable uncertainties will remain.

On the unusual recent global warming paradigm: Much of the "unusual" 20th century temperatures implied by several proxy-based estimates seems to depend on the inclusion of particularly controversial proxy series, i.e., the Yamal chronology or bristlecone/foxtail series (Section 3.5). If such trends are genuinely climatic then they should not be dependent on the inclusion of particular series.

In addition, most proxy-based estimates do not show the strong global warming of recent decades suggested by the thermometer-based estimates (Section 2.4). This suggests that either there are problems with the thermometer-based estimates (something we discuss elsewhere[B6–B9]), or the proxy-based estimates are unable to detect recent warming, in which case it is plausible that they might have also missed earlier warm periods.

However, there are also other significant contradictions between estimates, which need to be investigated. In Section 5, we saw that some estimates suggest temperatures in the 15th century may have been relatively warm, or at least mild. But this is not shown in other estimates. This suggests an ambiguity. Indeed, McIntyre & McKitrick noted[A45] that the *hockey stick study's* conclusion that 15th century temperatures were colder than the 20th century could be reversed with relatively minor and reasonable alterations to the study (Sections 4.1 and 4.3).

We should recognise that estimating climatic conditions of the last millennium or so, is a very challenging research problem. Many of the assumptions which have been commonly made for such studies have been inadequately justified and there are significant discrepancies between thermometer-based temperature estimates and the proxy-based estimates (Section 2.1). Serious problems and inconsistencies exist with many of the individual proxy series used (Section 3). Finally, pseudoproxy analyses reveal that none of the current reconstruction methods are perfect, although they do at least offer us a useful way to assess reconstruction methods (Section 4.2).

These uncertainties might initially seem intimidating, and lead researchers to take premature short-cuts and assumptions, in the hopes of getting a quick answer. But, the scientific community should embrace these uncertainties, rather than trying to brush them aside: *"If a man will begin with certainties, he shall end in doubts: but if he will be content to begin with doubts, he shall end in certainties"* - Francis Bacon, Sr. (1561-1626)

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Those references which have gone through a "peer review process", e.g., journal articles, are prefixed by "A", while those which have not, e.g., blog posts, are prefixed by "B".

Peer reviewed

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