2

Temperatures during the past few millennia

A recurring theme in most of Isaac Asimov's science-fiction novels is the search for historical roots in the fog that obscures the past. These books have been very successful and are very readable, partly because they are well conceived and well written, but also because they evoke an empathetic response from our natural desire to understand our origins and roots. Similarly, the history of climatic variations leaves behind a fog that is difficult to penetrate. Many incredibly ingenious proxy methods have been devised to peer into the past. However, none is entirely satisfactory, and many uncertainties remain. In addition to proxy data, there are many anecdotal accounts in historical records that indirectly infer information about past climate (e.g., extent of glacier expansion and contraction in the Swiss Alps, paintings showing skaters on lakes that presently don't freeze, etc.). All of these have been utilized in the imperfect attempt to estimate past climates.

Today, we are concerned about potential climate changes that might occur in the future. To seek background data, we search for variability in past climates. However, random changes in weather from year to year and decade to decade are so large as to mask the underlying trends in climate change (if there is such a thing as climate). Climatologists have employed sophisticated statistical techniques to identify underlying trends, combining large numbers of proxies. The confounding thing is that the more proxies that are used, the more one averages out local and temporal variability by mixing the noise of many proxies with the signal of a few good ones. The inevitable result is a relatively flat temperature in the past, and, when this is combined with an exaggerated profile of recently measured rising temperatures, one obtains the "*hockey stick*" profile.

2.1 USE OF PROXIES TO ESTIMATE HISTORICAL TEMPERATURES

In the context of historical temperatures, proxies are residual data from processes that occurred in the past, when they were dependent on local temperatures at the times they took place, and the evidence is preserved in the present in an accessible form. In all cases, extraction of implied past temperature data from confounding influences requires considerable analysis and manipulation. As a result, the

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credibility and reliability of such proxy data vary widely from data set to data set, as well as in the eye of the beholder. For example, there are trees that are several thousand years old. The growth (width and density) of tree rings depends on the temperature prevailing during the growth period. By examining old tree rings corresponding to historical times, one can infer past temperatures. However, tree growth is also affected by other factors (water availability, humidity, wind, cloudiness, CO_2 content in the atmosphere, nutrients, etc.). These add noise to the temperature signal. Hence, it is not a simple matter to extract accurate historical temperature data from tree rings (or other proxies, for that matter). Some prominent proxies include: tree rings (width, density, stable isotope composition), ice cores (oxygen isotope ratios, gas content in bubbles), ocean sediments (isotope ratios), pollen, boreholes and corals.

2.2 PROXIES AND CLIMATE

2.2.1 Processing proxy data

The common approach to historical climate reconstruction from proxies is to establish a relationship between actual temperature measurements and the variability of the proxy over this recent period of overlap (calibration period). This provides a transfer function that enables the proxies to be used to infer the past climate in historical times when proxy data are available but direct temperature measurements are not. Ideally this works out as shown in Figure 2.1. Actually, one never obtains nice smooth curves as shown. Real data have many wild oscillations about the trend line. The fit of the proxy data to the measured data almost always leaves a great deal to be desired.

During the calibration period, measurements of the temperature are available for the locality where the proxy is located. A mathematical connection is made between

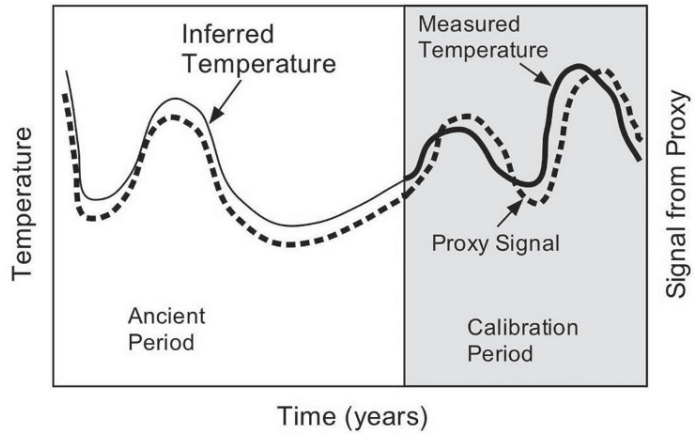

Figure 2.1. Concept of calibration period for a proxy.

the proxy signal and the temperature during this period. The proxy signal extends back in time prior to the calibration period. It is assumed (without proof) that the same relationship between proxy and local temperature holds in the past, and from this, past temperatures are estimated prior to the calibration period.

However, in many cases, the "fit" between the proxy and the measured temperature during the calibration period is not nearly as good as that depicted in Figure 2.1. There is usually a wide scatter in the data points and the putative relationship between proxy and temperature is not always clear. This is because many factors other than temperature often affect the proxy signal. Furthermore, in almost all published papers, the details of the proxy–temperature relationship during the calibration period are either not presented, or are so complex as to defy simple evaluation.

Over the past several decades, quite a number of scientists have analyzed data from one or several proxies and derived estimates of climate history in various localities and regions. Since much of the available data are from the northern hemisphere (NH), a good deal of this analysis pertains mainly to the NH.

2.2.2 Challenges in using proxies

Ogilvie and Jonsson (2001) noted that essentially all current calibrations of proxies to large-scale instrumental measurements have been made over periods of rising temperature. They raised the concern that a different calibration response might arise when the procedure is extended to an untested climate regime associated with a persistent cooling phase. They also raised other issues as well.

Jones *et al*. (1998) presented an extensive review of proxies. The spatial and timescale constraints on proxies were described in some detail. It was concluded that each reconstruction

> "... is probably limited in its ability to reproduce past temperature variations faithfully on the longest of timescales. This limitation varies from proxy to proxy and it is virtually impossible to quantify the degree to which this has occurred because instrumental series are not long enough."

It was found that, for most proxies, the correlation with instrumental data during the calibration period was not as good as reported in the journal articles.

2.2.3 Combining multiple proxies

Most proxies provide estimates of ancient temperatures at specific localities or regions. Climatologists are interested in the overall global climate, which necessitates developing a synoptic view of temperatures around the globe. The challenge is to find ways to combine temperature estimates from multiple proxies at various locations, over variable time periods.

Starting as early as the late 1970s and following through the 1980s and 1990s, culminating in a pair of very influential papers in 1998 and 1999 (Mann *et al*., 1998, 1999), and continuing to this day, a loosely allied cadre of scientists has attempted to

statistically combine large numbers of proxies (indeed, all the proxies that were available) into a reconstruction of global (or at least NH) average temperatures for the past millennium or two. They have assembled as many as 1,000 proxies into a database. These proxies include a variety of geographical locations, ranges of time, and degrees of credibility. A major question is how should these proxies be combined? Measurements at different locations, particularly different latitudes, will have different absolute temperatures and different temperature trends with time. Another major question is how to assign weights to various proxies based on their degree of credibility. Equally challenging is the question of how (or whether) to include documentary information that is typically discontinuous and often anecdotal in nature. These scientists typically employed sophisticated statistical approaches for combining proxy data sets into reconstructions of past NH or global average temperatures. The underlying basis for these approaches is the assumption that each proxy supplies an estimated temperature as a function of time (T_E) that contains a temperature signal (T_S) plus noise (T_N):

$$T_E = T_S + T_N.$$

It is further assumed that, if one utilizes a collection of proxies, the signals T_S will have similar trends for the various proxies while the trends for the noise T_N will vary randomly from proxy to proxy, sometimes plus and sometimes minus. Hence, if one adds up a number of proxies, the signals will tend to reinforce while the noise will tend to cancel out, leading to an estimate of T_S with less noise. Since the noise is typically quite large, sophisticated statistical mathematical schemes have been utilized to extract the signal according to this hypothesis.

Scientists who process proxies with complex statistical procedures have produced a steady stream of journal articles that justify their procedures (e.g., Rutherford *et al*., 2005). These articles seem to rarely show the actual original proxies during the calibration period, but only the result of feeding them into their analysis machines. The end result is typically a relatively flat meandering curve of temperature over the past millennium with a sudden rise in the 20th century (the so-called ''hockey stick''). In the latest in this series of self-justifying reports, Jones *et al*. (2009) provided a very lengthy review of the use of proxies to unravel the climate of the past millennium. The review covered (1) high-resolution proxy disciplines (trees, corals, ice cores, and documentary evidence), (2) various approaches for combining multiple climate proxy records to provide estimates of past hemispheric climates, and (3) use of climate model simulations of the past millennium. The end result for each proxy is a wiggly line representing a plot of temperature vs. year for a location. They then faced the problem of combining a large number of wiggly lines into a regional or global climate representation. The major stumbling blocks in combining multiple wiggly lines are (1) the spatial and temporal diversity and sparseness of the data, (2) the fact that most wiggly lines are heavily laden with local variations, and (3) large areas of the globe are underrepresented in the database. As a result, when large numbers of independent wiggly lines from various regions and time periods are simply summed and averaged democratically, the result tends to average out differences due to the wide

variety of phases and amplitudes of the wiggles. Jones *et al*. (2009) describe a number of sophisticated approaches for ''reconstructing the underlying spatial patterns of past surface temperature changes at global scales'' and ''assimilating proxy records into reconstructions of the underlying spatial patterns of past climate change''. Basically, this means extracting T_S from $T_S + T_N$. However, as several critics have shown, the resultant regional or global climate reconstruction does indeed depend upon the method used for reconstruction, and, depending on the method used, almost any result can be obtained. Furthermore, the net result of combining many wiggly lines with variable amplitude and phase tends to be a relatively flat profile. When this flat profile for the past millennium is combined with a measured upward trend of temperature vs. time in the 20th century, the inevitable result is a hockey stick type of figure (relatively flat for the prior 1,000 years with a sudden upturn in the 20th century). It is also remarkable that Jones *et al*. (2009) made no mention of major criticisms of methods used to combine multiple wiggly lines but simply pretended that such criticisms do not exist. While it is true that most of these criticisms do not appear in the published literature, the reason for this is that it is difficult to get climate papers published that do not support the alarmist position.

2.3 THE LITTLE ICE AGE AND THE MEDIEVAL WARM PERIOD

2.3.1 Proxy evidence for the LIA and the MWP

Historic proxy studies have distinguished two periods of particular interest in the past millennium. One is the putative *Medieval Warm Period* (MWP) centered near 850–1050, which was supposedly an unusually mild climate. The other was the so-called *Little Ice Age* (LIA) from perhaps 1600 to about 1850 (depending on location) when temperatures were unusually cold. Apparently, the Earth was not uniformly warm during the MWP or uniformly cold during the LIA at all locations at all times. The same is true for the warming of the 20th century in which a third of the measurement stations report that their regions cooled during the 20th century while the other two-thirds warmed (Muller *et al*., 2011a). There has not been any period over the past 2,000 years in which all regions of the Earth warmed or cooled in lock-step.

A number of independent proxy studies show evidence of distinct MWP and LIA. For example, the GISP2 ice core record shows evidence of a MWP and a LIA (Rapp, 2012). Thorsteinsson showed evidence for the MWP and LIA in the Camp Century ice core. Dansgaard (2005) claimed that the MWP and the LIA ''were recognizable'' and ''stand out clearly'' in the Camp Century ice core. He also presented the data shown in Figure 2.2. More recently, Vinther *et al*. (2010) revisited the matter of using Greenland ice cores to infer temperature variations over the past few thousand years. (This paper was partly based upon the Ph. D. dissertation ''Greenland and North Atlantic climatic conditions during the Holocene—as seen in high resolution stable isotope data from Greenland ice cores'' by Bo Møllesøe Vinther at the University of Copenhagen in 2006):

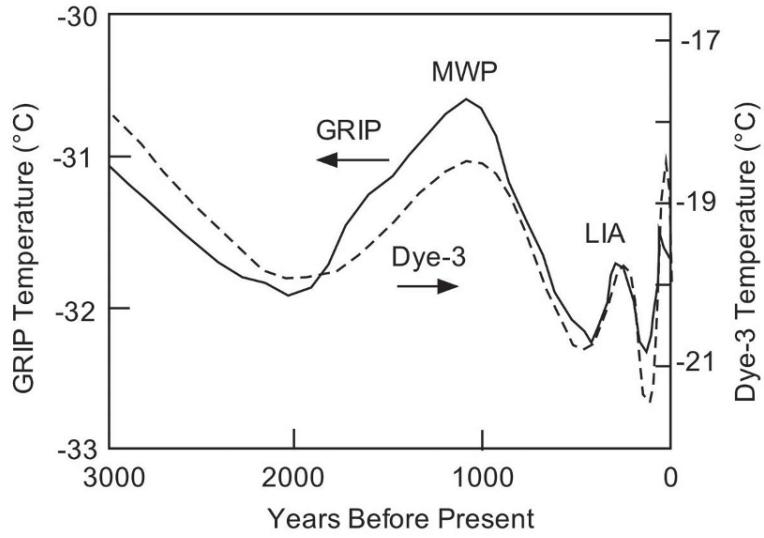

Figure 2.2. Ice core records showing LIA and MWP (Dahl-Jensen *et al*., 1998).

> "These authors worked with 20 ice core records from 14 different sites, all of which stretched at least 200 years back in time, as well as near-surface air temperature data from 13 locations along the southern and western coasts of Greenland that covered approximately the same time interval (1784–2005), plus a similar temperature dataset from northwest Iceland (said by them to be employed—in order to have some data indicative of climate east of the Greenland ice sheet)" (NIPCC, 2011).

Vinther *et al*. (2010) proceeded to demonstrate that "Greenland winter temperatures are much more variable than summer temperatures and thus dominate the annual average variability". So, they utilized winter ice core measurements of $\delta^{18}O$ at three sites on the Greenland ice sheet to examine the variability of climate from year 600 to year 2000. They found that the winter climate was highly variable with rather wild swings and, even with 50-year smoothing, the oscillations were large. Nevertheless, temperatures from about years 800 to 1000 were comparable to those of today, and temperatures from about 1400 to the late 19th century were demonstrably lower. This provides further support for the existence of a MWP and a LIA.

Richey *et al*. (2007) used Mg/Ca analyses in the white variety of the planktic foraminifera delta, which were obtained from the northern Gulf of Mexico as a measure of historical sea-surface temperatures. The results are shown in Figure 2.3.

Loehle (2007) produced a reconstruction of past temperatures that avoided the use of tree-ring proxies. His results are shown as Figure 2.4. The MWP and LIA are clearly delineated and the MWP is indicated to be warmer than the present.

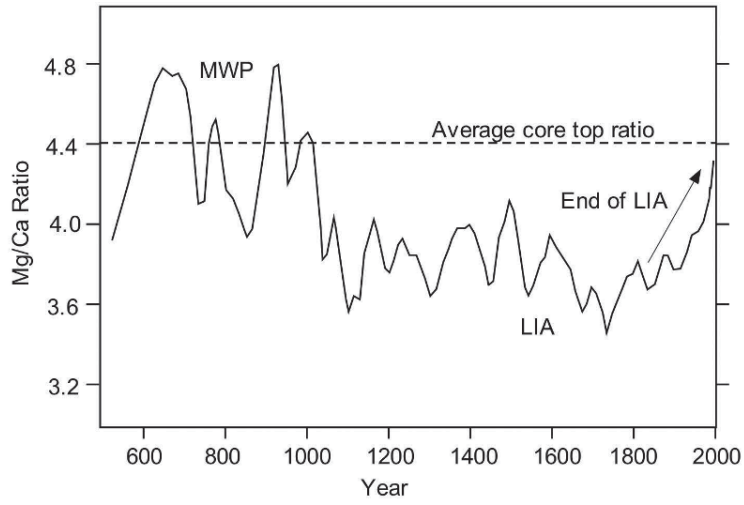

Figure 2.3. Mg/Ca analyses in the white variety of the planktic foraminifera delta, which were obtained from the northern Gulf of Mexico as a measure of sea-surface temperatures (Richey *et al*., 2007).

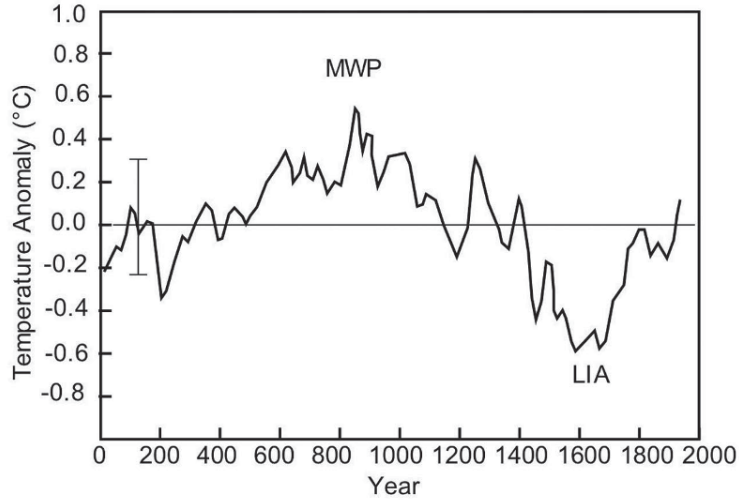

Figure 2.4. Mean temperature anomaly for 18 non-tree-ring proxy series (Loehle, 2007).

Yang *et al*. (2009) updated previous results for arid central Asia with new data over the last 2,000 years. They reported:

> "The most striking features are the existence of the *Medieval Warm Period* (MWP) and the *Little Ice Age* (LIA). The MWP was recorded in the 9–12th centuries and was accompanied by an anomalously dry climate, whereas the LIA extended from the 15–18th centuries and was accompanied by pluvial conditions."

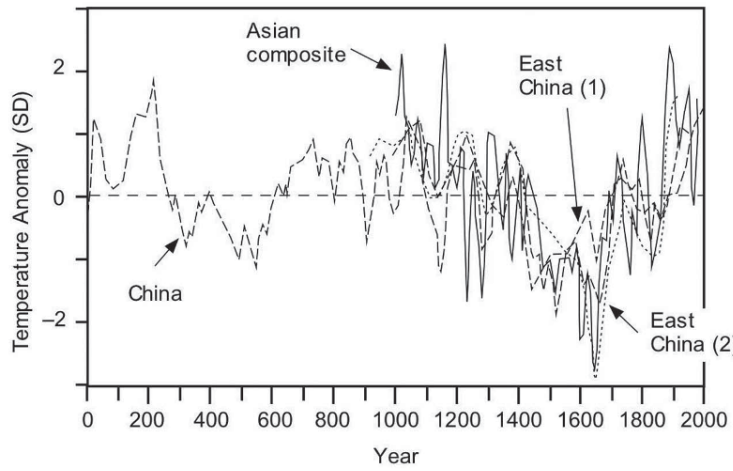

Figure 2.5. Estimates of historical temperatures in Asia (adapted from Yang *et al*., 2009).

Their result is shown in Figure 2.5.

Esper *et al*. (2012a, b) developed a 2,000-year summer temperature reconstruction based on 587 high-precision maximum latewood density (MXD) series from northern Scandinavia. The record was "developed over three years using living and sub-fossil pine (*Pinus sylvestris*) trees from 14 lakes and 3 lakeshore sites at latitudes $>65^{\circ}$N, making it not only longer but also much better replicated than any existing MXD time series". The reconstruction was calibrated against regional June–July–August (JJA) temperature (1876–2006) and spanned from 138 BC to AD 2006. The calibration curve is shown in Figure 2.6. It is to the credit of this team that, unlike most paleoclimatologists, they provided revealing data on the calibration period.

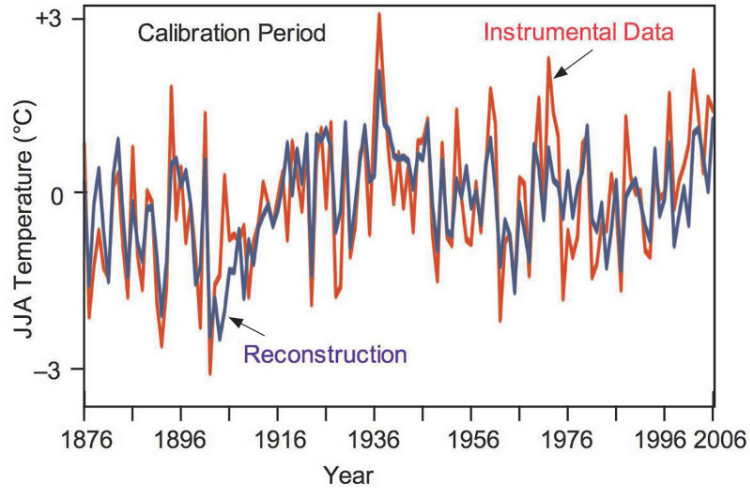

Figure 2.6. Calibration curve for northern Scandinavian tree-ring data (Esper *et al*., 2012a, b)).

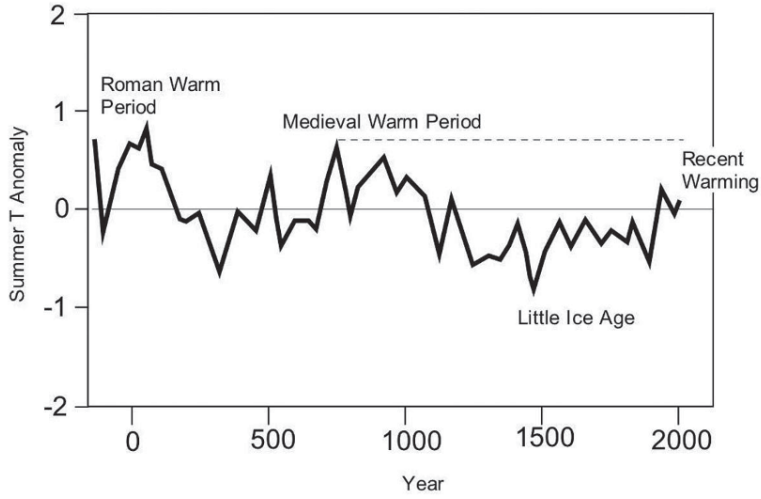

Figure 2.7. Reconstruction of 2,000-year history of Northern Scandinavian temperatures from tree ring data (Esper *et al.*, 2012a, b).

The resultant estimate of JJA temperatures for the past 2,000 years showed considerable noise from year to year with variations of $\pm 1^{\circ}$C being typical. Using a 100-year filter, they obtained the curve shown in Figure 2.7. These results indicate that past temperatures during Roman times and during the MWP were higher than the present, and there is a recognizable LIA. Esper *et al.* (2012a, b) chose to fit a straight line to the data, thus showing a fairly constant millennial-scale cooling trend. However, this interpretation is affected by the relatively high temperatures that occurred at the starting point in Roman times. Had the starting point been, say, AD 400, the trend would have been oscillatory with a positive lobe from AD 400 to AD 1200 and a negative lobe from AD 1200 to AD 1900. The interpretation of a fairly constant millennial-scale cooling trend might suggest a sudden change in the 20th century due to human influence, whereas the cyclical interpretation might suggest a new positive lobe beginning near AD 2000 (see Figure 2.8).

Idso (2008) provided extensive evidence in favor of the existence of the MWP and the LIA. In addition, prior to the MWP, there were numerous temperature excursions comparable with or greater than current global warming.

Shindell (2007) studied paleoclimatic data from a number of sources and concluded:

> "Historical data spanning the past millennium show substantial variations in aridity in the dry bands of the subtropics ... Palaeoclimatic records from a variety of sources and subtropical locations suggest that the MWP was generally marked by drier conditions, including prolonged droughts, which became less prevalent during the LIA. These records are supported by additional sediment and lake level records, including some showing wetter conditions near the equator, as well as fire residue and cultural records."

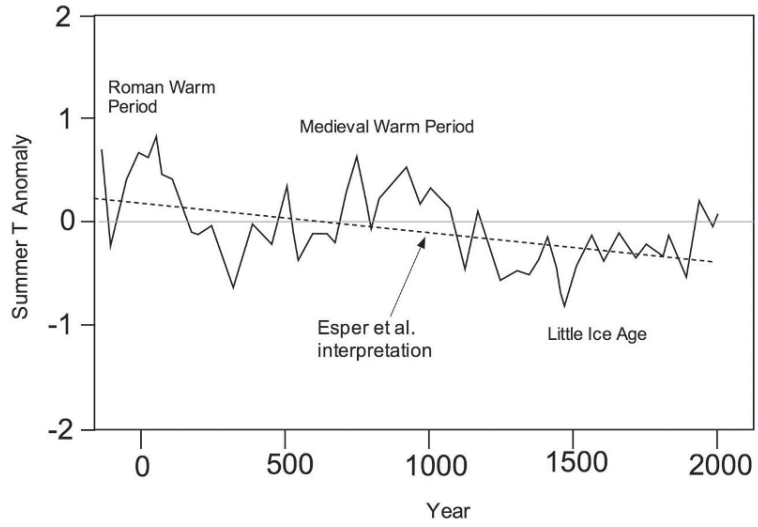

Figure 2.8. Reconstruction of 2,000-year history of Northern Scandinavian temperatures from tree-ring data with linear interpretation by Esper *et al.* (2012a, b).

Weckström *et al.* (2006) studied temperature patterns over the past eight centuries in Northern Fennoscandia inferred from sedimentary diatoms. Although temperature is not necessarily regarded as the strongest environmental variable affecting the distribution of diatoms, it was claimed that temperature has been shown to be a relatively powerful variable in this study area. The results show a pronounced MWP prior to 1200 and a wide flat bottom temperature from 1650 to 1900, representing the LIA. The temperature rise after 1900 is significant, but does not reach the temperatures of the MWP. Tree-ring data do not show this behavior.

Rørvik *et al.* (2009) examined sediment cores from a Norwegian fiord to infer temperature changes over the past 1,000 years. They found that "The periods from c. AD 500 to 790 and c. AD 1500 to 1940, stand out as cold periods".

Kobashi *et al.* (2010) derived Greenland temperatures over the past 1,000 years using nitrogen and argon isotope data, rather than oxygen isotope data. Their procedure is complex and appears to involve a number of assumptions. Nevertheless, they addressed these issues in considerable detail. Their conclusion was: "The data show clear evidence of the *Medieval Warm Period* and *Little Ice Age* in agreement with documentary evidence."

Barclay *et al.* (2009) presented data on the Tebenkof glacier in Alaska that clearly show the existence of a MWP and a LIA.

Cronin *et al.* (2010) used Mg/Ca ratios from ostracodes and oxygen isotopes from benthic foraminifera as proxies for temperature and precipitation-driven estuarine hydrography to prepare a 2,400-year paleoclimate reconstruction from Chesapeake Bay. This was compared to other paleoclimate records in the North Atlantic region to evaluate climate variability during the MWP and LIA. To the credit of these authors, they showed a detailed comparison of their proxy with

temperature records (many paleoclimatologists hide these data). However, the comparison was very unimpressive to this writer, and the value of their proxy is highly debatable. Nevertheless, using several sediment cores, they consistently found warm temperatures between years 600 and 1000, and colder temperatures from 1300 to 1600. In some cases, temperatures from 600 to 1000 were warmer than today and, in some, today's temperatures were warmer. As they showed, other proxy-based estimates at other NH locations produced variable results. The accuracy of these proxies is uncertain.

Lüdecke (2011) published a new reconstruction of temperatures over the past two millennia. Two long-range annual records were utilized: "a stack of tree rings and further biological proxies" (AD 0–1979) and a record of stalagmites from the Spannagel Cave (AD –90 to AD 1935). The variance in the cave data was seven times greater than that of the proxy data. However, both indicated a clear MWP and LIA. Neither indicated warming today as being greater than that of the MWP.

Additional insights may be gained from proxies at Antarctica. Hall *et al*. (2010) obtained moss, peat, and reworked shells from Antarctica ice that indicated that the Antarctic Peninsula had retreated to or beyond present levels 700–1000 YBP and earlier, suggesting additional evidence for a MWP.

The website: *http://co2science.org/data/mwp/mwpp.php* provides an extensive list of peer-reviewed scientific journal articles pertaining to the MWP, and provides brief summaries of the findings of each paper. The locations of these studies are plotted on an interactive map of the globe. Studies are categorized by the degree to which they support the notion of a MWP as well as by geographical location (Africa, Antarctica, Asia, Australia/New Zealand, Europe, North America, NH, Oceans, and South America). An amazingly large number of studies exist. The majority of studies find good evidence for a pronounced MWP although a few studies find no evidence that the MWP was as warm as present-day temperatures. Many of these studies also provide evidence on the LIA. In addition, the studies by Idso, Carter and Singer (2011) and Idso (2008) provide extensive evidence in favor of the existence of significant MWP and LIA.

2.3.2 Anecdotal inferences on the MWP and the LIA

Fagan (2000) provided many anecdotal descriptions of the climate during the MWP and the LIA. How accurate or general these may be remains an open question.

Fagan described the mild period prior to the LIA. The overseas conquests of the Norse from about 800–1200 took place in "unusually mild and stable weather". He went on at length to describe the reduced pack ice around Iceland when "winter and summer temperatures were usually higher than today". By contrast, during the great cold of 1350–1380, sea ice came so close to land that Greenland polar bears came ashore on Iceland. He said: "For five centuries [9th through 13th centuries], Europe basked in warm, settled weather … Compared with what was to follow, these centuries were a climatic golden age."

Fagan said: "In the 13th century, Greenland and Iceland experienced increasing cold. Sea ice spread southward around Greenland and in the northern Atlantic,

creating difficulties for Norse ships ...'' Icelandic recollections of the LIA are typified by: ''In the extreme winter of 1695, ice blocked the entire coast in January and stayed all summer.''

He also described changes in weather patterns other than temperature: drought and excessive rainfall. There was a deluge seven weeks after Easter in 1315. Rain fell essentially continuously from May through August, followed by a cold September, resulting in widespread famine and dislocation.

Fagan said:

> ''The *Medieval Warm Period* saw long successions of warm, settled summers. Then, starting around 1310, and continuing for about five and a half centuries, the climate became more unpredictable, cooler, occasionally stormy, and subject to sporadic extremes—the LIA. Anecdotal accounts of the cold during the *Little Ice Age* abound in paintings of the period, showing skaters on frozen lakes and streams that do not presently freeze.
>
> ''Between 1680 and 1730, the coldest cycle of the *Little Ice Age*, temperatures plummeted, the growing season in England was about five weeks shorter than it was during the twentieth century's warmest decades. The number of days each winter with snow on the ground in Britain and the Netherlands rose to between twenty and thirty, as opposed to two to ten days through most of the twentieth century. The winter of 1683–84 was so cold that the ground froze to a depth of more than a meter in parts of southwestern England and belts of sea ice appeared along the coasts of southeastern England and northern France. The ice lay thirty to forty kilometers offshore along parts of the Dutch coast. Many harbors were so choked with ice that shipping halted throughout the North Sea. Conditions around Iceland were now exceptionally severe. Sea ice often blocked the Denmark Strait throughout the summer. In 1695, ice surrounded the entire coast of Iceland for much of the year, halting all ship traffic. The inshore cod fishery failed completely, partly because the fish may have moved offshore into slightly warmer water, but also because of the islanders' primitive fishing technology and open boats. On several occasions between 1695 and 1728, inhabitants of the Orkney Islands off northern Scotland were startled to see an Inuit in his kayak paddling off their coasts. On one memorable occasion, a kayaker came as far south as the River Don near Aberdeen. These solitary Arctic hunters had probably spent weeks marooned on large ice floes. As late as 1756, sea ice surrounded much of Iceland for as many as thirty weeks a year ... The cold polar water spread southward toward the British Isles. The cod fishery off the Faeroe Islands failed completely, as the sea surface temperature of the surrounding ocean became 5°C cooler than today. Just as it had in the 1580s, a steep thermal gradient developed between latitudes 50° and 61–65° north, which fostered occasional cyclonic windstorms far stronger than those experienced in northern Europe today. The effects of colder *Little Ice Age* climate were felt over enormous areas, not only of Europe but the world.'' (Fagan, 2000).

There are anecdotal indications that the so-called *Medieval Warm Period* (MWP) from about 800 to perhaps 1200 was warmer than any period that followed

it (e.g., grapes suitable for wine-making were reportedly grown in England, and the tree line in Scandinavia was 100–200 m higher than it is at present (Crowley and Lowery, 2000), although some believe that the late 20th century was warmer than the MWP. The degree of warmth in the MWP remains uncertain. There is considerably better anecdotal evidence that the so-called *Little Ice Age* (LIA) that followed the MWP (approximately 1400–1850) was considerably colder than the MWP, although there are uncertainties as to how consistently cold and how widespread the LIA was. The warmth of the MWP has been estimated by two means: (1) global climate models based on rather uncertain forcings, or (2) proxy analysis based on relatively few proxies available of uncertain veracity.

However, Crowley (2002) showed that historical written reports cannot always be taken at face value. An example is cited regarding the often-misused freezing of the River Thames. Between 1408 and 1914, the Thames in London froze over 22 times. Century counts are: 1400s (two times), 1500s (five), 1600s (nine), 1700s (five), and 1800s (one). Taken at face value, this would seem to imply that the 1600s and 1700s were coldest. However, there were a number of modifications to this bridge, including reductions in the number of piers of London Bridge in 1756 that reduced ponding effects. Replacement of the bridge between 1825 and 1835 widened the piers further and removed the small weir, enabling the tide to encroach farther upstream. No complete freezes have occurred since then. Changes to the river and its channel are important factors that must be considered alongside cooler winters as causes of freeze-over. For example, in the winter of 1962–1963, the third coldest since 1659 in the Central England Temperature (CET) record, the river only froze upstream of the modern tidal limit (20 km upstream of central London). In the two coldest CET winters in 1683–1684 and 1739–1740, the Thames froze, but it also froze during nine other winters between 1659 and 1820. Painter *et al.*, (2013) argue that the decline of the LIA was likely driven by black carbon deposits on alpine glaciers from expanding industrialization of Western Europe in the mid-to-late 19th century.

2.3.3 Challenges to the notion of the LIA and MWP

In the past decade, some climatologists emphasized the lack of uniformity in evidence for the MWP and the LIA, and thereby attacked the very notions of the MWP and the LIA, arguing either that they were regional, minor, and variable, or in some cases they were claimed to be non-existent.

However, as Soon and Baliunas (2003a, b) pointed out:

> "The term *Medieval Warm Period* has been the subject of considerable controversy. Its nature and even its existence has been questioned ... as has that of the *Little Ice Age* ... They were not periods of unbroken cold and warmth respectively. Climate varied on small scales both spatially and temporally, as it has also in the twentieth century. Nevertheless, climatic conditions were such during the *Little Ice Age* that mass balances were sufficiently predominant for the glaciers to remain enlarged, although their fronts oscillated. Similarly during the *Medieval Warm Period* climatic conditions caused mass balances to be

> negative, and volumes of glaciers to be reduced, so that they retracted substantially, though their fronts no doubt fluctuated, as they have been observed to do during the warming of the twentieth century.''

One can deal with the MWP and the LIA in various ways. Grove (2001) reduced the difficulty in deciphering the nature of the MWP and the LIA by not directly referring to climate, but rather by limiting the definition to the extent to which glaciers extended globally and remained primarily enlarged or primarily retracted, while their fronts fluctuated about these forward or backward positions.

Various proxy studies have derived different time periods, and different spatial extents of the putative LIA. Ogilvie and Jonsson (2001) pointed out ''the difficulty in defining exactly the onset of the LIA''. However, it is widely agreed that the end of the LIA was either late in the 19th century or early in the 20th century. They emphasized the many discrepancies and lack of coherence in data purporting to define the temporal extent of the LIA (and the MWP as well). However, it is not clear how much of the variability in the observed timing of the LIA is due to true variability and how much is due to difficulty in interpreting proxy data. Ogilvie and Jonsson (2001) said:

> ''This lack of agreement could be due in part to uneven distribution and character of the evidence available, to the dating techniques used, and their resolution, and possibly due to differing degrees of effort devoted to unraveling glacial history.''

There is no precise onset and there is no way to define the LIA except as a multi-century period when temperatures were predominantly (but certainly not exclusively) relatively low compared with the eras that preceded and followed the LIA.

It is understood that neither the LIA nor the MWP were periods of unbroken cold and warmth, respectively. Climate varied on small scales both spatially and temporally, as it has also in the 20th century. Nevertheless, climatic conditions may have been such during the LIA that mass balances were sufficient for the glaciers to remain predominantly enlarged, although their fronts oscillated. Similarly, during the MWP, climatic conditions would have caused the volumes of glaciers to be reduced, so that they retracted substantially, although their fronts no doubt fluctuated, as they have been observed to do during the warming of the 20th century.

Soon and Baliunas (2003a, b) carried out a detailed study of a number of proxies that indicated that the LIA and the MWP existed as a distinguishable climatic anomaly in almost all regions of the world that were assessed. Furthermore, they concluded that most of the proxy records do not suggest the 20th century to be the warmest or the most extreme.

Soon and Baliunas (2003a, b) provided a very long table listing the various proxies used in the study. For each proxy, they provided the spatial extent, latitude, and longitude (where applicable), type of proxy, reference, and qualitative evaluations of whether MWP and LIA trends were discernible. These included 14 worldwide proxies, and >100 proxies that are regional or local. The results were provided in several figures accompanied by a lengthy discussion (about 25 pages) of

detailed information regarding specific proxy results that led to these figures. These results indicated that:

- The proxy data suggest that the LIA existed as a distinguishable climatic anomaly in almost all regions of the world that were assessed. Only two records, did not exhibit any persistent or unusual climatic change over this period.
- The MWP is a distinguishable climatic anomaly with only two unambiguous negative results.
- Most of the proxy records do not suggest the 20th century to be the warmest or the most extreme in its local representations. There are only three unambiguous findings favoring the 20th century as the warmest of the last 1,000 years. An interesting feature is that the warmest or most extreme climatic anomalies in the proxy indicators often occurred in the early to mid-20th century, rather than throughout the century.

The reaction of the *paleoclimatic cabal* was quick and forceful. In a series of emails that were not revealed until they were hacked in November 2011, *cabal* members strategized to repair the damage from the Soon and Baliunas paper. In one email from Malcolm Hughes to a dozen members of the *cabal*, he cautioned that "an appeal to the National Academy of Sciences (NAS) could be counterproductive—remember the poor treatment of high-res paleo in the NAS report requested by the White House the other year". (Note that he is apparently referring to the Wegman Report that was sanctioned by the NAS.) Michael Mann, in his usual arrogant stance, referred to "two awful papers written by those clowns" and, yet, what could be more awful than Mann's publications? Mann also referred to their paper as "an assault on the science of climate change"—which is exactly what his papers constitute. The outcome of this *cabalistic* exchange was the publication by Mann and Jones (2003) that has little technical content and represents mainly an affirmation of the faith of the *cabal* in its orthodoxy. Many websites lit up with the news: "*Leading Climate Scientists Reaffirm View that Late 20th Century Warming Was Unusual and Resulted From Human Activity*."[1]

This report was authored by the founding members of the *cabal*: Michael Mann, Caspar Ammann, Kevin Trenberth, Raymond Bradley, Keith Briffa, Philip Jones, Tim Osborn, Tom Crowley, Malcolm Hughes, Michael Oppenheimer, Jonathan Overpeck, Scott Rutherford, and Tom Wigley. The report was not made public but was available only to journalists. Dozens of websites blared this headline but few details were revealed. The point was: 13 *cabalists* can hardly be wrong—or can they?

Crowley and Lowery (2000) argued that anecdotal reports as well as studies of individual records from MWP suggest that the present warmth of the 20th century is not unusual and therefore cannot be taken as an indication of forced climate change from greenhouse gas emissions. But Crowley and Lowery then asked the question: "Were all of these changes synchronous, with hemispheric amplitudes comparable to

[1] (e.g.: *http://www.agu.org/news/press/pr_archives/2003/prrl0319.html*).

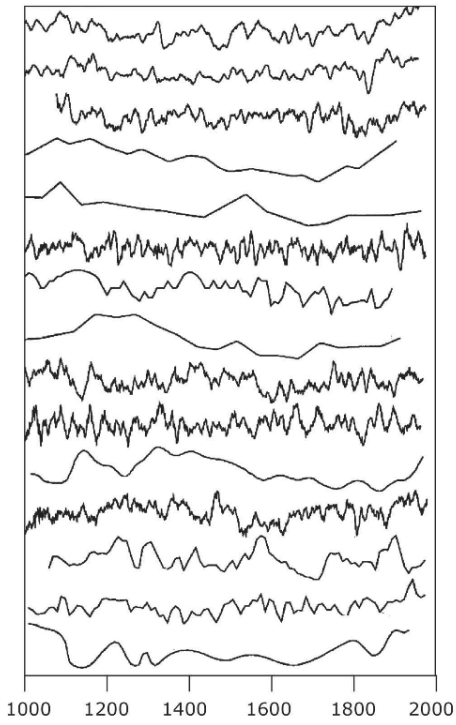

Figure 2.9. Fifteen individual proxies from various locations (adapted from Crowley and Lowery, 2000). Vertical scales are temperature anomalies.

or warmer than present?" However, this question seems to imply that present warming is spatially universal and synchronous—which it is not. (Indeed, the BEST study found that a third of reporting land sites experienced a decrease in temperature during the 20th century while the global average temperature advanced by roughly 0.6°C.) Crowley and Lowery revisited the controversy regarding the existence of the putative MWP by carrying out another proxy analysis, incorporating additional time series not used in previous hemispheric compilations. The 15 proxies used in the study are shown in Figure 2.9. It is to the credit of Crowley and Lowery that the individual proxies are shown; this is not often the case when assemblages of multiple proxies are analyzed. They combined the various proxies to obtain Figure 2.10. However, the process used to combine proxies is not clear to this writer.

Based on this result, Crowley and Lowery (2000) reached the rather incredible conclusions:

> "Despite clear evidence for medieval warmth greater than present in some individual records, the new hemispheric composite supports the principal conclusion of earlier hemispheric reconstructions and, furthermore, indicates that maximum medieval warmth was restricted to two–three 20–30 year intervals, with composite values during these times being only comparable to the mid-20th century warm time interval. Failure to substantiate hemispheric warmth greater than the present consistently occurs in composites because there are significant offsets in timing of warmth in different regions; ignoring these offsets can lead to serious errors concerning inferences about the magnitude of Medieval warmth and its relevance to interpretation of late 20th century warming."
>
> "Because of uncertainties in the proxy-instrumental temperature calibration, it is still difficult to unequivocably [*sic*] assert that the late 20th century warming is significantly greater than the peak warmth of the *Medieval Warm Period*. But there is even less justification to assert the opposite—it is not possible to make a robust statement that the *Medieval Warm Period* was warmer than the last two decades."

These conclusions seem far-fetched to this writer because:

- The huge variation from proxy to proxy suggests that these divergences may not represent true differences in climate from place to place, but rather, noise and error in the proxies themselves.
- Taking averages of highly divergent individual proxies must tend to average out variations, and add uncertainty and large error bars to the resultant average.
- The belief that the averages shown in Figure 2.9 can be trusted to the extent that differences between 20-year periods can be affirmed seems unwarranted by the lack of precision and consistency of the underlying data.
- The insistence that the MWP must perforce involve uniformly high temperatures at all locations for the entire period is unreasonable. The issue is not whether such uniform warmth occurred, but rather, allowing for spatial and temporal variations, the preponderance of evidence favors relative warmth compared with other eras. Furthermore, as we have pointed out, even the current warming in the 20th century is far from uniform, spatially and temporally.

McIntyre (2007) examined the results of Crowley and Lowery (2000) in some detail. McIntyre prepared a new figure, similar to Figure 2.10, except that it showed contributions from each of the 15 individual proxies with color-coding. McIntyre pointed out:

> "A distinctive 'hockey-stick' shape can be discerned in the 4 lowest records. Indeed, whatever '*hockey stick*iness' exists in Crowley and Lowery (2000) is entirely due to these 4 series, which consist of 2 bristlecone pine series, Briffa's Polar Urals series and Thompson's Dunde series ... The bristlecone pine series are prominent in the MBH99 reconstruction and the Polar Urals series in the Jones *et al*. (1998) series. Both series have problems [as discussed elsewhere in McIntyre, 2007]."

McIntyre (2007) then modified the color-coded version of Figure 2.10 by omitting four suspect proxy series. Instead of deleting the Sargasso Sea and Central Michigan proxies, both of which are claimed by McIntyre to be well linked to

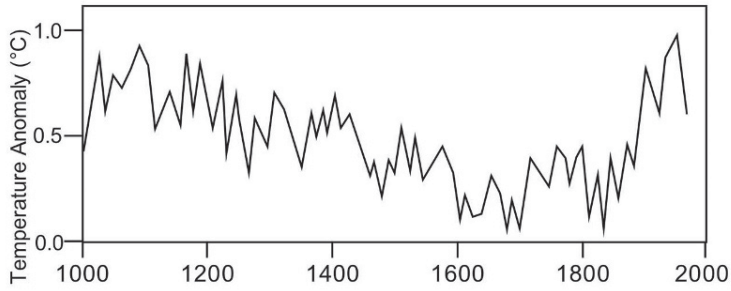

Figure 2.10. Derived NH temperature anomalies (adapted from Crowley and Lowery, 2000).

temperature, the two bristlecone pine series were excluded (as not being good temperature proxies) and the first century of the Polar Urals series was excluded on quality control grounds. Without the contribution of the bristlecones and Polar Urals, the MWP peak is comparable with the 20th-century peak.

Hegerl *et al.* (2007) added to the work of Crowley and Lowery (2000) using "updated records, a modified reconstruction method, and a new calibration technique". The stated goal of the study (as evidenced by the title of the paper) was to detect human influence on the climate:

> "The reconstruction consisted of three individual segments. A baseline reconstruction used 12 decadal records and covered the period to 1505. One longer, less densely sampled land temperature reconstruction ... was based on seven records back to A.D. 946, and [the third] consisted of five records back to A.D. 558."
>
> As is usual in such studies, they did not show the actual comparisons between proxies and temperatures during the calibration period so there is little basis to judge their adequacy *a priori*. Nevertheless, Hegerl *et al.* (2007) arrived at a slightly modified hockey stick result. Their hockey stick had a very minor MWP and a $\sim 0.5^{\circ}$C LIA. They used Jones' "trick" of tacking on the recent instrumental record to the proxy results. However, they show an instrumental record that gained almost 1.5°C since 1900, that is far out of line with other estimates, and provides a very exaggerated view of the rise in temperature in the 20th century. It defies the logical mind to imagine how 12 or 7 or 5 proxy records, each of dubious credibility, centered in Europe, and with no representation from the Southern Hemisphere or the 70% of the Earth covered by oceans, could adequately define the global climate over 1,500 years."

What is missing from the proxy analysis of Hegerl *et al.* (2007) (as well as every other published proxy analysis that I am aware of) is a presentation of the comparison of each proxy at each location with the temperature as measured at that location during the calibration period (as well as after the calibration period). The variations from proxy to proxy are enormous. It seems likely that these vastly different patterns have little to do with temperature. But, if they do properly represent temperature at each location, and the temperature patterns vary by that much, we might ask how many proxies (locations) are needed to approximate a global average temperature? Certainly, use of only 15 proxies appears on the face of it to be grossly and incredibly inadequate.

2.4 GLOBAL AND HEMISPHERIC AVERAGE TEMPERATURES IN RECENT MILLENNIA

2.4.1 The "MBH" model

Realizing that there exists a number of local, regional, and hemispheric proxies, with variable spatial and temporal extent, Mann, Bradley, and Hughes (1998, 1999) attempted a comprehensive analysis of the history of global average temperatures using a multi-proxy network consisting of "widely distributed high-quality annual-resolution proxy climate indicators, individually collected and formerly analyzed by many paleoclimatic researchers". The network included annual-resolution dendroclimatic ice-core, ice-melt, and long historical records previously assembled, combined with other coral, ice-core, dendroclimatic, and long instrumental records. This was intended to integrate as many proxy sources as possible into a single comprehensive view of how a single global average temperature (or NH average temperature) varied over the past millennium. A number of subsequent related studies have also been published by the same group, as well as other allied groups. The final result is a reconstruction of a single NH or global average temperature over the past one or two millennia with a so-called *hockey stick* structure: a rather flat profile for most of the millennium, prior to the 20th century, with a significant rise in the 20th century.

Mann, Bradley, and Hughes (1998) has been referred to as "MBH" after the names of the three authors of the principal paper. Subsequently, Mann, Bradley, and Hughes (1999) extended the period of analysis from 1400 back to 1000, and Mann and Jones (2003) added an additional millennium back to 200. Mann *et al*. (2008) updated previous results. There are also a number of other relevant papers by other investigators.

MBH is a compact paper, full of jargon, and difficult to follow. However, this is a characteristic shared by many papers that deal with large data sets for historic Earth temperatures. Wegman, Scott, and Said (2006) said:

> "The papers of Mann *et al*. in themselves are written in a confusing manner, making it difficult for the reader to discern the actual methodology and what uncertainty is actually associated with these reconstructions. Vague terms such as 'moderate certainty' give no guidance to the reader as to how such conclusions should be weighed. While the works do have supplementary websites, they rely heavily on the reader's ability to piece together the work and methodology from raw data. This is especially unsettling when the findings of these works are said to have global impact, yet only a small population could truly understand them."

Wegman, Scott, and Said (2006) also said: "The description of the work in Mann, Bradley, and Hughes (1998) is both somewhat obscure and as others have noted, incomplete."

The reference period for calibration of proxies with actual temperature data was 1902–1980. The various proxies were more numerous in recent times and far less

Table 2.1. Number of proxies vs. earliest date according to Mann, Bradley, and Hughes (1998, 1999).

Earliest date	*Number of proxies*
1000	12
1400	22
1450	24
1600	57
1700	74
1763	93
1820	112
1854	219
1902	1,082

numerous in the more distant past. The number of proxies vs. earliest date is shown in Table 2.1. This array of proxies is vastly inadequate to represent the global climate. Of the dozen proxies that extended from 1000 to 1400, four were ice cores from a single small ice cap in Peru, and three were tree rings from the southwest U.S. How could that possibly lead to a global climate? These facts were well hidden in propaganda for the hockey stick, like a pea under walnut shells.

Each of the proxy data sets had variable geographical distribution. The task was to combine these into a uniform function that best expresses the putative single global average temperature over a long time span. The process used for data reduction is too complex to discuss in any detail here. As is usual in such studies, these references worked with variances from the mean, rather than actual temperatures. However, a crucial factor was that they chose the mean during the calibration period (1902–1980) rather than the mean for the entire data set. As we shall see, this had major repercussions regarding the form and credibility of the result. Another key aspect was the use of principal components analysis (PCA) to identify the primary trends in data containing scatter and noise.

The final result from Mann, Bradley, and Hughes (1998) is shown in Figures 2.11 and 2.12. Note that the mean is the mean for the calibration period 1902–1980, and therefore most of the data (1400–1920) lie below the mean. We will have more to say about this in the following sections. These *hockey stick* figures were published in subsequent papers with a relatively flat profile prior to 1900 and a sudden rise after 1900. Note that the "*X*" at the far right of Figure 2.12 is meant to be the current temperature. As shown, it is 1.1°C higher than the 1895 temperature, whereas it is widely believed that this temperature differential is more like 0.7°C. This exaggerated the shape of the hockey stick.

Mann and Jones (2003) extended the work of Mann, Bradley, and Hughes (1998, 1999) back to year 200. Their result is similar to that shown in Figure 2.12 with the addition of essentially no change in temperature from year 200 to year 1000. Taken at face value, these figures would suggest: (1) there was no MWP, (2) there was a very minor LIA, (3) Earth temperatures have been remarkably stable for 2,000 years, and (4) the only significant change in Earth temperature took place in the

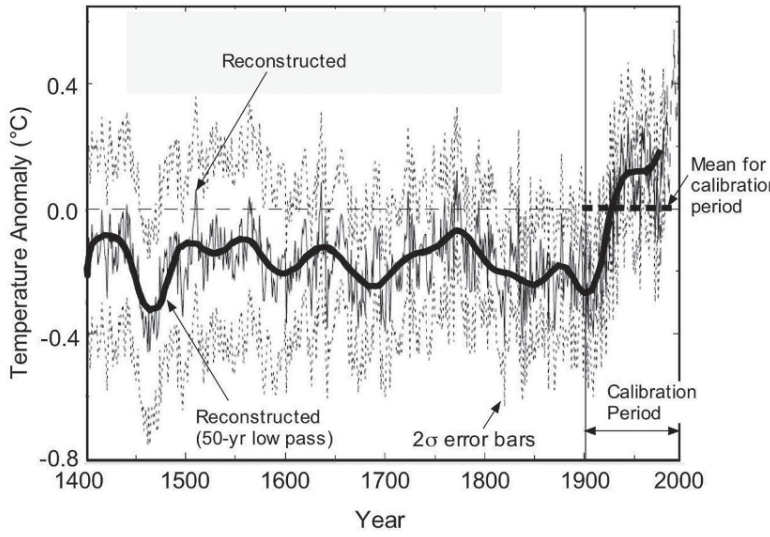

Figure 2.11. Reconstructed temperatures since 1400 (Mann *et al.*, 1998). Note that the mean is for 1902–1980. Also note that the 2σ error bars are so wide that they could hide almost any imaginable temperature curve.

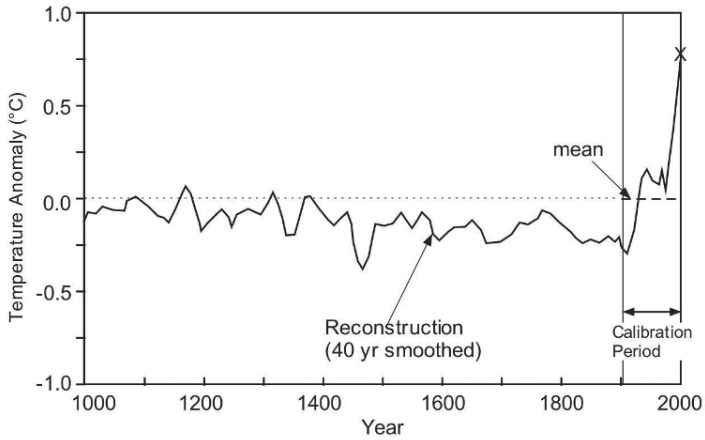

Figure 2.12. Temperature anomaly vs. year since AD 1000 (adapted from Mann *et al.*, 1999). The *X* at the far right is their estimate for 1998. The measured temperature rise from 1895 to 2008 is around 0.7°C, whereas Point *X* suggests a rise of 1.1°C. Note that the mean is for 1902–1980.

20th century with a sudden and decisive sharp rise after 1900. However, as we previously mentioned, MBH chose the mean for the calibration period (1902–1980) rather than the mean for the entire data set. As we shall see, this had major repercussions regarding the form and credibility of the result.

2.4.2 Other related models

A number of other groups published reconstructions of historical temperatures using similar methods. For example, Jones, Osborn, and Briffa (2001) obtained similar results using similar data-processing schemes. It is particularly noteworthy that Jones, Osborn, and Briffa (2001) decided not to show some of the proxy data late in the 20th century because it ticked sharply downward and conflicted with the desire to emphasize recent global warming (as we will discuss in a later section). Esper *et al.* (2005) discussed differences between various reconstructions based primarily on tree rings and presented a comparison. There is considerable variation in amplitude of the MWP and the LIA from study to study.

Moberg *et al.* (2005) indicated that, although differences in the amplitude of centennial temperature variability have been discussed in the literature, the picture with relatively small variability prior to the 20th century (i.e., the *hockey stick*) ''is arguably best known by a wider audience. One reason for this is the prominent role that the multi-proxy reconstruction by MBH had in the latest IPCC report and in public media''. However, they went on to point out that recent findings suggest that considerable underestimation of centennial NH temperature variability may result when regression-based methods (like those used by MBH) are applied to noisy proxy data with insufficient spatial representation. Moberg *et al.* (2005) also referred to well-documented difficulties in reliably reproducing multi-centennial temperature variability based on tree-ring proxies. Note the emphasis on data that ''are best known'' due to promotion by the IPCC and the media. von Storch *et al.* (2004) used a coupled atmosphere–ocean model simulation of the past 1,000 years to test empirical reconstructions of historical temperatures, specifically those of MBH. They found that centennial variability of the NH temperature is underestimated by the MBH regression-based methods. Their results also suggest that actual centennial variability may have been at least twice as large as the variability obtained in the MBH studies. Juckes *et al.* (2006, 2007) provided an extensive survey of a number of recent temperature reconstructions based on proxies. Juckes *et al.* (2006) presented a number of graphs of reconstructions of historical temperatures. However, all of these were based on MBH-type models in which the mean was chosen only for the calibration period (20th century) and, as a result, almost all of the temperature data for the past millennium (except for the 20th century) lie below the mean.

A comparison of the results of Mann *et al.* (2008) with results of other models shows wide variation from model to model (see Figure 2.13).

2.4.3 Fallacies in reconstruction of millennial temperatures

As we have pointed out, generally, the published papers on reconstruction of millennial temperatures tend to be very terse and full of jargon. The MBH papers are particularly bad in this respect. These papers present their results in small graphs with poor resolution but provide little insight into the calibration periods of specific proxies. Comparisons of proxies with temperatures during the calibration

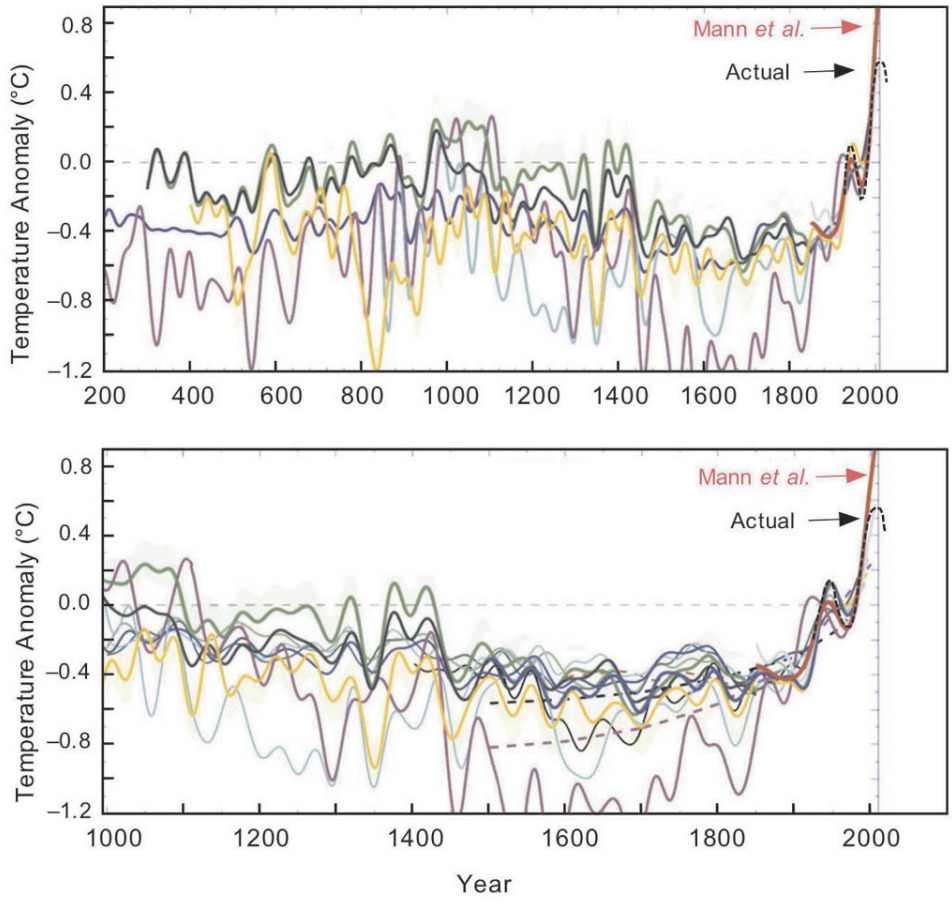

Figure 2.13. Estimates of historical temperatures by Mann *et al*. (2008) and others. Mann *et al*. (2008) exaggerated the temperature rise at the far right. The actual temperature change is shown as a black dashed line.

period are rarely provided and probably for good reason; the comparison is likely to be poor.

The only way to really understand what was done is to go back to their original data and follow the original procedures. As more and more of these reconstructions appeared in the literature with their typical hockey stick results, McKitrick (2005a) and McIntyre and McKitrick (2005, 2006, 2007) took it upon themselves to review these reconstructions by working with the original data in detail. These original studies by McIntyre and McKitrick continue to this day in the form of sporadic entries on the blog: *climateaudit.org*. The first obstacle they ran into was obtaining the data from the authors. Publications in journals are highly compressed and do not provide adequate means for others to reproduce the claimed results of the paper. Some journals require that authors archive the detailed data for access by the public

but this is rarely enforced. Sensing that McIntyre and McKitrick (M&M) were antithetical to the hockey stick results, authors of papers on reconstruction of millennial temperatures resisted providing M&M with data and script from their work. Evidently, they were defensive about their work and did not cooperate in allowing their work to be checked. When M&M utilized the *Freedom of Information Act* (FOIA) in an attempt to obtain data generated by government-funded work in the U.S. and England, the authors of papers enlisted help from politicians to pervert and circumvent the FOIA on specious grounds.

2.4.3.1 The fallacy of choosing the wrong mean

After much perseverance, M&M succeeded to a considerable degree in penetrating the MBH data and procedure. In the course of doing this, they uncovered several major errors in the MBH approach. The principal problem was summarized by McKitrick (2005a). A paraphrased rendition of some of his remarks is given in the next paragraph.

In a conventional PCA, the temperature data are standardized by subtracting the mean of entire data set and dividing by the standard deviation of the entire data set. This re-centers and re-scales all the data to a mean of zero and measures deviations from the mean in units of the standard deviation. In the MBH program, a scaling was applied, but rather than subtracting the mean of the entire data set over all years, they subtracted the mean of the 20th-century portion used for calibration, and then divided by the standard error of the 20th-century portion. While this may appear at first glance as innocuous, it has important consequences for the results derived from this procedure. The overwhelming majority of individual proxy series do not have the form of hockey sticks, but appear as random noise, and, since they don't change much in the 20th century, this procedure did not make much difference for them. For these proxies, the mean of the calibration period is roughly the same as the mean of the whole series (as is the standard error) so either way of standardizing yields more or less the same result. But a few of the proxy series trend upward in the 20th century. For these, the MBH method has a huge effect. Since the mean of the 20th century portion is higher than the mean of the whole series, subtracting the 20th-century mean de-centers the series, shifting it off a zero mean. This, in turn, inflates the deviations from the mean of these series with increases in the 20th-century. PCA algorithms inflate the weights of proxies with the highest deviations. If one proxy series in the group has a relatively high level of deviation from the mean, its weight in the PC1 gets inflated. The MBH algorithm did just this. The PCA procedure would, in effect, sift through a data set and identify series with a 20th-century uptrend, and then load almost all the weight onto these series. In effect, it data-mines for hockey stick trends.

Consider the hypothetical set of proxies shown in Figure 2.14. If PCA based on the mean for the calibration period is used for the flat proxies, no problem arises. However, when the mean for the calibration period is used for the proxy with an uptrend during the calibration period, the majority of deviations from the mean (b) will be much greater than deviations calculated from the mean for the entire data set

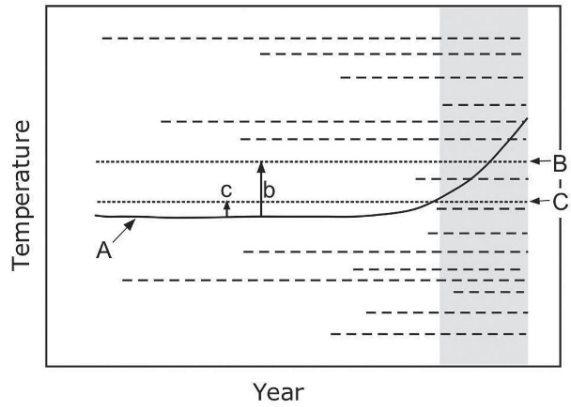

Figure 2.14. Hypothetical set of proxies where all proxies except one are flat and only one rises. The calibration period is shown as the gray rectangle. Flat proxies are shown as horizontal dashed lines. The one proxy (*A*) with variance is shown as a solid curve. Dotted line (*B*) is the mean for proxy (*A*) over the calibration period, while dotted line (*C*) is the mean for proxy (*A*) over the entire series. Deviations of proxy (*A*) from the two means are shown as (*b*) and (*c*).

(*c*). The point here is that, for the hypothetical set of proxies shown in Figure 2.14, PCA based on the mean for the calibration period will produce a "trend" similar to the one proxy with a slope and will essentially zero out the contribution of all the horizontal proxies to the estimated trend. Yet the preponderance of evidence is that the trend representing the overwhelming majority of data is actually horizontal. In cases such as that shown in Figure 2.14, PCA emphasizes the proxy with the greatest trend but it is not representative at all of the whole data set.

Figure 2.15 provides an example of the data-mining effect. It shows 2 of the 90 full-length series in the MBH database. The top panel is a tree-ring chronology from a stand of bristlecone pines at Sheep Mountain, California. The bottom panel is a tree-ring chronology from Mayberry Slough, Arkansas. In the bottom panel, the mean over the last 80 years is roughly equal to the mean for the previous 500 years, but, in the top panel, the post-1900 mean is above that for the pre-1900 portion. The MBH algorithm attributes 390 times as much weight to the top series as it does to the bottom series in the first principal component (PC1).

As it turns out, of 1,082 proxies used by MBH, only a handful exhibit the form shown in the upper panel of Figure 2.15, and all of these are tree-ring proxies that probably suffer from the potential CO_2 fertilization problem in the 20th century.

To test the MBH data-mining algorithm, M&M ran an experiment in which they input only trendless random red noise, simulating the data one would obtain from trees in a climate that is only subject to random fluctuations with no warming trend. In 10,000 repetitions, they found that a conventional PC algorithm (using the mean for the entire data set) almost never yielded a hockey stick-shaped PC1, but the MBH algorithm using the mean for only the calibration period yielded a pronounced hockey stick-shaped PC1 more than 99% of the time. The MBH algorithm efficiently

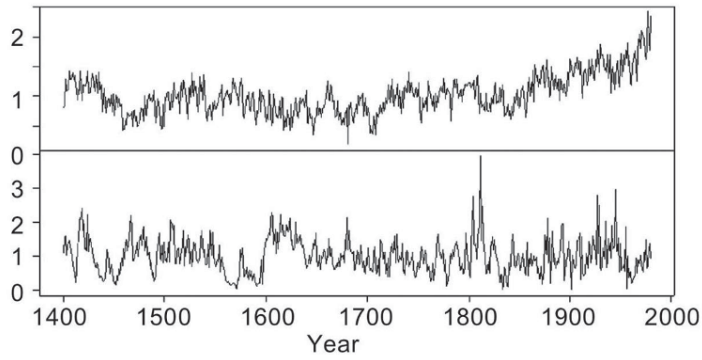

Figure 2.15. Two tree-ring temperature anomaly series from the MBH data set. Top: Sheep Mountain, CA. Bottom: Mayberry Slough, Arkansas (*climateaudit.org*).

looks for those kinds of series and flags them for maximum weighting. It concludes that a hockey stick is the dominant pattern even when pure noise is the input!

M&M extended their study in two ways. First, they showed that the MBH data-mining procedure did not just pull out a random group of proxies—it pulled out an eccentric group of bristlecone pine chronologies. These trees (the Sheep Mountain series in Figure 2.15 is an example) all turned out to exhibit a 20th-century growth spurt that has not been fully explained, but is likely to be at least partly due to CO_2 fertilization and is known not to be a temperature signal since it does not match nearby temperature records. The original authors who made the measurements (and others) have stressed that these series do not constitute proper climate proxies. So, M&M examined the consequences to the MBH results if these 20 bristlecone pine proxies were excluded. The result showed no hockey stick at all. Without these proxies with their rising shapes in the 20th century to mine for, the MBH method generates a result just like that from a conventional PC algorithm, and shows the dominant pattern is not hockey stick-shaped at all. In other words, without the bristlecone pines, even the flawed procedure of MBH would not have had a hockey stick shape.

Since the MBH papers purported to be the ultimate reference for estimation of the Earth's climate for one or two millennia, these results are likely to play a pivotal role in influencing the understanding of climatology. As such, the data and methodologies used by MBH should be readily available for replication and evaluation by others. McKitrick (2005b) provided an informal review of the way that he and McIntyre became interested in the Mann hockey stick, and their efforts to replicate the MBH results in order to understand the basis for their findings. In his discourse, McKitrick (2005b) described the difficulties in obtaining the required information, and the obstructive attitude of the MBH team. It became evident that the MBH team had manipulated the data unwittingly to greatly amplify the weight assigned to the few proxies with hockey stick form. It also became evident that the peer-review process did not penetrate into the MBH papers, and probably operates at a rather superficial level in most cases. M&M were thwarted by the journal *Nature*

in their attempts to publish their criticisms and *Nature* crassly and cynically bowed to pressure from the *paleoclimatic cabal* and allowed the misleading publications of MBH to stand unchallenged.

A.W. Montford wrote a book that provides a very detailed history, background, and review of the entire hockey stick saga and the ensuing ''climategate'' revelations (Montford, 2010). Montford goes into considerable detail on the specifics of the proxy data and how they were processed, as well as the work by M&M in unraveling what MBH actually did, and the errors and misconceptions in the MBH analysis. He describes the resistance put up by the *paleoclimatic cabal* and the culpability of journals in shielding them from justified criticism. His penetration into the whole grisly mess is far deeper than I have attempted in this book. As the story unfolds, Montford shows that these paleoclimatologists, with their lousy data, and worse methods of processing the lousy data, have hoodwinked the science community and the world at large into believing their results.

In a more recent paper, Mann *et al*. (2008) updated their previous work by including additional proxies of various types. The spatial distribution of these was heavily concentrated in the U.S. and Europe (about 85%) with very few in the rest of the world (about 15%). As is usual in papers authored by Mann and co-workers, the paper is difficult to decipher. Oceans, which cover 70% of the Earth, were claimed to be included in some of the studies but it is not clear how ocean temperatures from a thousand years ago were obtained and averaged over all the oceans—if indeed that is what was done. It is difficult to understand how they incorporated ocean data such as they may be from the terse description given in the paper. Of the 1,209 proxies utilized, 59 extended back 1,000 years and 25 extended back 2,000 years from the present. The mean duration of a proxy was about 270 years. Some of their reconstructions utilized all proxies, and some were restricted to a subset of proxies that passed ''a screening process for a local surface temperature signal. The screening process required a statistically significant correlation with local instrumental surface temperature data during the calibration interval''. The period 1850–1995 was used for calibration.

When a typical set of proxies from various regions is compared, the differences between proxies are huge compared with the similarities (assuming that similarities exist at all). The proxies used by Mann *et al*. (2008) were no exception. Figure 2.16 shows some of the proxies that they utilized. Evidently, the variations from proxy to proxy outweigh any consistent signal that may underlie these time series. Hence, this set of time series represents a data set with low signal-to-noise ratio, and simply adding up these proxies is bound to produce little more than noise. Nevertheless, Mann *et al*. (2008) remained undaunted. They applied a variety of sophisticated statistical methods in an attempt to unravel a signal from the noise. However, as the saying goes, it is difficult to ''convert a sow's ear into a silk purse'', or, in a more modern vernacular, it may be a case of ''garbage in–garbage out''. As Burger and Cubasch (2005) showed, almost any desired result can be obtained from interpreting this very noisy data, depending on how they are processed.

It is noteworthy that the temperature anomalies shown in Figures 2.11 and 2.12 are essentially all negative prior to about 1950, which proves that the mean that they

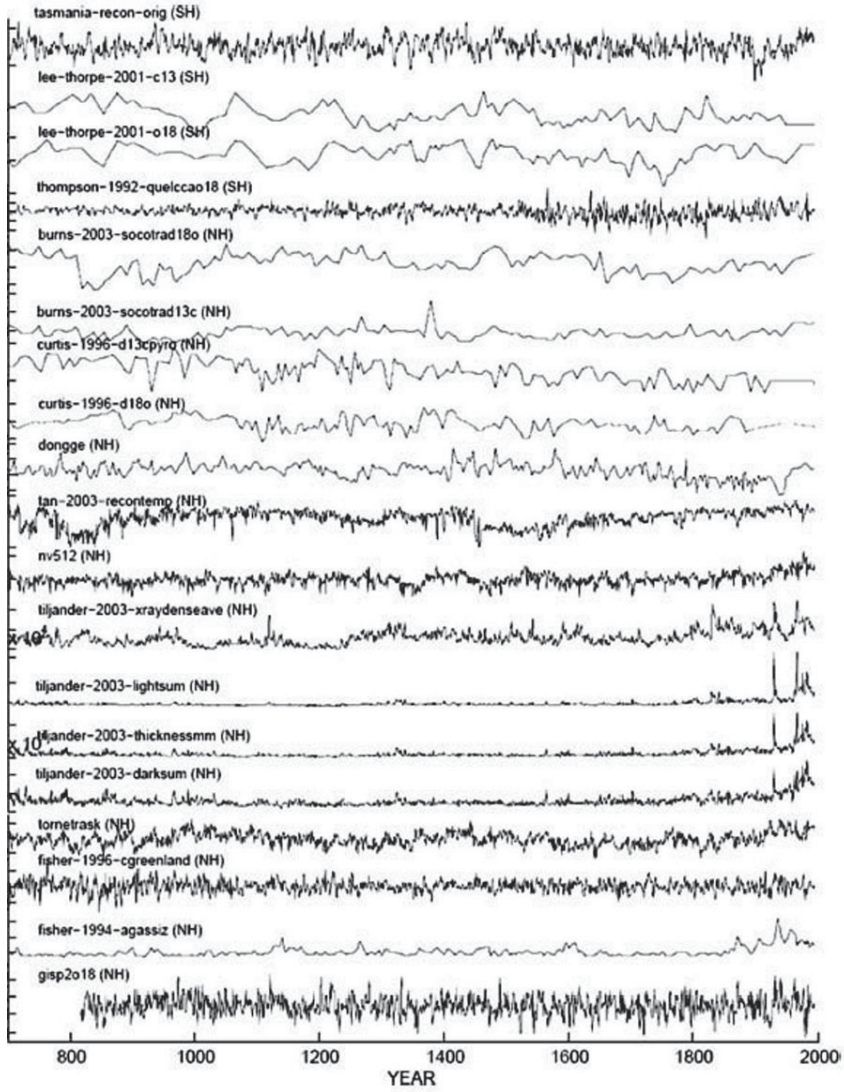

Figure 2.16. Some of the proxies used by Mann *et al*. (2008).

used to calculate anomalies was based on the calibration period, not the full data set (temperatures were higher on average during the calibration period). This casts doubt on the entire statistical procedure. In this connection, no mention was made of the criticisms of the methodology made by M&M, Wegman, and others, nor were any of their concerns addressed. Like the Wizard of Oz when exposed, they seemed to say: ''pay no attention to that man behind the screen''.

Clearly, the results of MBH and related papers are faulty. Juckes *et al*. (2006) attempted to deal with the criticisms of M&M (2003) claiming: ''... the deficiencies

in the description of the data used and possible irregularities in the data itself. These issues have been largely resolved in [Mann *et al*., 2004]''. However, aside from deficiencies in the data, which Mann, Bradley, and Hughes (2004) did not resolve except to obfuscate the matter, the critical issue of using the wrong mean, resulting in mining for *hockey stick* results was not even mentioned. Juckes *et al*. (2006) is just another in a series of papers using flawed statistics in a feeble effort to provide a pseudo-basis for their contention that we are in a state of unprecedented runaway global warming. There is no great point in reproducing their graphs here since they all display *hockey stick*s of one form or another.

It is noteworthy that Richard Muller said:

> ''... carbon dioxide from burning of fossil fuels will prove to be the greatest pollutant of human history. It is likely to have severe and detrimental effects on global climate. I would love to believe that the results of Mann *et al*. are correct, and that the last few years have been the warmest in a millennium.'' (*http://muller.lbl.gov/TRessays/32-Global_Warming_Bombshell.htm*)

Hence, he was far from being a confirmed skeptic. Nevertheless, he went on to roundly criticize the methods of Mann *et al*. and concluded that the hockey stick is actually an ''artifact of poor mathematics'' and said: ''A phony hockey stick is more dangerous than a broken one—if we know it is broken''.

It is also worthwhile to review the response of *Nature* magazine to M&M when they attempted to publish their critique of Mann *et al*. (1998). On January 2004, M&M submitted their critique as a letter to *Nature*. One referee provided a favorable review. The other offered some confusion emphasizing the complexity of the details, but said: ''In general terms I found the criticisms raised by M&M worthy of being taken seriously. They have made an in depth analysis of the MBH reconstructions and they have found several technical errors that are only partially addressed in the reply by Mann *et al*.'' *Nature* issued a ''favorable revise and resubmit'' to which M&M responded in March 2004 with a revised manuscript. *Nature* then asked M&M to reduce the manuscript to 800 words. This was difficult, but was achieved and reduced manuscript was submitted in April 2004. In August 2004, *Nature* declined to publish the article that now (for reasons unexplained) needed to be reduced to 500 words. The main reason given was that the matters involved were ''too technical'' for a science journal. In other words, in a matter concerning the legitimacy and validity of the most widely accepted model of the Earth's climate over the past millennium, *Nature* decided that they would allow the erroneous publication by Mann *et al*. to stand unchallenged because the issues involved were too complicated. The ironic thing was that the *paleoclimatic cabal* could then claim that the criticisms of M&M could not be taken seriously because they were not published in a peer-reviewed journal.

Esper, Cook, and Schweingruber (2002a) started out by repeating the mantra of the global-warming alarmists:

> ''... the MBH reconstruction indicates that the 20th century warming is abrupt and truly exceptional. It shows an almost linear temperature decrease

from the year 1000 to the late 19th century, followed by a dramatic and unprecedented temperature increase to the present time. The magnitude of warmth indicated in the MBH reconstruction for the MWP, 1000–1300 is uniformly less than that for most of the 20th century.''

However, Esper, Cook, and Schweingruber (2002a) (ECS) admitted: ''the MBH reconstruction has been criticized for its lack of a clear MWP.'' It was admitted that critics doubt that tree-ring records can preserve long-term, multi-centennial temperature trends. However, as usual in papers written by members of the *paleoclimatic cabal*, no mention is made of the devastating criticism of the MBH reconstruction made by McIntyre (2007). ECS then went on to present a defense of tree-ring reconstructions using centuries-long ring-width trends in 1,205 radial tree-ring series from 14 high-elevation and middle to high-latitude sites distributed over a large part of the NH extra-tropics. While ECS intended to support Mann, Bradley, and Hughes (1999), the large differences between their results and those of MBH lead this writer to the opposite conclusion. This raises the question whether any reconstruction based on proxies is credible. Furthermore, the anomalies in the result of ECS are mostly negative, suggesting that the mean used for data processing was not the mean for the entire time period, but only for the calibration period.

Nevertheless, based on their result, ECS reached the following conclusions:

(1) Multi-centennial temperature variability in long tree-ring records can be preserved if the appropriate tree-ring data and proper methods of analysis are used.
(2) The MWP appears to be more temporally variable than the warming trend of the last century and may have begun in the early 900s.
(3) The warmest period covers the interval 950–1045, with the peak occurring around 990.
(4) Past comparisons of the MWP with the 20th-century warming back to the year 1000 may not have included all of the MWP and, perhaps, not even its warmest interval.

McIntyre (2007) examined the data in ECS in considerable detail and wrote at length on their analysis. The issues are intricate and detailed and beyond the scope of the present write-up. McIntyre commented on the difficulty in obtaining the original data: ''It's obviously been pulling teeth to get data from Esper. After only two years of trying, I've recently obtained all but one site chronology ... and gobbledy-gook about methodology.'' Using the 13 site chronologies that he had available, McIntyre plotted the individual proxies as shown in Figure 2.17. This is sometimes referred to as a ''spaghetti chart''.

McIntyre pointed out that only 2 of the 13 series have strongly elevated closing values. They both entail foxtail pines (interbreeding cousins of bristlecone pines) both from sites very close to Sheep Mountain, California. He cast considerable doubt on the validity of these two proxy sites. McIntyre then went on to present individual plots for each proxy, and perform a simple average. These results show that the proxies vary widely, and cast doubt on the consistency and credibility of the

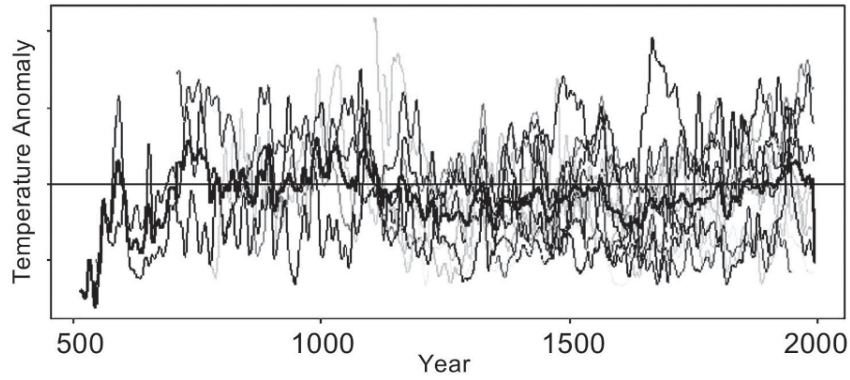

Figure 2.17. "Spaghetti chart" of individual proxies (except Mongolia) (adapted from McIntyre, 2007).

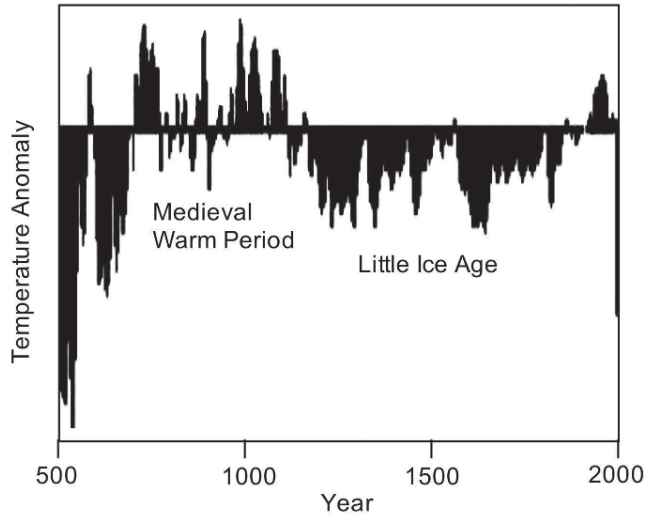

Figure 2.18. Simple average of proxy data from Esper *et al*. (2002) (adapted from McIntyre, 2007).

various proxies. While ECS provides us with assurance that "multi-centennial temperature variability in long tree-ring records can be preserved if the appropriate tree-ring data and proper methods of analysis are used", Figure 2.17 suggests otherwise.

McIntyre (2007) presented a simple average of all the proxies as shown in Figure 2.18. The result suggests an MWP and an LIA. Nevertheless, it seems evident that ECS used statistical data manipulation that unreasonably and illegitimately overemphasized the weighting of the two suspect proxies with high closing values.

Some *paleoclimatic cabalists* have argued that the MBH procedure is supported by other, more recent studies that also lead to a hockey stick. However, amazingly

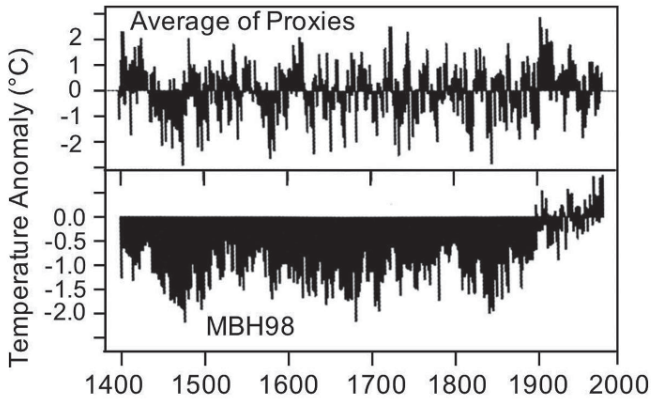

Figure 2.19. Temperature anomalies from MBH. The lower graph is the result of MBH98, while the upper graph is a simple average of their proxies (Montford, 2010).

enough, none of these further studies paid any attention at all to the M&M criticisms of the MBH procedure, and they blithely went ahead and used the mean of only the calibration period, making the same mistake as MBH, over and over again. Proof of this assertion is the fact that, in all of these reconstructions, the temperature anomaly remains starkly negative at all times prior to the calibration period. Montford (2010) supplied Figure 2.19 attributed to M&M. The lower graph is the result of MBH98 while the upper graph is a simple average of their proxies.

Paleoclimatologists who use PCA to extract a "trend" from the very noisy data seem to lose sight of the fact that, in cases where only a few proxies show a trend, PCA will weight these heavily to the exclusion of large numbers of proxies that do not show a trend. While Figure 2.14 is a rather extreme case, it illustrates a point. The object should not be to extract a "trend" but rather to represent the information contained in the entire data set. The average of all proxies in Figure 2.14 will be a line that is horizontal across most of the pre-calibration period and has a very slight rise through the calibration period due to the one proxy that rises. PCA will simply select the one proxy with a trend and ignore the data in all the horizontal proxies. In that respect, PCA is a useless, misleading method. But the reality is that the true trend is not the one PCA picks out, but rather, the average of all the data.

2.4.3.2 Hiding the decline

Tree-ring proxies are important in attempting to discern historical temperatures over the past millennium or two because they sometimes date back 2,000 years or more. Hence, tree-ring proxies are prominent in the MBH and other related reconstructions of global temperatures over one or two millennia. However, as we pointed out previously, tree growth is also affected by other factors (water availability, humidity, wind, cloudiness, CO_2 content in the atmosphere, nutrients, etc.) that add noise to the temperature signal. Hence, it is not a simple matter to extract accurate historical temperature data from tree rings (or other proxies, for that matter). There is ample

evidence that the climatologists who have developed models for global temperatures over the past millennium or two have had a vested interest in proving that rising temperatures in the 20th century are unique and unprecedented, thus suggesting that natural causes cannot account for this change, and it must be attributed to growth of greenhouse gas concentrations. We can speculate on their motives. One might be a true idealistic desire to save the world from what they believe is an impending catastrophe due to global warming. Another might be the crass fact that funding for climate research will be proportional to the degree of catastrophe that is predicted. Whatever their motives, unfortunately, the behavior of tree-ring proxies was not supportive of this belief. Tree-ring proxies showed aberrations at various times, but the most serious problem was that tree-ring data typically show a downward trend in the late 20th century, while measured global temperatures were rising. The goal of the alarmists was to preserve the hockey stick, which they felt was necessary to show that rising greenhouse gases in the 20th century produced continuously rising temperatures. The "solution" to the problem of this divergence was "the trick" of not showing the down-trending proxy data in the late 20th century, and replacing it with measured temperatures that were rising.

> "I've just completed Mike's Nature trick [Michel Mann's publication in Nature where he replaced tree-ring proxy data with actual data because the tree-ring data went in the 'wrong' direction] of adding the real temperatures to each series for the last 20 years (i.e. from 1981 onwards) and from 1961 for Keith's to hide the decline." (Excerpt from email by Phil Jones)

It is particularly revealing to note some results of Briffa *et al.* (2001). Figure 2.20 shows eight different reconstructions using various procedures with one preferred reconstruction. Note that all reconstructions decline in the second half of the 20th century while measured temperatures rise. Briffa *et al.* (2001) also compared their results with reconstructions by others as shown in Figure 2.21. The divergence is readily seen and Jones's "trick" produces the hockey stick.

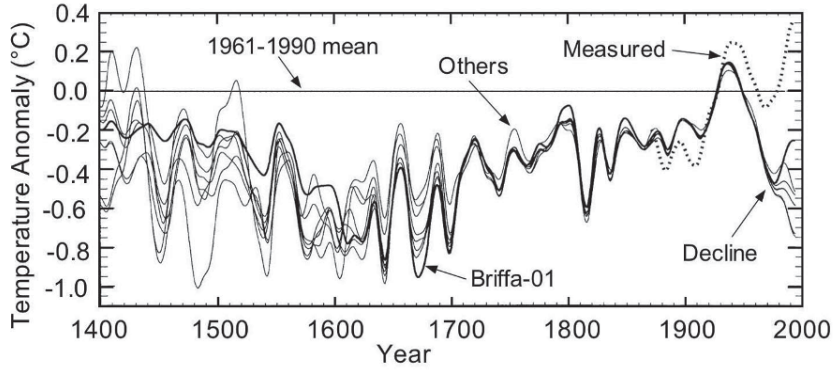

Figure 2.20. Eight reconstructions of historical northern non-tropical summer temperatures using various procedures. The heavy line (Briffa-01) is "preferred" (adapted from Briffa *et al.*, 2001).

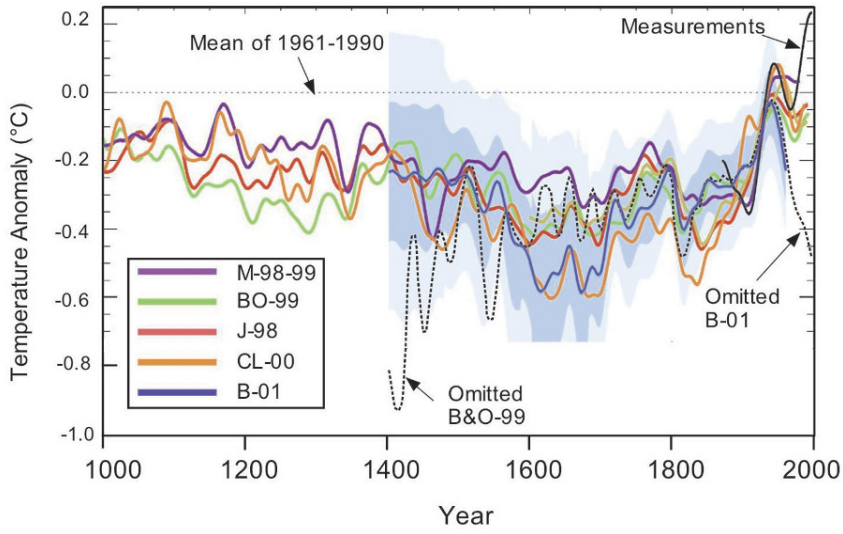

Figure 2.21. Comparison of reconstructions. M-98-99 = Mann *et al.* (1998, 1999). BO-99 = Briffa and Osborne (1999). J-98 = Jones *et al.* (1998). CL-00 = Crowley and Lowery (2000). B-01 = Briffa *et al.* (2001) (adapted from Briffa *et al.*, 2001).

Hegerl *et al.* (2007) also used Jones's "trick" of tacking on the recent instrumental record to the proxy results. Mann, Bradley, and Hughes (1998) and Mann, Bradley, and Hughes (1999) also cleverly substituted the measured temperatures for the modeled temperatures (Jones's "trick") to exaggerate the rise in the 20th century and thus accentuate the *hockey stick*.

The ultimate test for reliability of proxies is how well they track temperatures. Of all the many papers on proxies that I have reviewed, very few if any have provided such data in any detail. Briffa *et al.* (1998) is an exception. They compared tree-ring proxies with temperatures at many sites in the NH from 1880 to 1990. They said:

> "When averaged over large areas of northern America and Eurasia, tree-ring density series display a strong coherence with summer temperature measurements averaged over the same areas, demonstrating the ability of this proxy to portray mean temperature changes over sub-continents and even the whole Northern Hemisphere. During the second half of the twentieth century, the decadal-scale trends in wood density and summer temperatures have increasingly diverged as wood density has progressively fallen. The cause of this increasing insensitivity of wood density to temperature changes is not known . . ."

Although Briffa *et al.* (1998) pointed out the discrepancy between tree-ring data and temperature after 1950, their assessment that proxies tracked temperatures prior to 1950 may be somewhat optimistic.

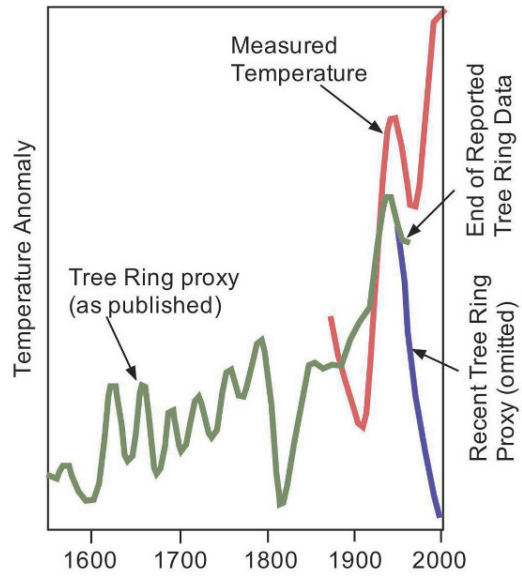

Figure 2.22. "Trick" of replacing tree-ring proxy by measured temperature (McIntyre, *http://climateaudit.org/2011/03/17/hide-the-decline-sciencemag/#more-13285*).

McIntyre[2] analyzed the data from several hockey stick reports (e.g. Briffa and Osborn, 1999). This is known as the "hide the decline" syndrome. Figure 2.22 shows the tree-ring proxy (combination of green and blue curves). However, when Briffa and Osborn were confronted with late-20th-century tree-ring data indicating a sharp drop in temperature, they craftily omitted the more recent data (blue curve) from their paper and ended the tree-ring data with the green curve. They then replaced the more recent tree-ring data by the measured curve (red curve).

Since the publication of these data in 1998, a number of additional papers have appeared dealing in one way or another with tree-ring proxies.

Jacoby *et al.* (2000) said:

> "Data from annual tree-ring widths are used to reconstruct May–September mean temperatures for the past four centuries. These warm-season temperatures correlate with annual temperatures and indicate unusual warming in the 20th century. However, there is a loss of thermal response in ring widths since about 1970."

Thus they admit to a divergence problem after 1970. However, when one examines their data prior to 1970, the correlation of tree-ring data with temperature even prior to 1970 is not impressive to this writer. D'Arrigo *et al.* (2006a) came to the defense of tree-ring proxies. They began their paper with the usual orthodoxy that "recent warming in the Northern Hemisphere appears to have been unprecedented over the past

[2] *http://climateaudit.org/2011/03/21/hide-the-decline-the-other-deletion/*.

millennium and that this warming is most likely a result of the anthropogenic release of greenhouse gases into the atmosphere''—which has not much to do with the reliability of tree-ring proxies. They mention that D'Arrigo *et al*. (2006b) used ''simple averaging of tree-ring records (after accounting for differences in mean and variance over time), followed by linear regression''. Simple averaging is a step in the right direction, but how good are the proxies as temperature indicators? As is usual in almost all papers on proxies, the data for the calibration period were not shown. They do allude to the divergence between tree-ring proxies and temperature reported by Briffa *et al*. (1998) and others cited in their paper, where they said:

> ''Theories for the cause (s) of this observed divergence, which may vary from site to site, include decreased temperature sensitivity due to warmer temperatures, drought stress, increased winter snowmelt and ozone effects. This divergence needs to be considered to avoid bias in dendroclimatic reconstructions; however it is not present everywhere. For example, temperature-sensitive elevational treeline sites in Mongolia and the European Alps exhibit dramatic growth increases in recent decades. Greater attention to site selection (e.g. avoidance of drought-prone sites) and careful comparison of adjacent sites with regards to their ecological characteristics can help circumvent this problem. [It has been] demonstrated that the divergence appears to be limited to the recent period (after $\sim$1950) and to trees from some northern locations (at some sites within $\sim$55-70°N), and that there is no evidence for a comparable divergence prior to this time (e.g. during the *Medieval Warm Period*). These observations suggest a unique, anthropogenic cause for the recent divergence and argue very strongly that tree-ring temperature reconstructions for the past millennium should not be called into question based on these recent observations.''

One problem with site selection is that, if one is attempting to estimate a global average temperature, one needs all the sites one can find. If only a few sites provide reliable data, how can one derive global or even hemispheric temperatures in the past? The claim that ''there is no evidence for a comparable divergence prior to this time (e.g. during the *Medieval Warm Period*)'' is sheer nonsense because there are no measured temperature data for that period and hence there is no way to ascertain whether such a divergence exists. D'Arrigo *et al*. (2006a) closed their paper with further homage to the orthodoxy of ''unusual recent anthropogenic warming on a hemispheric to global scale'' but their defense of tree-ring proxies falls flat.

To hide the divergence between proxies and reality, MBH terminated their calibration phase in 1980 even though more recent data were available. (Unfortunately, the proxies went down while measured temperatures went up after 1980.)

Wilmking and Singh (2008) discussed the ''divergence effect'' between measured temperatures and tree-ring proxies in the second half of the 20th century and pointed out that this ''seriously questions the validity of tree-ring based climate reconstructions, since it seems to violate the assumption of a stable response of trees to changing climate over time''. In their study, they claimed to have

> "... eliminated the 'divergence effect' in northern Alaska by careful selection of individual trees with consistently significant positive relationships with climate (17% of sample) and successfully attempted a divergence-free climate reconstruction using this subset."

However, they did admit:

> "The majority of trees (83%) did not adhere to the uniformitarian principle as usually applied in dendroclimatology. Our results thus support the notion that factors acting on an individual tree basis are the primary causes for the 'divergence effect' (at least in northern Alaska)."

Unfortunately, even the small subset of 17% of trees that are claimed to show good consistency with temperatures over the last century provide somewhat doubtful consistency. The diagram provided by Wilmking and Singh (2008) in their Figure 2 is a tiny little diagram that compresses the excursions between the temperature and tree-ring curves. Nevertheless, accepting the claim that 17% of the trees show good correlation with temperature for the sake of argument, the question arises as to whether it makes sense to select a subset of trees that happen to fit the temperature curve, and use these for estimating temperatures 1,000 years or more ago. Apparently, Wilmking and Singh suggest that there occurs a "mixture of trees with stable and non-stable climate growth relationships" and the ones with stable relationships provide a basis for estimating past climates. However, it may be equally likely that all the tree-ring records are randomized by other variables than temperature, and, by happenstance, about 17% of the records happen to have correlation coefficients with temperature that satisfy the criterion adopted by Wilmking and Singh (which is not impressive to this writer). There is then no great reason to believe that even these 17% of trees would remain as accurate temperature indicators over much longer periods. If you have a theory and desire to test it against data, you can cherry pick a small fraction of the data that seems to agree with the theory, but that is not science.

2.4.3.3 Sparse data set

Aside from all the other problems in reconstruction of millennial temperatures, the data set from which the analyses were conducted was very sparse. When MBH09 extended the time scale of MBH98 back from year 1400 to 1000, they depended on just 13 proxy series. Four were ice cores from a single small ice cap in Peru, and three were derived from southwestern U.S. tree rings. How could one possibly claim to have estimated global temperatures from such a sparse data set?

2.4.3.4 Lack of uniqueness

Burger and Cubasch (2005) is a difficult paper to read. It is full of jargon and uses a number of acronyms that are not defined. It is likely that only a reader who is intimately connected to statistical processing of long-term historical climate data could follow this paper in detail. Nevertheless, it appears to be an important paper

and must be considered here. Burger and Cubasch (2005) examined the mathematical procedure used by MBH for the NH temperature reconstruction and noted that there were six key junctures where a fork in the road occurred, and MBH had to choose one or the other pathway for the ensuing computations. Since any one choice of path at one juncture could be combined with any other choice of path at another juncture, and there are six junctures, there must be a total of $2^6 = 64$ possible pathways to carry out the entire calculation.

We will not describe all of the junctures and choices here, but it is important to mention that one of the junctures was the choice of alternatives: (1) the MBH calculation of temperature anomalies based on the mean over the calibration period vs. (2) calculation of the mean for the entire time span of the data. As M&M have shown, use of only the calibration period mines for *hockey stick* results.

The various pathways can be described by means of six-digit binary numbers. The MBH method is described by one of these 64 binary numbers. Burger and Cubasch (2005) said: ''No *a priori*, purely theoretical argument allows us to select one out of the 64 as being the 'true' reconstruction.'' Burger and Cubasch (2005) also argued that the alternate paths at each juncture are ''a priori sound, with numerous applications elsewhere, and can hardly be dismissed purely on theoretical grounds''. However, in regard to the juncture where one chooses the mean as a basis for calculating anomalies, it appears from the results of M&M that use of the mean for only the 20th century is fundamentally wrong *a priori*, and therefore the assertion by Burger and Cubasch (2005) is incorrect in this specific instance. The choice of the time period for the mean is not one of reasonable alternatives, but rather a choice of right vs. wrong.

The results of Burger and Cubasch (2005) are shown in Figure 2.23. Unfortunately, there is no key given to identify which of the 64 pathways correspond

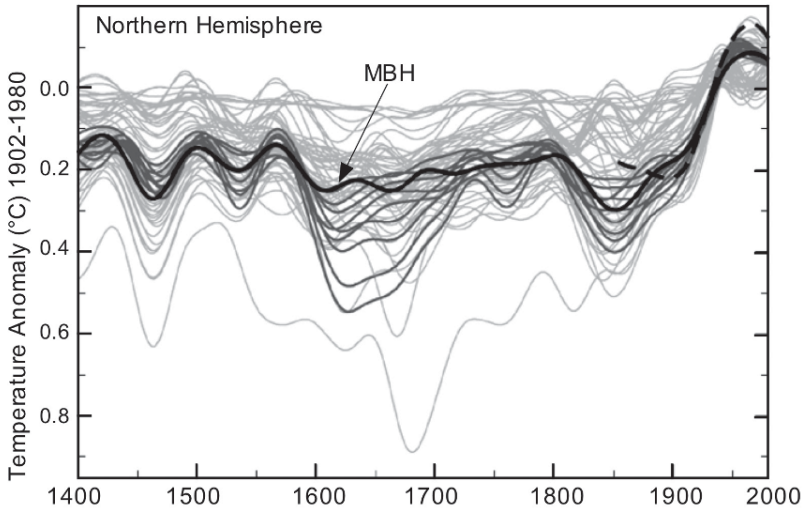

Figure 2.23. Temperature reconstructions (adapted from Burger and Cubasch, 2005).

to the various temperature reconstructions. A worrisome feature is that the zero line appears to be the mean of the 20th-century data and most anomalies are negative. This leads one to wonder whether they actually did calculations with the mean for the whole time period, and, if they did, why are the anomalies (for non-MBH options) not centered vertically on the zero line? Burger and Cubasch (2005) concluded:

> "Any robust, regression-based method of deriving past climatic variations from proxies is therefore inherently trapped by variations seen at the training stage, that is, in the instrumental period. The more one leaves that scale and the farther the estimated regression laws are extrapolated the less robust the method is. The described error growth is particularly critical for parameter-intensive, multi-proxy climate field reconstructions of the MBH type. Here, for example, co-linearity and over-fitting induce considerable error already in the estimation phase."

It appears that almost any result can follow from an integration of proxies, and no proxy result can be trusted.

2.4.3.5 Other criticisms

Zorita and von Storch (2005) examined methods used to construct historical temperatures. Unfortunately, it was difficult for this writer to follow all the arguments and jargon in the paper. However, it is apparent that Zorita and von Storch (2005) found significant problems with the methods used by MBH to infer long-term historical temperature records. Zorita and von Storch (2005) started with a model that the authors developed for a 1,000-year record of global temperatures called ECHO-G. The methods used by MBH were tested by following the MBH procedure, in which all data were considered to be deviations from the 1900–1980 mean value, even though this period is not representative for the temperature history of the past millennium. They followed the procedure of MBH but utilized different levels of noise as inputs. Inevitably, the methods of MBH led to a so-called *hockey stick* type of figure and grossly underestimated the variability of temperature in the past. Mann *et al.* (2007) argued against the results of Zorita and von Storch (2005), and Zorita *et al.* (2007) rebutted this criticism.

Esper *et al.* (2005a, b) concluded:

> "Our understanding of the shape of long-term fluctuations is better than commonly perceived, but the absolute amplitude of temperature variations is poorly understood ... Overall, amplitude discrepancies are of the order of the total variability estimated over the past millennium ..."

Even this conclusion seems optimistic.

Typically, when a set of proxies from various regions is compared, the differences between proxies are huge compared with the similarities (assuming that similarities exist at all). The proxies used by Mann *et al.* (2008) were no exception. Figure 2.16 shows some of the proxies that they utilized. Evidently, the variations

from proxy to proxy outweigh any consistent signal that may underlie these time series. Hence, this set of time series represents a data set with low signal-to-noise ratio, and simply adding up these proxies is bound to produce little more than noise. Nevertheless, Mann *et al*. (2008) remained undaunted. They applied a variety of sophisticated statistical methods in an attempt to unravel a signal from the noise. As shown in Figure 2.23, almost any desired result can be obtained from interpreting this very noisy data, depending on how it is processed. Figure 2.21 shows results compiled by Mann *et al*. (2008) from their calculations, along with other estimates of historical temperatures. Several aspects of this figure are worth noting:

(1) There is considerable divergence between various estimates.
(2) None of the estimates shows a strong MWP. This may be due to a scarcity of data dating back 1,100 years.
(3) Most of the estimates show a rather weak LIA, probably due to averaging too many proxies.
(4) The steep upward curve at the far right, stated to be the measured temperature, is upwardly exaggerated.

In some of their results, Mann *et al*. (2008) provided estimates of the uncertainties in their results as a standard deviation envelope around the linear curve of temperature vs. time. In their Figure S11, they indicate that when alternate calibration periods are included, the total standard deviation of a temperature estimate for any year is typically about $\pm 0.6^{\circ}$C, which suggests that any variations smaller than 0.6°C are uncertain by a significant amount. Since the uncertainties in calculations other than those of Mann *et al*. (2008) are likely to be at least as great, it must be concluded that, when an uncertainty of at least $\pm 0.6^{\circ}$C is imposed on the spread of estimates from various models, the resultant envelope would be large enough to hide almost any historical temperature profile that one desires.

It is noteworthy that the temperature anomalies shown in Figure 2.21 are essentially all negative prior to about 1950, which proves that the mean that they used to calculate anomalies was based on the calibration period, not the full data set (temperatures were higher on average during the calibration period). This casts doubt on the entire statistical procedure. In this connection, no mention was made of the criticisms of the methodology made by M&M, Wegman and others, nor were any of their concerns addressed.

There seems to be an inverse relationship working here: the more proxies that are included, the lower is the signal-to-noise ratio in the product. As Mann and associates relentlessly add new proxies, the quality of the result diminishes.

2.4.3.6 The Wegman Report

A team led by Professor Edward J. Wegman performed an independent examination of the *hockey stick* controversy. They produced a lengthy report, full of details. According to Wegman, Scott, and Said (2006):

> ‘‘The controversy of Mann’s methods lies in that the proxies are centered on the mean of the period 1902–1995, rather than on the whole time period ...

Principal component methods are normally structured so that each of the proxy data series are centered on their respective means and appropriately scaled. The first principal component attempts to discover the composite series that explains the maximum amount of variance. The second principal component is another composite series that is uncorrelated with the first and that seeks to explain as much of the remaining variance as possible. The third, fourth, and so on follow in a similar way. In the MBH approach the authors make a simple seemingly innocuous and somewhat obscure calibration assumption. Because the instrumental temperature records are only available for a limited window, they use instrumental temperature data from 1902–1995 to calibrate the proxy data set. This would seem reasonable except for the fact that temperatures were rising during this period, so that centering on this period has the effect of making the mean value for any proxy series exhibiting the same increasing trend to be de-centered low. Because the proxy series exhibiting the rising trend are de-centered, their calculated variance will be larger than their normal variance when calculated based on centered data, and hence they will tend to be selected preferentially as the first principal component. Thus, in effect, any proxy series that exhibits a rising trend in the calibration period will be preferentially added to the first principal component."

Wegman, Scott, and Said (2006) went on to say:

"The centering of the proxy series is a critical factor in using principal components methodology properly. It is not clear that the MBH Team even realized that their methodology was faulty at the time of writing the MBH paper. The net effect of the de-centering is to preferentially choose the so-called *hockey stick* shapes. While this error would have been less critical had the paper been overlooked like many academic papers, the fact that their paper fit some policy agendas has greatly enhanced their paper's visibility. Specifically, global warming and its potentially negative consequences have been central concerns of both governments and individuals. The *hockey stick* reconstruction of temperature graphic dramatically illustrated the global-warming issue and was adopted by the IPCC and many governments as the poster graphic. The graphic's prominence together with the fact that it is based on incorrect use of PCA puts Dr. Mann and his co-authors in a difficult face-saving position. We have been to Michael Mann's University of Virginia website and downloaded the materials there. Unfortunately, we did not find adequate material to reproduce the MBH materials."

Wegman, Scott, and Said (2006) performed a calculation similar to that of M&M by comparing the results of an analysis of the North American tree network PC1 using the MBH data with centering based either on the calibration period mean or the mean for the whole time span of the data set. The result is shown in Figure 2.24. In addition to the *hockey stick* shape of the upper panel, it is worth noting that the lower panel exhibits considerably more variability. PCA seeks to identify the largest contributor to the variance. The MBH offset of the mean value shifts the main variance from the bulk of the data set to the 20th-century data.

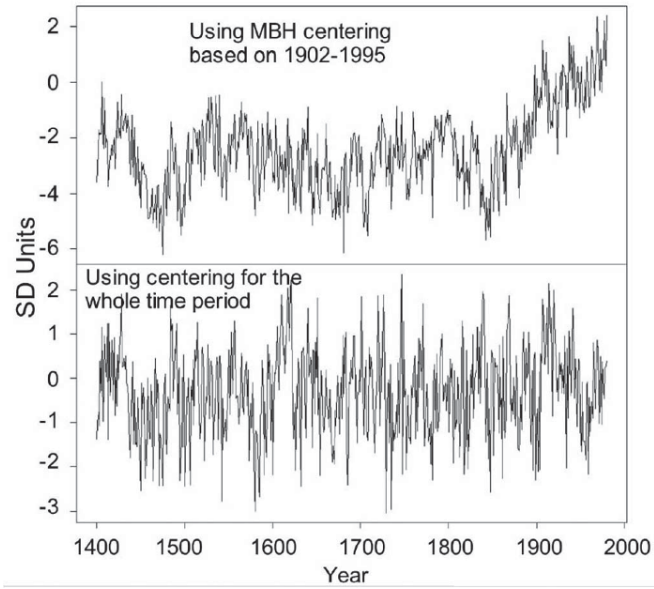

Figure 2.24. Comparison of rework of the North American tree network PC1 using MBH centering vs. using centering across the whole time span of the data set. The *hockey stick* is shown to be an artifice (adapted from Wegman *et al*., 2006).

The findings of Wegman, Scott, and Said (2006) are quite lengthy and only a very brief summary is given here:

(1) In general, they found the writing of MBH somewhat obscure and incomplete. (This writer found the same.)
(2) In general, they found the criticisms by M&M to be valid and their arguments to be compelling.
(3) Use of the temperature profile in the 1902–1995 time span for centering leads to misuse of the PCA. However, the narrative in MBH on the surface sounds entirely reasonable, and could easily be missed by someone who is not extensively trained in statistical methodology.
(4) The cryptic nature of some of the MBH narratives requires that outsiders would have to make guesses at the precise nature of the procedures being used.
(5) Much of the discussion on the *hockey stick* issue has taken place on competing web blogs. Web blogs are not an appropriate way to conduct science and thus the blogs give credence to the fact that these global-warming issues have migrated from the realm of rational scientific discourse. Unfortunately, the factions involved have become highly and passionately polarized.
(6) Generally speaking, the paleoclimatology community has not recognized the validity of the M&M papers and has tended to dismiss their results as being developed by biased amateurs. The paleoclimatology community seems to be tightly coupled, and has rallied around the MBH position.

(7) The widely quoted assessments that the decade of the 1990s was the hottest decade in a millennium and that 1998 was the hottest year in a millennium cannot be supported by a proper rendition of the MBH analysis ... The paucity of data in the more remote past makes the hottest-in-a-millennium claims essentially unverifiable.
(8) Use of bristlecone pine proxies are inappropriate because they were probably CO_2-fertilized. It is not surprising therefore that this important proxy in MBH yields a temperature curve that is highly correlated with atmospheric CO_2. There are clearly confounding factors for using tree rings as temperature signals.
(9) There are other detailed statistical problems with the MBH treatment that require specialized knowledge to understand.

McIntyre revisited the Wegman Report in May 2011. He pointed out that the combination of criticisms of the MBH hockey stick by the *climateaudit.org* website and the Wegman Report generated a great deal of controversy on the blogs. The amazing thing is that, as McIntyre put it:

> "Rather than conceding even seemingly indisputable points, Mann and his associates contested every single issue—even the seemingly indisputable and elementary observation that Mann's principal components mined datasets for hockey stick shaped data. To this date [May 23, 2011], neither Mann nor any of his associates has conceded the point."

McIntyre also discussed the question: "... given the defects of Mann's principal components, how did the methodology pass peer review and then remain unchallenged by specialists in the field?" He stated further:

> "The Wegman Report hypothesized that this failure was due to the inter-connectedness of climate scientists through co-authorship and, in particular, by the extent of Mann's network of co-authorship, a level of inter-connectedness that the Wegman Report seemed to think as not existing in their own field. Wegman speculated that members of Mann's closest circle ('clique' in network terminology) reviewed papers of other members of the clique, resulting in non-independent and weak peer review, which, in turn, had resulted in the failure to identify the incorrectness of Mann's principal components in both the original article and subsequently."

It is noteworthy that not only did the *paleoclimatic cabal* ignore the criticisms of its hockey stick and blithely continue to promulgate their incorrect results, but they also went on the attack to attempt to besmirch the criticizers on a personal level. The mechanism for doing this was to accuse Wegman (and myself) of plagiarism in our writings. This seems to have originated on the *deepclimate.org* website where the scurrilous person hiding behind a cloak of anonymity known as "DC" apparently stayed up nights comparing word for word the criticisms of MBH with references to identify passages that were copied or slightly modified without attribution. This was further propagated by a report written by John Mashey. DC and Mashey succeeded in their goal, which was to divert attention from the technical errors in the hockey

stick, and focus attention on the issue of trumped-up claims of plagiarism. There have been thousands of nonsensical blog entries about plagiarism in connection with the Wegman Report, whereas the perpetration of false science by the *paleoclimatic cabal* has mainly been ignored.

Bouville (2008) wrote a treatise on plagiarism. He said:

> "... even though ... copying other people's intellectual contribution is wrong, they do not apply to the copying of words. Copying a few sentences that contain no original idea (e.g. in the introduction) is of marginal importance compared to stealing the ideas of others. The two must be clearly distinguished, and the 'plagiarism' label should not be used for deeds that are very different in nature and importance."

The point is that plagiarism is only a serious malpractice when an intellectual concept is stolen for personal gain. When background material is presented without attribution, that is an inadvertency or an indiscretion, but not a crime. The thrust of the Wegman Report was twofold: (1) the hockey stick was based on bad science, and (2) collusion between members of the *paleoclimatic cabal* allowed the hockey stick to get repeatedly published despite the errors in the methods used. There was no plagiarism in these elements of the Report. Unfortunately, some of the introductory and background material was not given proper attribution. In the spirit of Bouville's paper, big deal.

It is interesting that both DC and Mashey accused me of plagiarism as well in this book. The second edition of my book: *Assessing Climate Change* contains 1,348 specific citations to references giving credit to authors for their work. It also includes 411 specific quotations of authors with their own words included in quote signs in my book. It is possible that, in a few places, I may have slipped up and used words from a paper and forgotten to give attribution. That was not, is not, and cannot be plagiarism. It was simply the very small fraction of inadvertent cases where I failed to give credit. Anyone with any sense can immediately see that, with 1,348 citations of references and 411 direct quotes, I could hardly have "intentionally, knowingly and recklessly" failed to reference the authors in a few cases. What did I have to gain? Why would I reference 1,348 but not the other 10 or 20? Ridiculous!

One of the henchmen involved in the attempt cast aspersions on me was a person named Samuel Cohen. Unaccountably, he sent an email to me out of the blue in October 2011:

> Dear Dr. Rapp, I am unclear about your affiliation with the University of Southern California. It is listed on your online CV, but no trace can be found on the USC website outside of an old seminar you gave at the behest of that demented crank George Olah. Can you clarify you status as a Research Professor there?
> Sincerely, Sam Cohen
> (By the way, Olah is a Nobel Laureate.)

The real meaning of this was that Mr. Cohen was under the impression that I was on the faculty at USC and he desired to poison that appointment by accusing me

of plagiarism. But he was (and remains) confused on my relationship to USC. As it turns out, I did have a one-year interim appointment in 2010 but that had lapsed by the time Mr. Cohen went on the attack. After I left USC, Mr. Cohen sent a scurrilous complaint to USC accusing me of plagiarism but it had no effect; I was already long gone. As he said in an email:

> "I assume you have seen the detailed complaint made to USC?"

I heard from Mr. Cohen again on 2/22/2012. He said:

> Dear Dr. Rapp,
> How's your good buddy Edward Wegman doing lately? Heard anything about the bullshit plagiarism charges that were brought against him? I guess we will now see how the NIH and Department of Defense handle sanctions. Could be fun to watch. They certainly will act faster than GMU. Talk to Ed lately? Sam

I tried to explain to him the difference between academic sloppiness in not making an attribution on well-known and widely accepted introductory material vs. stealing someone's intellectual innovation for personal gain. I quoted Bouville (2008), who wrote a treatise on plagiarism. The point is that plagiarism is only a serious malpractice when an intellectual concept is stolen for personal gain. When background material is presented without attribution, that is an inadvertency or an indiscretion, but not a crime. The thrust of the Wegman Report was twofold: (1) the hockey stick was based on bad science, and (2) collusion between members of the *paleoclimatic cabal* allowed the hockey stick to get repeatedly published despite the errors in the methods used. There was no plagiarism in these elements of the Report.

To Mr. Cohen, there is no distinction between failing to give attribution on some quotes vs. stealing ideas for personal gain. In fact, if you used one word from another publication, that would be plagiarism in his view. I asked Mr. Cohen what he ever accomplished in his life and he said:

> "Besides turning Wegman in to the NIH for plagiarism for the CSDA article, a few other things such as becoming a full professor at a top-tier research university, that sort of stuff."

He seems proud to proclaim that he "turned in" Ed Wegman. Evidently, he tried to bestow the same favor on me. When I asked him if he had no shame (à la McCarthy trials), he responded:

> "I feel no shame whatsoever. I did what I consider right, and my actions are supported by the vast majority of the academic community. My actions have been lauded by many, many people. The McCarthy allusion is false."

The orthodoxy seems to have enlisted Mr. Cohen as their hatchet man, using accusations of plagiarism as their weapon. And, while these charges prevail, the real message that Wegman was technically correct that the hockey stick is bogus gets lost. Meanwhile, Cohen admitted:

"I don't know Wegman and I have no real understanding of his work."

Cohen claims to be " an expert in academic and research misconduct". Despite his admitted lack of knowledge, Mr. Cohen went on to proclaim:

"The so-called hockey stick has been validated many times, most recently by the group at UC Berkeley (*http://berkeleyearth.org/*). They were experts, they were skeptical, they found that the paleoclimate data supported global warming, they were honest in both their approach and their conclusions (as so many denier are not), so I trust they did the work correctly and I trust their conclusions."

The Berkeley group never dealt with long-term climate change. They never dealt with the hockey stick. They only dealt with measured temperatures over the past couple of hundred years. One-third of their sites reported cooling, not warming. The *climateaudit.org* site has clearly demonstrated that the hockey stick is *phonus balonus*. Mr. Cohen also said:

"The hockey stick model is a fact. No one has ever demonstrated that it is an inaccurate representation of global temperatures, as far as I know. If you have citations to the primary literature demonstrating this, please send them to me."

Now it matters little what Sam Cohen thinks about the hockey stick but this exchange of emails provides some insight into the sinister thinking and actions of the *deepclimate.org* mentality. These people are stupid and dangerous. What is most sinister about Sammy is that he seems to have taken delight in disparaging professional scientists who committed minor indiscretions and wringing his hands in joy, relishing his attempts to destroy their reputations and their careers. As he said: "It could be fun to watch" (the downfall of Ed Wegman). This man is a cruel, vicious, nasty hangman. The *deepclimate.org* crowd has enlisted him to damage the reputations of those who disagree with them, while never refuting the arguments of those who disagree with them. Sammy is certain the hockey stick is correct yet he admits he does not know squat about it. What makes Sammy run?

We have previously reviewed the response of *Nature* magazine to M&M when they attempted to publish their critique of Mann *et al*. (1998). As we said, "in a matter concerning the legitimacy and validity of the most widely accepted model of the Earth's climate over the past millennium, *Nature* decided that they would allow the erroneous publication by Mann *et al*. to stand unchallenged because the issues involved 'were too complicated' ". There is at least the appearance (and more likely the reality) that the *paleoclimatic cabal* was in complete control of the situation.

2.4.3.7 The paleoclimatic cabal

There is strong evidence that the climatologists who earn their living by reconstructing paleoclimates over the past few millennia are in frequent communication with one another and are mutually supportive of their various efforts. There is nothing fundamentally wrong with a collegial relationship between scientists in a field and this can, in principle, be a very positive thing. However, in the case of

paleoclimatology, the relationship fostered very belligerent, malicious, insidious, unprofessional behavior by the principals. For several years, the various paleoclimatologists published their reconstructions and acted as reviewers for one another's manuscripts. All was well. The hockey stick was widely accepted and became one of the pivotal supporting foundations of global-warming alarmism. Then, in around 2005, the *climateaudit.org* website began reviewing these studies in great detail and found that they were all flawed due to (1) use of a mean for only the calibration period, (2) hiding the decline and using Jones's "trick" of substituting measured temperatures for proxies, as well as (3) various other problems discussed on *climateaudit.org*. This threatened to undermine years of work upon which the paleoclimatologists' reputations were based. Instead of admitting their errors and fixing them, they dug in and became defensive (and indeed paranoid) at first, and then went on the offense against their critics. The most defensive of them all was Michael Mann. Even a *cabalist* in an email cautioned: "but he would probably go ballistic" in regard to any criticism of his work.

Ball (2007) was critical of how Phil Jones (Head, Climate Research Group, East Anglia University) came up with his estimate of uncertainty in his temperature reconstructions and wrote to Jones asking for an explanation. Ball (2007) claimed that Jones replied in an email:

> "We have 25 or so years invested in the work. Why should I make the data available to you, when your aim is to try and find something wrong with it?"

When a world-leading climatologist is more concerned with protecting his *turf* than finding *truth*, things have taken a very bad turn. Jones also said in an email to Michael E. Mann, professor of climatology, Penn State University:

> "And don't leave stuff lying around on anonymous download sites—you never know who is trawling them. McIntyre and McKitrick have been after the *Climatic Research Unit* ... data for years. If they ever hear there is a *Freedom of Information Act* now in the United Kingdom, I think I'll delete the file rather than send it to anyone."

These scientists would rather destroy data than allow others to check up on them. In 2009, 2010, and 2011, extensive sets of emails between principal figures in the *paleoclimatological cabal* were made public (by unknown, but clearly illegal means). These emails revealed a deeply imbedded agreement amongst these climatologists to promulgate their orthodoxy that the Earth's climate has hardly wavered over the past 2,000 years, and that CO_2 was the principal cause of unprecedented global warming in the 20th century. The collection of emails is now referred to as "*climategate*". As Mosher and Fuller (2010) pointed out, they:

> "... ruthlessly suppressed dissent by insuring that contrary papers were never published and that editors who didn't follow their party line were forced out of their position. When *Freedom of Information* requests threatened to reveal their misbehavior, the emails showed them actively conspiring to delete emails to frustrate legitimate requests for information. Worst of all, one

> scientist threatened to delete climate data rather than turn it over, and that data is still missing.''

The defensive posture of the *cabal* seems to have been to disclose nothing, prevent others from delving into their work, totally ignore criticisms, and continue publishing bad science, acting as reviewers for one another's manuscripts. Amazing at it seems, even to this day, I am not aware of any principal paleoclimatologist responding to or even admitting that a criticism was made of their use of the mean for only the calibration period. McIntyre revisited the Wegman Report in May 2011. He pointed out that the combination of criticisms of the MBH hockey stick by the *climateaudit.org* website and the Wegman Report generated a great deal of controversy on the blogs. The incredible thing is that, as McIntyre put it:

> ''Rather than conceding even seemingly indisputable points, Mann and his associates contested every single issue—even the seemingly indisputable and elementary observation that Mann's principal components mined datasets for hockey stick shaped data. To this date [May 23, 2011], neither Mann nor any of his associates has conceded the point.''

Dr. Phil Jones, head of the Hadley CRU in 2009, said that the U.S. Department of Energy was funding his data collection—and that officials there agreed that he should not have to release the data. In a 2009 email, he said:

> ''Work on the land station data has been funded by the U.S. Dept of Energy, and I have their agreement that the data needn't be passed on. I got this [agreement] in 2007.''

Two months later, Jones said that the information ''has to be well hidden. I've discussed this with the main funder (U.S. Dept of Energy) in the past and they are happy about not releasing the original station data''. Evidently, the U.S. Department of Energy is in cahoots with the *cabal* to evade the requirements of the FOIA as well as the basic tenets of science. It should be emphasized that this is not a case of intellectual property produced by some brilliant new concept. It merely represents collecting climate data and storing them in columns. If the U.S. Department of Energy paid for it, it should be in the public domain.

The *cabal* also went on the offense. Since the principal figures in the *paleoclimatological cabal* were widely published, they tended to be chosen as reviewers for new manuscripts submitted to journals. They were able to act in collusion to prevent contrary papers from being accepted for publication and put pressure on editors who did not cooperate. The *cabal* refereed one another's papers submitted to journals, communicated improperly in a mutual back-scratch environment subverting the peer-review process, pressured journal editors not to publish papers contrary to the orthodoxy, conspired to write rebuttals to any papers that did slip through their barrier to publication of contrary views, and conspired to act in partnership to disparage and ridicule anyone with contrary findings. A particularly egregious episode in the shenanigans of the *cabal* is documented in great

detail at the Bishop Hill website.[3] The challenge to the hockey stick by McIntyre needed to be rebutted in time for inclusion in the 2007 IPCC Report. Two Mann associates, Caspar Amman and Eugene Wahl, were chosen to do this. However, they missed the IPCC deadline and their papers were originally rejected by journals (for good reason—they are misguided). However, the *cabal* managed to circumvent the IPCC deadline and manipulate the journals to their advantage.

In response to the many charges of malfeasance by the *cabal* members that appeared on the blogs, several reviews of the *climategate* activities were carried out by vested interests (e.g. the so-called "Russell Report", *www.cce-review.org/*). These generally provided a whitewash that was only to be expected, considering who were on the review boards.

Kevin Trenberth, Senior Scientist as the National Center for Atmospheric Research has emerged as a defender of the *cabal*.[4]

He did admit to "lack of openness in sharing data and violations of the Freedom of Information Act" but he pointed out that five investigations failed to find any of the alleged misconduct. Unfortunately, these five investigations were conducted by friends of the *cabal*. He also asserted that "scientists would not make up stuff that could be disproven by others!" but the nature of paleoclimatic data is that they are not susceptible to proof, disproof, verification, or validation, and hence are a very safe field to work in. He cited an excerpt from a Phil Jones email:

> "I can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow—even if we have to redefine what the peer-review literature is!"

and implied that this was Jones's invention and he (Trenberth) had nothing to do with this. Whether this is true or not, this excerpt reveals the intellectual environment of the climate *cabal*. However, I have to agree with one slide in Trenberth's presentation that says the Internet is "An open sewer of untreated, unfiltered information and the American public is incapable of deciphering between facts, fiction and opinion".

In late 2011, additional emails between *cabalists* were hacked. Some of these exposed some chinks in the armor. Self-doubt began to creep in. Tim Osborne was quoted as saying: "Also, we set all post-1960 values to missing in the MXD data set (due to decline), and the method will infill these, estimating them from the real temperatures—another way of 'correcting' for the decline, though may be not defensible!" Richard Alley (who is not really a *cabalist*, but a fellow traveler in alarmism) was quoted as saying:

> "Unless the 'divergence problem' can be confidently ascribed to some cause that was not active a millennium ago, then the comparison between tree rings from a millennium ago and instrumental records from the last decades does not seem to be justified, and the confidence level in the anomalous nature of the

[3] *http://bishophill.squarespace.com/blog/2008/8/11/caspar-and-the-jesus-paper.html*.
[4] *www.cgd.ucar.edu/cas/Trenberth/Presentations/ClimategateS.pdf*.

> recent warmth is lowered. I think the best way to sum up all of this is: Where does all this lead us? It is very likely that the NH mean temperature has shown much larger past variability than caught by previous reconstructions. We cannot from these reconstructions conclude that the previous 50-year period has been unique in the context of the last 500-1000 years. *Of course we all know that the IPCC reports differently*.'' (emphasis added)

Another hacked email was reported to have said:

> ''I am afraid the Mike [Mann] and Phil [Jones] are too personally invested in things now (i.e. the 2003 GRL paper that is probably the worst paper Phil has ever been involved in—Bradley hates it as well), but I am willing to offer to include them if they can contribute without just defending their past work—this is the key to having anyone involved. Be honest. Lay it all out on the table and don't start by assuming that ANY reconstruction is better than any other.'' (Email refers to Mann and Jones, 2003).

Jonathan Overpeck was quoted as saying:

> ''... what Mike Mann continually fails to understand, and no amount of references will solve, is that there is practically no reliable tropical data for most of the time period, and without knowing the tropical sensitivity, we have no way of knowing how cold (or warm) the globe actually got.... Unsatisfying, perhaps, since people will want to know whether 1200 AD was warmer than today, but if the data doesn't exist, the question can't yet be answered. A good topic for needed future work.''

Tim Osborne was quoted as saying: ''Also we have applied a completely artificial adjustment to the data after 1960, so they look closer to observed temperatures than the tree-ring data actually were.''

It seems to me to be unfortunate that, since publishing that excellent article in 2003, Dr. Soon has published some articles and made a number of public presentations expounding a very extreme skeptical point of view. He has been quoted as saying:

> ''Most of the weather and climate variations we observed are essentially related to the sun and the changing seasons—not by CO_2 radiative forcing and feedback. The climate system is constantly readjusting naturally in a large way—more than we would ever see from CO_2. The CO_2 kick [impact of CO_2 emissions] is extremely small compared to what is happening in a natural way. Within the framework of a proper study of the sun–climate connection, you don't need CO_2 to explain anything.''

I personally think he is completely misguided in this viewpoint. But his views are his right and privilege. It is unfortunate that organizations allied with the *cabal* went on campaign to besmirch Soon's reputation and the Internet is full of accusations and attacks. In fact, if you dial ''willie soon climate change'' into Google, you obtain mainly derogatory claims (apparently planted by Greenpeace) saying, for example,

"Climate skeptic Willie Soon received $1M from oil companies, papers show ...".[5] What these websites don't reveal is that *cabal* members have received many tens of millions of dollars to fund their alarmist research.

2.4.3.8 The climate alarmism cabal

Initially, the *cabal* consisted of paleoclimatologists. However, as time progressed, the *paleoclimatic cabal* was joined by other climatologists not necessarily involved in reconstruction of past climates, who had vested interests in climate alarmism, and viewed attacks on the *paleoclimatic cabal* as destructive to their cause. So the *paleoclimatic cabal* expanded to become the *climate alarmism cabal* dedicated to propagating the orthodoxy of alarmism. A new set of emails within *climategate* appeared in 2011. An exchange of emails between members of the *climate alarmism cabal* was revealed. Members appear to include (amongst others): Tom Wigley, Jonathan Overpeck, Caspar M. Ammann, Raymond Bradley, Keith Briffa, Tom Crowley, Malcolm Hughes, Phil Jones, Tim Osborn, Kevin Trenberth, Ben Santer, Steve Schneider, Malcolm Hughes, Michael E. Mann, Andrew Dessler, and Michael Oppenheimer (see *http://junkscience.com/2011/11/27/climategate-2-0-mann-suggests-harvard-take-action-against-soon-baliunas/*). The goals of the *climate alarmism cabal* seem to be to prevent contrary papers from getting published, to harass editors that pass contrary papers, to immediately combat any contrary papers or influential blog entries with counter papers and blog entries, and, unfortunately, in some cases it appears that attacks of a more personal nature might be considered. They have pompously and arrogantly claimed that their interpretations are *climate science* while work by other climatologists reaching different conclusions is something other than *climate science*. We see evidence of this in many publications and press releases. In particular, in regard to the effect of clouds, Dessler (2011) said: "In recent papers, Lindzen and Choi (2011), and Spencer and Braswell (2011) have argued that ... clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to *climate science* may be required." In other words, he regards "*climate science*" as that which the orthodoxy subscribes to. It is not his interpretation of climate science—it *IS CLIMATE SCIENCE*!

Another bizarre aspect of Dessler's publication was discussed by Pielke, Sr. (*http://pielkeclimatesci.wordpress.com/2011/09/06/comments-on-the-dessler-2011-grl-paper-cloud-variations-and-the-earths-energy-budget/*). He said: "Dessler's paper was received 11 August 2011 and accepted 29 August 2011. This is some type of record ... and indicates that the paper was fast-tracked. This is certainly unusual ...", to say the least. He went on to say:

> "It is not clear whether the Editor of *GRL* included Roy Spencer as one of the referees, [and if they did not] they were derelict in their responsibilities. Dessler's paper should have been submitted to *Remote Sensing* as a Comment [on Spencer's paper]. Then Roy Spencer would submit a Reply."

[5] *E.g., http://www.examiner.com/seminole-county-environmental-news-in-orlando/harvard-astro-physicist-dismisses-agw-theory-challenges-peers-to-take-back-climate-science#ixzz1fDDWbbz1.*

We are now witnessing a phenomenon in climatology publications that is occurring repeatedly. The climatology orthodoxy seems to have united into an informal association dedicated to (1) prevent contrary analyses and interpretations from being published, and (2) to quickly respond to those few contrarian publications that slip through their net with vitriolic attacks on the paper on orthodoxy blogs, and in the literature via rapid rebuttal publications such as that of Dessler (2011). It seems evident that many editors are in cahoots with the orthodoxy; certainly the editor of *GRL* is, and the editor of *Remote Sensing* who let Spencer and Bradwell's paper through the net, suddenly resigned for unclear reasons.

One topic that gets *cabal* members upset is the claim by some contrarians that persistent El Niños since 1976 were dominant in causing warming in the NH in the latter part of the 20th century. If this were true, it would suggest that the role of CO_2 in climate change may be far less than the orthodoxy believes. Thus, when the article by McLean *et al*. (2009) appeared in the literature suggesting an important role for El Niños as a dominant cause of warming in the NH in the latter part of the 20th century, it produced great animosity and consternation amongst the members of the *climate alarmism cabal*. McLean *et al*. (2009) concluded that the El Niño index

> "... is a dominant and consistent influence on mean global temperature. Shifts in temperature are consistent with shifts in the [El Niño index] that occur about 7 months earlier. The relationship weakens or breaks down at times of volcanic eruption in the tropics ... Since the mid-1990s, little volcanic activity has been observed in the tropics and global average temperatures have risen and fallen in close accord with the [El Niño index] of 7 months earlier. Finally, this study has shown that natural climate forcing associated with ENSO is a major contributor to variability and perhaps recent trends in global temperature, a relationship that is not included in current global climate models."

According to their estimates, changes in the El Niño index could account for about 70% of the variance in the global tropospheric temperature over the past 50 years. This paper was reviewed and accepted by three independent referees. One referee commented in part: "I found the paper to be well-organized, well-written, and clear on the importance of the research ... The findings are likely to be of interest to a wide variety of readers." A second referee commented in part: "This very clear and well-written manuscript is an analysis of the relationship between MSU-derived and radiosonde-based tropospheric temperature variability and the Southern Oscillation, as modified by major tropical volcanic eruptions."

After McLean *et al*. (2009) was published, a flurry of emails was exchanged between *cabal* members, strategizing on how to carry out damage control for their orthodoxy by preparing a rebuttal. Soon afterwards, a group of *cabalists* (Grant Foster, James Annan, Phil Jones, Michael Mann, Jim Renwick, Jim Salinger, Gavin Schmidt, and Kevin Trenberth) decided to prepare a rebuttal, and, to ensure speedy publication, they pressured the editor of the JGR and suggested the following persons as possible reviewers for their submitted critique: Ben Santer, Dave Thompson, Dave Easterling, Tom Peterson, Neville Nicholls, and David Parker (with Tom Wigley, Tom Karl, and Mike Wallace also mentioned). All of these were

professionally associated in some way to the Foster *et al*. group and are thought to be members of the *climate alarmism cabal*. Phil Jones commented: ''All of them know the sorts of things to say—about our comment and the 'awful original', without any prompting.'' (They all subscribe to the same orthodoxy.) McLean *et al*. describe the whole sordid story at *scienceandpublicpolicy.org/originals/censorship_at_agu.html*.

In their rush to rebut the original McLean article, the *climate alarmism cabal* posted their rebuttal on a website, in violation of *Journal of Geophysical Research* (JGR) rules. The results of McLean *et al*. (2009) would seem to be a major stumbling block for alarmists who attribute most of the warming of the 20th century to greenhouse gases. It is therefore not surprising that the alarmists struck back with members of the *cabal* publishing Foster *et al*. (2010), that claimed that the results of McLean *et al*. (2009) ''are seriously in error'' and concluded ''In fact, the general rise in temperatures over the 2nd half of the 20th century is very likely predominantly due to anthropogenic emissions of greenhouse gases''. Foster *et al*. (2010) fell back on climate models that attribute only 15–30% of temperature variation in the 20th century to variability of the El Niño index. Foster *et al*. (2010) constituted a rather vicious criticism of McLean *et al*. (2009), but JGR refused to publish McLean's response. Evidently, the JGR is acting in collusion with the *alarmist cabal*, and probably regrets that McLean *et al*. (2009) ''slipped through''. McLean (2010) provides all the details.

McLean *et al*. attempted to rebut the criticism by Foster *et al*. (2010), but the JGR refused to publish it. Their rebuttal, which will be referred to as ''McLean2010'', is available at: *icecap.us/images/uploads/McLeanetalSPPIpaper2Z-March24.pdf* and *http://scienceandpublicpolicy.org/originals/censorship_at_agu.html*.

There are several important aspects of this episode that require further elaboration. These include (1) technical aspects, (2) attitudes and collusion amongst *cabal* alarmists, and (3) collusion of the JGR with *cabal* alarmists.

In regard to technical aspects, the issue revolves about methods used for filtering in statistical processing of data. Foster *et al*. (2010) appear to have made some valid criticisms of specific details, but these do not negate the strong correlation of the El Niño index with climate change. Perhaps the contribution of the El Niño index is less than the 70% claimed by McLean *et al*. (2009), but clearly the El Niño index is an important factor in climate change. It seems doubtful that climatology has sufficient data and analytical insight to pin down its quantitative share in influencing climate change. A number of authors, even members of the *alarmist cabal*, have admitted that climate models do not adequately account for El Niño effects. McLean (2010) presented excerpts from the *climategate* emails that clearly show that the *alarmist cabal* regarded McLean *et al*. (2009) as a threat to their orthodoxy, and they colluded together to disparage McLean *et al*. (2009). The *cabal* regards itself as a police force to eradicate any contrary evidence or analysis that would refute their emphasis on greenhouse gases.

McLean *et al*. submitted a response to the published comment by Foster *et al*. but the JGR sent their response to the Foster *cabal* for review—like asking the fox to guard the henhouse. Needless to say, the McLean response was rejected and never

published by JGR, although it appears at *http://scienceandpublicpolicy.org/*. One does not need to be an expert on statistics of large data sets to see that persistent El Niños since about 1976 have contributed significantly to warming in the NH (see Figures 3.33 and 3.37 to 3.39). The science of climatology is not capable of assigning accurate estimates of the percentage contribution of El Niños to total warming. Skeptics suggest perhaps 70%; alarmists suggest less than 30%.

A rather parallel situation occurred in regard to the paper by Douglass *et al.* (2007) that examined measured tropospheric temperature trends and compared them with "Climate of the 20th Century" model simulations. They concluded that observed temperature trends were in significant disagreement with model predictions in most of the tropical troposphere. These conclusions contrasted strongly with those of recent publications by *cabalists*. It has been claimed that a major problem for climate models is the disparity between the temperature trends observed at the Earth's surface and the much smaller trends observed in the lower troposphere that is just the opposite of what global climate models (GCM) predict. (Figure 3.34 shows that the forcing due to doubling CO_2 from the pre-industrial value is much higher at the tropopause than at the Earth's surface). Douglass *et al.* (2007) compared tropical temperature trends with climate model predictions for temperatures in the so-called "characteristic emission layer" (CEL) (2–6 km altitude) where the role of water vapor is most important. Over the period from 1979 through 2004, the models predicted a rising temperature trend of roughly 0.2°C to 0.3°C per decade, whereas satellite temperature measurements indicate essentially no increase below 10 km altitude, and a negative trend above 10 km. This was cited as evidence of the inadequacy of current climate models.

It has come to pass that a few determined skeptics (Douglass, Lindzen, McLean, Spencer, McIntyre, *et al.*) continue to publish contrarian papers (in those rare cases where the *cabal* does not succeed in censoring publication), and immediately thereafter, a flurry of emails is exchanged between *cabal* members (Mann, Jones, Schmidt, Trenberth, *et al.*) castigating the skeptics, and strategizing to achieve damage control to protect their orthodoxy that rising CO_2 is essentially the sole cause of global warming. The most pugnacious and aggressive of these is Michael Mann. It is ironic that his own research, responsible for the *hockey stick*, is far less believable than the works of those he would criticize.

After publication of Douglass *et al.* (2007), the *cabal* came forth with Santer *et al.* (2008) as a rebuttal. This paper begins with the sentence: "There is now compelling scientific evidence that human activities have influenced global climate over the past century" which, aside from the fact that the statement is not true, reveals the orthodoxy to which the authors subscribe religiously. The details of the statistical processing of large data sets are complex. The issue is whether tropical tropospheric temperatures have risen more than surface temperatures as climate models would predict for the effect of greenhouse gases on climate. Douglass *et al.* (2007) concluded that models and data disagreed to "a statistically significant extent". Santer *et al.* (2008) claimed to achieve a "partial resolution of the long-standing 'differential warming' problem", although they also said:

> "We may never completely reconcile the divergent observational estimates of temperature changes in the tropical troposphere. We lack the unimpeachable observational records necessary for this task. The large structural uncertainties in observations hamper our ability to determine how well models simulate the tropospheric temperature changes that actually occurred over the satellite era. A truly definitive answer to this question may be difficult to obtain."

Yet, this did not prevent Santer *et al*. from producing a so-called "Fact Sheet" that said "We've gone a long way towards such a reconciliation" (between climate models and tropical tropospheric temperatures).[6]

In 2009, McIntyre[7] pointed out that, when the data used by Santer *et al*. (2008) that ended in 1999 are extended through 2008, the discrepancy reported by Douglass remains, and "the claim by Santer *et al*. (2008) to have achieved a 'partial resolution' of the discrepancy between observations and the model ensemble mean trend is unwarranted". McIntyre also noted the difficulty in obtaining data from Santer *et al*., and indicated that the *International Journal of Climatology* (IJC) was stalling in responding to him. It appears that this article will never pass through the *cabal*'s lock on the IJC, and McIntyre had to be content with merely archiving his article (*http://arxiv.org/abs/0908.2196*). Yet, alarmists continue to refer to Santer *et al*. (2008) as evidence that climate models have been adequately tested.

Douglass and Christy[8] presented evidence for their claim that Ben Santer, Phil Jones, Timothy Osborn, Tom Wigley, and 13 other *climate alarmism cabal* members apparently conspired to compromise the peer-review process, with the willing cooperation of the editor of the IJC, Glenn McGregor. This evidence involved dozens of emails over nearly a year, suggesting "(a) unusual cooperation between authors and editor, (b) misstatement of known facts, (c) character assassination, (d) avoidance of traditional scientific give-and-take, (e) using confidential information, (f) misrepresentation (or misunderstanding) of the scientific question posed by Douglass *et al*. (2007), (g) withholding data, and more." Douglass and Christy provide the entire sordid story; there is no need to reproduce the details here.

An example of the need by the *climate alarmism cabal* to respond to challenges by contrarians is the paper by Santer *et al*. (2011). This paper was concerned that measurements indicated that tropospheric temperatures had not risen since 1998 despite continued increases in CO_2 concentration. The paper had 17 authors in an expression of support by the *cabal*, although it is difficult to figure out what contributions (if any) were made by the various authors. The listing of these authors seems to be more a political statement than a scientific statement. The goal was to produce an analysis that concludes that temporary periods with no temperature gain may be viewed as a temporary fluctuation superimposed on an ever-present

[6] *https://publicaffairs.llnl.gov/news/news.../NR-08-10-05-factsheet.pdf*.
[7] *http://climateaudit.org/2009/01/27/submited-article-on-tropical-troposphere-trends/* and *http://climateaudit.org/2009/04/14/tropical-troposphere-march-2009/*
[8] *http://www.americanthinker.com/2009/12/a_climatology_conspiracy.html*

underlying upward trend due to rising greenhouse gas concentrations. The logic of the paper is quite shaky, however, as we discuss in Section 3.4.3.2.

Lindzen and Choi (2009) examined data on the outgoing radiation budget from the Earth Radiation Budget Experiment (ERBE) in the tropics in an attempt to determine whether observations of the Earth's radiation imbalance can be used to infer feedbacks and climate sensitivity. From this, they concluded that the climate sensitivity is considerably lower than the values predicted by climate models. Later, Lindzen and Choi admitted: "This work was subject to significant criticism by Trenberth *et al*. (2009), much of which was appropriate." As a result, they wrote a revised paper (Lindzen and Choi, 2011) that was "an expansion of the earlier paper in which the various criticisms are addressed and corrected. . . ." As might be expected, Lindzen and Choi (2011) found that feedbacks were primarily negative, resulting in relatively low climate sensitivity. This is contrary to the alarmist position that feedbacks are positive leading to higher climate sensitivity (and therefore produce a greater increase in global temperature as greenhouse gas concentrations increase). The manuscript by Lindzen and Choi (2011) was rejected by the *Proceedings of the National Academy of Sciences* (PNAS). The revelation of the reviewers and their comments led to a very extensive series of blog entries at *climateaudit.org*. In the course of these blog entries, we find (along with the usual trivia) several nuggets of information worth mentioning. Lindzen is a member of the NAS and it is very rare that a paper submitted by a member would be rejected (96% are accepted). In a highly unusual move, the PNAS rejected Lindzen's suggestion for reviewers, and instead chose reviewers who were obviously antagonistic to Lindzen's viewpoint. The reviews of this paper were incredibly detailed and penetrating. It seems likely that papers expressing the alarmist agenda glide through the review process with little friction and no depth of review. One blog contributor was a reviewer for the paper by Wahl and Ammann (2007). His review was discarded by the *Journal of Climate* because it was not in conformity with the alarmist agenda. It appears that most of the papers in climatology are based on inadequate data: lacking in spatial and temporal coverage. The sophisticated data processing used to cover this up, whether filtering, smoothing, use of principal components, or otherwise, hides the fact that the foundations are typically very weak. Had other landmark papers in climatology that are repeatedly referred to in biblical tones been given the same kind of penetrating review as Lindzen's manuscript, they would also have been rejected. Indeed, most of the literature in climatology would have to be cleared out. Finally, the Lindzen and Choi paper was published in the *Asia-Pacific Journal of Atmospheric Science*.

The entire set of pirated emails in *climategate* provides strong evidence that there is indeed a *climate alarmism cabal*, including both paleoclimatologists who seek to show that the Earth's climate has been relatively constant for thousands of years prior to the 20th century, climate modelers who seek to use climate models to infer that most of the 20th century warming was due to greenhouse gas buildup, and the members of this *cabal* are dedicated to their preconceived beliefs, conspire with one another to prevent publication of dissenting views, conspire with one another to oppose and rebut dissenting papers that slip through their net of referees for major journals, and make frequent alarmist press releases in a losing effort to win over the

public. It seems possible that the motivation for all this is to create a climate of fear so that governments will exponentially increase funding for climate research; in that respect, they have been very successful.

2.4.4 Proxy analysis

Esper, Cook, and Schweingruber (2002a) started out by repeating the mantra of the global-warming alarmists:

> "... the MBH reconstruction indicates that the 20th century warming is abrupt and truly exceptional. It shows an almost linear temperature decrease from the year 1000 to the late 19th century, followed by a dramatic and unprecedented temperature increase to the present time. The magnitude of warmth indicated in the MBH reconstruction for the MWP, 1000–1300 is uniformly less than that for most of the 20th century."

However, Esper, Cook, and Schweingruber (2002a) admitted that "the MBH reconstruction has been criticized for its lack of a clear MWP". It was admitted that critics doubt that tree-ring records can preserve long-term, multi-centennial temperature trends. However, as usual in papers written by the *paleoclimatic cabal*, no mention is made of the devastating criticism of the MBH reconstruction made by McIntyre (2007).

Esper, Cook, and Schweingruber (2002a) then went on to present a defense of tree-ring reconstructions using centuries-long ring-width trends in 1,205 radial tree-ring series from 14 high-elevation and middle- to high-latitude sites distributed over a large part of the NH extra-tropics. Their final result is shown in Figure 2.25.

Esper, Cook, and Schweingruber (2002a) (Figure 2.26) concluded that MBH did not miss an MWP, but rather, "it had a reduced expression of the LIA". They took comfort from the fact that some of the short-term "bumps" in the RCS and MBH curves were synchronous but there are significant differences between the two reconstructions.

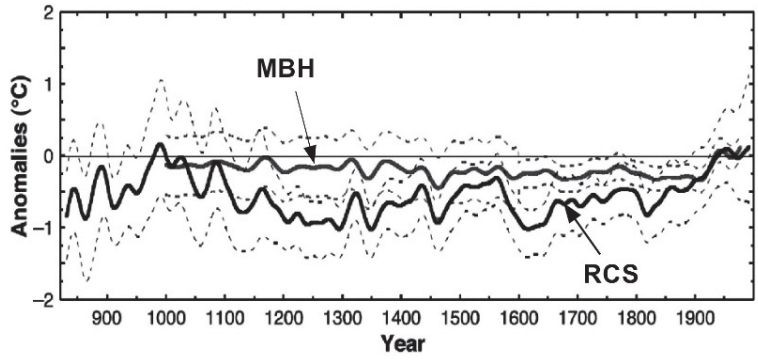

Figure 2.25. Comparison of temperature reconstructions by Esper *et al.* (2002) ("RCS") with that of MBH (1999) (adapted from Esper *et al.*, 2002).

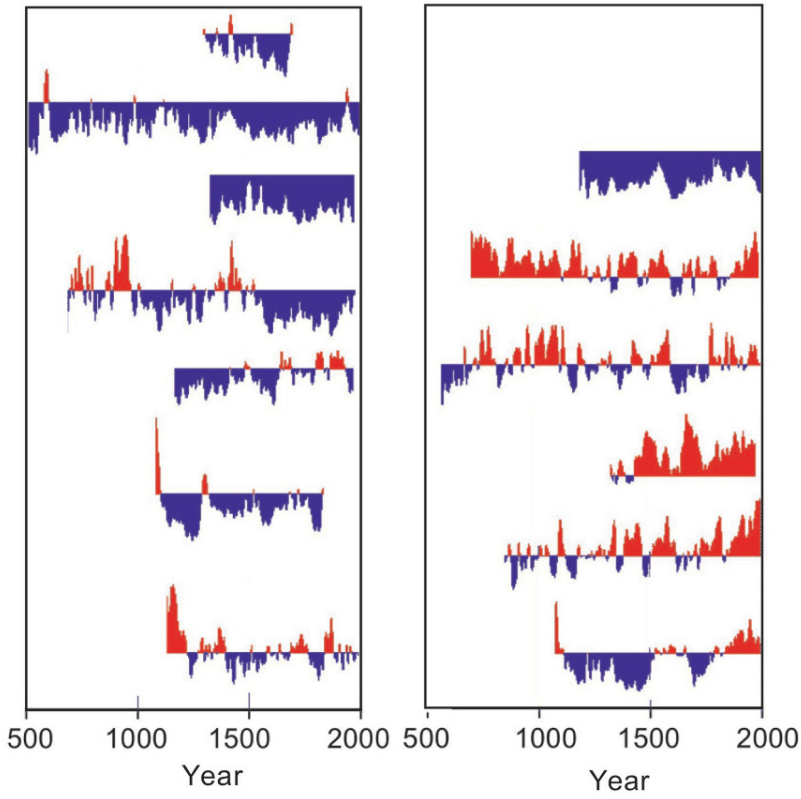

Figure 2.26. Rendition of individual proxies from Esper *et al*. (2002) as adapted by McIntyre (2007).

While Esper, Cook, and Schweingruber (2002a) intended to support Mann, Bradley, and Hughes (1999), the large differences between their results lead this writer to the opposite conclusion. This raises the question of whether any reconstruction based on proxies is credible. Furthermore, note that the anomalies in Figure 2.25 are mostly negative, suggesting that the mean used for data processing was not the mean for the entire time period and, as we have shown, this introduces significant doubt about the veracity of the results.

Finally, in Figure 2.27, McIntyre (2007) presented a simple average of all the proxies. The result suggests an MWP and an LIA. Nevertheless, it seems evident that Esper, Cook, and Schweingruber (2002a) used statistical data manipulation that unreasonably and illegitimately overemphasized the weighting of the two suspect proxies with high closing values.

McShane and Wyner (2010) (M&W) analyzed the methods of combining proxies into estimates of global average temperature from the point of view of mathematical statisticians. In discussing a typical hockey stick graph, M&W said:

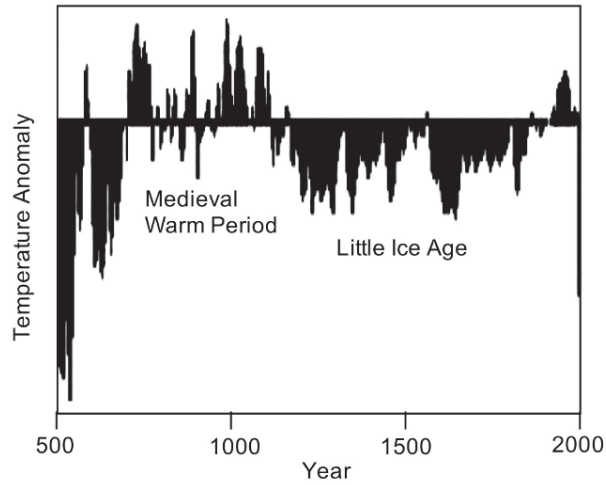

Figure 2.27. Simple average of proxy data from Esper *et al*. (2002) (adapted from McIntyre, 2007).

> "... the [proxy result] closely matches the [measured temperatures] from 1850 AD to 1998 AD because it has been calibrated to the instrumental period which has served as training data. This sets up the erroneous visual expectation that the reconstructions are more accurate than they really are. A careful viewer would know to temper such expectations by paying close attention to the reconstruction error bars given by the wide gray regions. However, even these are misleading because these are, in fact, point-wise confidence intervals and not confidence curves for the entire sample path of surface temperature. Furthermore, the grey regions themselves fail to account for model uncertainty."

What this paragraph means is that the hockey stick is a clever piece of public relations but it does not have the valid implications that a naive reader might suppose. The apparent agreement between the model and the data in the period after 1850 was forced by tuning the model to the data. The estimated implicit uncertainty in the data is optimistic, but, even so, is so wide as to encompass almost any past temperature profile.

In addition, it is noteworthy that M&W did not discuss the quality of the data *per se*, or comment on possible wide divergence in reliability of different proxies.

M&W went on to say:

> "... the task [of analyzing past proxy data to infer global average temperature history] is highly statistical [and] extremely difficult. The data is spatially and temporally auto-correlated. It is massively incomplete. It is not easily or accurately modeled by simple autoregressive processes. The signal is very weak and the number of covariates greatly outnumbers the number of independent observations of instrumental temperature."

This paragraph attests to the fact that estimating the global average temperature over an ~1,000-year period requires detailed spatial and temporal data, and the proxy data are very sparse. It is not easy to detect a signal from the noise and there are factors other than temperature that affect the signal. Climatologists and statisticians have used sophisticated methods to try to extract trends from this sparse mess of crude data, but it is difficult to produce a silk purse from a sow's ear. M&W said:

> "McIntyre and McKitrick (2003, 2005) (M&M) [reviewed] the original Mann *et al.* (1998) study [and showed] that it (i) used only one principal component of the proxy record and (ii) calculated the principal components in a 'skew'-centered fashion such that they were centered by the mean of the proxy data over the instrumental period (instead of the more standard technique of centering by the mean of the entire data record). Given that the proxy series is itself auto-correlated, this scaling has the effect of producing a first principal component that is hockey-stick shaped and, thus, hockey stick shaped temperature reconstructions. That is, the very method used in Mann *et al.* (1998) guarantees the shape of [the hockey stick figure]. M&M made a further contribution by applying the Mann *et al.* (1998) reconstruction methodology to principal components computed in the standard fashion. The resulting reconstruction showed a rise in temperature in the medieval period, thus eliminating the hockey stick shape.".

Mann and hs colleagues vigorously responded to M&M to justify the hockey stick (Mann *et al.*, 2004). According to M&W:

> "Mann *et al.* (2004) argued that one should not limit oneself to a single principal component as in Mann *et al.* (1998), but, rather, one should select the number of retained principal components through cross-validation on two blocks of held-out instrumental temperature records (i.e., the first fifty years of the instrumental period and the last fifty years). When this procedure is followed, four principal components are retained, and the hockey stick re-emerges even when the PCs are calculated in the standard fashion."
>
> "The furor reached such a level that Congress took up the matter in 2006. The Chairman of the Subcommittee on Oversight and Investigations formed an ad hoc committee of statisticians to review the findings of M&M. Their Congressional report (Wegman *et al.*, 2006) confirmed M&M's finding regarding skew-centered principal.... In his Congressional testimony (Wegman *et al.*, 2006), committee chair Edward Wegman excoriated Mann *et al.* (2004)'s use of additional principal components beyond the first after it was shown that their method led to spurious results: 'In the MBH original, the hockey stick emerged in PC1 from the bristlecone/foxtail pines. If one centers the data properly the hockey stick does not emerge until PC4. Thus, a substantial change in strategy is required in the MBH reconstruction in order to achieve the hockey stick, a strategy that was specifically eschewed in MBH ... a cardinal rule of statistical inference is that the method of analysis must be decided before

> looking at the data. The rules and strategy of analysis cannot be changed in order to obtain the desired result. Such a strategy carries no statistical integrity and cannot be used as a basis for drawing sound inferential conclusions'."

There are two points here. One is that, by implication, MBH changed the rules of the game to achieve a desired result in an unscientific, unprofessional manner. The other is that, by doing this, the hockey stick shape was relegated to the fourth level of PCA, a very weak inference of low credibility. Michael Mann, in his rebuttal testimony before Congress, continued to stubbornly defend his turf, and why shouldn't he, since his whole scientific reputation depended on it? He claimed that many subsequent peer-reviewed studies confirmed his findings, but all of these studies used variants of his approach and were peer-reviewed by members of the *paleoclimatic cabal*. In particular, they all used skew-centering, as evidenced by the fact that the data were not spread roughly equally across the zero line. Furthermore, as climategate has revealed, peer review in paleoclimatology is mainly a mutual affirmation by members of the club. This was the backdrop for the study by M&W to reexamine the issues involved in reconstructing past global climates from proxy data.

M&W began with the statement: "We are not interested at this stage in engaging the issues of data quality. To wit, henceforth and for the remainder of the paper, we work entirely with the data from Mann *et al.* (2008)." M&W then went on to support this database, saying:

> "We *assume* that the data selection, collection, and processing performed by climate scientists meets the standards of their discipline. Without taking a position on these data quality issues, we thus take the dataset as given. We further make the assumptions of linearity and stationarity of the relationship between temperature and proxies, an assumption employed throughout the climate science literature. . . ." (Italics added).

However, the validation of these assumptions in the literature leaves much to be desired. The precision of various proxies in representing temperature, and only temperature, remains a mystery unstated. One thing is certain: not all proxies are equally credible. M&W were concerned with the statistical processing of data, assuming the data were adequate. More likely, the data were poor in quality, so that even the best statistical analysis would result in GIGO.[9] Each proxy provides an estimate (often a rough estimate) of temperature for one locality. Temperatures are known to vary widely with locality. As M&W demonstrate, the various proxies in different localities vary widely in form. It challenges one's imagination to think that these wildly different proxies can provide much useful information about global average temperature. M&W began with a discussion of the over-determined system in which there are many more proxies than years in the instrumental temperature record. If, for example, one takes a particular year during the instrumental record, say 1950, there might be, say, 200 proxies for temperature that year. But there is only

[9] GIGO = garbage in; garbage out.

one global average temperature. In attempting to calibrate these 200 proxies against the single global average temperature, there is no unique solution to this over-determined problem. Certainly, one cannot hope to set all the proxies at all locations to the global average temperature. In some cases, one can segregate proxies that compare better with historically measured temperatures at their localities, but the connection to the global average temperature remains weak. Another approach involves dividing the calibration period (1850–1998) into segments, calibrating proxies over one segment, and testing the result over the other segments. While sophisticated analysts such as Mann *et al.* and M&W have used various procedures to try to work around this problem, the simple truth is that there is no solution.

The bases of all these manipulations by Mann *et al.* are the assumptions that each proxy measures a local or regional temperature, that each such local or regional temperature is the sum of a global average temperature influence term and a regional bias term, that the regional bias terms are either positive or negative, and that, when averaged over many proxies, they sum up to zero. Hence, adding up many proxies produces a measure of the global average temperature according to this theory. Unfortunately, these assumptions do not seem to have been validated. Even a casual examination of the actual proxies shows that they vary wildly from one to another. What seems to happen is that even if these assumptions were true (a consummation devoutly to be wished), the regional bias terms are orders of magnitude greater than the global average term, and the global average term is buried in an ocean of noise generated by the regional bias terms. Mann and company have attempted to use complex methods to extract the signal from the noise but, as M&W and many commentators have shown, the process suffers from many ills, not least of which is the imperfection of the proxies themselves. I would quote Carl Wunsch:

> "Sometimes there is no alternative to uncertainty except to await the arrival of more and better data." (Wunsch, 1999).

As Steve McIntyre[10] pointed out:

> "The fundamental problem in paleoclimate is not the need for some novel multivariate method, but better proxies and reconciliation of discordant existing 'proxies'. . . . Team reconstructions use highly stereotyped proxies over and over again in different guises. . . ."

McIntyre also pointed out that M&W unwittingly adopted "the Mann *et al.* 2008 data set, which quixotically introduced the Tiljander sediments, the modern portion of which was contaminated with bridge-building sediments".

The details of the statistical analysis in M&W (and the 13 commentaries) are quite complex and are only intelligible to specialists. This writer was not able to follow all the intricacies of the analysis, most of which were permeated with jargon. One possibility raised by M&W was:

[10] *climateaudit.org*.

> "... it is possible that the proxies are ... too weakly connected to global annual temperature to offer a substantially predictive (as well as reconstructive) model over the majority of the instrumental period. This is not to suggest that proxies are unable to detect large variations in global annual temperatures (such as the differences that distinguish our current climate from an ice age). Rather, we suggest it is possible that natural proxies cannot reliably detect the small and largely unpredictable changes in annual temperature that have been observed over the majority of the instrumental period."

This appears to be the actual case. Proxies seem able to detect very large excursions in temperature, such as occur in transitions between Ice Ages and interglacials, but probably are not able to resolve small temperature changes within an interglacial. The conclusions reached by M&W include the following.

The problem of back casting historical temperatures from proxy measurements calibrated during a limited period of overlap between temperature measurements and proxy measurements is very complex and requires very sophisticated statistical analysis that might be beyond the capability of climate scientists. M&W attempted to bring to bear such a sophisticated statistical analysis on the problem:

> "... we conclude unequivocally that the evidence for a 'long-handled' hockey stick (where the shaft of the hockey stick extends to the year 1000 AD) is lacking in the data. The fundamental problem is that there is a limited amount of proxy data which dates back to 1000 AD; what is available is weakly predictive of global annual temperature. Our back-casting methods, which track quite closely the methods applied most recently in Mann *et al*. (2008) to the same data, are unable to catch the sharp run up in temperatures recorded in the 1990s, even in-sample.... Consequently, the long flat handle of the hockey stick is best understood to be a feature of regression and less a reflection of our knowledge of the truth."

The main contribution of M&W was "to seriously grapple with the uncertainty involved in paleoclimatological reconstructions". According to them, the challenges include:

> "(i) a short sequence of training data,
> (ii) more predictors than observations,
> (iii) a very weak signal, and
> (iv) response and predictor variables which are both strongly auto-correlated.
>
> The final point is particularly troublesome: ... the number of truly independent observations (i.e., the effective sample size) may be just too small for accurate reconstruction. Climate scientists have greatly underestimated the uncertainty of proxy-based reconstructions and hence have been overconfident in their models.... Proxy based models with approximately the same amount of reconstructive skill produce strikingly dissimilar historical back-casts: some of these look like hockey sticks but most do not. Natural climate variability is not well understood and is probably quite large. It is not clear that the proxies

currently used to predict temperature are even predictive of it at the scale of several decades let alone over many centuries.''

Thirteen independent groups or individuals wrote commentaries on the M&W paper. Evidently, there is very little objectivity in paleoclimatology, as evidenced by the facts that the establishment warmist climatologists vigorously defended Mann *et al*., the statisticians made abstruse mathematical comments, and several climatologists exterior to the paleoclimatological *cabal* indicated support for M&W.

One aspect of this controversy is the use of PCA. One starts with a set of data from various proxies at various locations over various time periods. If one adds these up with equal weight, one obtains mainly mush—a smear of sparse data with neither direction nor structure. Then, PCA is applied. While one might naively treat all proxies equally, PCA assigns weights to the various proxies on the basis that those proxies with the least tendency toward trend are given low weight, and those proxies with the greatest tendency toward a trend are given greater weight. As M&M and Wegman showed, in the extreme case, MBH gave some proxies hundreds of times the weight of other proxies. The data set was very sparse to begin with, and PCA further reduces the dimensionality of the data set to put a microscopic focus on those few proxies that demonstrate a strong trend, some of which were suspect tree-ring proxies. How can a weak, sparse data set be improved by throwing out most of the data? The statisticians might respond by saying they have identified the proxies that generate the trend for the whole set, but, considering the uncertainty and unreliability of all proxies, this seems like a very biased, counter-productive approach. PCA gives climatologists and statisticians fodder to play with, but the whole process seems to add up to GIGO. In short, this writer thinks that the use of PCA as a method in this application is highly suspect.

Gavin A. Schmidt, Michael E. Mann, and Scott D. Rutherford, claimed that M&W used faulty data in their analysis but they provided little evidence that their own data set was adequate. In order to find any trends in the noisy data, they were forced to go to the 10th or 4th principal component—an extremely weak statistical inference. There is no way they can escape from the fact that the data are very sparse, noisy, and divergent. They seemed most intent on insisting the medieval warmth was minimal and that the decade 1997–2006 was the warmest in 1,000 years—the same old warmist mantra. It may well be true that 1997–2006 was the warmest decade since the height of the MWP, but the height of the MWP was about 1,200 years ago, and not in the past 1,000 years. Furthermore, the issue is not whether 1997–2006 was warm; we know that it was. The issue is whether its deviation from average was unprecedented, and the data are not adequate to answer this.

Amazingly, after all the discussion raised by M&M and Wegman on the problems inherent in centering and standardizing over the calibration period, rather than the full length of the data, Eugene R. Wahl and Caspar M. Ammann described centering and standardizing over the calibration period by MBH as a ''reasonable judgment''. They then went on to discuss technical aspects of PCA that are beyond the scope of this discussion, including taking PCA to the second or even the fourth

component in a desperate effort to evoke a trend. Somehow, they came up with a silk purse starting with a sow's ear.

The responses by Alexey Kaplan; Peter Craigmile and Bala Rajaratnam; Doug Nychka and Bo Li; Jason E. Smerdon; Martin P. Tingley; Jonathan Rougier; Murali Haran and Nathan Urban; and Richard A. Davis and Jingchen Liu were abstruse and mainly of use only to specialists. Some of these were critical of methods used by M&W but they did not justify methods used by Mann *et al*. For example, Haran and Urban said that their criticisms of M&W equally applied to Mann *et al*.

L. Mark Berliner made some brief comments. He said:

> "I join the authors in expressing dissatisfaction with some paleoclimate analyses. I endorse their claim that there has been underestimation of uncertainty in paleoclimate studies. The implication that additional participation of the statistics community is needed is undeniable."

He questioned the assumption of linearity between proxies and temperature, even as used by M&W. He also described the reliance on principal components as "highly questionable".

Lasse Holstrom said: "The authors demonstrate convincingly that the data used in Mann *et al*. (2008) does not allow reliable temperature prediction using this approach and that purely random artificial proxy records in fact perform equally well or even better."

Jason E. Smerdon criticized some of the statistical methods used by M&W but he did not seem to offer any support for the methods of Mann *et al*.

Martin P. Tingley began with:

> "M&W find that under certain scenarios … randomly generated series are as predictive of past climate as the commonly used proxies. They conclude that 'the proxies do not predict temperature significantly better than random series generated independently of temperature'.... If this assertion is correct, then M&W have undermined all efforts to reconstruct past climate, which are based on the fundamental assumption that natural proxies are predictive of past climate. I disagree with M&W's conclusion and provide an alternative explanation: [their procedure] is simply not an appropriate tool for reconstructing paleoclimate."

Note the pejorative tone in this opening statement as if M&W were akin to the Grinch who stole Christmas. But, as in the case of Smerdon, Tingley seems to have criticized some of the statistical methods used by M&W but he does not seem to offer any support for the methods of Mann *et al*.

M&W wrote a "rejoinder"—a sort of rebuttal—after 13 comments by other authors were sent in to the *Annals of Applied Statistics*. In this rejoinder, they said:

> "... Wahl and Ammann (WA) note 'there is an extensive literature contradicting McShane and Wyner (2011a)'s assertions about low or poor relationships between proxies and climate'. On the other hand, Tingley asserts 'each proxy is weakly correlated to the northern hemisphere mean (for two reasons: proxies generally have a weak correlation with local climate, which in

turn is weakly correlated with a hemispheric average)' and Davis and Liu (DL) state 'there is just not much signal present'.''

How can one explain such a vast difference between these conclusions? M&W attempt to explain it:

> ''This contrast can be explained at least in part by context. Our paper addresses the specific task of reconstructing annual temperatures over relatively short epochs during which temperatures vary comparatively little. Nevertheless, such contrasts are suggestive of the important frontiers for research and we hope our paper and this discussion will lead to advances on these fronts.''

However, this explanation does not do justice to the reality that the warmists (e.g., Wahl and Ammann, 2007) are dedicated to the hockey stick and the hockey stick requires that proxies be representative of past temperatures, whereas objective scientists observe the weak connection between proxies and temperature.

M&W's rejoinder then goes on at length to rebut the various commentaries by 13 authors and the details are abstruse and only appropriate for specialists.

In their conclusion, M&W characterized the assumptions made by the hockey stickers (linearity, stationarity, data quality, etc.) as ''questionable, perhaps even indefensible''. They also said: ''... we reiterate our conclusion that 'climate scientists have greatly underestimated the uncertainty of proxy-based reconstructions and hence have been overconfident in their models'.'' They closed with: ''Finally, and perhaps most importantly, the NRC assumptions of linearity and stationarity outlined in our paper are likely untenable and we agree with Berliner in calling them into question.''

Subsequent to the paper by M&W, Li *et al.* (2010) entered the fray with a lengthy and somewhat obscure discussion that involves combining a climate model with proxy data. This paper generated a number of published comments. Smith (2011) followed this with a paper of his own. These papers deal with complex aspects of wringing out statistical inferences from large noisy data sets. McIntyre[11] commented on these papers. He pointed out that none of this work dealt with the adequacy of the underlying data as to whether the tree-ring data (in particular) are truly representative of temperature. He said that ''indices of tree growth ... in many cases, are more responsive to precipitation than temperature. Academics in this field are far too quick to assume that things are 'proxies' when this is something that has to be shown''. McIntyre showed that the data used in these analyses, relying heavily on Graybill bristlecone chronologies, are highly suspect and provide the source of hockey stick form in the results. Thus, the most comprehensive, sophisticated statistical analyses are efforts in futility if the underlying proxies are not good measures of temperature. There are at least three issues here that go beyond complex statistics: (1) the relationship between proxy signals and temperature over the entire calibration period; (2i) the uncertainty introduced in applying the proxy-temperature relationship outside the calibration period; and (3) the lack of full regional and

[11] *http://climateaudit.org/2011/06/09/richard-smith-2011-and-the-graybill-bristlecones/*.

temporal coverage by the proxies. There has been, and remains, a notable lack of detail on the degree of conformity between proxies and temperature during the calibration periods. As we have pointed out previously, if one combines a set of noisy "proxies" that are highly random by any sophisticated statistical algorithm, one will end up with a relatively flat profile. Then, tacking on a rising measured temperature in the 20th century, one must obtain a hockey stick as the result.

2.4.5 Evidence for the MWP and the LIA

The term "*Little Ice Age*" (LIA) has been widely used to describe a period from roughly 1300 (or later) to about 1850 (or perhaps a bit later) that evidence indicates was, on average, colder than the preceding or following eras, particularly in the NH. Matthews and Briffa (2005) discussed the LIA in some detail. It is believed that there was no uninterrupted, centuries-long cold phase following a similar, uninterrupted, centuries-long *Medieval Warm Period* (MWP). While the LIA was marked by prevailing cold temperatures, there were fluctuations within the LIA both spatially and temporally, and this has provided ammunition for some purists to debate whether there actually was an LIA. Matthews and Briffa (2005) indicated that the term "*Little Ice Age*" (LIA) has been defined in various ways according to the context and there is no single widely accepted interpretation of the term. In particular, depending on whether the emphasis is on glacierization or climate, some differences will inevitably result. Some authors suggest that the term should be used cautiously, some say it should not be used at all, and some say it should be allowed to disappear from use or should be avoided because of its limited utility. However, the existence of the LIA is a barrier of sorts to those who subscribe to the *hockey stick* version of global temperature history, and those who disparage the LIA seem to have an alarmist view of global warming.

One serious problem with Matthews and Briffa (2005) is that temperatures for the LIA are compared with a standard based on the average for 1961–1990. Since LIA temperatures are clearly lower than 1961–1990 temperatures, this serves to unduly emphasize the temperature rise of the 20th century. But the thing that makes the LIA unique is that temperatures during the LIA were lower than the long period that preceded it as well as the 20th century that followed it, and that is why it is colloquially called an LIA. *From the perspective of global warming, the important point is how 20th century temperatures compare with the warm period prior to the LIA—not how they compare with temperatures during the LIA*.

As Matthews and Briffa (2005) pointed out:

> "The expanded state of glaciers, relative to today, during the last few hundred years is an incontrovertible fact ... glaciers on all continents, from the tropics to the polar regions, were characterized by glacier expansion and subsequent retreat. However, beyond the European Alps, and to a lesser degree in Scandinavia and North America, data on the precise timing of variations in glacier size during this broad time interval are still patchy. Consequently, several controversial issues remain, including: (1) the timing of the onset (and end) of

the LIA; (2) the amplitude and timing of glacier variations within the LIA; (3) the degree of synchroneity [*sic*] between glaciers from the different regions; and (4) the attribution of cause(s) in terms of large-scale climate forcing . . . Greatest reliance must . . . be placed on the geographically restricted evidence available from the European Alps, where the historical sources are sufficient in quality and quantity to answer not only the question of onset but also questions about when the '*Little Ice Age*' glaciers reached their maximum extent and what amplitude of glacier variations occurred within the LIA period."

Grove (1988) provided 1,000 pages of evidence for the LIA. Grove (2001) asserted that the greatest amount of information about the LIA can be gleaned from the Swiss Alps, where historical records are unusually rich and moraine dating is good. Many ice fronts extended below the current tree line and were in full view of settlements for hundreds of years, or even abutted onto farmland. Written records, paintings, and drawings made by both local observers and visitors are plentiful. Identification of the calendar dates at which many *in situ* trees were killed by advancing ice, together with their ages at death, has been made possible by multiple dendrochronological analyses.

The following paragraph is excerpted from Grove (2001):

"The most complete record of the Late Holocene fluctuation history of any glacier in the world comes from intensive investigations of the deposits of the Grosser Aletsch [Figure 2.28]. This reconstruction was based on a great variety of evidence. Some samples were taken from the outer rings of trees overrun by the advancing ice, which could be absolutely dated by reference to dendrochronological series. This chronology is accordingly more accurate than one based only on radiocarbon samples. Retraction during the MWP was ended by rapid advance, starting after 1250 and culminating around 1350. Although the MWP here was fragmented by a marked glacial advance between 1050 and 1150, this was not comparable in scale with those of later centuries and therefore

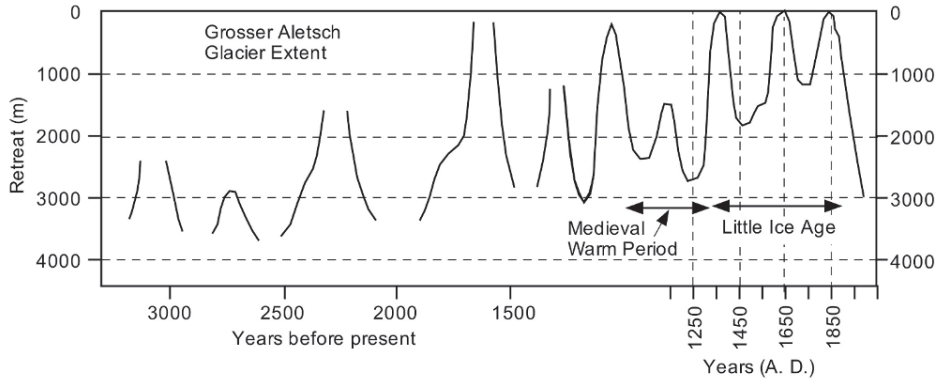

Figure 2.28. Retreat of the Grosser Aletsch Glacier, Swiss Alps, over the last 3,000 years based on documentary and proxy evidence (adapted from Matthews and Briffa, 2005).

has to be considered an interruption of medieval warmth rather than part of the LIA. The Grosser Aletsch advanced about 40 m per year after 1300, to reach its greatest extension around 1370–80. This 14th-century advance brought the front as far forward as did those in the seventeenth and nineteenth centuries. The major extension phases were separated by withdrawals that were insufficient to return the front to positions comparable with those of the warm period. It is evidently reasonable to view the whole period between the start of expansion in the thirteenth century and the great retreat of the late nineteenth and twentieth centuries as one complex LIA with each century scale fluctuation itself made up of smaller scale oscillations, such as those recorded during monitoring of the positions of glacier fronts.''

Several aspects of Figure 2.28 are critical when using it to define the LIA in the Swiss Alps (Matthews and Briffa, 2005). First, the three glacier high stands of around 1350, 1650, and 1850 were remarkably similar in extent. Second, previous glacier maxima, including those in the 3rd, 7th, 9th, and 12th centuries, were less extensive. Third, the size of the glacier during the retreat phases between the LIA high stands remained much greater than in the earlier retreat phases. The data support the notion of a change toward a more glacierized region at the end of the 12th century, and so marking the onset of the central European LIA. This change has also been interpreted as marking the end of the ''*Medieval Warm Period*'', and a similar pattern and timing are supported on a centennial timescale by the somewhat less complete records from other Alpine glaciers. It must nevertheless be concluded that, even in the Swiss Alps, differences between the glacier variations during the LIA and those before the LIA were a matter of degree rather than of kind.

It can be seen from Figure 2.28 that, since the mid-19th century, mountain glaciers have been in a fairly steady retreat. Anon. (M) and Kotlyakov (1996) confirm that the retreat of the glaciers preceded large-scale buildup of CO_2.

Polissar *et al*. (2006) also noted three major glacial advances from Andes lake sediments from the period 1300–1750.

Grove (2001) pointed out that the Gorner Glacier has advanced and retreated in harmony with the Grosser Aletsch, showing only very minor differences. Grove also mentions that other large Alpine glaciers have been shown to have been similarly affected by early LIA advances. The Grindelwald was advancing around 1338. The Rhone Glacier advanced to a maximum between 1350 and 1400, which was slightly more extensive than any occurring later. Farther east, the Gurgl reached a maximum in about 1300. In the Austrian Alps, the Gepatschferner, Gurglerfemer, and Simonykees glaciers are also known to have advanced during this period.

Grove (2001) concluded that the LIA started in the second half of the 13th century and its three culminations, including the first in the 14th century, were very similar in scale. The LIA was not a unique event with a discrete beginning and a discrete end. Several phases of glacial expansion during the Holocene, comparable in scale with those of the LIA, have been traced in both the Swiss and Austrian Alps.

Grove (2001) also attempted to estimate the degree to which the LIA was applicable to regions other than the Swiss Alps. An extensive survey of available data for Canada, Greenland, Iceland, and Scandinavia provided fragmentary insights. It was concluded:

> "Though no reconstructions of the glacial history of the last millennium are as complete as the most detailed from Switzerland, evidence that the LIA had begun in the thirteenth century, with the first culmination in the fourteenth century, is widespread."

Joerin, Stocker, and Schluchter (2006) studied sub-fossil remains of wood and peat from six Swiss glaciers found in pro-glacial fluvial sediments. They discovered 12 phases of glacier recessions during the Holocene. Trees and mires grew where glaciers exist at present and, therefore, glaciers were smaller at that time than the present. The 12 major recession periods occurred at 9850–9600, 9300–8650, 8550–8050, 7700–7550, 7450–6550, 6150–5950, 5700–5500, 5200–4400, 4300–3400, 2800–2700, 2150–1850, and 1400–1200 years before present. They emphasized that "this natural variability of glacier extent, which occurs on a centennial timescale, is superimposed on a much longer term ... trend towards increased glacier extent culminating in the '*Little Ice Age*' ".

A reconstruction covering the last 600 years based on selected tree-ring density series mainly from high-latitude land areas is shown in Figure 2.29. However, it is not clear how the tree-ring data were scaled. It appears to this writer that the data from tree rings might fit the measured temperatures better over its interval if the entire tree-ring curve is lifted about 0.1°C.

According to Matthews and Briffa (2005), Figure 2.29 demonstrates a distinct LIA climate from about 1570 to 1900 when NH summer temperatures (April to September) fell significantly below the 1961–1990 mean. Matthews and Briffa said:

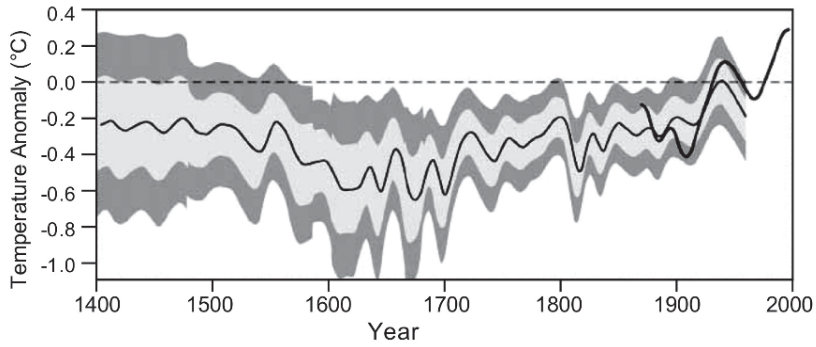

Figure 2.29. Tree-ring density reconstruction of NH (lands north of 20°N) summer temperatures (April to September) since AD 1400 (thin continuous line). Units are °C anomalies referenced to 1961–1990 mean (dashed line). Shaded areas show 68% and 95% confidence levels. Measured temperatures (thick line) are also shown (adapted from Matthews and Briffa, 2005).

> "It would appear that there is a tenable statistical basis for belief in at least the main phase of the '*Little Ice Age*' as at least a hemispherical cold period ... and that, in terms of summer temperature, most of the seventeenth century was of the order of 0.5°C below the 1961–1990 mean. The question of whether the event was global remains more open."

In a later passage, Matthews and Briffa said:

> "Indeed, we show here, for the first time in map form, that the majority of the Northern Hemisphere experienced a relatively low mean summer temperature for more than three centuries (1570 to 1900), and that the LIA was not merely or even mainly a European phenomenon."

But perhaps more importantly, Figure 2.29 is somewhat misleading because temperatures for the LIA are compared with a standard as the average for 1961–1990. As we previously pointed out, since LIA temperatures are lower than 1961–1990 temperatures, this serves to unduly emphasize the temperature rise of the 20th century. As long as one uses 1880 as a base year for comparison, 20th-century temperatures will show an increase. But is this a departure from the normalcy of the LIA, or was the LIA an aberration, with the 20th-century rise in temperature being a return to normalcy?

In Section 2.3.1, we provided a number of examples of proxy evidence for existence of the MWP and LIA.

While alarmists have denigrated the LIA as being minor and localized to Europe, there is some evidence that the LIA extended to tropical regions and the Southern Hemisphere. Lane *et al*. (2011) said: "Climate change during the LIA ... was once thought to be limited to the high northern latitudes, but increasing evidence reflects significant climate change in the tropics. ... Our results from Hispaniola further emphasize the global nature of LIA climate change. ..." Rabatel *et al*. (2008) reported on evidence of the LIA in Bolivia. Thompson *et al*. (2006) found evidence of the LIA in the Andes.

2.4.6 Asian climate records

Esper, Schweingruber, and Winiger (2002b) reported results on more than 200,000 ring-width measurements from 384 trees obtained from 20 individual sites that were analyzed to reconstruct regional climatic variation patterns in western central Asia since 618. A prolonged centennial trend toward better growing conditions was observed over the last 300 years (1700–2000) in the western central Asia tree ring records. This trend is of a lesser magnitude compared with conditions before 1100 and suggests the existence of a MWP. These early and recent benign periods were separated by a prolonged period of poor growth, which presumably reflects the LIA in western central Asia. These results support the hypothesis of the existence of the MWP and the LIA. The warmest decades since 618 appear to be between 800 and 1000, whereas the coldest periods were recorded in the first half of the 17th century.

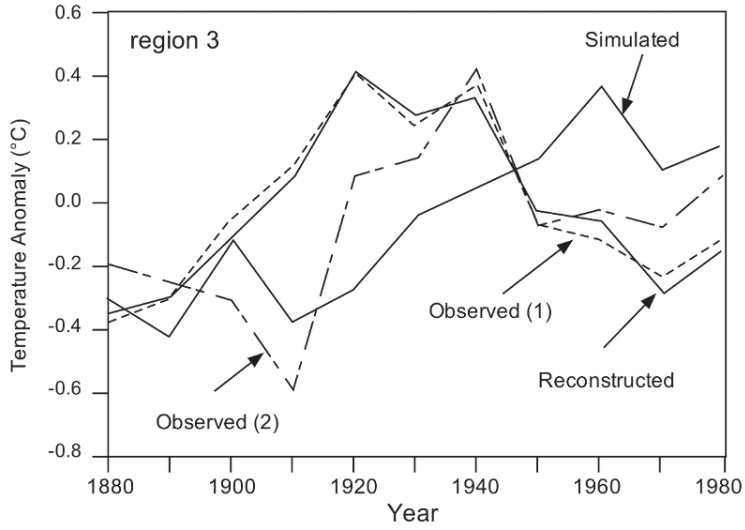

Figure 2.30. Basic data for the calibration period for Region 3 (South China) (adapted from Liu *et al*., 2005).

Zhang *et al*. (2003) reported on a 2,326-year tree-ring chronology that is currently the longest annually resolved climate proxy record on the Qinghai–Tibetan Plateau and in China. Their results indicate that the climate on the plateau has undergone oscillations and, sometimes, very rapid swings during the last two millennia.

Liu *et al*. (2005) carried out a study of historical temperatures in China since 1550. China was divided into 10 districts that are relatively homogeneous. With proxy data from historical documents, tree rings, and ice cores, reconstructions of the temperature series were made for eight of these regions. The reconstructions of 10-year mean temperature anomalies were based on proxies via statistical techniques that rely on establishing empirical relationships between modern observations and environments during the calibration period (1880–1979). However, Liu *et al*. (2005) reported: ''Unfortunately, this evidence is very uncertain.'' The basic data for one of the Chinese regions (Region 3) are shown in Figure 2.30. Note how the ''simulated'' data accentuate the rise during the 20th century. However, it is difficult for this writer to understand how the ''simulated data'' were derived.

Liu *et al*. (2005) provided ''reconstructed'' and ''simulated'' data from 1590 to 1980 for each of the eight regions. It is not clear to this writer how these were derived. Interestingly, when these data are compared with Figure 2.31 for Region 3, there doesn't seem to be much correlation.

Liu *et al*. then went through an empirical orthogonal analysis (EOF) that is a form of PCA[12] and ended up with Figure 2.31. The connection of this figure to Figure 2.30 appears to be ''lost in translation''.

[12] But the analysis was unintelligible to this author.

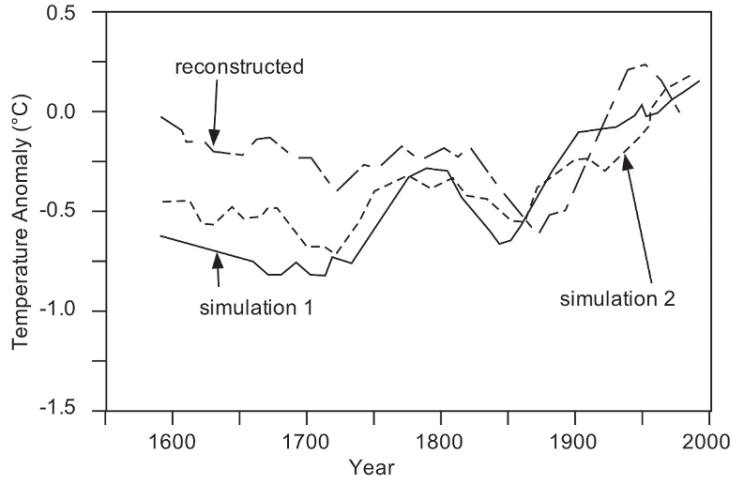

Figure 2.31. Longer-term data for Region 3 (adapted from Liu *et al.*, 2005).

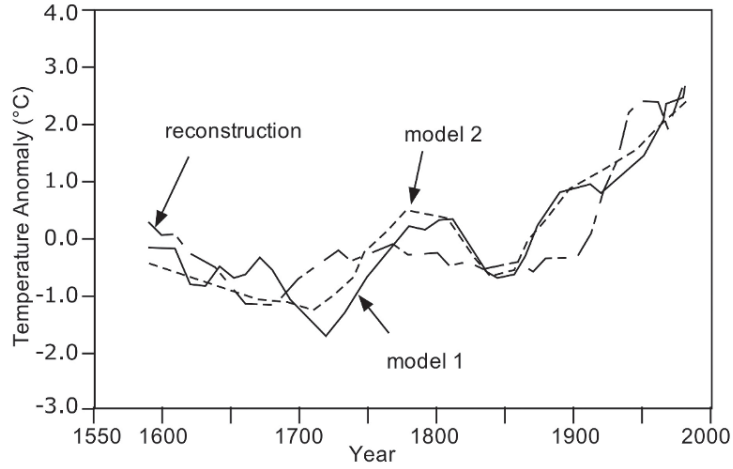

Figure 2.32. Estimated temperature profiles for all of China (adapted from Liu *et al.*, 2005).

The final result for the eight regions of China is given in Figure 2.32, showing a moderate *hockey stick* form. However, there is some evidence of a significant LIA, particularly between 1650 and 1750. Furthermore, the absolute values of the temperature anomalies seem very high in this figure. Without access to the original temperature data (1880–1980) from which the fundamental proxy relations were (presumably) derived, it is difficult to evaluate the veracity of this result. In particular, a concern is that, in statistical processing, they may have used the mean for the 1880–1980 period as the basis for calculating anomalies for the entire period 1590–1980, which could lead to exaggeration of the *hockey stick* syndrome. This writer is unable to penetrate the methodology enough to resolve this issue.

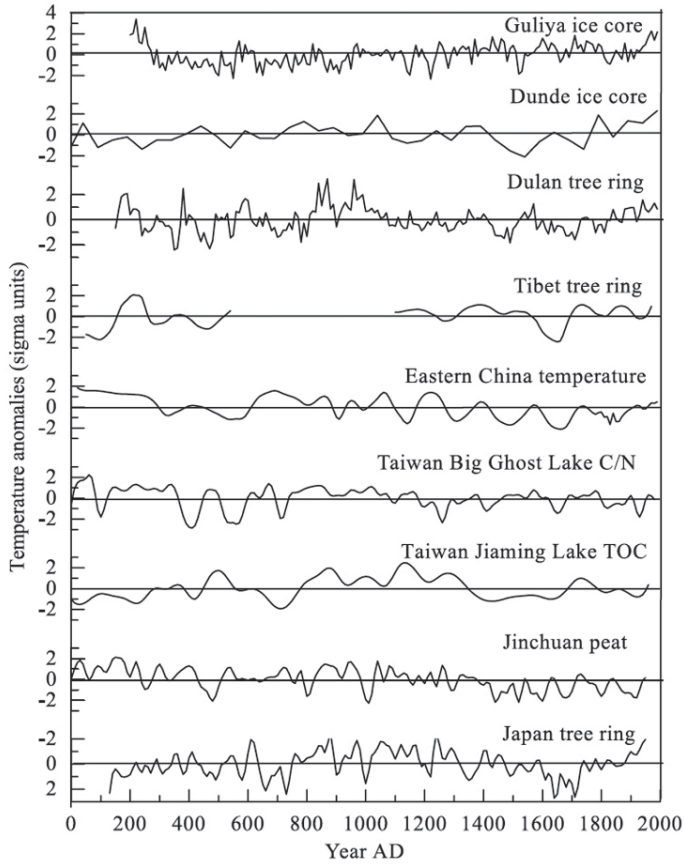

Figure 2.33. Two-thousand-year histories of average temperature for Chinese and vicinity areas (adapted from Yang *et al*., 2002).

Yang *et al*. (2002) established three alternate China-wide temperature composites covering the last 2,000 years by combining multiple paleo-climatic proxy records obtained from ice cores, tree rings, lake sediments, and historical documents.

The basic proxy records are shown in Figure 2.33 for nine regions in and around China. As is usual with proxy data, there is considerable variation from region to region. Some of this could be real, while some could be the result of "noise" in the signal. There is a tendency for the time period around 1000 to be warm, but this is not uniform across all proxies. Similarly, there is a dip in temperature after 1600 in some of the proxies.

Yang *et al*. (2002) tried several methods to reconstruct regional paleo-temperature series from the nine data sets. One approach was to simply average all nine data sets with equal weighting. This was referred to as "Complete" China. A second approach ("Weighted" China) weighted the seven Chinese data sets in

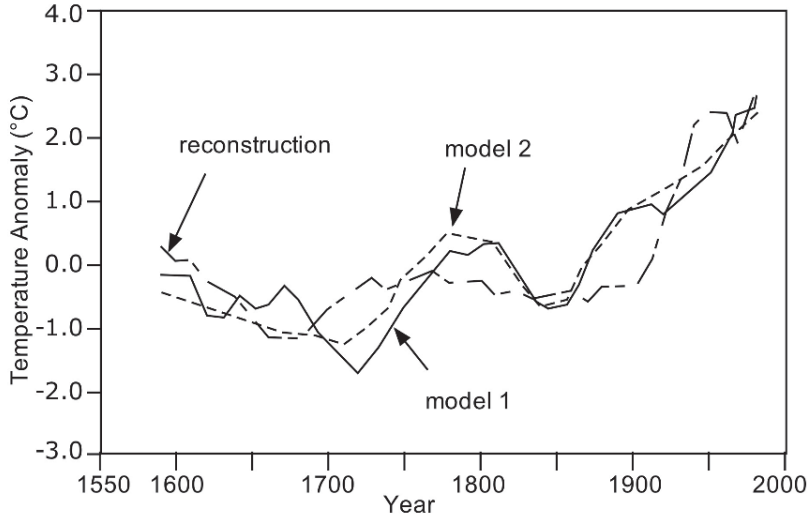

Figure 2.34. Comparison of several temperature reconstructions for China (adapted from Yang *et al*., 2002).

proportion to the implied areas covered by the data. A third approach ("H-res" China) averaged only the higher-resolution Chinese data sets. These are shown in Figure 2.34.

There is good correlation between the three reconstructions. A cool period occurred between about 300 and 800. A MWP occurred between 800 and 1100. An LIA was pronounced from 1400 to 1700. A relatively flat neutral environment occurred from 1700 to 1900. The temperature rose again after 1900. Although the 20th-century temperature appeared to reach (or exceed) medieval levels around 2000, the number of temperature series dropped off sharply in the 20th century.

Yang *et al*. (2002) compared their results for China with several global temperature reconstructions, as shown in Figure 2.35. Yang *et al*. claimed good correlation with the global reconstructions, but the Chinese data show a much more pronounced LIA, a slightly more suggestive MWP, and a lesser *hockey stick* rise after 1900. This is exactly what one would expect, considering that the global studies used means for the 20th century (rather than for the whole data set) that skews the result and generates a *hockey stick* artifice (see Section 2.4.3.1).

Ge *et al*. (2010) reconstructed regional temperatures in China as far back as 500 to 2,000 years ago from proxies, principally tree rings, stalagmites, lake sediments, and historical documents. These results display considerable diversity from region to region. However, a few features are somewhat consistent across the regions. The LIA is shown roughly as a "double dip" in temperature from 1600 to 1700 and from 1800 to 1900. A warm period, at least comparable to today, occurred variably from about 1000 to 1200.

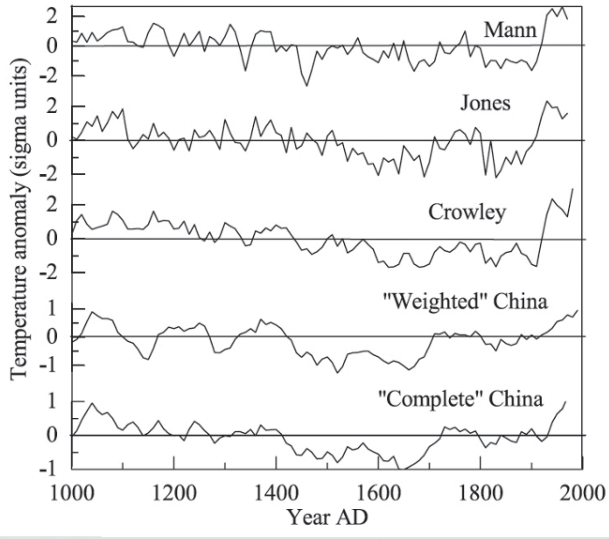

Figure 2.35. Comparison of several temperature reconstructions for China and the Northern Hemisphere (adapted from Yang *et al*., 2002). Mann = Mann, Bradley, and Hughes (1999); Jones = Jones *et al*. (1998); Crowley = Crowley and Lowery (2000).

2.4.7 Regional approaches to the MWP and the LIA

One problem with attempting to deal with a single average global or hemispheric temperature is that most of the regional variations tend to cancel out, and one ends up with only small apparent net changes. One can place these changes under a magnifying glass and derive great concern from a global variation of a few tenths of a degree, but the utility of that concept seems to be limited. When the writer lived in Texas, if a cold front moved through, the temperature could drop by 25°C in one hour. Summer temperatures were over 40°C and winter temperatures dipped below freezing. There is no utility to a Texan in knowing that global temperatures changed by, say, 0.3°C. Understanding how the climate of the Earth has varied with time, and how the Earth responds to changes in the Sun as well as to anthropogenic effluents and activities, requires a more regional approach, as discussed in Soon and Baliunas (2003a, b). The changes in global average temperature don't even begin to describe the regional and local hardships and problems that derive from climate change.

The different sensitivities of proxies to climate variables and the potential time-dependence of the proxy-climate correlation require careful calibration and verification on a location-by-location basis; the emphasis on local results by Soon and Baliunas (2003a, b) avoided the difficulty of inter-comparing disparate proxies but did not generate a synoptic global view. Thus, Soon and Baliunas (2003a, b) gave up on quantitative synthesis of many proxies into single global or NH average temperatures, because, even for the same location, different proxies may yield

different climate expressions simply because of their different sensitivities to local climatic variables. Soon and Baliunas (2003a, b) suggested that a compact mathematical representation of individual proxy variations (e.g., Mann*et al*., 1998), without full understanding of proxy-climate calibration relations, may yield overconfident results. Furthermore, democratically combining large numbers of proxies of highly variable quality tends to dilute the reliable proxies, producing little more than noise.

Ljungqvist (2011) pointed out:

> ''Considerable effort has been made during the last decade to reconstruct global or northern hemispheric temperatures for the past 1,000 to 2,000 years in order to place the observed 20th century warming in a long-term perspective. Less effort has been put into investigating the key question as to what extent earlier warm periods have been as homogeneous in timing and amplitude in different geographical regions as the present warming. It has been suggested (by some, particularly Mann *et al*.) that late-Holocene long-term temperature variations, such as the *Medieval Warm Period* (MWP) and the *Little Ice Age* (LIA), have been restricted to the circum-North Atlantic region (including Europe) and have not occurred synchronic in time with warm and cold periods respectively in other regions This view has, however, been increasingly challenged through the ever growing amount of evidence of a global (or at least northern hemispheric) extent of the MWP and the LIA that have become available. A main obstacle in large-scale temperature reconstructions continues to be the limited and unevenly distributed number of quantitative paleo-temperature records extending back a millennium or more. The limited number of records has rendered it impossible to be very selective in the choice of data. Paleo-temperature records used in a large-scale temperature reconstruction should preferably be accurately dated, have a high sample resolution and have a high correlation with the local instrumental temperature record in the calibration period. The number of long quantitative paleo-temperature records from across the globe, of which a majority are well suited for being used in large-scale temperature reconstructions, have been rapidly increasing in recent years. Thus, it has now become possible to make regional temperature reconstructions for many regions that can help us to assess the spatio-temporal pattern and the MWP and LIA. Only by a regional approach can we truly gain an understanding of the temperature variability in the past 1–2 millennia and assess the possible occurrence of globally coherent warm and cold periods.''

Ljungqvist (2011) then went on to produce six regional reconstructions of temperatures over the past two millennia. These included:

(1) warm-season temperatures of Scandinavia north of 60°N;
(2) warm-season temperatures for northern Siberia;
(3) annual mean temperatures for Greenland;
(4) warm-season temperatures for the Alps region of Central Europe;

(5) annual mean temperatures for China;
(6) annual mean temperatures for the whole of the North American continent.

The specific proxies used by Ljungqvist are listed in his paper. ''Only proxy records with reasonably high resolution (multi-decadal or better) were utilized and records with lower resolution were instead used for the purpose of verifying the reconstructions.'' Unlike many others who produced reconstructions, Ljungqvist did not use PCA methods that apply weights to the various proxies, thus further reducing an already sparse data set. Instead, he used a method that basically averages overlapping proxies (''composite-plus-scale''). According to von Storch *et al*. (2007), this method, though simpler, ''clearly displays a better performance''. His results for all six regions clearly show a significant MWP and a LIA. For Scandinavia north of 60°N, northern Siberia, and Greenland, the MWP was clearly warmer than current temperatures and the LIA bottomed out between 1600 and 1850. Current temperatures in Central Europe and China exceed those in the MWP. Current temperatures in North America are approaching those of the MWP.

Ljungqvist concluded:

> ''Temperature changes, on centennial time-scales, occurred rather coherently in all the investigated regions—Scandinavia, Siberia, Greenland, Central Europe, China, and North America. ... Large-scale patterns as the MWP, the LIA and the 20th century warming occur quite coherently in all the regional reconstructions presented here but both their relative and absolute amplitude are not always the same. Exceptional warming in the 10th century is seen in all six regional reconstructions. Assumptions that, in particular, the MWP was restricted to the North Atlantic region can be rejected. Generally, temperature changes during the past 12 centuries in the high latitudes are larger than those in the lower latitudes and changes in annual temperatures also seem to be larger than those of warm-season temperatures. In order to truly assess the possible global or hemispheric significance of the observed pattern, we need much more data. The unevenly distributed paleotemperature data coverage still seriously restricts our possibility to set the observed 20th century warming in a global long-term perspective and investigate the relative importance of natural and anthropogenic forcings behind the modern warming.''

However, the rate of temperature increase in the 20th century appears to be greater than that of the MWP. Some of Ljungqvist's results for various regions are shown in Figure 2.36. A comparison of Ljungqvist's results for the NH with those of Moberg *et al*. (2005) and Mann *et al*. (2008) is shown in Figure 2.37.

Ljungqvist's results show a peak temperature around year 950 that is 0.12°C warmer than the temperature in year 2000. His estimated temperature during the depth of the LIA in year 1680 was 0.75°C lower than that in year 2000, and 0.87°C lower than that at the height of the MWP in year 950.

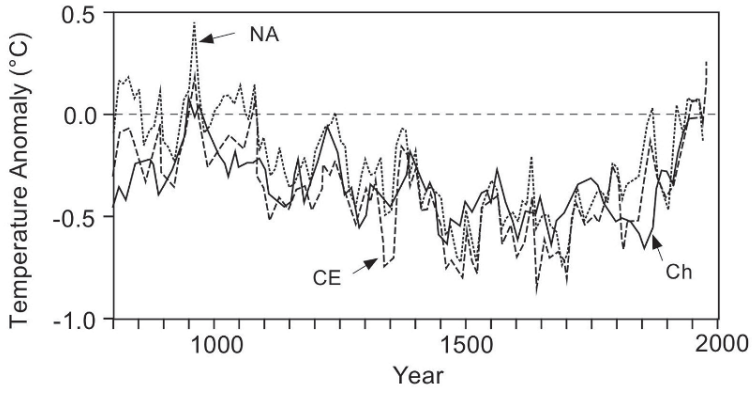

Figure 2.36. Annual mean temperature reconstructions for North America (dotted line); China (solid line); and Central Europe (dashed line) (adapted from Ljungqvist, 2011).

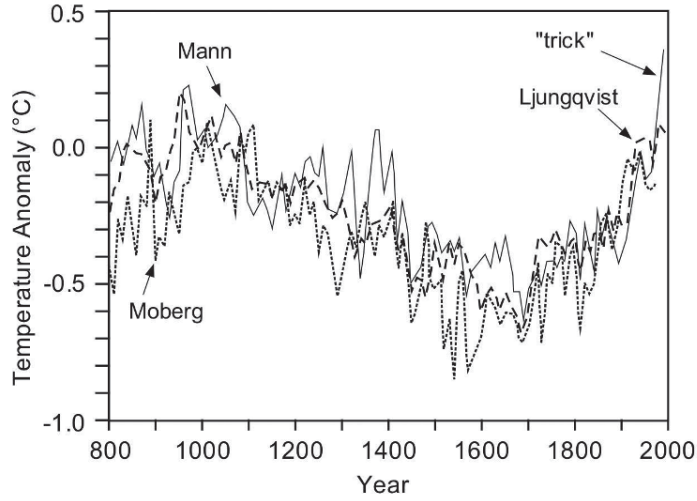

Figure 2.37. Temperature reconstructions for the NH as estimated by decadal means of Moberg *et al.* (2005); the "error-in-variables" (EIV) regression method variant of Mann *et al.* (2008); and and the extra-tropical Northern Hemisphere reconstruction by Ljungqvist (2011) (adapted from Ljungqvist, 2011). The "trick" was used by Jones and Mann to hide the discrepancy between measured temperatures (trick line) and proxies in the late 20th century.

2.4.8 Borehole measurements

Another approach for estimating past millennial temperatures is analysis of borehole temperatures. According to Pollack and Huang (2000), the fundamental concept behind subsurface temperatures as a climate proxy can be succinctly stated: if the Earth's atmosphere experiences a warming or cooling, the soil and rock in contact with the atmosphere will feel this change. Such temperature changes at the Earth's

solid surface then propagate into the subsurface by heat conduction through the soil and rock. The process is analogous to the warming of a cold ceramic cup after hot tea is poured into it. The interior surface of the cup experiences an increase in temperature, which then propagates through the wall of the cup and can be sensed a short time later on the exterior surface. Similarly, variations of temperature at the Earth's surface associated with climate change can be thought of as a time-varying boundary condition on the upper boundary of the solid Earth. But, whereas heat conducts through a cup in just minutes, temperature fluctuations at the Earth's surface take several hundred years to penetrate the upper few hundred meters of the subsurface. The Earth filters out high-frequency energy fluctuations and retains only the long-term trends of surface energy imbalance, recording surface changes as perturbations of underground temperature as a function of depth. These changes in the energy balance at the Earth's surface are reflected in geothermal records whenever the underlying physical processes are sustained.

The present temperature distribution below the surface is the end product of variable surface temperature acting over past epochs of time to transfer heat by conduction down into the subsurface. The deeper that a temperature is measured the more it encompasses a longer duration of surface temperature effects:

> "Many thousands of boreholes around the world have been subjected to temperature logging in the course of determining the terrestrial heat flux. Thus, an abundance of observations exists, but because of the many investigators and different measurement practices and techniques, the data are heterogeneous. The heterogeneity arises from different borehole depths, different logging depth intervals, and variable information about thermo-physical properties, subsurface geological structure, and surface site characteristics. Even with such heterogeneity, however, quality data are sufficiently abundant and the analysis tools sufficiently flexible to allow credible climate reconstructions from these data at many sites around the world." (Huang *et al*., 1997, 2000).

Kilty (1997) explained how borehole measurements are used to estimate the ground surface temperature (GST), say, over the past 1,000 years. The system involves circular reasoning because researchers begin with a model and use the observed borehole temperatures to find the parameters of the model:

> "However, a common problem with obtaining the history of surface temperature from borehole temperatures is that heat conduction destroys information regarding long past temperature quite completely, and, therefore, many different temperature histories explain the borehole data equally well. Quite a few of these histories oscillate in temperature wildly. By including a penalty for deviating from the initial model the program drives the final solution toward some unique result, and, if the initial model is smooth, the solution is also smooth." (Kilty, 1997)

One can allow either a loose or a tight constraint on variations from the original model. Kilty (1997) described the process of recovering past temperature from boreholes as an "ill-conditioned problem". Many doubts are raised about the

veracity of borehole measurements in resolving past temperature variations. Only through circular reasoning by limiting variations from an assumed model can a unique result be obtained. Ogilvie and Jonsson (2001) also discussed the difficulties with borehole measurements, including the "uncertain relation between ground temperatures and atmosphere temperatures".

Jo Nova[13] provided a valuable review and summary. As Nova points out, there is "vastly more detail in ice cores, and we wouldn't bother with boreholes at all if there were glaciers conveniently located all over the world". ... "The pro side of boreholes is that there are thousands of measurements, and they are spread all over the land masses of the globe (all bar Antarctica). On the downside, it's hard to calibrate, and doesn't include the ocean". And they produce "highly smoothed past temperatures". Jo Nova reported that "it takes about 100 years for [a surface] perturbation to reach a depth of 150 m, and 1,000 years to reach 500 m depth. Boreholes are handy because they assess land areas that have few other proxies".

Nova reviewed a series of published papers by Huang and co-workers from 1997 to 2008. The latest paper is Huang *et al.* (2008). The borehole data show a broad peak temperature around year 1300 and a minimum around year 1770, corresponding roughly to the MWP and LIA. However, proxies suggest that the peak at the MWP was about 300 years earlier. Over the years, changes were reported in the relative height of the MWP and depth of the LIA borehole results. In addition, each study seemed to employ a different selection of borehole data. Noting that, as the years went by, the height of the MWP peak temperature declined, Nova raised the suspicion that the analyses were somehow tainted to make current temperatures higher than those during the MWP. The results of 1997–2000 indicated a much higher MWP peak temperature than the results of 2008. Data from Huang *et al.* (2008) are shown in Figures 2.38 and 2.39.

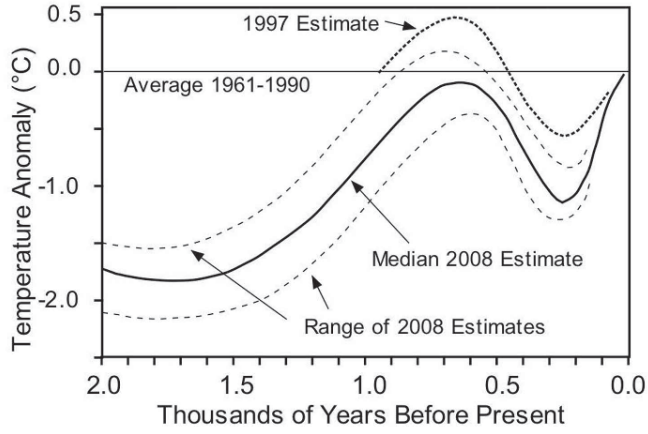

Figure 2.38. Borehole estimate of global average surface temperature over the past 2,000 years (Huang, 2008).

[13] *http://joannenova.com.au/2012/11/the-message-from-boreholes/#more-24964*.

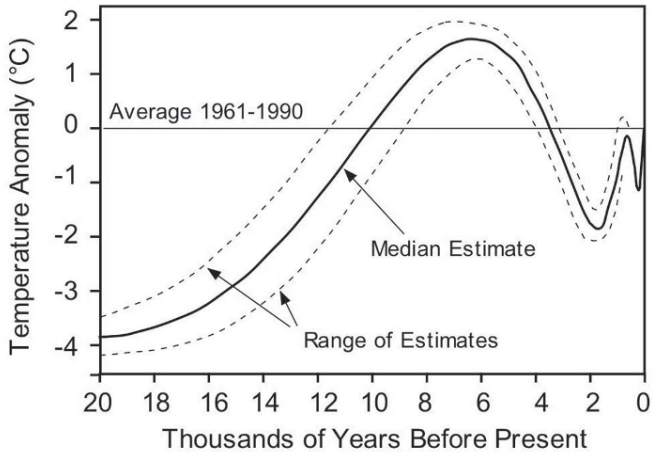

Figure 2.39. Borehole estimate of global average surface temperature over the past 20,000 years (Huang, 2008).

2.4.9 Arctic environment change

Overpeck *et al.* (1997) presented a compilation of paleoclimatic records from lake sediments, trees, glaciers, and marine sediments that provided a view of circum-Arctic environmental variability over the last 400 years. However, the instrumental record of Arctic climate change is brief and geographically sparse. In their article, the authors used the paleo-environmental record to assess the climate events of this century from the perspective of the last four centuries. They compiled a variety of complementary paleo-environmental indicators of climate from around the entire Arctic. This perspective permits the visualization of natural sub-decadal to century-scale climate variability in the circum-Arctic region. The results are shown in Figures 2.40a, b. It can be seen that, in general, the period from 1600 to 1925 was cold, although some intermittent warm periods were interspersed in some of these time series. Nevertheless, 22 out of 29 series were predominantly cold during this era, and the other 7 were variable. Therefore, there is clear evidence of an LIA.

The data after 1925 vary with the time series as follows:

(1) Strong warming: 12 series.
(2) Warming followed by moderate cooling (or variable): 13 series.
(3) Warming followed by strong cooling: 4 series.

In 17 out of 29 series, the predominant trend in the late 20th century was cooling. In 4 of the 17, the cooling was strong. We may therefore conclude that, in Arctic areas:

(1) the era 1600–1925 was relatively cold;
(2) compared with 1600–1925, the era that followed showed considerable warming;
(3) warming after 1925 was not consistent, and the majority of Arctic sites late in the 20th century were either in a cooling trend or were variable.

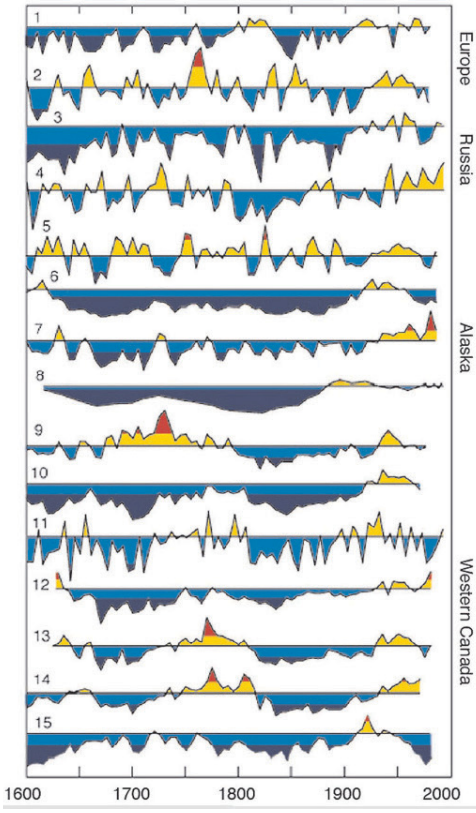

Figure 2.40a. (Left) Standardized 400-year proxy-climate records of surface air temperature. Red indicates temperatures greater than one standard deviation warmer than average for the reference period (1901–1960), whereas dark blue indicates at least one standard deviation colder than this average (based on Overpeck, *et al.*, 1997, with permission of *Science* J)

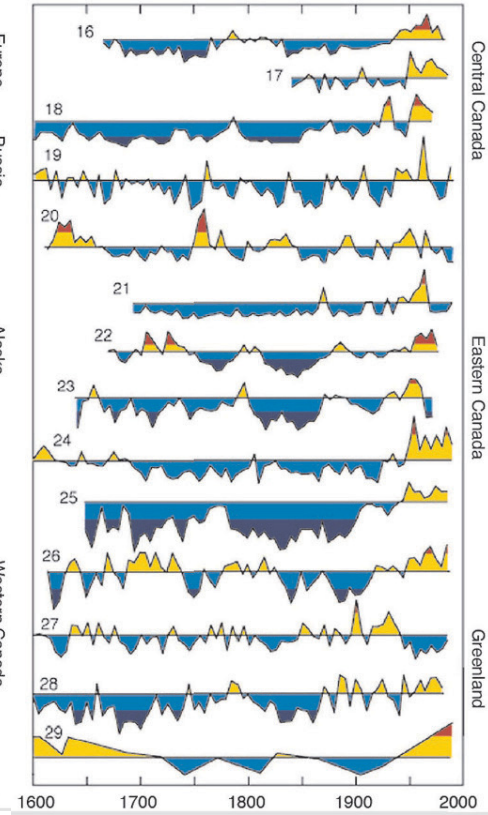

Figure 2.40b. (Right) Same as 2.43a but for sites in Canada east to Greenland. All series are presented as five-year averages except for sites 8 and 29, which are plotted at their original lower resolution. All time series represent surface air temperature except for site 29, which represents sea temperature (based on Overpeck, *et al.*, 1997, with permission of *Science* J)

Overpeck *et al.* (1997) seem to have been biased toward emphasizing global warming. They reported:

> "From 1840 to the mid-20th century, the Arctic warmed to the highest temperatures in four centuries. This warming ended the *Little Ice Age* in the Arctic and has caused retreats of glaciers, melting of permafrost and sea ice, and alteration of terrestrial and lake ecosystems."

However, they admitted that peak temperatures occurred around 1945, but did not discuss the cooling that occurred after that date. The data do not seem to

support their conclusions.[14] Overpeck *et al*. said: ''Half of the post-1840 warming (about 0.75°C) took place from 1840 to 1920. . . .'' Overpeck *et al*. struggled to find a rationale for this warming. The cooling trend from 1950 to 1970 was also a concern. They suggested that ''the observed slowdown in warming from 1950 to 1970 may have been influenced by the increase in Arctic tropospheric aerosols that occurred after 1950''. Aerosols have been proposed after the fact to deal with recent cooling (see Section 3.4.5).

Miller *et al*. (2010b) wrote a review of ''Temperature and precipitation history of the Arctic'' in which they report large temperature fluctuations (as much as ± 1°C during the past two millennia.

In their discussion of historical climate in Iceland, Ogilvie and Jonsson (2001) defined three separate viewpoints regarding Icelandic climatic change. One may be termed the uniformitarian view. As this name implies, it suggests a fairly constant climate, with only minor variations. A second may be called the ''deterioration'' view. This involves a transition from a relatively favorable early climate to an unfavorable later climate (i.e., an LIA). A third may be termed the ''relative'' view. This emphasizes that climate is constantly changing and suggests that even if there were relatively long cold or warm time periods, they nevertheless encompass large annual to decadal variability. Ogilvie and Jonsson provided an extensive review of journals written by Icelanders or Europeans from 1790 through the 1920s.

One 1914 book was definitely uniformitarian and claimed that ''the climate of Iceland did not change markedly from Iceland's settlement to 1914''. If that were true, it would cast significant doubt on the applicability of the LIA to Iceland.

Another 1914 book by a different author reached the opposite conclusion. For example, it was claimed that the first inhabitants of Iceland were able to grow grain but this later became impossible as the climate worsened. Several other writers (from Denmark and Norway) at a later date reached the uniformitarian conclusion. The sources of information become increasingly diffuse as one goes back in time. Ogilvie and Jonsson (2001) provide historical data on various aspects of Iceland. The sea ice index was plotted from 1600 to 1850, and from 1850 to 1990. Over the period 1600–1850, the sea ice index was a minimum during the 17th century and was highest from 1750 to 1850. Over the period 1850–1990, the sea ice index was highest before 1920, dropped to near zero from 1920 to 1967, and increased after 1967. The decrease starting around 1920 can hardly be due to greenhouse gases. The increases after 1967 are contrary to the expectation from greenhouse alarmists.

As in most studies of climate, Ogilvie and Jonsson (2001) provide many crosscurrents and conflicting indications that are highly stimulating but not fully revealing. Figure 2.41 shows the variation of Iceland temperatures since 1830.

[14] Some time ago, I was watching a boxing match on television. The announcer was enamored with the favorite and kept emphasizing how well he was doing, whereas my view indicated quite the opposite. Then there is the story of the boxer who returned to his corner between rounds and his trainer told him that his opponent never laid a glove on him. He complained that the referee must be hitting him because someone was administering a beating to him. Sometimes preconceived viewpoints affect what we perceive.

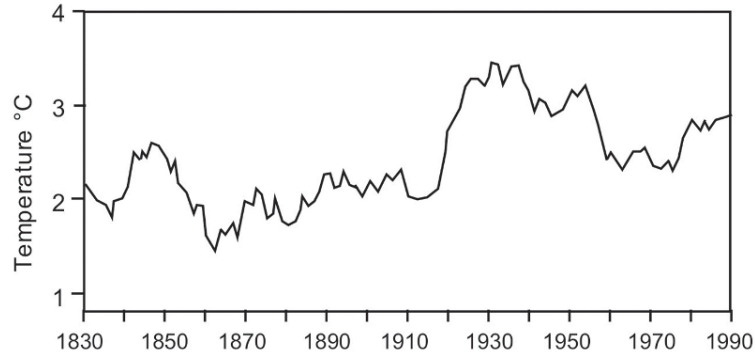

Figure 2.41. Ten-year running average Iceland temperatures (adapted from Ogilvie and Jonsson, 2001).

Table 2.2. Space–time matrix of temperature data.

Year⇓	Longitude = 0°					5°				360°				Proxies			
Lat⇒	0	5	10	…	70	0	5	…	70	0	5	…	70	1	2	…	N
850																	
851																	
852																	
…	Pseudo "real" data													Pseudo proxy			
1854																	
1855																	
1856					Real Data												
…																	
1980																	

Temperatures after 1855 were measured. Prior to that date, proxies were used. There was a rise around 1920, but, after 1950, temperatures fell. The rise around 1920 can hardly be due to greenhouse gases.

2.4.10 How reliable are proxy methods?

In order to test the robustness of methods for processing proxy data, Mann *et al.* (2005) set up a model for proxy-processing evaluation. Smerdon and Kaplan (2007) commented on this process. The description that follows is based on Smerdon and Kaplan (2007).

An artificial model is generated in which real temperature data are used for a calibration period, and a climate model is used to create real data prior to the calibration period. The climate model data are treated as if they were real for purposes of testing. A number of pseudo-proxies are created by selecting some of the

real data for particular locations, and adding noise to them. The point here is that a proxy is treated as an absolutely accurate temperature series at one location, but with noise superimposed. Then, the challenge is to reconstruct the real data from the pseudo-proxies.

They considered a time period from 850 to 1980. For each year, a temperature is assigned to each spatial cell covering a $5^\circ \times 5^\circ$ range of latitude and longitude. The latitude varies from 70°N to the equator and the longitude goes from 0° to 360° in 5° steps, as shown in Table 2.2. In actuality, we cannot know the actual temperatures over such a long time period. The true temperatures are known from 1855 to 1980 as shown by the rows 1855–1980 (for columns 0° to 360°) in Table 2.2. A global climate model is used to fill in the earlier data from 850 to 1854 (rows 850 to 1854 for columns 0° to 360°). Even though this data set is not real, it is treated as real for purposes of testing proxy processing. Then, a set of pseudo-proxies is constructed by selecting some of these time series, each pseudo-proxy representing one time series for a single latitude/longitude combination as shown in the far right columns. To emulate a real case, noise is added randomly to the various pseudo-proxies.

Then, the question is, given a set of pseudo-proxies as a starting point: how well does the data-processing scheme reproduce the original ''true'' data set?

Basically, in any statistical processing of proxy data, one begins by centering the data about its mean and normalizing it (scaling the deviations from the mean) in units of the standard deviation. This is called ''standardization''. There exists a calibration period during which there exist both proxies and actual measurements, and there is a longer period of time over which proxies are used to infer temperatures. One can standardize based on either the mean for the shorter calibration time period, or the mean over the entire data set. Smerdon and Kaplan (2007) found that, when standardization was carried out over the entire data set, the results were consistent and varied only slightly with the addition of noise to a data set. However, when the mean for only the calibration period was used for standardization, the results varied widely when noise was added. But Smerdon and Kaplan (2007) mentioned that, when standardization is carried out over the entire data set:

> ''Such a decision may sound benign, [but] it amounts to knowing the mean and standard deviation of the target field prior to the calibration interval—a luxury that would obviate the need for a reconstruction in the first place.''

Smerdon and Kaplan (2007) argued that the case where standardization is accomplished over the entire data set is artificial because one never has the full data set in reality. This would seem to suggest that the only practical way to process proxy data is based on standardization over the calibration interval, and this method is problematic because it produces variable results depending on the noise involved. As in the case of the M&M findings, the more noise that is added to the data, the more the result approaches a *hockey stick* form.

von Storch *et al.* (2007) also carried out a psuedo-proxy study. This was an outgrowth of their previous study (Zorita and von Storch, 2005). Their pseudo-reconstructions were performed in a climate simulation of the past millennium with

the climate model ECHO-G. They said: "This is not the same simulation used by von Storch *et al.* (2004) in their pseudo-proxy analysis of the Mann *et al.* (1998) reconstruction method, but a simulation with the same model using different initial conditions." They admitted that "There still exists a large uncertainty in the amplitude of past TSI [total solar irradiance] at centennial timescales" but that doesn't matter because however inaccurate the climate model may be, it is still taken as truth in order to determine the ability of reconstruction models to replicate it. They tested three methods for reconstruction of the past climate from proxies. These included (1) the MBH method (inverse linear regression of proxies from principal components), (2) direct linear regression of principal components from proxies, and (3) the so-called "composite plus scaling" (CPS) that in some ways amounts to simple averaging of proxies. The results of the climate model were taken as factual. Noise was added to these representations of actual data to generate proxies, since proxies always contain a certain amount of noise. Given these proxies, the question was how well the reconstruction method reproduced the "factual" data. They concluded:

> "The results of the three reconstructions methods of the Northern Hemisphere temperature in the ECHO-G simulation for both pseudo-proxy networks from simple white-noise and red noise models ... are that all pseudo-reconstructions ... underestimate the past variations of the mean temperature, and the estimated temperature in the centuries previous to the instrumental period is too warm. The bias, however, depends on the noise model, on the calibration variant and on the size of the pseudo-proxy network.
>
> "However, the simple method Composite plus Scaling provides, in the conditions tested in this study, better results and is more robust against changes in the proxy network and noise characteristics."

The ultimate test for reliability of proxies is how well they track temperatures. Of all the many papers on proxies that I have reviewed, very few if any have provided such data in any detail. Briffa *et al.* (1998) is a notable exception. They compared tree ring proxies with temperatures at many sites in the NH from 1880 to 1980 as shown in Figures 2.42 and 2.43.

> "When averaged over large areas of northern America and Eurasia, tree-ring density series display a strong coherence with summer temperature measurements averaged over the same areas, demonstrating the ability of this proxy to portray mean temperature changes over sub-continents and even the whole Northern Hemisphere. During the second half of the twentieth century, the decadal-scale trends in wood density and summer temperatures have increasingly diverged as wood density has progressively fallen. The cause of this increasing insensitivity of wood density to temperature changes is not known ..."

Although Briffa *et al.* pointed out the discrepancy between tree ring data and temperature after 1950, their assessment that proxies tracked temperatures prior to 1950 might be somewhat optimistic. The time period was short and the variability of temperature was rather small.

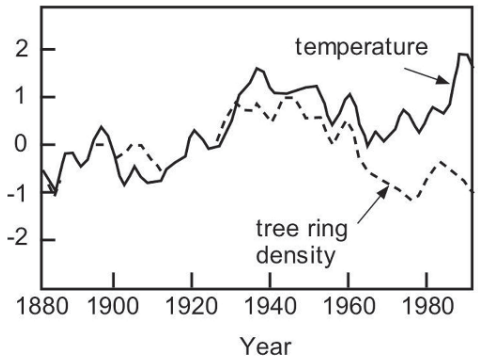

Figure 2.42. Comparison of tree-ring density with temperature for NH (Briffa *et al*., 1998).

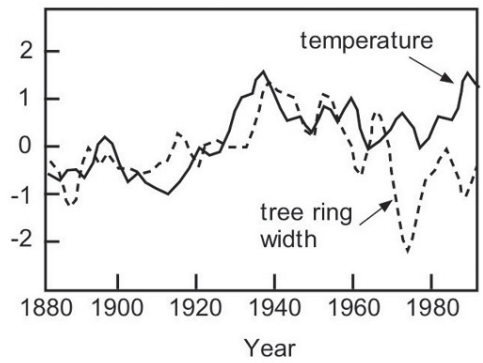

Figure 2.43. Comparison of tree-ring width with temperature for NH (Briffa *et al*., 1998).

Since the publication of this data in 1998, a number of additional papers have appeared dealing in one way or another with tree ring proxies.

Jacoby *et al*. (2000) said:

> ''Data from annual tree-ring widths are used to reconstruct May–September mean temperatures for the past four centuries. These warm-season temperatures correlate with annual temperatures and indicate unusual warming in the 20th century. However, there is a loss of thermal response in ring widths since about 1970''.

Thus they admit to a divergence problem after 1970. However, when one examines their data prior to 1970, the correlation of tree ring data with temperature even prior to 1970 appears somewhat subjective to this writer.

D'Arrigo *et al*. (2006a) came to the defense of tree ring proxies. They began their paper with the usual orthodoxy that ''recent warming in the Northern Hemisphere appears to have been unprecedented over the past millennium and that this warming is most likely a result of the anthropogenic release of greenhouse gases into the atmosphere''—which has not much to do with the reliability of tree ring proxies. They mention that D'Arrigo *et al*. (2006b) used simple averaging of tree-ring records (after accounting for differences in mean and variance over time), followed by linear regression''. Simple averaging is a step in the right direction, but how good are the proxies as temperature indicators? As is usual in almost all papers on proxies, the data for the calibration period were not shown. They do allude to the divergence between tree ring proxies and temperature reported by Briffa *et al*. (1998) and others cited in their paper, where they said:

> ''Theories for the cause (s) of this observed divergence, which may vary from site to site, include decreased temperature sensitivity due to warmer temperatures, drought stress, increased winter snowmelt and ozone effects. This divergence needs to be considered to avoid bias in dendroclimatic reconstructions; however it is not present everywhere. For example, tempera-

> ture-sensitive elevational treeline sites in Mongolia and the European Alps exhibit dramatic growth increases in recent decades. Greater attention to site selection (e.g. avoidance of drought-prone sites) and careful comparison of adjacent sites with regards to their ecological characteristics can help circumvent this problem. [It has been] demonstrated that the divergence appears to be limited to the recent period (after ~1950) and to trees from some northern locations (at some sites within ~55-70°N), and that there is no evidence for a comparable divergence prior to this time (e.g. during the *Medieval Warm Period*). These observations suggest a unique, anthropogenic cause for the recent divergence and argue very strongly that tree-ring temperature reconstructions for the past millennium should not be called into question based on these recent observations''.

One problem with site selection is that if one is attempting to estimate a global average temperature, one needs all the sites one can find. If only a few sites provide reliable data, how can one derive global or even hemispheric temperatures in the past? The claim that ''there is no evidence for a comparable divergence prior to this time (e.g. during the *Medieval Warm Period*)'' is unmitigated nonsense because there are no measured temperature data for that period and hence there is no way to ascertain whether such a divergence exists. D'Arrigo *et al*. (2006a) closed their paper with further homage to the orthodoxy of ''unusual recent anthropogenic warming on a hemispheric to global scale'' but their defense of tree ring proxies falls flat.

Wilmking and Singh (2008) discussed the ''divergence effect'' between measured temperatures and tree ring proxies in the 2nd half of the 20th century and pointed out that this ''seriously questions the validity of tree-ring based climate reconstructions, since it seems to violate the assumption of a stable response of trees to changing climate over time''. In their study they claimed to have

> ''... eliminated the 'divergence effect' in northern Alaska by careful selection of individual trees with consistently significant positive relationships with climate (17% of sample) and successfully attempted a divergence-free climate reconstruction using this subset''.

However, they did admit:

> ''The majority of trees (83%) did not adhere to the uniformitarian principle as usually applied in dendroclimatology. Our results thus support the notion that factors acting on an individual tree basis are the primary causes for the 'divergence effect' (at least in northern Alaska)''.

However, even the small subset of 17% of trees that are claimed to show good consistency with temperatures over the last century provide somewhat doubtful consistency. The diagram provided by Wilmking and Singh (2008) in their Figure 2 is a tiny little diagram that compresses the excursions between the temperature and tree ring curves. Nevertheless, accepting the claim that 17% of the trees show good correlation with temperature, the question arises as to whether it makes sense to select a subset of trees that happen to fit the temperature curve, and use these for

estimating temperatures a thousand years or more ago. Apparently Wilmking and Singh suggest that there occurs a "mixture of trees with stable and non-stable climate growth relationships" and the ones with stable relationships provide a basis for estimating past climates. However, it may be equally likely that all the tree ring records are randomized by other variables than temperature, and by happenstance, about 17% of the records have correlation coefficients with temperature that satisfy the criterion adopted by Wilmking and Singh (which is not impressive to this writer). There is then no great reason to believe that even these 17% of trees would remain as accurate temperature indicators over much longer periods. In other words, select a theory, choose a small subset of data that agrees with the theory, ignore the majority of data that disagree with the theory, and claim the theory is verified!

2.4.11 The paleoclimatic *cabal*

Wegman, Scott, and Said (2006) have suggested that the field, temperature history of the Earth, is dominated by a cadre (*cabal*) that is vitally concerned about the potential impacts of global warming, and supports the *hockey stick* result, as well as the procedure used to derive it. Wegman, Scott, and Said (2006) said:

> "If there is a tight relationship among the authors, and there are not a large number of individuals engaged in a particular topic area, then one may suspect that the peer review process does not fully vet papers before they are published. Indeed, a common practice among associate editors for scholarly journals is to look in the list of references for a submitted paper to see who else is writing in a given area and thus who might legitimately be called on to provide knowledgeable peer review. Of course, if a given discipline area is small and the authors in the area are tightly coupled, then this process is likely to turn up very sympathetic referees. These referees may have co-authored other papers with a given author. They may believe they know that author's other writings well enough that errors can continue to propagate and indeed be reinforced."

It was concluded:

> "It is immediately clear that Mann, Rutherford, Jones, Osborn, Briffa, Bradley, and Hughes form a clique, each interacting with all of the others."

Other cliques were identified as well. It seems clear that this *cabal* has considerable control over which papers are approved for publication in journals on the topic of the history of temperature of the Earth.

As McLean (2007b) said:

> "The peer-review process was established for the benefit of editors who did not have good knowledge across all the fields that their journals addressed. It provided a 'sanity check' to avoid the risk of publishing papers which were so outlandish that the journal would be ridiculed and lose its reputation. In principle this notion seems entirely reasonable, but it neglects certain aspects of human nature, especially the tendency for reviewers to defend their own (earlier)

> papers, and indirectly their reputations, against challengers. Peer review also ignores the strong tendency for papers that disagree with a popular hypothesis, one the reviewer understands and perhaps supports, to receive a closer and often hostile scrutiny. Reviewers are selected from practitioners in the field, but many scientific fields are so small that the reviewers will know the authors. The reviewers may even have worked with the authors in the past or wish to work with them in future, so the objectivity of any review is likely to be tainted by this association. Some journals now request that authors suggest appropriate reviewers, but this is a sure way to identify reviewers who will be favorable to certain propositions … In 2002 the editor-in-chief of the journal *Science* announced that there was no longer any doubt that human activity was changing climate, so what are the realistic chances of this journal publishing a paper that suggests otherwise? The popular notion is that reviewers should be skilled in the relevant field, but a scientific field like climate change is so broad, and encompasses so many sub-disciplines, that it really requires the use of expert reviewers from many different fields. That this is seldom undertaken explains why so many initially influential climate papers were later found to be fundamentally flawed. In theory, reviewers should be able to understand and replicate the processing used by the author(s). In practice, climate science has numerous examples where authors of highly influential papers have refused to reveal their complete set of data or the processing methods that they used. Even worse, the journals in question not only allowed this to happen, but have subsequently defended the lack of disclosure when other researchers attempted to replicate the work."

In peer-reviewing manuscripts submitted to science journals, the situation is in some ways analogous to the criminal justice system. There are two extremes: (1) make sure that you catch all criminals even if, in the process, you convict some innocent people; or (2) make sure that you do not convict any innocent people even if some criminals escape. A sane justice system seeks some middle ground between these extremes. In a similar way, we could hypothesize two extremes in reviewing scientific manuscripts: (1) make sure that every publication is *bona fide* even if, in the process, some valid papers are rejected; or (2) make sure that every valid paper is published even if a few bad papers sneak through the review process. It seems evident that the *cabal* has adopted the philosophy that only *bona fide* papers should be published, and they have appointed themselves as sole arbiters of what is *bona fide*, based on whether the manuscript supports the alarmist position.

It is noteworthy that M&M submitted a letter to *Nature* about a flaw in the MBH procedure. After a long (eight-month) reviewing process, M&M were notified that *Nature* would not publish this letter. *Nature* concluded it could not be explained in the 500-word limit, and one of the referees said he found the material was quite technical and unlikely to be of interest to general readers. Instead, MBH were permitted to make a coy disclosure in their July Corrigendum. In an online Supplement (but not in the printed text itself), they revealed the non-standard method, and added the unsupportable claim that it did not affect the results. There is

at least the appearance (and more likely the reality) that the *paleoclimatic cabal* was in complete control of the situation.

Mann *et al*. (2005) was written by the MBH team (1) to argue against von Storch *et al*. (2004), and (2) to claim that it makes little difference whether the proxy data are standardized by utilizing only the mean of the calibration period, or the mean of the entire historical data set. What would be humorous, if it were not sad, is the fact that the *paleoclimatic cabal* continues to relentlessly publish learned articles in defense of their methodologies, while pretending all the while that the valid criticisms of M&M do not exist.[15] Thus, for example, Mann *et al*. (2005) concluded:

> "Two widely used statistical approaches to reconstructing past climate histories from climate proxy data such as tree rings, corals, and ice cores are investigated using synthetic pseudo-proxy data derived from a simulation of forced climate changes over the past 1200 yr. These experiments suggest that both statistical approaches should yield reliable reconstructions of the true climate history within estimated uncertainties, given estimates of the signal and noise attributes of actual proxy data networks."

Mann *et al*. (2005) went on to say:

> "We find no evidence for the suggestion (e.g., von Storch *et al*., 2004) that real-world proxy-based temperature reconstructions are likely to suffer from any systematic underestimate of low-frequency variability. Our findings suggest that both standard methods that have been used in proxy-based reconstruction are likely to provide a faithful estimate of actual long-term hemispheric temperature histories, within estimated uncertainties."

M&M were never mentioned. Mann *et al*. (2007) continued the relentless defense of flawed methods:

> "Our results reinforce previous conclusions that CFR methods, correctly implemented and applied to suitable networks of proxy data, should yield reliable reconstructions of past climate histories within estimated uncertainties."

Mann *et al*. (2008) continued the charade.

In late 2009 and early 2010, an extensive set of emails between principal figures[16] in the *paleoclimatological cabal* was made public (by unknown, but clearly illegal, means). These emails revealed an apparent deeply imbedded agreement amongst these climatologists to promulgate their orthodoxy that the Earth's climate has hardly wavered over the past 2,000 years, and that CO_2 was the principal cause of unprecedented global warming in the 20th century. As Mosher and Fuller (2010) pointed out, these climatologists:

[15] Why am I reminded of Frank Morgan as the Wizard of Oz admonishing Dorothy to "pay no attention to that man behind the screen …"?

[16] Phil Jones and Michael Mann are the principal figures in the *cabal*, but several others were involved, such as Kevin Trenberth, Tom Wigley, Ray Bradley, Ben Santer, Gavin Schmidt, Jonathan Overpeck, and there are others.

> "... ruthlessly suppressed dissent by ensuring that contrary papers were never published and that editors who didn't follow their party line were forced out of their position. When *Freedom of Information* requests threatened to reveal their misbehavior, the emails showed them actively conspiring to delete emails to frustrate legitimate requests for information. Worst of all, one scientist threatened to delete climate data rather than turn it over, and that data is still missing."

Some of the worst gaffes were committed by Phil Jones (Hadley Climate Research Unit), who said (amongst other things):

> "And don't leave stuff lying around on anonymous download sites—you never know who is trawling them. McIntyre and McKitrick have been after the Climatic Research Unit ... data for years. If they ever hear there is a Freedom of Information Act now in the United Kingdom, I think I'll delete the file rather than send it to anyone.
>
> "I've just completed Mike's Nature trick [Michel Mann's publication in *Nature* where he replaced tree-ring proxy data with actual data because the tree-ring data went in the 'wrong' direction[17]] of adding the real temperatures to each series for the last 20 years (*i.e.* from 1981 onwards) and from 1961 for Keith's to hide the decline." [See Figure 2.22.]
>
> "We have 25 or so years invested in the work. Why should I make the data available to you, when your aim is to try and find something wrong with it?"

Mosher and Fuller (2010) provide great detail on this saga, colloquially known as "*climategate*". The Internet is full of commentary on this sorry situation in which scientists appear to have acted unprofessionally, unscientifically, and in some cases illegally. Kevin Trenberth, Senior Scientist at the National Center for Atmospheric Research, has emerged as a defender of the *cabal*.[18] He did admit to "lack of openness in sharing data and violations of the Freedom of Information Act" but he pointed out that five investigations failed to find any of the alleged misconduct. Unfortunately, these five investigations were conducted by friends of the *cabal*. He also asserted that "scientists would not make up stuff that could be disproven by others!" but the nature of paleoclimatic data is that they are not susceptible to proof, disproof, verification, or validation, and hence are a very safe field to work in. He cited an excerpt from a Phil Jones email:

> "I can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow—even if we have to redefine what the peer-review literature is!"

He implied that this was Jones's invention and he (Trenberth) had nothing to do with this. Whether this is true or not, this excerpt reveals the intellectual environment of the climate *cabal*. However, I have to agree with one slide in Trenberth's

[17] See: *http://climateaudit.org/2009/11/20/mike%E2%80%99s-nature-trick/*.
[18] *www.cgd.ucar.edu/cas/Trenberth/Presentations/ClimategateS.pdf*.

presentation that says the Internet is "An open sewer of untreated, unfiltered information and the American public is incapable of deciphering between facts, fiction and opinion".

The *cabal* refereed one another's papers submitted to journals, communicated improperly in a mutual back-scratch environment subverting the peer-review process, pressured journal editors not to publish papers contrary to the orthodoxy, conspired to write rebuttals to any papers that did slip through their barrier to publication of contrary views, and conspired to act in partnership to disparage and ridicule anyone with contrary findings. Several books present excerpts from the emails and provide interpretations of their implications, which are generally referred to as "climategate" (e.g. Mosher and Fuller, 2010).

One topic that annoys *cabal* members is the claim by some climatologists that persistent El Niños since 1976 were dominant in causing warming in the NH in the latter part of the 20th century. If this were true, it would suggest that the role of CO_2 in climate change may be far less than the orthodoxy believes. Thus, when the article by McLean *et al.* (2009) appeared in the literature suggesting an important role for El Niños as a dominant cause of warming in the NH in the latter part of the 20th century, it produced great animosity and consternation amongst the members of the *cabal*. This paper was reviewed and accepted by three independent referees. One referee commented in part: "I found the paper to be well-organized, well-written, and clear on the importance of the research ... The findings are likely to be of interest to a wide variety of readers." A second referee commented in part: "This very clear and well-written manuscript is an analysis of the relationship between MSU-derived and radiosonde-based tropospheric temperature variability and the Southern Oscillation, as modified by major tropical volcanic eruptions." After the paper was published, a flurry of emails was exchanged between *cabal* members, strategizing on how to carry out damage control for their orthodoxy by preparing a rebuttal. Soon afterwards, a group of *cabalists* (Grant Foster, James Annan, Phil Jones, Michael Mann, Jim Renwick, Jim Salinger, Gavin Schmidt, and Kevin Trenberth) decided to prepare a rebuttal, and, to ensure speedy publication, they pressured the editor of the *Journal of Geophysical Research* and suggested the following persons as possible reviewers for their submitted critique: Ben Santer, Dave Thompson, Dave Easterling, Tom Peterson, Neville Nicholls, and David Parker (with Tom Wigley, Tom Karl and Mike Wallace also mentioned.) All of these were professionally associated in some way to the Foster *et al.* group. Phil Jones commented: "All of them know the sorts of things to say—about our comment and the 'awful original', without any prompting." (They all subscribe to the same orthodoxy). McLean *et al.* describe the whole sordid story.[19] In their rush to rebut the original McLean article, the *cabal* posted their rebuttal on a website, in violation of JGR rules. Figures 3.33 and 3.35 to 3.39 provide clear evidence of the interaction of El Niños with climate.

[19] *scienceandpublicpolicy.org/originals/censorship_at_agu.html*.

2.4.12 The blogs

I note that, typically, whenever a climate scientist publishes a new paper or puts out a press release (however, in my day, no self-respecting scientist pushed his own wares via press releases), a flurry of websites appears reporting on, or commenting on, the original, but rarely providing a link to the original. These can be found via a Google search. Those websites that are configured to rank high in Google's prioritization scheme appear near the top, and finding a link to the original publication can range from difficult to impossible.

A similar thing happens when you are planning a trip. You seek a specific hotel on Google but you get many hotel compendium sites and it is difficult to reach the website of that specific hotel. In my opinion, there is something dreadfully wrong with Google's prioritization algorithm. Their principal basis for prioritizing a website seems to be based on how many other websites link to the website in question; they regard this as a vote of confidence by the public. What happens though, is that institutional sites and sites frequented by bloggers dominate, and sites run by individuals or specific commercial establishments, often containing the most content, get low priority. Thus, if you try to find any specific site that does not have extensive multi-links, it is likely to be buried in an ocean of heavily linked sites dealing with the general topic of the specific site you are trying to reach.

Blog sites are an amazing phenomenon. There are many websites on various topics (one important topic being climate change) that operate in the following way. Somebody (often anonymous, sometimes revealed) runs the site. That person posts a paragraph or an essay *du jour* on the blog, usually controversial, and, immediately, typically, hundreds of followers of that blog site send in cryptic remarks in response, sometimes one-liners, but sometimes considerably longer. Many climate blogs are dominated by followers of a single persuasion: alarmist or skeptic. Many of the responders to blogs hide behind the cloak of anonymity using a pseudonym. It is difficult to tell whether they are scientists or janitors. My experience with such sites is that the overwhelming majority of remarks seem to be pretty stupid or trivial, or both. Many get bogged down with trivia and tangential aspects. Some turn nasty and become personally abusive. On rare occasions, they may contain valid content. Some of these websites make false or misleading accusations against serious people, and their followers, hungry for any hint of impropriety or scandal, love it. In regard to climate change, many of the entries are nasty, insulting, insinuating, and unfriendly. We previously quoted Trenberth saying that the Internet is ''An open sewer of untreated, unfiltered information and the American public is incapable of deciphering between facts, fiction and opinion''.

Quite a number of blogs have evolved out of the controversies surrounding putative global warming. Some of these represent global-warming skeptics and some represent the *paleoclimatic cabal*. The great majority of these blogs are filled with short sound bites submitted by people who have not done the research needed in order to speak authoritatively on any subject, and, indeed, the bulk of the banter is nonsense. However, a few sites deserve special mention because the postings are often technically competent and worthy of review. (That does not necessarily imply

that posting by followers in response to the original posting are competent.) One blog, which is anti-establishment, is *www.climateaudit.org*, run by Steve McIntyre (of M&M). This website is distinguished from many other climatological websites in that McIntyre has the skill and invests the time to actually investigate the minute details of published papers from their large data sets. Many published climatological papers depend on large data sets and it is impractical for most others to check the veracity of the manipulations of the data. However, McIntyre does this, and, in the process, he has discovered a considerable amount of improper analysis by highly recognized climatologists. In essence, McIntyre has become the *de facto* reviewer for complex statistical analyses of climate data, although most climatologists try to ignore his critiques—which appear to always be on target. Some of his attempts to obtain original data from climatologists have been thwarted by evasion, obfuscation, and illegal violations of the FOIA (typically with approval by institutional managers). At least one technical paper was withdrawn after McIntyre published a critique on his blog. However, McIntyre restricts his blog to a relatively narrow range of subjects utilizing large data sets.

McIntyre has penetrated into the data and details and presents authoritative analyses. He has evolved to become the established arbiter of published climate data. Most published climate papers are complex and typically utilize extensive data sets. Even if journal reviewers were objective (which they typically are not), they would not have time (and typically skill) to penetrate into the details of the data and the analysis. Thus, they content themselves with a more cursory overview of the paper and rubber-stamp it if it was written by seemingly competent people. Worse still, is the situation when the reviewers and the authors have a cozy relationship in which they approve one another's manuscripts for publication. Clearly, those who work in the field of climatology (typically professors at universities) are beset by the need to obtain funds, supervise student research, make presentations, and teach courses. They do not have time to provide the review needed to assure that climate papers are sound and credible. Only a retired person, dedicated to reviewing other people's work, has the time to do this. McIntyre acts to keep the field honest. However, the *cabal* nevertheless continues to manipulate the journals according to its agenda. The scientific method is only fulfilled in a report if the data and procedures are made available for checking and reproduction by others. McIntyre provides specific instances where important papers in climate change have not made the data available and his attempts to acquire the data were parried by the *paleoclimatic cabal*. McIntyre also presents a few examples where scientists responded fully and promptly to his inquiries for detailed data. In general, the business of processing very large extended data sets by means of sophisticated statistical procedures does not lend itself easily to review by others. But the efforts made by workers in the field to make their data available leave much to be desired.

McIntyre provides detailed accounts of his difficulties in acquiring original proxy data used in published papers by members of the *paleoclimatic cabal* (Osborn, Briffa, Jones, Crowley, Lowery, Esper, Moberg, Juckes, Mann, *et al.*). In general, obtaining proxy data from these authors required endless requests and cajoling, and even then was only sporadically successful. In one case, he could not get any

satisfaction from the authors, and had to resort to 25 emails to the journal *Science*, and even then he was not able to recover the required data. He indicated that in several important cases, the authors utilized data from previous publications, but the data so acquired were not original, and had been processed (sometimes in ways that are suspect and difficult to trace). Seeking the original data required going back to multiple authors, leading inevitably to frustration.

Aside from the problem of acquiring the data, McIntyre found many inconsistencies and oddities in the handling of proxy data by the *cabal* members. He details these at length, although this writer found much of it difficult to absorb. One way or another, these manipulations of data all seemed to have a singular end result: amplification of the recent late-20th-century warming trend, and damping of the MWP, leading to the mantra that we are currently experiencing the warmest climate in over 1,000 years. It is particularly revealing to note some results of Briffa *et al*. (2001). Figures 2.20 and 2.21 show eight different reconstructions using various procedures with one preferred reconstruction. Note that all reconstructions decline in the second half of the 20th century while measured temperatures rise. The divergence is readily seen and Jones's "trick" of replacing the proxy data with measured data produces the hockey stick.

The major alarmist establishment blog is *http://realclimate.org*, run by Gavin Schmidt, which presents the viewpoints of the global-warming alarmists in a seemingly authoritative manner. Like its polar opposite (*climateaudit.org*), the *realclimate.org* blog is very extensive and detailed, and it is not possible to adequately describe all the material on this website. Only a few comments will be made.

Al Gore's film *An Inconvenient Truth* received a glowing review from *realclimate.org* including "admirable", and "for the most part he gets the science right". Al Gore received the Nobel Peace Prize, presumably partly based on this film. See Appendix I for a review of this film that provides the opposite view.

The *realclimate.org* blog provides a section entitled "Myth vs. Fact regarding the *hockey stick*". It is claimed that:

> "Numerous myths regarding the so-called *hockey stick* reconstruction of past temperatures, can be found on various non-peer reviewed websites, internet newsgroups and other non-scientific venues. The most widespread of these myths are debunked below."
>
> **Myth #0** *Evidence for modern human influence on climate rests entirely on the "hockey stick" reconstruction of Northern Hemisphere mean temperatures indicating anomalous late 20th-century warmth.*
> The response to this putative myth on *realclimate.org* is vague and confused. A better response from the alarmist position would be this: Even if we ignore the *hockey stick* and accept that global temperatures varied significantly in the MWP and the LIA, there is a significant difference today from those periods: CO_2 and CH_4 concentrations are much higher. The major issue regarding modern human influence on climate is whether the climate models are credible that predict significant future temperature growth from increases in CO_2 and CH_4 concentrations.

Myth #1 *The hockey stick reconstruction is based solely on two publications by climate scientist Michael Mann and colleagues (Mann* et al., *1998, 1999)*.
The *realclimate.org* response to this "myth" is that "this is patently false". To support their position, they mention: "nearly a dozen model-based and proxy-based reconstructions ... by different groups all suggest that late 20th century warmth is anomalous in a long-term (multi-century to millennial) context". However, the other publications typically utilized PCA with the mean chosen only for the calibration period, leading inevitably to some form of *hockey stick* if some of the proxies had an upward trend in the 20th century. It is not the number of papers that counts here. As Bob Foster emphasized, truth in science is not a matter of voting. The issue here is whether the reconstruction is correct, independently of whether the reconstruction was done in 2, 20, or 200 papers. This putative "myth" is irrelevant; there is no myth. Competent scientists do not doubt the hockey stick because it does not have enough publications to back it up. They doubt it because it has been shown to be based on incorrect math and inadequate data.

Myth #2: *Regional proxy evidence of warm or anomalous (wet or dry) conditions in past centuries contradicts the conclusion that late 20th-century hemispheric mean warmth is anomalous in a long-term (multi-century to millennial) context*.
This "myth" is presumably an allusion to the papers by Soon and Baliunas. The *realclimate.org* makes the point that Soon and Baliunas were rebutted by "a group of more than a dozen leading climate scientists" as though to say "our team is bigger than yours so it must be right". In addition, the "leading climate scientists" are members of the *paleoclimatic cabal*.

The rebuttal claimed that regional anomalies cannot characterize global anomalies. However, global anomalies are merely statistical averages of regional anomalies. Since temperature anomalies vary widely with location and time, a good deal of information is lost by averaging over all data because variations tend to get averaged out. As long as we understand that periods like the MWP and the LIA were not continuous and were not uniformly distributed, examination of regional anomalies one at a time can build up a much better and incisive picture of climate change than a concocted single average global temperature. And, from the regional studies, we learn that, while there were large spatial and temporal variations during the MWP and the LIA, the preponderance of the evidence suggests predominant warmth during the MWP and predominant cold during the LIA. Even in the 20th century, a century of predominant warming, one-third of all land measurement stations reported a decrease in temperature over that period. Climate is determined by a predominance of regional climates, not by unanimity of regional climates.

Myth #3: *The hockey stick studies claim that the 20th century on the whole is the warmest period of the past 1,000 years*.
The rebuttal claimed that "this is a mischaracterization of the actual scientific conclusions". It is claimed that "it is not the average 20th century warmth, but the magnitude of warming during the 20th century, and the level of warmth

observed during the past few decades, which appear to be anomalous in a long-term context''. However, this response does not jibe with statements made by alarmists.

Mann, Bradley, and Hughes (1999) said: ''... our results suggest that the latter 20th century is anomalous in the context of the last century. The 1990s was the warmest decade and 1998 the warmest year at moderately high levels of confidence.''

The IPCC Report said: ''The 1990s are likely to have been the warmest decade of the millennium in the Northern Hemisphere and 1998 is likely to have been the warmest year.''

Singer and Avery (2007) quote a number of similar alarmist claims from various sources.

- ''Nineteen ninety-nine was the most violent year in the modern history of weather. So was 1998. So was 1997. And 1996 ...''
- ''A nine-hundred-year-long cooling trend has been suddenly and decisively reversed in the past fifty years. ... Scientists predicted that the Earth will shortly be warmer than it has been in millions of years.''
- ''A climatological nightmare is upon us. It is almost certainly the most dangerous thing that has ever happened in our history.''
- ''Climate extremes would trigger meteorological chaos-raging hurricanes such as we have never seen, capable of killing millions of people; uncommonly long, record-breaking heat waves; and profound drought that could drive Africa and the entire Indian subcontinent over the edge into mass starvation.''

Myth #4: *Errors in the hockey stick undermine the conclusion that late 20th-century hemispheric warmth is anomalous.*

The *realclimate.org* response to this was: (1) the validity of the hockey stick is affirmed by the large number of researchers who agree with it; (2) the correction (see Mann *et al*., 2004) was not an admission of the criticisms of M&M, but only admitted to very minor data issues; (3) spurious allegations made by M&M are of no value because (a) M&M are not paleo-climatic specialists, (b) their articles were not published in legitimate science journals, (c) as proof of their lack of veracity, their submitted article was rejected by *Nature*.

To this, I would reply that (1) the affirmations are by members of the *paleoclimatic cabal* who are in league with one another; (2) it is agreed that Mann, Bradley, and Hughes (2004) was not responsive to the criticisms of M&M, more to the discredit of the *paleoclimatic cabal*; (3a) the validity of the arguments by M&M does not depend on their field of endeavor—but rather their knowledge of PCA which appears to be better than that of the *paleoclimatic cabal*; (3b) the validity of the articles does not depend on where they were or were not published (furthermore, this is a self-serving response because the *paleoclimatic cabal* appear to have control over the publication of manuscripts that criticize their methods); (3c) was it *Nature* that rejected the article or was it the reviewers for *Nature* who are members of the *paleoclimatic cabal*?

There are a great many more articles on the *realclimate.org* website. It is not practical to review more of them in this book. Only one more will be cited here.

In their zeal to alert the world to their perceived dangers of global warming, the *paleoclimatic cabal* have promulgated the beliefs that the temperature variations in the MWP and LIA were minor, and it is claimed that even the Holocene Optimum was restricted to summers in the NH. As a result, unfounded claims have been made about the late 20th century being the warmest period in the past 1,000 years, and possibly for the entire Holocene (10,000 years).

The *realclimate.org* blog stated:

> "The [Holocene Optimum] is a somewhat outdated term used to refer to a subinterval of the Holocene period from 5000–7000 years ago during which it was once thought that the Earth was warmer than today. We now know that conditions at this time were probably warmer than today, but only in summer and only in the extra-tropics of the Northern Hemisphere."

NOAA also stated: "In summary, the mid-Holocene, roughly 6,000 years ago, was generally warmer than today, but only in summer and only in the northern hemisphere."

McIntyre rebutted these statements based in part on Stott *et al.* (2004). Vollweiler *et al.* (2006) determined that global temperatures about 7,700 YBP and 3,500 YBP were warmer than today's temperatures. Sundqvist *et al.* (2010) found "a large majority of the investigated temperature reconstructions indicate that temperatures were warmer at the mid-Holocene (6000 YBP $\pm$ 500 yrs) compared to the preindustrial period (1500AD $\pm$ 500 yrs), both in summer, winter and the annual mean. By taking simple arithmetic averages over the available data, the reconstructions indicate that the northern high latitudes were 0.9°C warmer in summer, 0.5°C in winter and 1.7°C warmer in the annual mean temperature at the mid-Holocene (6000 YBP) compared to the recent pre-industrial". Kaufman *et al.* (2004) measured the spatio-temporal pattern of peak Holocene warmth over 140 sites across the Western Hemisphere of the Arctic with "clear evidence for warmer-than-present conditions at 120 of these sites". At the 16 terrestrial sites where quantitative estimates were obtained, local summer temperatures were on average 1.6°C higher than the average of the 20th century. It is noteworthy that about 8,000 YBP, the CO_2 concentration was about 260 ppm, and, since that time, it rose essentially linearly to about 285 ppm in the pre-industrial era (Indermuhle, *et al.*, 1999). There is no correlation at all between temperature and CO_2 concentration during this period.

The blog run by Professor Judith Curry is unique in some ways because it attempts to steer a middle ground and provide a forum for diverse views. Her postings cover a very wide range of topics ranging from detailed technical issues to socio-economic and political aspects. Her postings are mostly intelligent and articulate. Her website provides a forum for discussion of all the major issues relevant to climate. However, like most blogs, it gets bogged down with hundreds of responses, many of which are *non sequiturs* (*http://judithcurry.com/*).

Other relevant climate websites include: *http://bobtisdale.blogspot.com/*; *http://*

pielkeclimatesci.wordpress.com/; *http://wattsupwiththat.com*/; *http://www.climate4you.com*/; and *http://www.drroyspencer.com*; *http://www.SEPP.com*.

At the other end of the scale is the worst climate blog of all: *http://deepclimate.org*/.

This blog is supported by rabid evangelical alarmists who post repeated idiotic messages, often making personal attacks on legitimate people, while hiding behind the cloak of anonymity. The blog is operated by a masked person called "DC" who is a scurrilous low-life.

2.5 CONCLUSIONS ON MILLENNIUM TEMPERATURE HISTORY

Scientists abhor a vacuum. They can't seem to shrug their shoulders and admit that we just don't know the answer to a vexing problem. They demand explanations, however speculative they may be. Thus, we have theories that have gelled into beliefs on how life started on the Earth, how life begins from inanimate matter, how the universe began, how much life exists in the universe, and how the climate of the Earth varied over past millennia.

There is evidence that the Earth has been primarily in a warming trend during much of the 20th century, although there was a definite hiatus in this rise from 1945 to 1978 and the warming has neither been continuous nor universal. The 20th century also saw a steady rise in CO_2 concentrations in the atmosphere, due presumably to the burning of fossil fuels, land clearing, and cement production. Many scientists (and others) have legitimately become concerned that the greenhouse effect due to this CO_2 increase may be responsible for some or most of this observed rise in temperature, and, if left unchecked, could possibly lead to disastrous consequences in the future. In principle, if a sufficiently good global climate model can be produced, the effect of rising CO_2 concentration on Earth temperature in the future can be calculated. Unfortunately, there are so many variables and unknowns in the Earth system that such estimates can only be made as very rough approximations. As in any detective story, if direct evidence is not available, one falls back on circumstantial evidence.

One central issue in this regard is a comparison of the observed temperature rise in the 20th century with estimated variations of temperature in the past millennium or so. If past temperature fluctuations were small compared with the temperature rise in the 20th century, it would suggest that the temperature rise in the 20th century might be unique, unprecedented, and likely to be due to factors unique to the 20th century (e.g., greenhouse gases). On the other hand, if past fluctuations prior to industrialization were as large as, or greater than, those observed in the 20th century, it might suggest that the temperature rise observed recently might (at least partly) just be another fluctuation such as has occurred in the past. While this argument is not ironclad in either direction, it does provide some valuable insights. Accordingly, the quest for better space–time resolution of historical temperatures over the past couple of millennia has become an important part of the effort to understand the causes of global warming.

A number of studies of historical temperatures were conducted in the past, either based on anecdotal records, models of solar variability and climate responses to variable solar intensity, or more likely, based on proxies for past temperature such as tree rings, ice cores, etc. Although numerous papers have pointed out the confounding factors inherent in proxies, "in the land of the blind, a one-eyed man is king."[20] Therefore, despite the problems inherent in the use of proxies, many studies of proxies have abounded in the literature. Some of these fragmentary glimpses of the past have evoked a picture of significant variations in the past climate, with a notable warm period during medieval times, and a relatively cold period called the LIA from about 1400 to about 1850, depending on the criteria used for selection.

The first major global, synoptic, encompassing study of historical global average temperatures from proxies was the "MBH" study reported in 1998 (Mann*et al*., 1998). This was an audacious effort, encompassing over 1,000 proxies, which provided an unprecedented breadth to the study of historical temperatures. Nevertheless, the number of proxies diminished sharply going back in time, and the global coverage more than 400 years ago was minimal. To aid in processing all these data, a sophisticated statistical data-processing methodology (PCA)[21] was utilized. This was particularly remarkable because it was primarily the product of a Ph.D. dissertation by Michael Mann at the University of Massachusetts. This initial paper was followed by several more that extended the analysis further back into the past. The end result of these studies was a historical temperature profile that had the so-called *hockey stick* shape with a relatively flat profile for 1,000 years or more, followed by a sudden sharp rise in the 20th century. These papers were compact, full of jargon, and difficult to follow. Sufficient data for others to make independent checks were typically difficult to obtain. Nevertheless, they were impressive papers and outwardly seemed to be well done. As a result of this work, Michael Mann was rapidly catapulted from a newly graduated Ph.D. to a position of fame and renown and almost instantly became recognized as a world leader in paleoclimatology.

A number of climate scientists (and others), hungry for evidence of human-induced global warming, seized on the MBH results as a landmark. The *hockey stick* figure was reproduced and disseminated widely, being offered up as strong evidence of CO_2-induced global warming in the 20th century. The *hockey stick* was adopted by the Intergovernmental Panel on Climate Change of the U.N. (IPCC), Al Gore, and, in general, a majority of the paleoclimatology science community. The claim was made that the warming in the 20th century was unprecedented, that the 1990s was the hottest decade on record, and 1998 was the hottest year in at least the past millennium or two and maybe millions of years. Montford (2010) said:

> "Every home in Canada was sent a leaflet quoting the [IPCC hockey stick] and warning of the dangers of climate change. School books told children that the hockey stick meant that the world had to change. Politicians told voters that only they could save people from the threat. ... Insurers, newspapers,

[20] In modern terms, we may say: "It is the only game in town."
[21] Also known as "empirical orthogonal functions" (EOFs).

magazines, pamphlets, and websites were all in thrall to its message; the hockey stick swept all before it.''

About five years later, M&M rained on the *hockey stick* picnic. M&M were experts in manipulating large noisy data sets, which is just the problem faced in reconstructing the Earth's climate from proxies. In a series of papers and informal reports, they clearly showed that:

(1) MBH made an innocent-looking mistake in the PCA by standardizing with a mean based only on the calibration period, instead of a mean based on the entire time period covered by the data. As it turns out, this unwittingly led to a chain of events that placed undue emphasis on a few highly suspect proxies that produced the *hockey stick* result while ignoring most of the proxy data in the study.
(2) Use of certain tree-ring data by MBH was unjustified because much of the observed growth in the 20th century was due to CO_2 fertilization and other factors, rather than a rise in temperature.
(3) When a proper recalculation of the MBH data is performed, the result shows that, although there was indeed a significant temperature increase in the 20th century, there were comparable high temperatures earlier in the past millennia.

Thus, the bases for the claim that the 20th century exhibited an unprecedented temperature rise, and that the 1990s and 1998 were the hottest in the past several thousand years, were undermined. In addition to this, there is another factor not usually discussed by the critics. The correlation of the proxies with measured temperature during the calibration period is usually poor, and the extrapolation backward in time for periods much longer than the calibration period is an unsupportable matter of faith. The use of all proxies democratically mixes in many poor ones with the few good ones, and produces mainly noise.

The responses to the findings of M&M are interesting. Instead of issuing a *mea culpa* and going on from there, Mann dug in his heels and protected turf from truth. He issued a response to the M&M charges that is a masterpiece of evasion and obfuscation, not even mentioning M&M or in any way dealing with their central issue. Most of the paleoclimatology community, which by and large adopted the *hockey stick* as its motif, cooperated by controlling which papers get published in the journals. In general, the U.N., Al Gore, and the climatological alarmists have simply ignored M&M and continued to vouchsafe the *hockey stick*, pretending that the criticism of M&M did not exist.[22] For those who are determined to raise the alarm to the world on the dangers of global warming, the *hockey stick* is too valuable as a public message to allow truth to interfere. In addition, if one standardizes against the calibration period, noisy data produce a *hockey stick* result if the data are rising during the calibration period. This raises questions about the reliability and utility of assembling proxy data into a global average temperature. Since the temperature was rising during the 20th century, use of 20th-century data for calibration or proxies can lead to misleading results. Use of PCA in the manner employed by MBH can exacerbate this problem.

[22] Think of *The Emperor's New Clothes*.

Anon. (M) presented a very detailed and generally objective review of surface temperature reconstructions for the past two millennia. Their conclusions are summarized below:

- "The instrumentally measured warming of about 0.6°C during the 20th century is also reflected in various proxy measurements."
- "Large-scale surface temperature reconstructions yield a generally consistent picture of temperature trends during the preceding millennium, including relatively warm conditions centered near 1000 (identified by some as the *Medieval Warm Period*) and a relatively cold period (or *Little Ice Age*) centered near 1700. The existence of a *Little Ice Age* from roughly 1500 to 1850 is supported by a wide variety of evidence including ice cores, tree rings, borehole temperatures, glacier length records, and historical documents. Evidence for regional warmth during medieval times can be found in a diverse but more limited set of records including ice cores, tree rings, marine sediments, and historical sources from Europe and Asia, but the exact timing and duration of warm periods may have varied from region to region, and the magnitude and geographic extent of the warmth are uncertain."
- "It can be said with a high level of confidence that global mean surface temperature was higher during the last few decades of the 20th century than during any comparable period during the preceding four centuries. This statement is justified by the consistency of the evidence from a wide variety of geographically diverse proxies."[23]
- "Less confidence can be placed in large-scale surface temperature reconstructions for the period from 900 to 1600. Presently available proxy evidence indicates that temperatures at many—but not all—individual locations were higher during the past 25 years than during any period of comparable length since 900. The uncertainties associated with reconstructing hemispheric mean or global mean temperatures from these data increase substantially backward in time through this period and are not yet fully quantified."[24]
- "Very little confidence can be assigned to statements concerning the hemispheric mean or global mean surface temperature prior to about 900 because of sparse data coverage and because the uncertainties associated with proxy data and the methods used to analyze and combine them are larger than during more recent time periods."[25]

[23] This statement reflects a generally prevailing implicit view that "the preceding four centuries" were normal, while the relatively higher temperatures at the end of the 20th century are comparatively abnormal. However, the preceding four centuries extend across the LIA, and therefore one might state the proposition differently: temperatures during the preceding four centuries were colder than they were at the end of the 20th century.

[24] This author cannot find any substantial evidence that temperatures were (as claimed) generally higher in the past 25 years than they were in 900.

[25] This author has very little confidence in estimates of temperature prior to 1600, let alone prior to 900.

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