

## The Beginning

Earth Day, April 22, 1998, was the day the hockey stick was born. On that day, Michael Mann, and co-authors Ray Bradley and Malcolm Hughes published a [paper](#) in the scientific journal *Nature* (referred to as MBH98). This paper claimed to reconstruct northern hemispheric temperatures from the past 600 years by examining temperature “proxies” found in nature. The most important of these proxies were tree rings, the size (and density) of which can be influenced by temperatures.

The results of this paper were dramatic. The temperature reconstruction it contained showed relatively flat temperatures for approximately 500 years followed by a sharp increase in temperatures over the last hundred years. The sharp increase formed a curve, which when attached to the end of a relatively flat line created the image of a “hockey stick.” It told the viewer current temperatures were higher than anything seen in hundreds of years. Looking at it, it was almost impossible to think anything other than, “Humans are causing a dramatic change in temperature.”

Not only was the resulting image powerful, it was also extremely definitive. MBH98 claimed modern warmth was unprecedented in 600 years with “roughly a 99.7% level of certainty.” This high degree of confidence was only reinforced by the paper saying it's conclusions weren't based on a single type of proxy (such as tree rings), but rather:

*the long-term trend in NH is relatively robust to the inclusion of dendroclimatic indicators in the network, suggesting that potential tree growth trend biases are not influential in the multiproxy climate reconstructions.*

The authors said their conclusions were almost absolutely certain. They said their results were so certain, you could throw out tree ring data (dendroclimatic indicators), their largest source of data, and they'd still get the same results. The authors were full of it. On page 51 of Michael Mann's book, he discusses an analysis he performed shortly after MBH98 was published:

*The tests revealed that not all of the records were playing an equal role in our reconstructions. Certain proxy data appeared to be of critical importance in establishing the reliability of the reconstruction—in particular, one set of tree ring records spanning the boreal tree line of North America published by dendroclimatologists Gordon Jacoby and Rosanne D'Arrigo.*

If “one set of tree ring records” was “of critical importance in establishing the reliability of the reconstruction,” the reconstruction could not have been “relatively robust to the inclusion of dendroclimatic indicators.” While Mann now casually admits the importance of such a small amount of data, neither he nor his co-authors ever made any effort to correct their paper on the point.

The next year, these authors published a new paper (MBH99), extending their hockey stick back another 400 years. In it, they concluded:

*The 1990s was the warmest decade [of the last millennium], and 1998 the warmest year, at moderately high levels of confidence.*

These two papers, collectively referred to as MBH, formed the basis for what is the most memorable image used in discussions of global warming. In 2000, Bill Clinton referenced it in his State of the Union Address. More importantly, in 2001, the International Panel on Climate Change (IPCC), an organization created by the United Nations to periodically release “assessments” of the state of knowledge on global warming, gave it prominent display. A summary made for government officials of its Third Assessment Report (TAR) even used MBH to conclude it likely “the 1990s has been the warmest decade and 1998 the warmest year of the millennium.” Out of hundreds of pages of documents, this image was selected to be the “public face” of global warming.

### Controversy

Controversy began when Stephen McIntyre and Ross McKittrick collaborated on a paper published in the journal *Energy & Environment* in 2003. They concluded:

*The particular “hockey stick” shape... is primarily an artefact of poor data handling, obsolete data and incorrect calculation of principal components.*

In response to this conclusion, Mann fabricated a story about McIntyre and McKittrick's results being based upon a faulty spreadsheet. However, he also said (page 123):

*The paper's dramatically different result from ours... was instead an artifact of the authors' having inexplicably removed from our network two-thirds of the proxy data we had used for the critical fifteenth-sixteenth century period.<sup>46</sup>*

This is untrue. McIntyre and McKittrick were forced to omit some data (but not two-thirds) because they couldn't figure out certain undisclosed methodological choices Mann had made. When McIntyre asked Mann to disclose what he had done, Mann refused. Because it was impossible to know what Mann had actually done, McIntyre simply emulated the process as best he could. Despite what Mann claims, the difference in methodology didn't affect any conclusions.

Peculiarly, the reference Mann [gives](#) says nothing about this claim. It says McIntyre and McKittrick were wrong, but gives totally different reasons.

This is especially strange as Mann wrote the article he references.

## The Follow-Up

*In this book, I attempt to tell the real story behind the hockey stick.*

*Michael Mann - Prologue*

Over the next two years, both “sides” of the controversy tried to support their position. Most notably, Mann was forced to publish a [corrigendum](#) by *Nature* in order to correct errors pointed out in his work, though he claimed, “None of these errors affect our previously published results.”

During this period, Mann and some of his colleagues started a blog, [RealClimate](#). McIntyre started a blog of his own, [ClimateAudit](#), to respond to things posted on RealClimate. Many discussions and arguments were made on these two web sites, eventually leading McIntyre and McKittrick to publish another paper in the journal *Geophysical Research Letters (GRL)*. Mann says of it (page 130):

*McIntyre and McKittrick had quietly dropped their erroneous original assertion (in their 2003 paper discussed in chapter 8 that the hockey stick was an artifact of bad data. Their new, albeit equally erroneous, assertion was that the hockey stick was an artifact of the conventions used in applying principal component analysis (PCA) to certain tree ring networks...*

While Mann claims the argument by McIntyre and McKittrick was new, it was one they had made in their first paper. The abstract of that paper states (emphasis added):

*The particular “hockey stick” shape... is primarily an artefact of poor data handling, obsolete data and **incorrect calculation of principal components.***

This fact is even acknowledged by Mann in his book (emphasis added - note #45)

*To be specific, they claimed that the hockey stick was an artifact of four supposed “categories of errors”:* “collation errors,” “unjustified truncation and extrapolation,” “obsolete data,” and “**calculation mistakes.**”

McIntyre and McKittrick had always been aware there was a problem with how Mann did PC calculations, so this was not a “new” issue. All that was new was McIntyre and McKittrick had realized the data errors they had found were mostly irrelevant to the MBH results. Having realized that, they began to focus more on the main issue, the PC calculations. In regard to this, Mann says (page 137):

*McIntyre and McKittrick used a different PCA convention in their 2005 paper. They centered the tree ring data over the long term (1400—1980). That's fine—in fact, long-term centering is actually the traditional convention...*

Clearly, Mann does not think they calculated their PCs incorrectly. Instead, he claims:

*Applying our selection rule to these data, using a modern centering convention indicated that the leading two PC series should be retained.... By misapplying a selection rule derived for one convention (modern*

*centering) to PCA results based on a different convention (long-term centering) [they] end up erroneously throwing out the proverbial baby with the bathwater.*

Both groups kept two PCs for the North America tree ring network (NOAMER). Mann claims this is wrong, that McIntyre and McKittrick should have kept more. This fairly simple claim is Mann's basis for dismissing the GRL paper:

*In effect, McIntyre and McKittrick had “buried” or “hidden” the hockey stick. They had chosen to throw out a critical pattern in the data as if it were noise*

In their GRL paper, McIntyre and McKittrick never said anything about how many PCs to keep. Instead, the paper is a discussion about how PCs were calculated with NOAMER being used as an example. Given Mann's claim has no possible connection to this paper, it's natural to try looking at another paper published by McIntyre and McKittrick that year. That paper was published in *Energy and Environment (EE)*, and it did discuss how many PCs get kept:

*If a centered PC calculation on the North American network is carried out (as we advocate), then MM-type results occur if the first 2 NOAMER PCs are used in the AD1400 network (the number as used in MBH98), while MBH-type results occur if the NOAMER network is expanded to 5 PCs in the AD1400 segment (as proposed in Mann et al., 2004b, 2004d). Specifically, MBH-type results occur as long as the PC4 is retained, while MM-type results occur in any combination which excludes the PC4. Hence their conclusion about the uniqueness of the late 20th century climate hinges on the inclusion of a low-order PC series that only accounts for 8 percent of the variance of one proxy roster.*

Rather than simply “throw out” data based upon a “selection rule,” McIntyre and McKittrick carefully considered what happens based on how much data is used. Mann's claim is exactly the opposite of the truth in relation to the EE paper (and is completely nonsensical in relation to the GRL paper). He continues this sort of misrepresentation (page 136):

*We employed a standard, objective criterion for determining how many PCs should be kept for each region.*

In the note he provides, he says the rule he used is called Preisendorfer’s Rule N. There is no evidence this rule was actually used on the tree ring networks. This claim first appeared years after the hockey stick was made, it wasn't supported by any of the program code released by Mann, and the [evidence](#) says it couldn't have been used.

### Mann Seeks Support

During any controversy, people are bound to join in on the arguments. The hockey stick controversy is no different. Mann refers to one example of this when he discusses work by Eugene

Wahl and Caspar Ammann (page 138):

*Wahl and Ammann demonstrated that the hockey stick was not an artifact of PCA conventions and that the basic result is robust as long as key proxy records are not thrown out (either explicitly as in the original 2003 McIntyre and McKittrick paper, or implicitly through the use of erroneous selection rules, as in their 2005 paper).*

Again, Mann repeats his misrepresentations of McIntyre and McKittrick's work. More importantly, he says Wahl and Ammann find the MBH “result is robust as long as key proxy records are not thrown out.” This is the exact result observed by McIntyre and McKittrick. It is the exact result Mann admits in his book. Everyone agrees if you keep that particular set of tree ring records, you get a hockey stick. If you remove it, you don't get a hockey stick.

Mann goes on to say Wahl and Ammann:

*showed that, had McIntyre and McKittrick subjected their alternative reconstruction to the statistical validation tests stressed in MBH98 and MBH99 (and nearly all related studies), it would have failed these critical tests.*

This is problematic in a number of ways. First, McIntyre and McKittrick never claimed to be offering an “alternative reconstruction.” They were merely testing what happened if certain changes were made. This serves the same purpose as running statistical validation tests. Both are ways of seeing how robust a paper's results are.

Second, Mann raises the issue of statistical validation tests. There are a variety of such tests, the two most important of which are RE and  $r^2$ . The closer the results of these tests are to one, the better the conclusion is. The problem is McIntyre had long criticized Mann for not publishing  $r^2$  verification scores which were practically 0 (very bad). Mann [calculated](#) these scores, but he never published the adverse results. When a committee formed by the United States House of Representatives [asked](#) Mann:

*Did you calculate the R2 statistic for the temperature reconstruction, particularly for the 15th Century proxy record calculations and what were the results?*

Mann simply [avoided](#) answering the question:

*I assume that what is meant by the “R2” statistic is the squared Pearson dot-moment correlation, or  $r^2$  (i.e., the square of the simple linear correlation coefficient between two time series) over the 1856-1901 “verification” interval for our reconstruction. My colleagues and I did not rely on this statistic in our assessments of “skill”*

He doesn't admit or deny calculating the scores. He simply says he and his coauthors didn't

“rely” upon them. Whether or not they relied upon the scores, those scores are obviously relevant to anyone looking at the hockey stick. It would especially have been relevant to the IPCC which [claimed](#) Mann and co-authors (emphasis added):

*estimated the Northern Hemisphere mean temperature back to AD 1400, a reconstruction **which had significant skill in independent cross-validation tests.***

Even worse, while the adverse results were hidden, Mann and his co-authors published  $r^2$  scores when they were helpful. Figure 3 of MBH98 shows  $r^2$  scores:

*Figure 3 shows the spatial patterns of calibration  $b$ , and verification  $b$  and the squared correlation statistic  $r^2$ , demonstrating highly significant reconstructive skill over widespread regions of the reconstructed spatial domain.*

Given all this, it is difficult to understand why Mann would bring up statistical validation tests. He has long hidden the fact his own work fails such tests, and there is no reason failing such tests would matter for McIntyre and McKittrick's results (they weren't doing an alternative reconstruction).

To add to the oddness, Wahl and Ammann actually show the failing MBH  $r^2$  scores.

## Congress

*A second flood, a simple famine, plagues of locusts everywhere  
Or a cataclysmic earthquake, I'd accept with some despair  
But, no, you sent us Congress, good God, sir, was that fair?  
Piddle, Twiddle and Resolve – 1776 (musical)*

By this point, the United States Congress had already gotten involved in the hockey stick debate. In 2006, it increased its involvement by commissioning two reports to study the controversy, the National Academy of Science (NAS) [Report](#) and the Wegman [Report](#).

The most important conclusion given by both of these reports deals with a methodological choice made by Mann involving principal component analysis (PCA). Mann used a non-standard implementation of PCA. His critics claimed this caused his method to “mine” for hockey sticks. If true, it would mean Mann's methodology inherently gave undue influence to that particular shape. Both reports acknowledged this criticism. The NAS Report said:

*As part of their statistical methods, Mann et al. used a type of principal component analysis that tends to bias the shape of the reconstructions.*

The Wegman Report said:

*The net effect of this decentering using the proxy data in MBH98 and MBH99 is to produce a “hockey stick” shape.*

Both reports agree the original hockey stick was created by a biased methodology. It sought hockey sticks in the data and gave them undue significance. Despite this, Mann says (page 164):

*The more extensive and authoritative NAS review, for example, had specifically dismissed the notion that PCA conventions had any substantial impact on our findings. As Bloomfield had put it at the NAS press conference, “the committee, while finding that the issues are real, [found] they had a minimal effect on the final reconstruction.”*

Rather than quote the NAS Report, Mann quotes a comment made in a press conference which isn't supported by the report. He does quote the report on page 161:

*The basic conclusion of Mann et al. (1998, 1999) that the late 20th century warmth in the Northern Hemisphere was unprecedented during at least the last 1,000 years has subsequently been supported by an array of evidence that includes both additional large-scale surface temperature reconstructions and pronounced changes in a variety of local proxy indicators*

However, this merely supports the conclusion of MBH, not the methodology. Indeed, nothing in the NAS Report actually supports Mann's work. Instead, it merely says other work reached the same

conclusions. This cannot possibly address the merits of Mann's work, a point expressed by Edward Wegman (the lead author of the report bearing his name):

*Method Wrong + Answer Correct = Bad Science.*

Despite this, Mann says:

*The NAS report was widely reported to be an affirmation of our work.*

### Projection

Mann flagrantly misrepresents the NAS report in regards to “bristlecones,” the type of tree the all-important tree ring data was taken from. Mann says of it (emphasis added - page 190):

*McIntyre also appealed to the conclusions of the 2006 NAS report to claim that our continued use of the very long bristlecone pine series was inappropriate. Yet this was a misrepresentation of what the NAS had concluded. The NAS panel expressed some concerns about so-called strip-bark tree ring records, which include many of the long-lived bristlecone pines. These trees grow at very high CO<sub>2</sub>-limited elevations, and there is the possibility that increases in growth over the past two centuries may not be driven entirely by climate, but also by the phenomenon of CO<sub>2</sub> fertilization – something that had been called attention to and dealt with in MBH99 (see chapter 4). The NAS report simply recommended efforts to better understand any potential biases by “performing experimental studies on biophysical relationships between temperature and tree-ring parameters”.*

This is a gross misrepresentation of the NAS report’s findings. From the very same page as the quote he offers (strip-bark is the type of bristlecones being discussed - page 52):

*While “strip-bark” samples should be avoided for temperature reconstructions, attention should also be paid to the confounding effects of anthropogenic nitrogen deposition (Vitousek et al. 1997)...*

McIntyre cited a conclusion from the very same page Mann was quoting from, yet Mann claims it was a misrepresentation. Clearly, the reverse is true. More importantly, it is clear bristlecones are a questionable data source. The NAS Report states this. It also acknowledges Mann's hockey stick was dependent upon bristlecones:

*For periods prior to the 16th century, the Mann et al. (1999) reconstruction that uses this particular principal component analysis technique is strongly dependent on data from the Great Basin region in the western United States.*

There's a final oddity to this issue. In a RealClimate [post](#), Mann's co-author Ray Bradley said:

*One final note: bristlecone pines often have an unusual growth form known as “strip bark morphology” in which annual growth layers are restricted to only parts of a tree’s circumference. Some studies have suggested that such trees be avoided for paleoclimatic purposes, a point repeated in a recent National*



*Academy of Sciences report (Surface temperature reconstructions for the last 2,000 years. NRC, 2006).*

Mann even commented on that blog post (inline response to comment #7), yet he now completely misrepresents the finding his co-author referred to in it.

His treatment of the Wegman Report is little better (page 164):

*The Wegman Report, commissioned by Joe Barton and published several weeks after the NAS report, seemed a transparent effort to further spread the attacks against our work. It uncritically repeated the old and tired McIntyre and McKittrick claim that the hockey stick was an artifact of the conventions used in a statistical (PCA) analysis...*

The most important fact about the Wegman Report is not actually found in the Wegman Report. Instead, it was [stated](#) by Gerald North, the chair of the panel which wrote the NAS report:

*CHAIRMAN BARTON. I understand that. It looks like my time is expired, so I want to ask one more question. Dr. North, do you dispute the conclusions or the methodology of Dr. Wegman's report?*

*DR. NORTH. No, we don't. We don't disagree with their criticism. In fact, pretty much the same thing is said in our report.*

The same point was reiterated by another member of the panel, Peter Bloomfield:

*MR. BLOOMFIELD. Thank you. Yes, Peter Bloomfield. Our committee reviewed the methodology used by Dr. Mann and his coworkers and we felt that some of the choices they made were inappropriate. We had much the same misgivings about his work that was documented at much greater length by Dr. Wegman.*

Mann claims the Wegman Report was just a repetition of McIntyre and McKittrick's arguments. The NAS panel agreed with the criticisms found in the Wegman Report...

## The Hockey Stick, Redux

*“Second verse, same as the first!”*

*I'm Henry the Eighth, I Am(song)*

In 2008, Mann published a [new](#) hockey stick. Mann describes it (page 190):

*With far more ice core and sediment records now available, we were able to obtain a meaningful reconstruction of the Northern Hemisphere average temperature for the past thirteen hundred years without using tree ring data at all. If tree ring data were used, the reconstruction could be extended, with some reservations, back over the past seventeen hundred years.*

Mann's critics claimed his original hockey stick was purely a product of a small amount of tree ring data. If his new reconstruction was free from that problem, it would be a major development. However, McIntyre almost immediately claimed to find problems with the new reconstruction:

*Stephen McIntyre wasted little time in launching a series of attacks on the PNAS paper, employing—it would seem—the strategy of throwing as much mud against the wall as possible and hoping that some would stick. Teaming up with his former coauthor Ross McKittrick, he submitted a short letter to the editor of PNAS claiming that our reconstruction used “upside down proxy data.”<sup>52</sup> That was nonsensical, as we pointed out in our response,<sup>53</sup> one of our methods didn't assume any orientation, while the other used an objective procedure for determining it.<sup>54</sup>*

As Mann explains, two different methodologies were used, Composite Plus Scale (CPS) and Error-in-Variables (EIV). However, there is nothing “nonsensical” about saying data was used upside down. The CPS methodology screens proxy records by directly comparing them to the local instrumental records. If the two records are similar enough, the proxy record gets used. The problem is proxy records can increase without meaning temperatures increased. If one were measuring the accumulation of snow/ice, larger values would be expected for cooling, not warming. This increase would be compared to an increase in instrumental temperatures, and thus the CPS methodology would treat the cooling as warming. This would cause the series to be used upside down.

EIV is similar to this, though it doesn't screen proxy series. Instead, it compares each proxy series to the temperature record and determines how similar the two are. If it finds a similar trend, but in the opposite direction, it “flips” the series upside down. This means both methodologies used by Mann are capable of using proxy series upside down despite the fact he claims such is “nonsensical.”

*McIntyre settled then on a more specific avenue of attack: our use of a small group of sediment records from Lake Korttajarvi in central Finland. But this was quite inconsequential and, ironically, we were the ones who had raised concerns about these particular data in the first place, not McIntyre. We had included*

*them for consideration only to be complete in our survey of proxy records in the public domain.*

The records Mann is referring to here are commonly called the Tiljander series. The four series are labeled Thickness, Lightsum, Darksum and XRD. Thickness and XRD are measured, but Lightsum and Darksum are derived from those measurements. This means using using all four series, which Mann did, results in double counting.

More importantly, these series were corrupted by human influence. The lake they were taken from began being influenced by farming and construction in the 1700s. The impact from human influence completely overwhelmed any temperature signal there may have been in the data. The people who originally published the series noted this and cautioned people not to use the modern portion of the series as a temperature proxy.

Since Mann's methodologies require calibrating proxy series to the instrumental record (1850-1995), it makes no sense to use series whose data has been corrupted in the modern periods. Any correlation which may be found is spurious, not caused by the proxy actually responding to temperature. Since the correlation is spurious, it could not have any connection to the temperature response the series were supposed to have before being corrupted. This is what caused two of the Tiljander series to be used with the opposite correlation as that suggested by the original authors.

### Mann's Nonsense

*In the online supplementary information accompanying publication of our PNAS article, we had both noted the potential problems with these records and showed that eliminating them made absolutely no difference to the resulting reconstruction.<sup>57</sup> McIntyre had thus attempted to fabricate yet another false controversy*

Mann acknowledged the authors warning not to use the data as he did, yet used it anyway. The reasoning he offers for such makes no sense: he says there is “no difference” if he uses the series. If there is no difference, why include them? The answer is simple. Using them makes a huge difference.

The main temperature reconstruction is barely affected by removing the Tiljander series because it includes the tree-ring data which was essential for Mann's original hockey stick. Mann's paper claims not to need that data to get a hockey stick, but that is only true if he includes the Tiljander series. If you remove both the Tiljander and tree ring series, there is no longer a hockey stick.

This point is confirmed by Gavin Schmidt, a coauthor of Mann's at RealClimate:

*Since the no-dendro CPS version only validates until 1500 AD (Mann et al (2008) ), it is hardly likely that the no-dendro/no-Tilj CPS version will validate any further back, so criticising how bad the 1000 AD*

*network is using CPS is hardly germane. Note too that while the EIV no-dendro version does validate to 1000 AD, the no-dendro/no-Tilj only works going back to 1500 AD (Mann et al, 2009, SI).*

A commenter at RealClimate noticed this remark and [asked](#):

*So just to be clear with regard to your response to 525. Under either method (CPS or EIV) it is not possible to get a validated reconstruction to before 1500 without the use of tree rings, or the Tiljander sediments.*

Schmidt responded:

*That appears to be the case with the Mann et al 2008 network.*

Mann himself has acknowledged this. From the [Supplementary Information](#) for a later paper:

*Additional significance tests that we have performed indicate that the NH land+ocean Had reconstruction with all tree-ring data and 7 potential “problem” proxies removed (see original Supp Info where this reconstruction is shown) yields a reconstruction that passes RE at just below the 95% level (approximately 94% level) back to AD 1300 and the 90% level back to AD 1100 (they pass CE at similar respective levels).*

The test used by Mann required his reconstruction pass at a 95% confidence level, and he acknowledges it can only do so by including either the tree ring data or the (nonsensically used) Tiljander series. The “false controversy” he claims McIntyre raised actually repudiates a central claim of Mann's paper:

*With far more ice core and sediment records now available, we were able to obtain a meaningful reconstruction of the Northern Hemisphere average temperature for the past thirteen hundred years without using tree ring data at all.*

### Nobody Agrees With Mann

Mann continues to spread disinformation (page 198):

*When Science in early September 2009 published an article by Darrell Kaufman and his colleagues showing the most dramatic hockey stick yet—a two-thousand-year reconstruction of Arctic temperature changes<sup>19</sup>—Stephen McIntyre and his forces went on the attack on the Internet,<sup>20</sup> immediately trumpeting the false claim that the work was compromised by bad data, despite the fact that whether or not the authors used the data in question made no difference to the result they obtained.*

The “data in question” are the Tiljander series which were once again used upside down. Despite Mann's claims, McIntyre never said this was the source of the Kaufman hockey stick (he primarily blamed another series, Yamal). Beyond that, a few months after the Kaufman paper came out, a corrigendum was [published](#). Included in it was this line:

*Record 20 was corrected to reflect the original interpretation of Tiljander et al. (S32) that X-ray density is related inversely to temperature.*

Mann claims McIntyre raised a “false claim” to attack a paper, yet the authors of that paper acknowledged his claim was correct. Even stranger, Ray Bradley was a coauthor on both Mann's 2008 paper and the Kaufman paper. Both papers made the same mistake, but only the Kaufman group admitted it.

### A Simple Point

For all the “technical” issues in Mann's papers, the controversy is actually very simple. Mann's papers give undue focus to a small amount of data. Even he and his supporters admit his original hockey stick was based entirely upon a small amount of tree ring data (which the NAS says should not be used to measure temperature). His latest hockey stick was the same, save he added upside down data which he couldn't possibly calibrate to temperature.

## Denouement

*“If you want a happy ending, that depends, of course, on where you stop your story.”*

*Orson Welles*

One of the most common defenses offered for Mann's hockey stick is other papers get the same result. The idea is if Mann got the right answer, criticisms of his work don't matter. This is a dumbfounding position, and the best response is that given above by Wegman:

*Method Wrong + Answer Correct = Bad Science.*

Mann's work has been highly publicized. If the glaring problems in it are overlooked, how can anyone trust other papers reaching the same conclusions? What is to stop those other papers from being just as flawed? Nothing. They cannot be ignored or dismissed because of flaws in Mann's work, but they should all be carefully examined rather than accepted on faith. Unfortunately, the same basic criticisms are leveled against practically every one of those reconstructions.

### General Issues

It would be impossible to discuss every paper showing a hockey stick. However, two general problems found in them can be covered. The first problem has already been shown, namely, mistakes don't get admitted. Mann's PC1 (the source of his hockey stick) was created with a biased methodology, and it was made up of data the National Academy of Scientists said should be avoided. Despite this, it was included in the latest IPCC report.

Related to the above, papers with hockey sticks tend to rely on the same data. In addition to bristlecones, a commonly used series is called Yamal. Mann discusses it (page 198):

*A more vicious attack was reserved for later that month. The matter concerned a tree ring temperature reconstruction for Russia's Yamal region that Keith Briffa and colleagues had published some years earlier; it once again showed recent warmth to be anomalous in a two-thousand-year context. At a time when Briffa was known to be seriously ill and not in a position to respond to any allegations, McIntyre publicly accused him of having intentionally cherry-picked tree ring records to get a particular result...*

*To support his “cherry picking” allegation, McIntyre had produced his own composite reconstruction—which happened to lack the prominent recent warming evident in Briffa's reconstruction.*

McIntyre didn't accuse Briffa of cherry-picking, and he didn't make “his own... reconstruction.” He simply did a sensitivity test. Testing to see what happens when you make a change is not the same as saying that change is “right.”

*How did he accomplish this? By deleting tree ring records of Briffa's he didn't seem to like, and replacing*

*them with other tree ring data he had found on the Internet, which were inappropriate for use in a long-term temperature reconstruction*

Mann claims the data added was “inappropriate for use in a long-term temperature reconstruction,” but it was no different than the data McIntyre removed. Mann also claims the data was “data he had found on the Internet.” Surprisingly, that's true. McIntyre found the data on the internet web [page](#) for the International Tree Ring Data Bank, the single largest repository for tree ring records...

As for the “tree ring records... he didn't seem to like,” they were 12 cores (tree rings measurements), a rather low amount. McIntyre removed them to see what would happen if a different site's data was used instead. This new site had 34 cores, a far better number, and it was from the same area. More importantly, McIntyre then added the 12 cores back in and got the same [result](#).

Put simply, McIntyre showed a series with a prominent hockey stick lost it's hockey stick shape if a little data from the same area was added. This series has been used in a [dozen](#) reconstructions. Is it any surprise those reconstructions got the same result as Mann got? All this shows is if you give a small amount of questionable data undue focus, you can get the same results Mann got by giving a small amount of questionable data undue focus.

The hockey stick was originally accepted without anyone verifying it. That was a mistake. Newer hockey sticks were accepted without anyone verifying them. That was a mistake. Will the same mistake be made with Mann's book?