Following the meeting with Review representatives on 9 April 2010, members of CRU were invited to comment on the points made in an Article by Prof McKitrick from the Financial Post:

http://network.nationalpost.com/np/blogs/fpcomment/archive/2009/10/01/ross-mckitrick-defects-in-key-climate-data-are-uncovered.aspx

CRU remarks on the article by McKitrick published in October 2009 in Financial Post

The McKitrick article is an opinion piece that was published on the Canadian news website of the "Financial Post" in October 2009. It is not a peer-reviewed science article. The article consists of a series of unsubstantiated and misleading claims. From a climatic perspective, it is very much at odds with mainstream thinking on climate change.

As directed by the Review Team, we comment only on those aspects of the article that relate directly to CRU work. We wish to note that there are other aspects of the article, particularly those criticising the work of others or making more general criticisms, with which we strongly disagree, though we do not detail our criticisms here.

The McKitrick article was published before the CRU emails were hacked. Professor Briffa has already responded to a number of the issues raised by McKitrick via an article published on the CRU web site, also before the CRU emails were hacked. This online response should be read in conjuction with the remarks given below, and is available here:

http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/

The article by McKitrick is copied below, with his text shown in the boxes. Our remarks are given below each relevant section.

ANALYSIS: DEFECTS IN KEY CLIMATE DATA ARE UNCOVERED
Financial Post, 1 October 2009
http://network.nationalpost.com/np/blogs/fpcomment/archive/2009/10/0 1/ross-mckitrick-defects-in-key-climate-data-are-uncovered.aspx
By Ross McKitrick

Beginning in 2003, I worked with Stephen McIntyre to replicate a famous result in paleoclimatology known as the Hockey Stick graph. Developed by a U.S. climatologist named Michael Mann, it was a statistical compilation of tree ring data supposedly proving that air temperatures had been stable for 900 years, then soared off the charts in the 20th century. Prior to the publication of the Hockey Stick, scientists had held that the medieval-era was warmer than the present, making the scale of 20th century global warming seem relatively unimportant. The dramatic revision to this view occasioned by the Hockey Stick's publication made it the poster child of the global warming movement. It was featured prominently in a 2001 report of the U.N. Intergovernmental Panel on Climate Change (IPCC), as well as government websites and countless review reports.

Steve and I showed that the mathematics behind the Mann Hockey Stick were badly flawed, such that its shape was determined by suspect bristlecone tree ring data. Controversies quickly piled up: Two expert panels involving the U.S. National Academy of Sciences were asked to investigate, the U.S. Congress held a hearing, and the media followed the story around the world. The expert reports upheld all of our criticisms of the Mann Hockey Stick, both of the mathematics and of its reliance on flawed bristlecone pine data. One of the panels, however, argued that while the Mann Hockey Stick itself was flawed, a series of other studies published since 1998 had similar shapes, thus providing support for the view that the late 20th century is unusually warm. The IPCC also made this argument in its 2007 report. But the second expert panel, led by statistician Edward Wegman, pointed out that the other studies are not independent.

They are written by the same small circle of authors, only the names are in different orders, and they reuse the same few data climate proxy series over and over. Most of the proxy data does not show anything unusual about the 20th century. But two data series have reappeared over and over that do have a hockey stick shape. One was the flawed bristlecone data that the National Academy of Sciences panel said should not be used, so the studies using it can be set aside. The second was a tree ring curve from the Yamal Peninsula in Siberia, compiled by UK scientist Keith Briffa.

Briffa had published a paper in 1995 claiming that the medieval period actually contained the coldest year of the millennium. But this claim depended on just three tree ring records (called cores) from the Polar Urals. Later, a colleague of his named F. H. Schweingruber produced a much larger sample from the Polar Urals, but it told a very different story: The medieval era was actually quite warm and the late 20th century was unexceptional. Briffa and Schweingruber never published those data, instead they dropped the Polar Urals altogether from their climate reconstruction papers.

In its place they used a new series that Briffa had calculated from tree ring data from the nearby Yamal Peninsula that had a pronounced Hockey Stick shape: relatively flat for 900 years then sharply rising in the 20th century.

In Briffa et al. (1995) a list of the coolest estimated summer temperatures relevant to the region of the northern Polar Urals was presented. The data available for AD 1032 did produce the lowest temperature estimate in the reconstructed series. Incidentally, the 2 warmest estimates are for 1045 and 1033. Though an extreme cool individual estimate is not necessarily incompatible with an estimate of period warmth, this reconstruction indicated generally cool conditions for the period around AD 1000.

The reconstruction of summer temperatures presented in Briffa et al. (1995) is based on both ring-width (TRW) and maximum-latewood-density (MXD) data from the eastern side of the northern Polar Ural Mts. Because of the way in which the data were processed, the low-frequency information (salient to the direct comparison of average warmth in the 20th century and earlier centuries) is contained in the density records (the TRW data having been processed to retain primarily shorter-timescale information). The low sample counts, and associated high-interannual variability of the data, particularly before AD 1100 suggest large uncertainty in the MXD chronology (though note that there are both TRW and MXD measurements for each sample). Despite the very high observed interannual association between the MXD and co-located instrumental data, the use of multiple least-squares regression to produce the past temperature estimates is also likely to result in some loss of long-timescale information (e.g. Osborn and Briffa, 2004).

It was for this reason that Briffa believed that the Yamal chronology, published in Briffa (2000), and which contained considerably *more* sample data processed in a way that could retain long-timescale information, was likely to provide a better representation of long-timescale changes in summer temperature than that shown in the 1995 Polar Urals paper. The lower sample replication and correspondingly lower chronology confidence associated with the 1995 Polar Urals temperature estimates, particularly in the early part of the reconstruction, were shown in Figure 1c of that paper. It was also stated in the paper that:

"So although the twentieth century was certainly unusually warm in the northern Urals, determining how unusual it was, in the context of the long record, is equivocal."

A later communication sent by Briffa to the journal *Nature* as a rebuttal of a submitted challenge by McIntyre and McKitrick to the accuracy of the crossdating of the Polar Urals data published in 1995, contained the following point.

"Despite MM's [McIntyre and McKitrick's] *incorrect inference regarding the poor quality of the crossdating, it is worth stressing that circumspection should also apply to the mean level of inferred temperature in the early section of the BEA* [Briffa et al. (1995)], *because of low replication of the data prior to about 1100. However, this is clearly indicated in Figure 1 of BEA and also in our subsequent publication (Briffa, 2000) where the low-frequency variability in the Yamal chronology and BEA Polar Urals temperature reconstruction is shown to differ: the Yamal series implying warmer conditions at this time.*

These remarks were forwarded to McIntyre by the journal on January 26, 2006. Hence McIntyre and McKitrick have long been aware of our concerns regarding the interpretation of our 1995 reconstruction and the disparity with the indication of summer warmth in the medieval period shown in our later published Yamal series (Briffa, 2000). We still consider the Yamal chronology provides a better indication of long-term summer temperature changes than the Briffa et al. (1995) reconstruction.

There is some irony in McKitrick's criticism of our use of the Yamal series which he infers was because it "*had a pronounced Hockey Stick shape: relatively flat for 900 years then sharply rising in the 20th century.*" The Yamal series actually implies that summers were comparatively warmer in the medieval period (~AD1000) than is indicated in the Briffa et al. (1995) Polar Urals reconstruction. Our subsequent use of the Yamal series is based on our judgement that it most likely provides a more reliable indication of century-to-century changes in summer warmth, including the evidence of a warm medieval interval in the Yamal region, than is provided by our 1995 publication which is heavily reliant on what we agree was a poorly replicated early density chronology, as is stated by McKitrick in this article.

Indeed, two published large-area composite ring-width series, produced by Briffa and colleagues, include Yamal data showing relative warm conditions around AD 1000, as do both of the large-area average data series themselves [see part F and the lower series in Figure 1 of Briffa (2000) and Figures 3b and 4 in Briffa et al. (2008)].

The importance we place on recognizing medieval warmth at high northern latitudes is clearly indicated in the following quotes from these two publications.

"*This exclusively tree-ring based 'Northern Chronology Average' series clearly shows the very high 20th century growth rates, but only marginally less high rates in the late 10th and 11th centuries.*" [from Briffa (2000) page 90]

"*Hence there is clear evidence for an interval of widespread enhanced medieval tree growth at high latitudes. While locally, this is apparently equivalent in scale to the recent growth levels in Avam-Taimyr and to early twentieth century growth levels in Yamal, when viewed over northwest Eurasia as a whole, the medieval high tree-growth phase seems not to be as strong or persistent as growth levels observed after 1920*". [from Briffa et al. (2008) page 6]

So our published data are not 'relatively flat for 900 years' as McKitrick states. They show clear evidence of relative summer warmth in medieval times. They do rise sharply in the early 20th century and growth remains relatively high thereafter.

> This Yamal series was a composite of an undisclosed number of individual tree cores. In order to check the steps involved in producing the composite, it would be necessary to have the individual tree ring measurements themselves. But Briffa didn't release his raw data.

McKitrick's statement is misleading. These tree-ring data had not been directly collected, measured or owned by CRU, but were obtained from their owners, and thus they were never CRU's to "release". The only request to Briffa for these data came from McIntyre. Briffa re-directed McIntyre to the rightful owner of those data. At his request, the Yamal data were sent to him by Hantemirov on 2nd February 2004 (the Review Team was shown an email attesting to this). Besides the evidence of Hantemirov's email, there is on-line support on McIntyre's blog site, Climate Audit, where McIntyre acknowledged that he received these data from Hantemirov in early 2004:
http://climateaudit.org/2009/10/05/yamal-and-ipcc-ar4-review-comments/#comment-197561

Also, contrary to McKitrick's assertion that 'Briffa and Schweingruber never published those data', the tree-ring data for the Polar Urals sites have been available from the International Tree Ring Data Bank

(ITRDB) since at least 2000. This can be verified by looking at the ITRDB website, where the Polar Urals raw data are available: ftp://ftp.ncdc.noaa.gov/pub/data/paleo/treering/updates/wsl/wsl-site-information.txt
We also note that McIntyre and McKitrick appeared to use some of these data when examining the accuracy of the crossdating of the Polar Urals data some 3 years before McKitrick wrote this article.

> Over the next nine years, at least one paper per year appeared in prominent journals using Briffa's Yamal composite to support a hockey stick-like result. The IPCC relied on these studies to defend the Hockey Stick view, and since it had appointed Briffa himself to be the IPCC Lead Author for this topic, there was no chance it would question the Yamal data.

Since McKitrick does not provide a list of the more than nine papers that supposedly used the Briffa Yamal series, it is difficult to evaluate this claim. The claim (possibly deliberately) overstates the importance of the Briffa Yamal series in the body of evidence for Northern Hemisphere temperature changes; for example, it was used in only four of the 12 reconstructions shown in Chapter 6 of the IPCC Working Group I AR4 (Jansen et al., 2007) and makes only a small contribution to most of these four. We discussed the importance of the Briffa (2000) Yamal chronology to this wider body of evidence in depth in October 2009:
http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/cautious/cautious.htm

The use of the Yamal chronology (Briffa 2000, Briffa et al. 2008) would not compromise the results of a multi-proxy reconstruction of large-scale temperature change because the evidence of past temperatures it provides has not been shown to be wrong either in the peer-reviewed literature or elsewhere. We re-iterate that the use of the Yamal series is valid, since we consider it represents the best indication of Yamal regional summer temperatures currently published and is superior to the unpublished "updated Polar Urals" chronology advocated by McIntyre.

The inference that the IPCC author team did not approach their task objectively and honestly is erroneous and insulting.

> Despite the fact that these papers appeared in top journals like Nature and Science, none of the journal reviewers or editors ever required Briffa to release his Yamal data. Steve McIntyre's repeated requests for them to uphold their own data disclosure rules were ignored.

As stated earlier, the data were not Briffa's to release and anyway McIntyre had the data in 2004.

> Then in 2008 Briffa, Schweingruber and some colleagues published a paper using the Yamal series (again) in a journal called the Philosophical Transactions of the Royal Society, which has very strict data-sharing rules. Steve sent in his customary request for the data, and this time an editor stepped up to the plate, ordering the authors to release their data. A short while ago the data appeared on the Internet. Steve could finally begin to unpack the Yamal composite.

Fritz Schweingruber was not, in fact, an author of the Briffa et al. (2008) article. These data were made available in 2009, but Yamal data had been sent to McIntyre five years earlier in 2004. McIntyre could have started working with these data in 2004, but he did not. The archiving of data in 2009 followed a request from the Royal Society to Briffa who contacted his Swedish, Finnish and Russian colleagues, and at this time they all agreed to archive. It is important to note that the data were never owned by CRU, despite repeated assertions that they were. Furthermore, the numbers of samples throughout the length of the Yamal chronology had already been published by Hantemirov and Shiyatov (2002). The sample size can be seen in the histograms in the upper part of the panels shown in their Figure 7. Briffa (2000) used the same data for the last 2000 years as in Hantemirov and Shiyatov (2002).

> It turns out that many of the samples were taken from dead (partially fossilized) trees and they have no particular trend. The sharp uptrend in the late 20th century came from cores of 10 living trees alive as of 1990, and five living trees alive as of 1995. Based on scientific standards, this is too small a sample on which to produce a publication-grade proxy composite. The 18th and 19th century portion of the sample, for instance, contains at least 30 trees per year. But that portion doesn't show a warming spike. The only segment that does is the late 20th century, where the sample size collapses. Once again a dramatic hockey stick shape turns out to depend on the least reliable portion of a dataset.

Once again McKitrick misrepresents the evidence and misleads the reader. As we have stated, the data are not 'relatively flat for 900 years' as McKitrick earlier states. The data show high tree growth around AD 1000 and in the early and late 15th century (and the mid 3rd and late 4th centuries before that). Tree-growth does apparently rise sharply in the 20th century, but not only in the last few years: it rises in the early 20th century when the statistical quality of the Yamal chronology is stronger, and growth remains relatively high thereafter.

It is true that the data show even higher growth in 1995 and 1996, but the evidence of relatively high overall tree growth over much of the 20th century is not dependent on these two values (see additional discussion of this issue at http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/).

> But an even more disquieting discovery soon came to light. Steve searched a paleoclimate data archive to see if there were other tree ring cores from at or near the Yamal site that could have been used to increase the sample size. He quickly found a large set of 34 up-to-date core samples, taken from living trees in Yamal by none other than Schweingruber himself! Had these been added to Briffa's small group the 20th century would simply be flat. It would appear completely unexceptional compared to the rest of the millennium.

This statement is untrue. When the data from the 17 trees referred to here are incorporated in the analysis of Yamal data, they do not substantially alter the published picture of increasing 20th-century tree growth in the area. Tree-growth rises in the early 20th century and remains generally high compared to levels of the previous millennium. This is demonstrated in considerable detail here: http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/.

Hence McKitrick's statement that "had these been added to Briffa's small group the 20th century would simply be flat. It would appear completely unexceptional compared to the rest of the millennium" is simply not supported by the evidence.

> Combining data from different samples would not have been an unusual step. Briffa added data from another Schweingruber site to a different composite, from the Taimyr Peninsula. The additional data were gathered more than 400 km away from the primary site. And in that case the primary site had three or four times as many cores to begin with as the Yamal site. Why did he not fill out the Yamal data with the readily-available data from his own coauthor? Why did Briffa seek out additional data for the already well-represented Taimyr site and not for the inadequate Yamal site?

Among the three 2000-year-long tree-ring width chronologies presented in Briffa et al. (2008) one, called Avam-Taimyr, represents a composite of sub-fossil and living tree data described earlier by other researchers working in central and eastern Taimyr. These include data from Bol'shoi Avam (~70° 30'N 93° 01'E, see Sidorova et al., 2007) and three locations further east that make up the published Taimyr chronology (~72° 00'N 101° 00'E, see Nauzbaev et al., 2002). The modern (i.e. living-tree) site data are from locations corresponding to those of the sub-fossil material. These include larch data from Balschoya Kamen (71° 12'- 71° 20'N and 93° 30'- 93° 50'E), some 100km from the location of the Bol'shoi Avam sub-fossil material. Living and sub-fossil tree data for the Taimyr chronology come from three locations each between 370 and 690km to the east of Balschoya Kamen (see the location maps in the above-cited

references).

Some historical context for our 2008 paper might shed some light on this issue. Some time ago we began work on a multi-institution paper intended to describe the sensitivities in producing tree-ring-based climate reconstructions to the methods of chronology construction and subsequent climate calibration, illustrated using the examples of various tree-ring chronologies across northern Eurasia. When we later received a request to submit a paper to a planned themed issue of the *Philosophical Transactions of the Royal Society* about 'The boreal forest and global change', Briffa and colleagues decided to use some of the material to hand in preparing a draft. It was intended that this should describe 3 continuous 2000-year ring-width series, each originally planned to represent the integration of a large-regional data set of sub-fossil and living tree data. The focus was to be on representing large-regional growth signals and initial comparisons with equivalent regional temperature data. The western, 'Fennoscandia', series would incorporate near tree-line pine data from northern Sweden and Finland; the Avam-Taimyr series would integrate larch data from near the Taimyr peninsula tree-line region. Between these we had intended to explore an integrated Polar Urals/Yamal larch series but it was felt that this work could not be completed in time and Briffa made the decision to reprocess the Yamal ring-width data to hand, using improved standardization techniques, and include this series in the submitted paper.

Subsequently, in response to the issues raised by McIntyre, we explored the use of additional ring-width data local to the Yamal sub-fossil data. This work established the general validity of the published Yamal chronology information, albeit with significant statistical uncertainty, including during the medieval time and the late 20th century.

We had never undertaken any reanalysis of the Polar Urals temperature reconstruction subsequent to its publication in 1995. We did use the data to validate the dating of the material used in our 1995 paper in response to McIntyre and McKitrick's unfounded challenge in 2005 (referred to earlier), and briefly to explore the potential for introducing chronology bias if subsets of data, standardized in the work described in Esper et al. (2002), are used to represent regional chronologies (e.g. see Briffa and Melvin, 2010, in press, pdf copy provided to the Review Team) [note that Polar Urals data are not cited in this paper].

When we became aware that McIntyre was advocating the use of a new so-called "Urals Esper Update" version of the Polar Urals chronology (e.g. see Figure 2 of McIntyre's submission to the Review Team) we did some preliminary analysis of it that strongly suggests this chronology to be unsatisfactory. McIntyre's version of the Polar Urals chronology appears to combine data from larch and spruce, with likely biases in the standardized indices used to form the chronology. The bias arises where a statistical model of expected ring width as a function of tree age used to correct the raw measurements is not appropriate for all of the samples from this location. This may produce a spurious overall trend in the series, while the amalgamation of spruce and larch data may produce inhomogeneous variability. There are also issues with sample replication which is weaker for much of McIntyre's chronology compared to the Yamal series, especially in the early medieval section prior to about AD 1100. The sample replication in the published versions of Yamal is poor in 1995 and 1996 (although it has been subsequently enhanced to 10 trees – see http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/). The Yamal data in these two years do not overly influence multi-proxy reconstructions, because they are either not used or are combined with other series, sometimes after being filtered. The Polar Urals chronology only spans the period 778 to 1990. McKitrick appears to assert that a sample of less than10 trees in any year is too small to produce a 'publication-grade proxy composite'. Leaving aside the complex issue of how one assesses the statistical quality of a chronology, we note that over the common period (778 to 1990) only 4 years of the Yamal tree-ring chronology have data from less than 10 trees, while the so-called "Urals Esper update" series advocated by McIntyre has some 250 years with less than 10 samples. So, we do not accept that McIntyre's chronology is necessarily superior as an indicator of changing regional tree growth

and, at the very least, it requires further examination once McIntyre has published his chronology in the peer-reviewed literature.

We still intend to publish an extended review paper that will compare and contrast features of the different published (and unpublished) versions of various regional composite chronologies in northern Eurasia and the effect on the character of climate reconstructions of calibrating them using different regression techniques. We also intend to submit for peer review the detailed results of analyses of the McIntyre version of the Polar Urals chronology that he uses in his submission to the UK Parliamentary and Muir-Russell Reviews.

> Thus the key ingredient in most of the studies that have been invoked to support the Hockey Stick, namely the Briffa Yamal series, depends on the influence of a woefully thin subsample of trees and the exclusion of readily-available data for the same area.

McKitrick is implying that we considered and deliberately excluded data from our Yamal chronology. The data that he is referring to were never considered at the time because the purpose of the work reported in Briffa (2000) and Briffa et al. (2008) was to reprocess the existing dataset of Hantemirov and Shiyatov (2002).

> Whatever is going on here, it is not science. I have been probing the arguments for global warming for well over a decade. In collaboration with a lot of excellent coauthors I have consistently found that when the layers get peeled back, what lies at the core is either flawed, misleading or simply non-existent. The surface temperature data is a contaminated mess with a significant warm bias, and as I have detailed elsewhere the IPCC fabricated evidence in its 2007 report to cover up the problem. Climate models are in gross disagreement with observations, and the discrepancy is growing with each passing year. The often-hyped claim that the modern climate has departed from natural variability depended on flawed statistical methods and low-quality data. The IPCC review process, of which I was a member last time, is nothing at all like what the public has been told: Conflicts of interest are endemic, critical evidence is systematically ignored and there are no effective checks and balances against bias or distortion.

In our earlier responses to the Review Team, we have stated that the IPCC procedures have been fully respected and followed, not just as stipulated but also in spirit. McKitrick's biased assertions in this paragraph are not supported by evidence in the scientific literature. The surface temperature record is not contaminated by a warm bias. The trends of the three surface datasets agree with each other, indicating a warming of 0.16 ºC per decade over the period 1979 to 2009. The independent satellite record shows a warming of 0.15 ºC per decade over the same period. IPCC did not fabricate evidence in the 2007 report. McKitrick does not back up any of these statements with any citations to articles in the peer-reviewed climate literature.

> I get exasperated with fellow academics, and others who ought to know better, who pile on to the supposed global warming consensus without bothering to investigate any of the glaring scientific discrepancies and procedural flaws. Over the coming few years, as the costs of global warming policies mount and the evidence of a crisis continues to collapse, perhaps it will become socially permissible for people to start thinking for themselves again. In the meantime I am grateful for those few independent thinkers, like Steve McIntyre, who continue to ask the right questions and insist on scientific standards of openness and transparency.

At the beginning, we said McKitrick's article is an opinion piece and that is all it should be taken to be. It is not substantiated, peer-reviewed science. The allegations it contains are incorrect.

We wish to re-iterate a point made in our earlier submission (1 March 2010 to the Muir-Russell Review Team) that there is no such thing as a definitive tree-ring chronology or proxy-based palaeoclimatic reconstruction. Research can only seek to produce statistically robust chronologies and timeseries and

take account of the uncertainty implicit in them when interpreting them as evidence of climate change. While we will continue in our attempts to do just this, we recognise that there is always scope to improve our work and our communication of its limitations and implications. However it is important to realise that we have never selected or rejected data (or methods) in order to support a preconceived (or "desired") picture of climate change. Rather, we contend that such an accusation might seem more appropriately directed towards some of our critics.

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