

# Response to “Summary of salient points...” and additional requested information

This document should be read in conjunction with the document provided by the Independent Review Team titled “Summary of salient points of interviews with Professors Philip Jones and Keith Briffa, Dr Tim Osborn and Tom Melvin, carried out by Professors Geoffrey Boulton and Peter Clarke.”

The Review Team’s “Summary of salient points...” began by stating:

**Interview carried out at UEA on 9<sup>th</sup> April 2010.**

*The UEA group were asked to verify that this is a correct record of our meeting, and to correct the record where they believe it not to be. These have now been received and are added to this note.*

*If they wish to make further comments, please send these separately, rather than attempting to amend this record. In addition, we have a number further questions in relation to the IPCC. Two will be sent later; one is included as paragraph 18.*

Much of the material provided below is either additional information requested by the Review Team or a summary of information that was presented at the 9<sup>th</sup> April meeting but which was not included in the Review Team’s “Summary of salient points...” This information is numbered to match the paragraph numbering used in the “Summary of salient points...” One area to note is paragraph 11 of the Review Team’s document which indicates that Professor Briffa stated during the interview that, in preparing the 2001 IPCC report, “others had misused his data.” The introductory text (copied above) may give the impression that there is complete agreement over the accuracy of the document. Professor Briffa, however, does not recollect making such a statement and did not intend to give the impression that he believes this. Regardless of this uncertainty over what was or was not stated during the interview, Professor Briffa is clear that he does not believe that “others misused his data” in this context.

## 1. Introduction

*No additional comments to add.*

## 2. Tornetrask Tree ring series

*The following comment is consistent with the “Summary of salient points...” but provides some additional context and support for the UEA response, as requested by the Review Team.*

Professor Briffa was asked about the Tornetrask series created in Briffa et al. (1992). What was the scientific reasoning that justified the adjustments to the most recent period, and which has been described as a “bodge” in Briffa and Melvin (2010)? Briffa explained that the paper used two independent sets of measurements from the Tornetrask trees, maximum latewood density (MXD) and tree-ring width (TRW). The inter-annual variability of both is significantly correlated with variability in local temperature. TRW changes are associated with growing season temperatures mostly in midsummer (particularly July) while MXD appears to respond to the changes during a longer season (April to September). Indices of both (TRW and MXD) correspond well with the high-frequency changes in temperature. Prior to 1750 the low-frequency signals were similar in both series but after 1750 indices of the MXD series fell relative to the TRW series. Because of the much higher replication in TRW, Briffa presumed the TRW were correct and made a linear adjustment to the MXD series to force the MXD series to agree with the TRW series. He referred to

this as the “bodge” in later work (Briffa and Melvin, 2010, a copy of which was supplied to the Review Team in the March 1, 2010 submission) which used improved processing methods that showed that the difference between TRW and MXD chronologies had resulted in part because the standardization technique used in the original paper (Briffa et al. 1992) to remove sample-age bias in the MXD data was itself biased. A brief presentation was made to the Review Team to illustrate this. This later work confirmed that it had been correct to adjust the MXD series in the earlier work and thus justified the original “bodge”. The manipulation had not been hidden, but had been clearly described in Briffa et al. (1992). Briffa directed the Review Team to our previous written submission of 1st March 2010 (page 7 in Section 1.2).

*The following comment summarises material that was covered during the meeting but that was not included in the “Summary of salient points...”*

Melvin and Briffa also gave a detailed explanation of why the latest published Tornetrask density chronology (Grudd 2008) also advocated by McIntyre in his submission to this review is potentially in error because it applies a biased ‘standardisation’ approach to updated density measurements. This evidence was also shown to the Review Team in a brief presentation. This evidence is not published yet.

### **3. Yamal Tree ring series**

*The following comment is consistent with the “Summary of salient points...” but provides some additional context and support for the UEA response.*

We note that the Yamal tree-ring series appears as an individual chronology as well as part of a ‘circum Arctic’ aggregation shown in Figure 1 of Briffa (2000) and as such the lay reader would be able to judge its contribution to that aggregate series in that Figure and in other multiproxy reconstructions where it is used.

### **4. Limitations of validity of the Briffa-Yamal series.**

*The following comment is consistent with the “Summary of salient points...” but provides some additional context and support for the UEA response.*

Briffa agreed that in the first publication of the CRU ‘reworked’ Yamal series [Briffa 2000], where he had reprocessed the data of Hantemirov and Shiyatov [later to be published in 2002], no indication of the chronology uncertainty was shown in the smooth curves (contained in Figure 1). Briffa informed the Review Team that the core counts for the Yamal chronology were, however, shown in Hantemirov and Shiyatov (2002) in their Figure 7 and have been available since that time. He repeated that the increased growth that suggests prominent warming in the Yamal region was apparent for much of the 20<sup>th</sup> century. It is not dependent on a few years at the most recent end of the chronology and the implied warming is shown to be robust in the analyses described in our posting (<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>) referred to in the CRU submission to this Review Panel on the 1st March 2010, a pdf copy of which was provided.

Details of core numbers and a simple indication of the spread between individual tree cores for a reworked version of these and additional data are indicated in the CRU on-line response to McIntyre. Briffa pointed out that the final 2 years of the Yamal chronology (1995 and 1996) are based on less than 10 sampled trees from 1 site and are highly uncertain but these years do not overly influence multi-proxy reconstructions because they are either not used at all (such as in Box 6.4, Figure 1 of the AR4) or are combined with two or more other series, and are sometimes also used as smoothed data. The overall evidence of significant growth increases in the trees in this

region is not dependent on only these last two years. Recent increased tree growth from about 1900 is maintained throughout the 20<sup>th</sup> century. This is based on evidence from many samples and different sites. Briffa reiterated that both versions of the Yamal chronology [published in Briffa 2000 and Briffa et al. 2008] present a defensible indication of relatively high recent tree growth and implied local warmth in the 20<sup>th</sup> century. Briffa requested that the Review Team re-examine the details of the Yamal tree growth and sample numbers provided in on-line response to McIntyre where it shows that the evidence published in 2000 and 2009 is robust. The issue of uncertainty in the Yamal series is greatly exaggerated, as is McIntyre's claim that the multi-proxy series using these data are flawed or present an exaggerated picture of 20<sup>th</sup> century warmth. Briffa argued strongly that McIntyre's specific evidence for implied falling temperatures in Yamal in the late 20<sup>th</sup> century is based on a very selective replacement of valid data from several sites from the region with data from a single (more remote) site (the KHAD data) that does not show increasing tree growth – a more biased analysis than any produced at CRU.

Including these KHAD data with the other data available from the region produces a result that is consistent with our earlier analyses of these data. Though 20<sup>th</sup> century values are associated with significant uncertainty they still provide strong evidence for unusual 20<sup>th</sup> century warmth in this area (Briffa referred the Review Team to Figures E and F of our online rebuttal to McIntyre).

**5. The allegation of inappropriately using the Briffa-Yamal series in order to promote the idea of recent global warming:**

*The following comment is consistent with the “Summary of salient points...” but provides some additional context and support for the UEA response, as requested by the Review Team.*

Briffa stressed that the Yamal TRW series (all CRU versions are virtually the same) does represent the most reliable picture of changing regional tree growth for the area over the last 1000 years at least. The picture of implied temperature change it provides is superior to the adjacent Polar Urals (Sob river) temperature reconstruction that CRU published in 1995 because this was based on TRW and MXD data with lower replication for much of their length and because the reconstruction provided more limited evidence of long-timescale changes than the Yamal (Briffa 2000, Briffa et al. 2008) series. The Yamal data are also superior to the chronology that McIntyre terms the “updated Polar Urals” (extracted from data described in Esper et al. 2002). McIntyre is the only person who has presented this version as a *better* option than Yamal as far as we know. CRU had not examined the “updated Polar Urals” series or assessed the quality of what was McIntyre's version of these data until his submission and his implied allegation that we effectively hid it is ridiculous. After examining it, it is Briffa's understanding that the McIntyre series was produced by extracting data from a larger (hemispheric) data set, statistically processed in a way that produces a biased chronology if only the series local to the Urals are extracted and averaged. The sample indices McIntyre used are of mixed species and biased with respect to the reference curve used to remove tree age effects in the measurement data. The chronology is poorly replicated in parts (more so than the Yamal series), and a calibration against local climate data has never been published as far as we know. Over a common period (1213 years between AD 778 and 1990), considering the larch samples, Yamal has 4 years with samples from less than 10 trees, while the updated Polar Urals chronology has 264 years with data from less than 10 trees, many of them in the Medieval period. Briffa and Melvin illustrated some of these points, though this evidence has not yet been published.

Briffa again stressed that the use of either of the CRU published versions 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. The Review Team has previously been directed to the online discussion of this issue (<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/> a copy of which was provided on

March 1, 2010).

## 6. Undue prominence and influence of the Briffa-Yamal series in IPCC reports.

*The following comment is consistent with the “Summary of salient points...” but provides some additional context and support for the UEA response, as requested by the Review Team.*

Members of CRU stated that they had no involvement in other research where they were not co-authors nor had they ever sought to influence other researchers’ choice of what proxy series to use in their research. The CRU reconstructions or chronologies were published in the normal way in the peer reviewed literature and it was for others to use them as they saw fit. Briffa stated again that he would advocate the use of the published Yamal series rather than his published Polar Urals reconstruction which is statistically poorer than the Yamal series. McIntyre’s version of the “updated Polar Urals” chronology is unpublished and flawed as had been extensively discussed. CRU researchers disagreed that the Yamal series dominates the shape of reconstructions included in the AR4, Chapter 6. Briffa stated that the Yamal series is only used in 4 of the 10 reconstructions shown in Figure 6.10 of Chapter 6 of AR4 and was not used in the 3rd IPCC Assessment. It was further stated that it does not dominate the reconstructions in the way it is said to, i.e. that if removed then it would likely make only a small difference relative to the overall evidence of temperature variability indicated by these reconstructions. CRU researchers had not explicitly tested this because they believed Yamal to be a valid representation of local temperature change. They did not believe McIntyre had tested it extensively either. In some cases, however, it is obvious that the effect of Yamal is minimal; e.g. only the high-pass filtered Yamal series was used by Moberg et al. (2005), so removing it or replacing it would have minimal effect on the shape of the long-term, low-frequency aspects of this reconstruction. The reconstruction that would be most affected is that based on the average of only three long tree-ring-width chronologies (Briffa, 2000), for which Yamal is one of the three series used. The AR4 report makes clear that this reconstruction is representative only of northern Eurasia.

Briffa said the Yamal series was a defensible indication of local summer temperature anomalies. McIntyre says that “CRU tree-ring proxies (in particular Tornetrask, Yamal/Polar Urals, Taymir) were used in all ten IPCC 2007 multi-proxy reconstructions”. It is hardly surprising that at least one chronology from one of these four locations is used in many multi-proxy reconstructions – though the precise choice of series varies and not all versions of regional chronologies are necessarily those processed by Briffa and colleagues. This does not demonstrate that any of them are invalid. A version of Tornetrask is used in 9 of the reconstructions and for Taymir it is 6.

Prof. Briffa stated that it is well known and openly discussed that reconstructions of ~1000 years ago are subject to significant uncertainties such as shown in the IPCC AR4 report Figure 6.10. Statements of likelihood in Chapter 6 follow IPCC published guidelines. The reconstructions used in the IPCC 4th report were not presented in the summary for policy makers, and in any case this summary was written by an independent team drawing upon the relevant chapters as they saw fit. In short the reconstructions shown in IPCC AR4 represented an honest aggregation of the published evidence, with explicit presentation of uncertainty in the third part of Figure 6.10 and in the text (relevant points are also contained in the CRU submission to the Review Team of 1<sup>st</sup> March 2010).

When McIntyre advocates substituting different versions of either the CRU Tornetrask or CRU Yamal series for versions he prefers, this may be because he knows that his “preferred” versions of the chronologies indicate a relatively warmer medieval period – yet we have clear scientific reasons why we believe his preferred versions of chronologies are flawed. True this is not published but we explained the reasons to the Review Team. We have been involved in responding to the needs of these and other inquiries for some considerable time now, but plan to publish this information in the

near future.

The Review Team will appreciate that we strongly refute McIntyre's claim of bias or 'cherry picking' and it is he who is misleading this Review by promoting the use of the "updated Polar Urals" chronology though he has presented no analysis of why, other than the fact that the medieval tree growth appears higher. The AR4 Chapter 6 made use of what were considered valid, published reconstructions available at the time.

In his submission McIntyre creates the impression that we in CRU had, or even created, an updated chronology for the Polar Urals which was at odds with another we had produced earlier (published in Briffa et al. 1995). In his submission (at Annex 11) he states that "*Neither CRU nor any other climate scientist ever published this update. The data at right has never been publicly archived and was obtained only through quasi-litigation at Science.*" and follows this with a statement in parentheses "*(One of the Climategate Letters expresses regret that the data was made available.)*".

This account conflates and distorts the issues. No CRU researcher produced the "updated" Polar Urals chronology. It was presented as an "Esper" chronology for this region by McIntyre himself. As we have explained, we believe there are serious flaws in the use of this version of these data as a regional chronology. Esper produced the underlying data, but as part of a larger analysis of average tree growth over a much wider region. McIntyre's quasi-litigation (whatever this was) was nothing to do with CRU. We cannot comment on the email expressing regret about the release of these data since it has not been identified; however, if any such email exists, we do not believe that it reflects CRU opinion.

#### **7. Tree data availability:**

*The following comment is consistent with the "Summary of salient points..." but provides some additional context and support for the UEA response, as requested by the Review Team.*

Professor Briffa said that McIntyre's remarks relate to the Fennoscandian and Russian data used in either Briffa (2000) or Briffa (2008). Briffa stated that these tree ring data had not been directly collected or measured by CRU. All belonged to other researchers in Stockholm, Helsinki, Ekaterinburg and Krasnoyarsk. Many details about these data, their originators and characteristics were described in a series of papers in a dedicated issue of the journal *Holocene* (2002). The only request for data from CRU by Mr McIntyre was re-directed to the rightful owner of those data. In the case of Yamal data, these were sent to him by Hantemirov on 2nd February, 2004 (the Review Team were shown an email attesting to this). The archiving of data in 2009 followed a request from the Royal Society to Briffa who again 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 CRU's to archive, but were obtained from their owners.

Briffa mentioned that besides the evidence of Hantemirov's email, he believed there was on-line support, on McIntyre's blog site, Climate Audit, where McIntyre acknowledged that he received these data from Hantemirov in early 2004 but no evidence of this was presented at the meeting. Subsequently, a relevant posting by McIntyre was located at the following URL:

<http://climateaudit.org/2009/10/05/yamal-and-ipcc-ar4-review-comments/#comment-197561>

An indication of the changing number of samples throughout the length of the Yamal chronology of Hantemirov and Shiyatov can be seen in the histograms in the upper part of the panels in Figure 7 of their 2002 paper. A pdf copy of Hantemirov and Shiyatov (2002) was provided separately, as requested. The same data used in Hantemirov and Shiyatov (2002) were also in by Briffa (2000)

and Briffa et al. (2008). Though the publication date of the first Briffa paper (i.e. 2000) preceded that of the Russian paper (2002) the research was contemporaneous. Briffa also referred the Review Panel to the CRU response on this issue in our posted rebuttal of 27<sup>th</sup> October 2009.

An additional claim of McIntyre's is that polar Urals data (as distinct from Yamal data) were not available for public examination (McIntyre, Parliamentary Submission, paragraph 11). CRU presented evidence to the Review Team in the form of copies of meta-data listings for these data holdings in the ITRDB showing that the raw measurement data were available in 2000 (<ftp://ftp.ncdc.noaa.gov/pub/data/paleo/treering/updates/wsl/wsl-site-information.txt>).

**8. There is no item 8 in the final version of the “Summary of salient points...”**

**9. Information for policy makers: the “hide the decline” email.**

*No additional comments to add.*

**10. Information for policy makers: the WMO 1999 statement.**

*The following comment is consistent with the “Summary of salient points...” but provides some additional context and support for the UEA response.*

This is not tantamount to withholding uncertainties from policymaking audiences because the uncertainty ranges **were provided**. Estimates of the uncertainties were given on page 4 of WMO (1999). WMO (1999) states on p4:

*Uncertainties increase earlier in the millennium due to the sparser and imperfect nature of proxy data (95% confidence errors of  $\pm 0.3^{\circ}\text{C}$  for 1000–1500, reducing to  $\pm 0.1^{\circ}\text{C}$  by the early 19th century on this 50-year time scale).*

The WMO 1999 statement was sent primarily to National Meteorological Services (NMSs). It may have been circulated outside NMSs, but the point is that, until our emails were hacked, it had almost no media coverage. We contend, therefore, that it had little influence on public or policymakers, especially in comparison with the IPCC assessment reports. The latter are extensively peer-reviewed and are the result of assessment over a number of years by teams of authors. The gravity of the IPCC reports is, as a result, very much greater than that of the 1999 statement.

The plot on the front cover shows 3 series. What we are talking about here is one of those series. The other two series are not affected by this issue. The series in question does not extend back before 1400, so it is not relevant to the discussion of the whole millennium. The two series that do extend back to 1000 do cover the Medieval Warm Period but are not strongly influenced by any recent tree-ring “decline”.

The issue with this Figure was discussed in our response to Issue 3 in our March 1 submission. In this submission we state that the 1999 WMO Statement makes this conclusion (in the text inside the WMO Document).

*“In the case of WMO (1999), which is the subject of the email that triggered this particular issue, the figure was used to support this statement: **“against the background of the millennium as a whole, the 20<sup>th</sup> century was unusually warm.”** To claim “unusual” warmth is considerably more conservative than to claim “unprecedented” warmth, and is appropriate in this case because a number of uncertainties in the reconstructions including the concern over the implications of the recent divergence prevent a more certain conclusion (such as unprecedented warmth – see also our response to question 1.5)”*

The claim by WMO 1999 was only that the 20th century was unusually warm – not that it was unprecedented.

#### **11. Information for policy makers: the IPCC report in 2001.**

The UEA group believe that the “Summary of salient points...” is inaccurate in paragraph 11. The Review Team and the UEA group disagree whether Professor Briffa stated during the interview that, in preparing the 2001 IPCC report, “others had misused his data.” **Regardless of this disagreement over what was or was not stated during the interview, Professor Briffa is clear that he does not believe that “others misused his data”.**

*The following comment provides some additional context and support for the UEA response.*

This paragraph refers to the IPCC Report in 2001. Professors Jones and Briffa were contributing authors on Chapter 2, but had no involvement in writing any of the text or producing the diagrams.

If Briffa gave the impression to the Review Team that he believes “others misused his data”, this was not his intention. He does not recollect making this statement, though he may have said that neither he nor anyone else at CRU had any control over how our data were used in the 2001 IPCC report or elsewhere. Briffa does not believe that “others misused his data”. The 2001 IPCC Assessment made explicit reference in the text to the problem of tree-ring divergence, providing a citation to the paper discussing the implications (Briffa et al. 1998). The 2007 IPCC report contained a further discussion of this issue and explicitly stated that Briffa et al. (2001) had excluded post-1960 data. We have already provided a detailed response (in our submission to the Review Team dated 1st March 2010) as to why this issue does not compromise the proxy-based assessments of large-scale medieval warmth as reported in the 2001 and 2007 IPCC Assessments.

The meaning of Briffa’s remark “I know there is pressure to produce a nice tidy story as regards apparent unprecedented warming in a thousand years or more in the proxy data but in reality the situation is not quite so simple” in email 0938031546 was made in the context of a discussion of the need to present caveats in the use of tree-ring data in the 2001 Assessment and giving his opinion of how the Policy Makers Summary should present the proxy temperature evidence. The caveats suggested were included in the final draft. Note that although Briffa was a contributing author of Chapter 2 of the Third Assessment Report (Folland et al., 2001) he did not contribute to the writing of specific text. Briffa had no role in deciding what went into the Policy Makers Summary for this Assessment and was never pressurised to comply with any decision on its content.

There is a reference to an email (1155402164) in the Section 1.2a of the meeting agenda as providing evidence that Briffa considered the “divergence” phenomenon “problematic in private”. Such an interpretation is unsupported by reading the email. It refers to a poorly worded draft response to one of the Reviewers Comments on Chapter 6 of the AR4. The draft had simply been poorly worded to imply something that was not intended and was later amended. This can be verified by reading the email in question.

The issue about whether the CRU reconstruction was “completely hidden by the graphics” is insignificant. See the discussion here:

<http://deepclimate.org/2010/05/11/how-to-be-a-climate-auditor-part-1-pretty%C2%A0pictures/>

#### **12. Peer review & scientific journals: Climate Research / Soon & Baliunas**

*The “Summary of salient points...” covers the issues related to Climate Research / Soon &*

*Baliunas. The following comment covers an additional issue that was raised at the meeting.*

The agenda of the meeting included an item (2.1) that stated "In an email of 2003 (1059664704) Professor Briffa sought rejection of papers on other than acceptable scientific grounds". This topic was discussed at the meeting and was also previously addressed in Section 5 of the CRU submission to the Review Team made on 1st March 2010. At the meeting Briffa explained why there was no substance to the allegation and provided (confidential) copies of correspondence relating to his handling of the review process of the one paper involved. The Review Team indicated that they accepted CRU's position and we were given to believe that this issue was resolved.

### **13. Peer review & scientific journals: Energy & Environment**

*No additional comments to add.*

### **14. Freedom of Information Requests for CRUTEM related information**

*No additional comments to add.*

### **15. Freedom of Information Requests for CRUTEM related information**

The "Summary of salient points..." indicated that UEA were unable to provide detailed answers at the time because they were not prepared with details of these particular cases. It was agreed to postpone this issue to allow UEA to study the file and respond later. The subsequent UEA response is summarised below.

The interview with Mr. Palmer and Mr. Colam-French was conducted with questioning based on a document prepared by Mr. Steve McIntyre. The interviewees did not have prior access to this document and, furthermore, the document made reference to correspondence that was never part of any FOIA request made to the University and which neither Mr. Palmer nor Mr. Colam-French had ever seen. This correspondence was intermingled with information pertaining to the FOIA requests themselves, which may have led the interviewers to assume incorrectly that Mr. Palmer and Mr. Colam-French were conversant with all the material referred to within the document used by the interviewers.

The "Summary of salient points..." covers the allegations that were raised during the meeting but does not reflect any of the responses that were provided by Mr. Palmer and Mr. Colam-French during the meeting. The responses supplied are given below.

A summary of all CRU FOI/EIR requests had been supplied by UEA to the enquiry.

#### **1. Eschenbach.**

The entire premise of the argument stated in 15 is faulty and based on a complete misreading of the evidence. There were two (2) requests only in 2007 relating to station data, one, by Mr. Eschenbach (FOI\_07-04), received on 25 January 2007, and the other by Mr. McIntyre (FOI\_07-09), received on Feb. 22, 2007. The Eschenbach request asked for the identities of all stations within the CRUTEM3 data set whilst the request of Mr. McIntyre only requested stations within the west Russian network, the Chinese network, and the Australian network. In both cases, the requester asked for both the station identity AND the raw station data associated with each station.

Mr. Eschenbach in particular, never withdrew or wavered in his request for the data associated with each station, and indeed this is the ground upon which his request foundered.

On 14 April 2007, Mr. Eschenbach wrote " I am asking for:

- 1) A list of the actual sites used by Dr. Jones in the preparation of the HadCRUT3 dataset, and
- 2) A clear indication of where the data for each site is available. This is quite important, as there are significant differences between the versions of each site's data at e.g. GHCN and NCAR."

On 21 April 2007, Mr. Eschenbach wrote " I had originally asked for the raw station data used to produce the HadCRUT3 dataset to be posted up on the UEA website, or made available in some other form" and " I wish to re-instate my original request, that the information itself be made available on your website or in some other form. I understand that a small amount of this data (about 2%, according to your letter) is not available due to privacy requests from the countries involved. In that case, a listing of which stations this applies to will suffice."

On 27 April 2007, Mr. Eschenbach wrote " I wish to clarify that my request #2, viz: 2) A clear indication of where the data for each site is available refers to the data actually used in the preparation of the HadCRUT3 database, and that it be available in downloadable form (or the original data provided)."

Further, on 20 April 2007, we wrote to Mr. Eschenbach and made the following offer "We can, however, send a list of all stations used, but without sources. This would include locations, names and lengths of record, although the latter are no guide as to the completeness of the series."

The emails received from Mr. Eschenbach on 21 and 27 April 2007 clearly indicate dissatisfaction with our answer of 20 April and in neither does he specifically accept our offer of the publication of the list of stations as being satisfactory to him.

This is why, pursuant to our Code of Practice and the Secretary of State's s.45 Code of Practice, this matter was referred on 27 April 2007 for a formal internal review conducted by Ms. Kitty Inglis.

The requester never, at any time, withdrew his request for station data, nor did he ever accept our offer of a list of stations in our letter of 20 April as being sufficient to meet his requirements.

## 2. McIntyre

As noted above, McIntyre asked for a subset of the station list. We responded that (a) the information was already provided by GHCN, and (b) the information from 'rural' data stations no longer exists in the form requested at the University of East Anglia.

On McIntyre's appeal, he clearly asked for a list of stations and we offered a list in our letter of 3 April 2007 which we told him had been put up on CRU's website on 11 April 2007.

Mr. McIntyre was only partially satisfied by this response and still wanted Part (b) of his request responded to in a fashion acceptable to him ("B) identification ... of the stations used in the gridded network which was used as a comparandum in this study"). At this stage, he appears to have dropped his original request for " the ... data for the stations used in the gridded network which was used as a comparandum in this study."

We responded on 18 April 2007 in regards the data stating that "... we do not have a copy of the station data as we had it in 1990." It would appear, in this case, that we answered a question that was not asked, but we did indeed provide a list of stations that was requested in Part A of Mr. McIntyre's request.

## 16. Issues relating to the IPCC.

*No additional comments to add.*

## 17. Issues relating to the IPCC.

*Responses to these questions have been provided separately by Professors Jones and Briffa.*

## 18. Issues relating to the IPCC.

*The following comment is consistent with the “Summary of salient points...” but provides some additional context and support for the UEA response, as requested by the Review Team.*

The Review team requested more information on the circumstances in which the additional paragraph in the urbanization section came to be introduced into Ch3 of AR4. This issue was partly responded to on 7 May 2010. There was also a response to a series of related questions in the 20 April 2010 response. Our response to a specific question in the 7 May 2010 document provides some of the requested details; we repeat this response and respond further below.

***When was the decision made to include the AR4 Report’s reference to M&M2004, and was this text seen by the whole writing team?***

*The decision to include MM04 (and de Laat and Maurellis) was made at the final meeting in Bergen, and as stated the text was seen by the whole writing team. It hadn’t been possible to include de Laat and Maurellis until then as it hadn’t been published until after the third Lead Author’s meeting. Discussion of MM04 can be seen in comments numbered 3-283 to 3-289 of the Second Order Draft of Chapter 3. In two of these comments (3-284 and 3-285) it was stated that we would refer to MM04 in Section 3.2.2.2 with some text, which would point out that the papers by MM04 and de Laat and Maurellis were biased. The fact that the chapter author team had now read de Laat and Maurellis is referred to in response to comment 3-289.*

*These comments can be viewed by going to this web site*

*<http://hcl.harvard.edu/collections/ipcc/>*

*and scrolling down to comments on the second order draft of Ch 3.*

*<http://pds.lib.harvard.edu/pds/view/7786376>*

*The relevant comments are on pages 36 to 40.*

*The first, second and final drafts for section 3.2.2.2 are given at the end. The final draft includes the new paragraph – and this follows from the response to comments 3-284 and 3-285. The comments were signed off by the two Review Editors for the Chapter.*

The text above was given in the May 7, 2010 response. As stated, the decision to include the text came as a direct result of the reviews received (some of the reference numbers are given above). Prof Jones doesn’t recall the discussion related to the decision to make a response (as Prof Jones was not the responsible person for section 3.2 as he has stated on numerous occasions – April 20 and May 7 and at the meeting on April 9). The person who took the lead on Section 3.2 was David Parker of the Hadley Centre. The decision may have been as a result of a suggestion by one of the Review Editors (Prof Brian Hoskins and Tom Karl) that a response should be made. This is purpose of the Review Editors – to go through all the responses to the comments and to determine if extra pieces of text are required. Whatever the exact reason, a response was made. The suggestion to make a response did not come from Prof Jones as he was not the responsible person for the section. We had over 2000 comments to respond to, and this particular issue was not one of the more contentious. We were not introducing any material that altered the conclusions of the chapter. The whole Chapter author team (and the Review Editors) were not present for every decision for each of the over 2000 comments.

In response to these reviews (see some of the links above) it was stated that the Chapter Team

would refer to MM2004 and to de Laat and Maurellis (as this paper was now published) and that these responses would include some additional text. This was an additional paragraph, which was sent to the Muir-Russell Review Team on May 7. The Review Editors agreed to the responses to all the comments, and for this issue the responses said some more text would be written. The revised text was written and this text was agreed after the Bergen meeting with the whole team of authors. This applied to all of the > 2000 review comments. As Prof Jones has stated on numerous occasions he was not the lead author responsible for the revised text, so he did not take the lead on the response to section 3.2. He did read through the whole chapter and signed off on all the changes, as did the rest of the author team. We confirmed this in the May 7 response.

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