From Geoffrey Boulton to Professor Phil Jones 15 April 2010

Dear Professor Jones

I write to follow up the evidence session that we had at CRU last Friday, when it was difficult to pursue issues relating to the IPCC in the detail that is needed by the ICCER. The purpose of this letter is to set out an issue on which we would like evidence from you.

The relevant terms of reference are: to "review CRU's policies and practices for acquiring, assembling, subjecting to peer review and disseminating data and research findings, and their compliance or otherwise with best scientific practice". In relation to this it has been alleged that IPCC procedures have been misused in attempting to prevent the publication of opposing ideas. This specifically relates to your role as coordinating lead author for chapter 3 in the IPCC Fourth Assessment Report (FAR).

As context for the allegation, we quote the Principles Governing IPCC Work adopted in 1999 and amended in 2003: "In preparing the first draft (of a Report), and at subsequent stages of revision after review, Lead Authors should clearly identify disparate views for which there is significant scientific or technical support, together with relevant arguments." An amplification of the IPCC approach has been quoted to us from Dr Pachauri's interview with an Australian reporter (http://www.abc.net.au/7.30/content/2009/s2700047.htm): *"Whatever we do is very transparent. Every stage of the drafting of our report is peer reviewed, and whatever comments we get from the peer review process are posted on the website of the IPCC, and the reasons why we accept or reject those comments are clearly specified. Where we accept a comment we say, "Yes. Accepted." Where we don't, we have to adduce very clear reasons why the authors don't agree with the comment. So it's a very transparent process."*

I have reviewed the allegations below and ask you to respond to them in detail, giving evidence to support your statements where possible. There is considerable overlap between them, particularly 2 & 3, but it would help if you could deal with them separately.

I stress that these allegations do not necessarily represent the views of the Review team, but are a reflection of the issues raised in submissions made to us, many of which have now been put on the ICCER website. It is our role to investigate them as rigorously as possible, which is the rationale for this letter.

Although the issues that are addressed below overlap with considerations of scientific debate, which is beyond our remit, it has been suggested that they reveal a pattern of behaviour designed to exclude improperly from IPCC consideration arguments that conflict with those of the CRU group. The basis for this allegation is the email quote in the following paragraph.

1. **Excluding a paper from appropriate consideration on improper grounds.**

On 8th June 2004, you sent an email to Mann: *"The other paper by MM (McKitrick & Michaels, 2004) is just garbage. … I can't see either of these papers being in the next IPCC report. Kevin (Trenberth, the other coordinating lead author for ch. 3 in FAR) and I will keep them out somehow – even if we have to redefine what the peer-review literature is!"* This paper argued that a large proportion of the measured recent warming was a consequence of increased economic activity and change sin land use.

When the IPCC released the First Order Draft in August 2005 the relevant section of the

Draft (Chapter 3, pages 3-9 to 3-10) contained no mention of the McKitrick and Michaels (2004) paper (or supporting work by de Laat and Maurellis). This was consistent with the intent expressed in the email. IPCC Expert Reviewer Vincent Gray criticized the omission as follows: (http://pds.lib.harvard.edu/pds/view/7795947?n=7&imagesize=1200&jp2Res=.25), as did expert review comments from McKitrick. In the IPCC Second Order Draft, released in March 2006, and despite reviewer demands, there was still no mention of these papers. McKitrick provided lengthy feedback objecting to this omission. In June 2006 the expert review period closed.

Questions.

- **Was the choice not to include reference to the M&M paper yours as Coordinating Lead Author?**
- **What was the justification for omitting to include a paper with very strong implications for understanding the nature of recent warming from the First and Second Order Drafts and from consideration by reviewers?**
- **Is this not a *prima facie* case of excluding views of which you disapproved?**

2. Dismissing opposing views on an inadequate foundation and thereby subverting IPCC principles of transparency and rigour.

When the final IPCC FAR was published in May 2007, it included a new paragraph in Chapter 3, on page 244, that referred to the McKitrick and Michaels (2004) and De Laat and Maurellis (2006) papers, and that had not been included in either of the drafts shown to reviewers. It is assumed that this was either written by you, or in consultation with Trenberth, but in any case, the two of you, as Coordinating Lead Authors, bear responsibility for its inclusion. It reads: *"McKitrick and Michaels (2004) and De Laat and Maurellis (2006) attempted to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying that urbanisation and related land surface changes have caused much of the observed warming. However, the locations of greatest socioeconomic development are also those that have been most warmed by atmospheric circulation changes (Sections 3.2.2.7 and 3.6.4), which exhibit large-scale coherence. Hence, the correlation of warming with industrial and socioeconomic development ceases to be* ***statistically significant*** **(**highlighting added**)**. *In addition, observed warming has been, and transient greenhouse-induced warming is expected to be, greater over land than over the oceans (Chapter 10), owing to the smaller thermal capacity of the land".*

Questions.

- **What is the justification for what appears as an *ad hoc* conclusion not based on published research that summarily dismisses an argument that is based on peer-reviewed research?**
- **Why were these conclusions not shown to or discussed with expert reviewers during the IPCC Report preparation?**
- **The references to sections 3.2.2.7 and 3.6.4 of the IPCC Report are misleading since neither section presents evidence that warming due to atmospheric circulation changes occurs in the regions of greatest socioeconomic development. Neither section even mentions industrialization, socioeconomic development, urbanization or any related term. How can they therefore be used to justify the stance of the above quotation?**
- **No justification is given for the claim of statistical insignificance, which has a precise meaning. Do you have a *p* value that justifies this statement, and if not, what does it mean?**

3. **Arbitrary searching for any support for a position that was in reality a foregone conclusion**

Your response to the Gray comment quoted above was: "the locations of socioeconomic development happen to have coincided with maximum warming, not for the reasons given by McKitrick and Michaels (2004) but because of the strengthening of the Arctic Oscillation and the greater sensitivity of land than ocean to greenhouse forcing owing to the smaller thermal capacity of land". (http://pds.lib.harvard.edu/pds/view/7795947?n=7&imagesize=1200&jp2Res=.25)

It has been suggested that this was *ad hoc* reasoning unsupported by any evidence. It has been suggested that the statement gives the impression that you had no credible reason to exclude the McKitrick and Michaels evidence, but were determined to do so nevertheless.

Evidence of your seeking any argument that could be explain away the results of McKitrick and Michaels and de Laat and Maurellis has been suggested to be your endorsement of Schmidt's hypothesis that spatial autocorrelation explains their results, even though this contradicts your own hypothesis that it is the Arctic Oscillation explains them, emphasizing that "it is all down to the calculation of spatial degrees of freedom." It has been suggested that you were prepared to accept anything that would create an appearance of scientific support for what was in reality a foregone conclusion.

Question.

Could you add to any comments made in response to 2) that would account for an apparently changing position on this issue?

4. **Compounding the failings of 2) by using an unsubstantiated premiss as a basis for an important statement in the Summary of Policymakers**

Global temperature trends are presented in Table 3.2 on page 243 of the IPCC Report. The accompanying text (page 242) states that the CRU data uncertainties "take into account" biases due to urbanization. The Executive Summary to the chapter (page 237) asserts that *"Urban heat island effects are real but local, and have not biased the large- scale trends...the very real but local effects are avoided or accounted for in the data sets used."* The influential Summary for Policymakers stated: *"Urban heat island effects are real but local, and have a negligible influence (less than 0.006°C per decade over land and zero over the oceans) on these values."* The supporting citation was to Section 3.2, which relied on the unsubstantiated material on page 244. IPCC Chapter 9 provides the summary of evidence attributing warming to greenhouse gases. The problem of CRU surface data contamination is set aside as follows (p. 693): *"Systematic instrumental errors, such as changes in measurement practices or urbanisation, could be more important, especially earlier in the record (Chapter 3), although these errors are calculated to be relatively small at large spatial scales. Urbanisation effects appear to have negligible effects on continental and hemispheric average temperatures (Chapter 3)."* The rationale for ignoring these potential data problems relies on a citation to Chapter 3, which in turn relied upon the apparently unsubstantiated evidence on page 244.

Question

- **The statement to policymakers is arguably the most important document produced by IPCC. It is vital that uncertainties are expressed clearly. Is the unequivocal statement for policymakers justified on the basis of rigorous**

science available to the FAR team, and if so, what is that evidence?

As we are hoping to complete our work by the end of May, it would be very helpful to have an early response, and particularly useful if you could let me know when that might be.

Yours sincerely

Geoffrey Boulton

Response (dated 20.4.10) from Prof Phil Jones to questions from Prof Geoffrey Boulton.

Preamble

Your questions appear to be much more ones of IPCC Science Assessment and the process by which it is achieved than of the work of CRU. In answering the questions, therefore, it is necessary to provide some background about IPCC and how it works. Additionally, some specific details about the IPCC process are included in some of the answers to your issues.

The IPCC Reports and the individual chapters are collectively produced, and it is not possible for an individual to manipulate this process, even if they are on the chapter writing team. IPCC Chapters are written by the Convening Lead Authors (CLAs), the Lead Authors (LAs) and Contributing Authors (CAs). How the process worked in practice for Ch 3 in IPCC's Fourth Assessment Report (AR4) is that the CLAs and LAs were given an outline from a previously held IPCC Plenary meeting (during 2004 and before the CLAs and LAs were selected). Chapter 3 of AR4 had two CLAs, ten LAs and 66 CAs. With Chapter 3 the two CLAs determined, based on the experience of the LAs, which of the LAs would write each section. Responses to comments on these sections were dealt with by the LAs concerned overseen by the CLAs. The two CLAs picked up the remaining sections of the chapter, where there was little experience within the assigned LA team. The CLAs and the LAs then decided which CAs they needed to contact. A few CAs contacted the writing team directly with their thoughts, which they are perfectly allowed to do under IPCC rules. The number of CAs increased during the drafting stages, again perfectly allowed under IPCC rules. Additionally there are Review Editors and their role will be introduced later.

There are four iterations of the Reports within an IPCC cycle: these are the Zeroth, First and Second Order Drafts and the Final Published version. Much development takes place from draft to draft, together with the inclusion of new published science.

Issue 1: Excluding a paper from appropriate consideration on improper grounds.

I admit to sending the email on June 8, 2004. I sent this on the spur of the moment and quickly forgot about it. It has no relation to my subsequent behaviour. Until the emails were hacked I'd forgotten I'd even written the email. No pattern of behaviour with respect to my IPCC work can be construed from this one email.

Before going any further, the claims in MM2004 are not backed up by scientific evidence. The rate of warming of the surface temperature record (HadCRUT3) is in close accord with estimates of temperatures for the lower troposphere from satellites (series produced by the University of Alabama at Huntsville, UAH and another group called Remote Sensing Systems, RSS). HadCRUT3 also agrees with other surface-based temperature datasets produced by NASA/GISS and NOAA/NCDC. For any period of rapid climate change, it would be expected that the land would change at a

faster rate than the oceans, as it has a markedly lower thermal inertia. In the Table, I give the temperature trends from 1979-2000 (the period used by MM2004) for CRUTEM3, HadSST2, HadCRUT3 and two satellite records (UAH and RSS). As shown in Ch 3 of IPCC's AR4, the trend of CRUTEM3 agrees very well with land-based records from NASA/GISS and NOAA/NCDC for various periods. Numerous studies (see later in response to Issue 2) show that much of the spatial pattern of trends in temperature can be explained by changes in the atmospheric circulation.

The Figure at the end shows the HadCRUT3 series compared to the two satellite temperature series for the period from 1979 to 2009.

MM2004 claim that removing the effects of economic covariates reduces the trend of land surface temperatures from 0.27 deg C/decade to 0.11 deg C/decade and by removing another factor reduces this to 0.06 deg C/decade. MM04 subsequently claim that this remaining trend is equivalent to the trend over the whole 20th century, but the global land temperature series is not well approximated by a linear trend over the last 100 years. If the CRUTEM3 trend is reduced by the factor claimed by MM2004, the land-based record then becomes incompatible with the ocean and the satellite record. MM2004 make no mention of this in their paper. In writing Ch 3 of AR4 (the Fourth Assessment Report, not the FAR, which refers to the First Assessment Report in 1990) the author team were mindful of this and were writing an assessment of the science and not a review. MM2004's analysis of the land surface temperature record is completely at odds with the rest of the surface and lower tropospheric temperature records. MM2004 also fails to take into account the effects of changes in the atmospheric circulation, which will be addressed in Issue 2.

Some of the same points can be used to argue against the two papers by de Laat and Maurellis (2004, 2006), although these two papers only present speculations about differences between surface and lower tropospheric temperatures and then only in the context of the validation of General Circulation Models. The de Laat and Maurellis (2006) paper was also not published at the time of the First Order Draft, so the Ch 3 drafting team were not aware of it until they were writing the Second Order Draft.

Issue 1 – Q1

The decision as to which papers to include was a collective one of the author team of Ch 3 of AR4, for the reasons outlined above. The decision, therefore, was not just mine. I did not write the text referred to in Issues 2 to 4 (as is assumed throughout the text you have summarized). I did, however, agree with its inclusion in the assessment as I was a part of the overall writing team and a CLA for Chapter 3.

Issue 1 – Q2

The question of justifying the inclusion/omission of specific papers should be addressed to the entire author team of this chapter. A justification for not discussing MM2004 in the earlier drafts is that it is scientifically flawed and this can be easily shown – see above and later in Issue 2. When responding to comments, it has to be remembered that the author team are writing an assessment within a specified space limit. Some comments (on other issues to that being discussed here and within other chapters) were responded to by stating that the conclusions of the papers were

incorrect or were not relevant to the text. IPCC authors are not obliged to say within the report itself why a paper was not included – but the reason (if the question had been raised by an expert reviewer) why some have, will be provided within the response file to the comments (see link below).

Issue 1 – Q3

This is nothing of the sort. This is a *prima facie* case of writing a Chapter for an important review that is based on sound science. It would be remiss of the chapter writing team to allocate space to the discussion of poor science, at the expense of other, more useful material – except in particular circumstances, and this was partly the case with MM04.

Issue 2: Dismissing opposing views on an inadequate foundation and thereby subverting IPCC principles of transparency and rigour.

Your preamble here (see also Issue 3) assumes that I wrote the text in conjunction with my co-CLA (Kevin Trenberth). As stated in response to Issue 1 this is incorrect. The whole Chapter is a collective report.

Q1 – This was not an *ad hoc* conclusion. We were responding to the reviewer's comments and stating why the paper was incorrect. The fact that MM2004 is in the peer-review literature does not mean it is good science. There are examples of poor science across all areas of science in the peer-review literature. Occasionally scientists submit comments on poor or incorrect papers, but this sadly is something of a rarity. With the plethora of journals it is becoming harder and harder to read and respond to all the literature. One could make a full time job of publishing criticisms of poor or incorrect papers.

Q2 – The comment/response files for each stage were not released after each review, but only released together when the final report was published in May 2007. You seem to be under the impression that expert reviewers saw responses to their comments at each stage. This has never been the case in any IPCC Report. This was an IPCC decision and all reviewers were aware of this when they made their reviews.

The paragraph in question (on p244 of Ch 3 of AR4) was included at the final stage. A section on the effects of urban heat islands on large-scale temperatures was included in all earlier drafts. It is obviously not possible to get reviews of the final draft undertaken (see IPCC guidelines), otherwise there would never be a final document.

As an aside, at the final draft stage, the Ch 3 writing team removed about 30% of the references and some small pieces of text in order for the Chapter to fit the page length constraints we had been given.

Q3 – These section references are not misleading. These sections present evidence, respectively, for patterns of temperature trends and for changes in atmospheric circulation patterns that have a strong influence on surface temperatures, especially when only a short 22 year period such as 1979-2000 is considered.

Section 3.2.2.7 states "*The lack of significant warming at about 20% of the locations (Karoly and Wu, 2005), and the enhanced warming in other places, is likely to be a result of changes in atmospheric circulation (see Section 3.6). Warming is strongest over the continental interiors of Asia and northwestern North America and over some mid-latitude ocean regions of the SH as well as southeastern Brazil.*" Strictly speaking, this text refers to the long period 1901-2005, but the principle is the same for the shorter period 1979-2005. Figures 3.9 and 3.10 demonstrate the geographical variations of annual trend for the two periods and of seasonal trends for the shorter period.

Section 3.6.4, Figure 3.31 shows the mainly strongly positive North Atlantic Oscillation index values in the 1990s. This Section also shows the influence of the North Atlantic Oscillation on surface temperatures in winter (December to March) in Figure 3.30. This pattern, given a trend to positive North Atlantic Oscillation values since 1979, projects strongly onto the observed trend pattern for winter in Figure 3.10, and also projects onto the annual trends because trends are greatest in winter (Figure 3.10). Section 3.6.4 makes this point explicitly by stating "*Following on from Hurrell (1996), Thompson et al. (2000) showed that for JFM from 1968 to 1997, the Northern Annular Mode accounted for 1.6°C of the 3.0°C warming in Eurasian surface temperatures*". Section 3.6.4 also ascribes some summer regional warming to atmospheric circulation: "*the trend towards persistent anticyclonic flow over northern Europe has contributed to anomalously warm and dry conditions in recent decades (Rodwell, 2003).*"

Therefore, Sections 3.2.2.7 and 3.6.4 between them describe a substantial atmospheric-circulation-related pattern of warming affecting Eurasia and North America, which also happen to be regions also of strong socioeconomic development. Additionally, patterns of temperature change in many other regions of the world are strongly influenced by the El Niño/Southern Oscillation (ENSO) phenomenon.

In summary, the atmospheric circulation has been shown, in numerous studies, to account for patterns of temperature change. Before undertaking the kind of analysis in MM04, it is essential to account for known signals (i.e. the NAO and ENSO and possibly others) and then examine the residuals.

I note that McKitrick (2010), included in a submission to you, claims to take circulation indices into account. As stated earlier the extraction of signals related to the atmospheric circulation should be undertaken first – taking out what is well-known for climatological reasons. The variability accounted for by the atmospheric circulation will be high-frequency in nature. Variability due to land-use changes (which, by the way, are likely to be poorly approximated by socio-economic indices) will be on longer timescales. This paper is also fundamentally flawed in its approach for the same scientific reasons and the journal name (*Statistics, Politics and Policy*) suggests it is unlikely to have been reviewed by a climatologist.

Q4 – The pattern of atmospheric-circulation-related warming appears similar to the geographical distribution of socioeconomic development. Such similarity makes it impossible to use purely statistical methods to ascribe patterns of warming trends to patterns of socioeconomic development. It remains possible, however, to ascribe patterns of warming trends to atmospheric circulation because its influence is in

accord with the laws of physics and can be detected in day-to-day weather variations, on which timescales socioeconomic trends are infinitesimal. As stated, it is essential to extract the known and understood influences first and then look at the residuals.

There is no need to calculate a p value for a statement that is based on the laws of physics.

3. Arbitrary searching for any support for a position that was in reality a foregone conclusion

The preamble here again assumes that I wrote the text in question and the responses. As I have mentioned before, I did not. Instead the responses to questions on Ch 3 were a collective response from the LAs and CLAs of the Chapter. The responses to the comments are available (http://pds.lib.harvard.edu/pds/view/7795947?n=7&imagesize=1200&jp2Res=.25) and these responses were signed off by the two Review Editors (Brian Hoskins and Tom Karl). It is the responsibility of the review editors to ensure that all comments on the drafts at the three stages of review (the initial closed review and the open expert and government reviews) are adequately responded to. This is the principal responsibility of the Review Editors. If the Review Editors wanted to comment on any section of the Chapter, then their comments go into the system as comments from anyone else would.

Q1 – There is an implication in the second paragraph that I have somehow changed my view of the issue in response to my review of a paper by Schmidt (2009) that post dates AR4. This is not so. I reviewed Schmidt (2009) and I indicated to the editor that I thought the paper was good and should be published with minor changes. This has absolutely no bearing on my views on the effect of urbanization on large-scale temperature records during the writing of Ch 3 of IPCC's AR4. *My view on this issue has not changed since 1990* – i.e. any effect is an order of magnitude smaller than overall observed warming. This view has been supported by many other studies which are referred to in Ch 3 of AR4. It is also endorsed by more recent studies (e.g. Parker, 2010). I have undertaken more assessment of possible urbanization effects in Chinese temperatures (Jones et al. 2008), where there might be more of an effect, but other unpublished work I'm doing at the moment indicates that China may be a special case.

A further point to make is that the supposedly unrelated criticisms of MM2004 (and similar work) are in fact related. The atmospheric circulation changes that have contributed to warming in some of the NH land mass are also contributions to the spatial coherence of the temperature field discussed by Schmidt (2009). It is inappropriate and simplistic to attempt to dissect these multiple criticisms of MM2004 into unrelated components, as they are clearly related. Rather than viewing the arguments within Ch 3 with the findings of Schmidt (2009), the panel might consider whether McKitrick is being opportunistic by using your enquiry to re-open his disappointment at how his research is viewed by others in the field and in the IPCC assessment.

4. Compounding the failings of 2) by using an unsubstantiated premise as a basis for an important statement in the Summary for Policymakers (SPM)

Q1 - The statements in the Executive Summary of Ch 3 were the result of the whole writing team of the Chapter. Chapter 9 was written by an entirely different writing team. The statement in the SPM was approved by the writing team for the SPM – this did not include any members of the Ch 3 writing team. McKitrick had opportunity to comment on the SPM, but it seems from searching the archive of comments he chose not to. There were 3 comments on the SPM (numbers 482, 864 and 1005) that made specific reference to MM2004 by others. These were rejected by the SPM writing team because the points being made were inconsistent with a large body of the climatological literature addressed in Ch 3.

The bullet statement with the word 'unequivocal' in the SPM is:

"Warming of the climate system is unequivocal, as is now evident from observations of the increases in global average air and ocean temperatures, widespread melting of snow and ice, and rising global average sea level."

The statement is made on much more evidence than just that given in Ch 3 (pp241-253) for land-based temperatures. Ocean temperatures are also discussed in Ch 3, snow area reductions and ice melting (retreating glaciers) were discussed in Ch 4 and sea level was discussed in Ch 5. The context of this bullet in the SPM is also implicitly referring to measurements of these variables back over the last 100 years, so not just to 1979-2000. The entire SPM was approved, sentence-by-sentence, by the IPCC Plenary in Paris in February 2007.

Finally, although not discussed in the 'unequivocal' bullet, the rate of warming at the surface is fully supported by the totally independent measurements of lower tropospheric temperatures from satellites (see Figure).

References

de Laat ATJ and Maurellis AN. 2004 : Industrial CO2 emissions as a proxy for anthropogenic influence on lower tropospheric temperature trends. *Geophysical Research Letters* **31**: L05204.

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Parker, D. E., 2010: Urban heat island effects on estimates of observed climate change, *Wiley Interdisciplinary Reviews: Climate Change*, *1*(1), 123-133, doi:10.1002/wcc.21.

Schmidt, G.A., 2009: Spurious correlations between recent warming and indices of local economic activity. *Int. J. Climatol.* **29**, 2041-2048.

Table: Trends (deg C/ decade) of various temperature series over the period 1979-2000 (as used by MM04) and for interest the longer period (1979-2009). The error ranges are the 5 to 95% range used by IPCC, but they do not take serial correlation into account, so may be slightly smaller than those quoted in AR4. Ch 3 of AR4 quotes values for 1979-2005.

Dataset	**1979-2000**	**1979-2009**
CRUTEM3 (Land)	0.205±0.086	0.221±0.046
HadSST2 (Sea)	0.125±0.045	0.135±0.026
HadCRUT3 (Land + Sea)	0.147±0.055	0.158±0.031
RSS (Land + Sea)	0.135±0.084	0.153±0.046
UAH (Land + Sea)	0.094±0.087	0.127±0.047

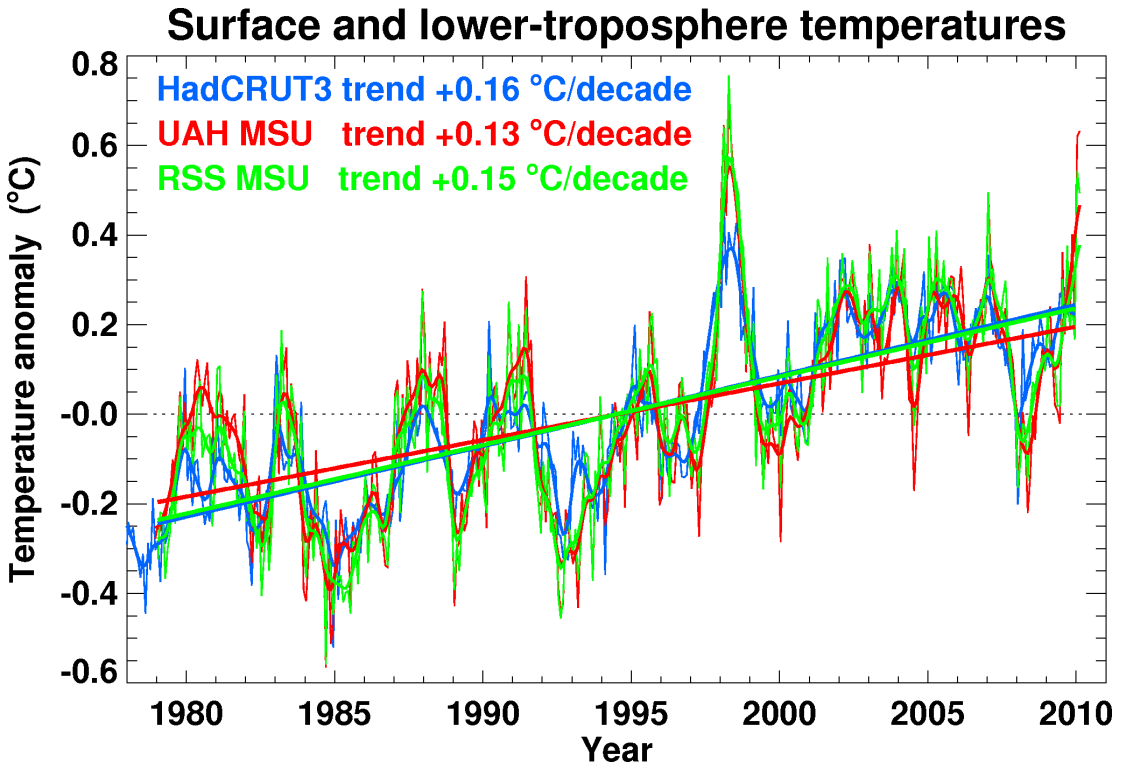

Figure – surface temperature trends compared to estimates from satellite records of the lower troposphere. The trends given in this figure are for 1979 to 2009. This figure is included as background science.