

MEMORANDUM CONCERNING DISCLOSURE OF CLIMATE DATA FROM THE CLIMATIC RESEARCH UNIT AT THE UNIVERSITY OF EAST ANGLIA

SECTION 1: INTRODUCTION

1. I hold a Ph.D. in economics from the University of British Columbia and I am a tenured full professor of economics at the University of Guelph in Ontario, Canada. My area of specialization is environmental economics, and I have published many peer-reviewed papers in social and physical science journals on the topic of climate change. Some of my research work examines the quality of surface temperature data supplied by the CRU to the Intergovernmental Panel on Climate Change (IPCC) for its assessment reports. I have published several peer-reviewed journal articles presenting statistical evidence that the CRU climate data (CRUTEM) are not free of biases due to urbanization, land use change and related socioeconomic processes.

2. In this memorandum I make the following submissions for your consideration. (a) In my view the CRU makes claims about the quality of its surface temperature data that cannot be verified based on the documentation on its web site. Nor is it possible from the publicly disclosed information to determine exactly what input data is used for the production of CRU climate data series and the extent to which it overlaps with other data products. (b) Published studies have specifically tested for and rejected claims that the CRU data products are adequately filtered for climatic measurement purposes. This is an active controversy in the scientific literature. (c) Professor Phil Jones, along with his coauthor Kevin Trenberth of the US National Center for Atmospheric Research, appears to me to have used his standing as a Coordinating Lead Author of the IPCC Report to prevent evidence calling into question CRU data used in the IPCC Report from being shown to IPCC expert reviewers. (d) After the close of the review process, misleading information was inserted into the text of the IPCC Report which had the effect of downplaying

evidence against the quality of the data referred to in (c) above. It is my understanding that Professor Jones had responsibility for the section in question.

3. With regard to your three questions, I comment as follows.

3.1 The timing and content of the emails shows that Jones was aware of a study that questioned the quality of CRU data prior to the preparation of the first IPCC draft.

3.2 In addition to its existing terms of reference the Independent Inquiry should consider whether CRU scientists whose responsibilities include providing climate data to the IPCC should not serve as IPCC Lead Authors (or Coordinating Lead Authors) on any Report or Chapter that assesses evidence for or against its quality for climatic research purposes.

3.3 The information needed to determine the independence of CRU data products compared to other major surface temperature series is not apparently available on the CRU web site, but it can be surmised that the input data are not fully independent across different products.

SECTION 2: QUESTIONS ABOUT CRU DATA QUALITY

4. The CRU publishes several versions of a data set purporting to show trends in climatic temperatures over land in 5x5 degree land-surface grid cells. I am concerned herein with controversies surrounding the post-1979 interval. Climatic data are not simply temperature records. It has been known for many decades that temperatures at land-based observational sites can be affected by changes in the land surface due to local deforestation, introduction of agriculture, road-building, urbanization, changes in monitoring equipment, measurement discontinuities, and so forth; as well as by local emissions of particulates and other air pollutants. These are considered *non-climatic* influences, since they cause purely local, and in principle reversible, changes in regional temperatures. Hence they must be filtered out of the local temperature record in order to reveal the *climatic* record. An ideal measurement of surface

climatic changes would require a monitoring site untouched by human development, the equipment for which was consistent and perfectly maintained over the entire measurement interval. However the actual data used to produce climate data sets almost never satisfies these ideals. Consequently, data sets published as “climate” records are not simply observations: they are the outputs of models that take weather records as inputs, apply adjustments aimed at removing non-climatic influences, group the resulting records into regional grids and then translate the data into deviations from a local averages, yielding what are called gridded climate “anomalies”.

5. The problems with raw temperature data are widely recognized, including by the CRU. The CRU web page (<http://www.cru.uea.ac.uk/cru/data/hrg/>) references data compilations called CRU TS 1.x, 2.x and 3.x which are not subject to adjustments for non-climatic influences. Users are explicitly cautioned not to use the TS data for measuring or analyzing climate change in the way apparently done in IPCC reports. The 1.2 release of this product provided a list of FAQ’s related to time series analysis (see <http://www.cru.uea.ac.uk/cru/data/hrg/timm/grid/ts-advice.html>). The first question, and its answer, are reproduced (in part) below.

Question One

Q1. Is it legitimate to use CRU TS 2.0 to 'detect anthropogenic climate change' (IPCC language)?

A1. No. CRU TS 2.0 is specifically *not* designed for climate change detection or attribution in the classic IPCC sense. The classic IPCC detection issue deals with the distinctly anthropogenic climate changes we are already experiencing. Therefore it is necessary, for IPCC detection to work, to remove all influences of urban development or land use change on the station data....If you want to examine the detection of

anthropogenic climate change, we recommend that you use the [Jones](#) temperature data-set. This is on a coarser (5 degree) grid, but it is optimised for the reliable detection of anthropogenic trends.

6. The implication is that the Jones data has been adjusted “for the reliable detection of anthropogenic trends.” Readers are referred to some academic papers for further explanation. The first is Brohan et al. (2005). This paper does not explain how the data are adjusted, instead it focuses on defending the claim that the potential biases are very small. Two references are cited in support of this point. One is by US scientist Thomas Peterson, which refers to the contiguous US only. Another is by David Parker of the Hadley Centre, whose argument relied on an apparent similarity between trends on windy and calm nights. No references to papers critical of Parker’s methods are cited. Section 2.3.3 of Brohan et al. states that to properly adjust the data would require a global comparison of urban versus rural records, but classifying records in this way is not possible since “no such complete meta-data are available” (p. 11), so the authors instead impose the assumption that the bias is no larger than 0.006 degrees per century. This *assumption* later appears in the 2007 IPCC Summary for Policymakers as a research finding (see paragraph 18 below).
7. Brohan et al. refer to a 2003 paper in *Journal of Climate* by Jones and Moberg, explaining the CRUTEM version 2 data product. This paper also has little information about the data adjustments. Reference is made to combining multiple site records into a single series, but not to removing non-climatic contamination. Moreover, the article points out (page 208) that it is difficult to say what homogeneity adjustments have been applied since the original data sources do not always include this information.

8. The other reference on the website is to a 1999 *Reviews of Geophysics* paper by Jones, New, Parker et. al. This paper emphasizes that non-climatic influences (therein referred to as “inhomogeneities”) must be corrected (Section 2, p. 37) for the data to be useful for climatic research. The part of the paper that provides information on the adjustments is Section 2.1, consisting of only 3 paragraphs, none of which explains the CRU procedures. The only explanatory statement is (page 174):

“All 2000+ station time series used have been assessed for homogeneity by subjective interstation comparisons performed on a local basis. Many stations were adjusted and some omitted because of anomalous warming trends and/or numerous nonclimatic jumps (complete details are given by Jones et al. [1985, 1986c]).”

9. Jones et al. [1985, 1986c] are technical reports that were submitted to the US Department of Energy, but they only cover data sets ending in the early 1980s, whereas the data under dispute herein is the post-1979 interval. Even if the adjustments were adequate in the pre-1980 interval it is likely impossible to have estimated appropriate empirical adjustments in the early 1980s for changes in socioeconomic patterns that did not occur until the 1990s.
10. In sum, the CRU cautions that unadjusted temperature data is inappropriate for the IPCC’s purpose, and for detection and attribution analysis more generally. The CRU refers users instead to the CRUTEM products. Yet the accompanying documentation does not appear to explain the adjustments made to make the data products reliable for such usage.
11. These references also provide tables of sources for the CRUTEM input data. It can be inferred from the tables that a substantial portion of the raw data are from the Global Historical

Climatology Network (GHCN) maintained by NOAA. These data are also used as inputs for the NASA and NOAA global temperature series. Hence the three global climate data series are not entirely independent. However the extent of overlap cannot be determined without knowing exactly which GHCN series are used for the CRU data set, which was one of the points subject to Freedom of Information requests in 2009. In addition, without provision of the non-GHCN source data, and a clear description of the adjustments applied to all input data, it is likely impossible to determine the overall independence between the CRU, GISS and NOAA series.

11. I have spent several years implementing statistical models to test the claim that the adjustments to CRU data are adequate. I have argued that an indication of inadequate adjustments would be a significant correlation between the spatial pattern of warming trends in climate data and the spatial pattern of industrialization/socioeconomic development. My 2004 paper in *Climate Research*, coauthored with Patrick J. Michaels, showed that such correlations are large and statistically significant, implying that the adjustments are likely inadequate. Our follow-up paper in the *Journal of Geophysical Research* in 2007 re-established these results on a new and larger global data base. Meanwhile in 2004 and 2006 a team of Dutch meteorologists (de Laat and Maurellis) also published research showing that gridded climate data sets appear to be contaminated by effects of industrialization. They used different methodologies, and we worked independently.

SECTION 3: THE JONES 2004 EMAIL AND SUBSEQUENT IPCC DRAFTS

12. CRU Email 1089318616.txt is available at <http://www.eastangliaemails.com/emails.php?eid=419&filename=1089318616.txt>. It appears to be a concatenation of two emails from Jones to Michael Mann with the second one dated July 8 2004. I reproduce the second one with the

statement of interest underlined. The reference to the 'MM' paper is to my 2004 *Climate Research* paper coauthored with Michaels.

From: Phil Jones <p.jones@xxxxxxxxxx.xxx>
To: "Michael E. Mann" <mann@xxxxxxxxxx.xxx>
Subject: HIGHLY CONFIDENTIAL
Date: Thu Jul 8 16:30:16 2004

Mike,

Only have it in the pdf form. FYI ONLY - don't pass on. Relevant paras are the last 2 in section 4 on p13. As I said it is worded carefully due to Adrian knowing Eugenia for years. He knows the're wrong, but he succumbed to her almost pleading with him to tone it down as it might affect her proposals in the future !

I didn't say any of this, so be careful how you use it - if at all. Keep quiet also that you have the pdf.

The attachment is a very good paper - I've been pushing Adrian over the last weeks to get it submitted to JGR or J. Climate. The main results are great for CRU and also for ERA-40. The basic message is clear - you have to put enough surface and sonde obs into a model to produce Reanalyses. The jumps when the data input change stand out so clearly. NCEP does many odd things also around sea ice and over snow and ice.

The other paper by MM is just garbage - as you knew. De Freitas again. Pielke is also losing all credibility as well by replying to the mad Finn as well - frequently as I see it.

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

Cheers

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In a UK Guardian article of February 2, 2010, Trenberth wholly disavows the underlined statement (<http://www.guardian.co.uk/environment/2010/feb/02/hacked-climate-emails-flaws-peer-review>).

13. The IPCC released the First Order Draft in August 2005. Since this was over a year after Jones' email to Mann it is clear he was aware of my study (it is not clear what is the second paper to which he refers, but it might have been one by de Laat and Maurellis, and I assume that it was). The relevant section of the IPCC Draft was Chapter 3, pages 3-9 to 3-10. Consistent with the intent expressed in the email there was no mention of either MM2004 or the de Laat and Maurellis work. IPCC Expert Reviewer Vincent Gray criticized the omission as follows:

The "corrections" to the surface temperature record have always been based on very poor evidence. The many references to studies on individual or regional stations which find the need for much higher corrections than are currently applied, are ignored. Now you have ignored the persuasive evidence of McKittrick and Michaels 2004 Climate Research 26 156-173 who have shown a significant influence on your "corrected" figures of a series of socioeconomic factors. You cannot just ignore this paper.
[Vincent Gray]

(<http://pds.lib.harvard.edu/pds/view/7795947?n=7&imagesize=1200&jp2Res=.25>) My expert review comments also criticized the omission.

14. The IPCC Second Order Draft was released in March 2006. Again consistent with the intent revealed in Jones' email to Mann, and despite reviewer demands, there was still no mention of our findings or those of deLaat and Maurellis. I provided lengthy feedback objecting to this omission. In June 2006 the expert review period closed.
15. The final, published IPCC report in May 2007 included a new paragraph that had not been included in either of the drafts shown to reviewers. I surmise that Professor Jones, as Coordinating Lead Author for Chapter 3, wrote the paragraph alone or in consultation with Trenberth, and bears responsibility for its inclusion in the published report.

McKittrick 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.** 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.

(IPCC 2007 Chapter 3 page 244, emphasis added).

16. The concept of “statistical insignificance” has a specific quantitative interpretation: it implies that an empirical test has been done yielding a p value greater than 0.1. The effects reported in MM2004 had p values on the order of 0.002 or 0.2%, indicating significance. The claim that our results were statistically insignificant is inaccurate and was made without any supporting citation. To my knowledge no study showing such a thing exists, and in fact I have a new paper forthcoming in a peer-reviewed statistics journal (see <http://sites.google.com/site/rossmckittrick>) countering the specific claim that accounting for atmospheric circulation effects undermines our previously-published results.
17. The highlighted portion of the inserted paragraph (see paragraph 15 above) is unsupported, and in the context appears to reflect a fabricated conclusion. It was not included in the drafts that underwent expert review. Moreover, 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. The final sentence in the quoted paragraph is irrelevant to the present discussion since the debate only concerns data over land: there is obviously no economic development over the open ocean.

18. The CRU data was crucial for some of the main conclusions in the published version of IPCC Report. 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 apparently unsubstantiated material. 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).

Again, the rationale for ignoring the issue of CRU data quality problems relies on a citation to Chapter 3, which in turn relied upon apparently unsubstantiated evidence.

19. I submit that sufficient evidence to disprove a claim of evidence fabrication would consist of the p value supporting the claim of statistical insignificance, the peer-reviewed journal article in which it was presented, and the page number where the study is cited in the IPCC Report. An inability to produce these things would, I submit, have relevance for answering the first Inquiry question.