

Submission to Commons Science and Technology Committee

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1. Introduction

[1.1] This submission describes some actions by Philip D. Jones, a professor at the University of East Anglia. Some related science issues are also briefly mentioned.

2. Review by Jones of my submission to *Energy & Environment*

2.1 Introduction

[2.1.1] In August 2007, I submitted an article to the journal *Energy & Environment*. The article concerned my allegation of fraud by a researcher at the University at Albany, Wei-Chyung Wang. Specifically, I alleged fraud in the following two research reports.

Jones P.D., Groisman P.Y., Coughlan M., Plummer N., Wang W.-C., Karl T.R. (1990), “[Assessment of urbanization effects in time series of surface air temperature over land](#)”, *Nature*, 347: 169–172.

Wang W.-C., Zeng Z., Karl T.R. (1990), “[Urban heat islands in China](#)”, *Geophysical Research Letters*, 17: 2377–2380.

[2.1.2] Each report analyses temperature data from some meteorological stations in China, over the years 1954–1983. (The first report also considers data from stations in the USSR and Australia; Wang was only involved in Chinese data, and so the other stations were not relevant for my article.) The first report is quite important: it is cited for resolving a major issue by the most recent (2007) assessment report of the IPCC.

[2.1.3] As is standard with scholarly journals, my article was sent to other researchers for comment—what is usually called *peer review*. Journal editors base their decision on whether or not to publish an article on the comments received from reviewers. Usually there are two reviewers: if the two agree, the editor almost always follows their recommendation. If the two disagree, the editor typically brings in a third reviewer, as a tie-breaker. In all cases, though, the final decision rests with the editor.

[2.1.4] For my article, one of the people asked to comment was Jones. Jones was asked in substantial part because he would obviously be very familiar with the issues, as he was the lead author of one of the two research reports.

[2.1.5] Jones sent his comments to the journal editor in early September. Afterwards, there was some discussion between Jones and me. A full copy of the discussion is at <http://www.informath.org/apprise/a5610/b0709.htm>. What follows is a treatment of the main issues in that.

2.2 Comment on Parker (2006)

[2.2.1] Here is a statement from my article.

The study of Jones et al. is not the sole study relied upon by the IPCC report for its conclusion about the insignificance of the urbanization effects. ... On the other hand, assumptions made in one of the other main studies, by Parker (2006), have since been strongly criticized, both in the peer-reviewed literature and on scholarly blogs.

The statement was followed by a footnote, which read as follows: “The only response from Parker of which I am aware is blogged at <http://www.climateaudit.org/?p=1813> (dated July 2007); this also references the main criticisms.” (Note: that is the submitted version of the footnote; the published version was slightly more detailed.)

[2.2.2] Jones commented on the foregoing as follows: “There are no peer-review publications that criticise Parker (2006)”. The comment is false. The most prominent publication is by Pielke et al. (*Journal of Geophysical Research*, 2007)—in a well-respected journal; there are also other publications that are related. Jones would certainly be expected to know this, since this is his area of research. Even if he did not know it, he could have consulted the footnote in my article.

2.3 The principal reference for Chinese data

[2.3.1] The principal reference for the Chinese data used by Jones et al. and Wang et al. is a report that was jointly sponsored by the U.S. Department of Energy and the Chinese Academy of Sciences. Concerning that, my article states the following: “The DOE/CAS report was formally published in full in 1991—Wang et al. and Jones et al. used a pre-publication version of the report”. Jones, in his review comments, said this: “The Tao *et al.* (1991) report was published after the two papers from 1990”. Here again, Jones seems to be trying to make it appear that there is a deficiency in my article, when there is none.

2.4 Unavailable station histories

[2.4.1] Here is another comment from Jones.

The 42-station pairs used in the two 1990 papers were selected by Professor Zeng (who was a co-author on Wang *et al.*, 1990).... In making her decision she did have access to the station histories and the site population values.

[2.4.2] This issue is discussed in my article at length. In particular, the 1991 report (and the 1997 revision) explicitly states that for 49 of the stations claimed to be

studied by Jones et al. and Wang et al. “station histories are not currently available”. It appears that Jones ignored this part of my article.

2.5 More attempted misdirection by Jones

[2.5.1] Another comment from Jones is the following.

All but one of the locations (i.e. one out of 34) for which Keenan cites the numbers of likely moves indicated in the site histories (on his web site, from Tao *et al.*, 1991), relate to the 42 sites of urban station data used in Jones *et al.* (1990). It is the rural sites that are crucial to the 1990 study, not the urban ones. The comparison in the Jones *et al.* (1990) paper was between the rural station data and the CRU gridded temperature data available at the time (i.e. 1990).

[2.5.2] Here is the relevant quote from Jones et al.: “We assembled a network of 42 station pairs of rural and urban sites.... stations were selected on the basis of station history: we chose those with few, if any, changes in instrumentation, location or observation times.” That assertion is untrue, for both the rural stations (40 have no histories at all) and the urban stations (9 have no histories; most of the remaining 33 had substantial moves). Again, the topic of my article is those untrue claims.

2.6 Further attempted misdirection by Jones

[2.6.1] A further comment from Jones is the following.

Nowhere in the paper, nor in the Appendix, does Keenan present the result of any analyses of temperature data for any of the two sets of 42 station records. I would have thought that this would be essential for any paper, making a constructive or useful contribution to the discussion of ‘urban’ biases.

[2.6.2] My article is about the fraud allegation. Urban biases are discussed only insofar as they relate to the article’s topic. Again Jones appears to be trying to distract attention away from the article’s topic.

2.7 Another issue with no relevance

[2.7.1] The next comment from Jones is this.

Site changes do influence the long-term homogeneity of the temperature series, but the magnitude of such biases can only be assessed by looking at the temperature data. In Brohan *et al.* (2006), we averaged all the homogeneity adjustments for all adjusted stations across the world. The histogram in Figure 4 in that paper shows that applied adjustments are slightly more likely to lead to cooling rather than warming (but this difference is probably not significant).

[2.7.2] My article is about the fraud allegation; that is stated in the title, for example. The comment above has no bearing on my article’s argument.

2.8 Subsequent work on urbanization effects in China

[2.8.1] The final comment from Jones follows.

The more recent papers on urbanization in China (i.e. published in the last few years) generally look at differences over the period from the early 1980s or just for the 1990s. Keenan doesn't refer to the paper by Li *et al.* (2004). One of the purposes of peer review is to point out selectivity in referencing. This paper adjusts some of the temperature data and concludes the urbanization effect is of the order of 0.06°C during the last 50 years.

[2.8.2] Here is what my article says.

Since the publication of Jones et al. (1990), there have been several studies on the effects of urbanization on temperature measurements in China. The most recent study, in 2007, is by GuoYu Ren and colleagues at the Laboratory for Climate Studies, China Meteorological Administration. This study concludes that a large part of the warming that has been measured in China is due to the effects of urbanization on measurement. (The study is also supported by the analysis of He et al. (2007) for the years 1991–2000.)

The most recent works—Ren et al. (2007) and He et al. (2007)—would be expected to discuss prior work, including Li et al. (2004); indeed they do, and they conclude that Li et al. were overly optimistic. Jones' claim to have found a deficiency is untrue.

2.9 Conclusions for first round

[2.9.1] Jones has not found any problems with my article. He has, however, repeatedly claimed to have found such. The editor of the journal forwarded my rebuttals of those claims to Jones, for further discussion.

[2.9.2] This practice by the editor is unusual. The editor, [Benny J. Peiser](#), was clearly making a strong effort to determine what is true and to give Jones an ample chance to criticize my article.

2.10 The 49 stations with no histories

[2.10.1] In the second round, Jones commented as follows.

Attached is Tao et al (1991). Nowhere in it does it explicitly state for 49 of the stations claimed to be studied by Jones et al. and Wang et al. are 'station histories not currently available. It says this for the 205. I'm attaching Tao et al.. It is a scanned pdf, so the find/search facility won't work. Zeng had the station histories for the 84 sites we used. They didn't have adequate resources in the 1989-90 period to digitise everything. Keenan has been told this.

The 49 stations are all in the 205; so Jones's comment is very misleading here. Zeng said in 1991 (and again in 1997) that there were no histories for those 205; this point is discussed at length in the Appendix of my article, which Jones's comment ignores.

2.11 More on rural sites

[2.11.1] Here is the next comment from Jones in the second round.

The data on Keenan's web site doesn't show that his statement for the rural sites to be true. He only has the station history for one urban sites. We chose those with few, if any, site moves.

They obviously could not have made the choice based on site moves, because for 49 sites—including 40 of the 42 rural sites—there were no histories of site moves.

2.12 Advice to the editor and conclusion

[2.12.1] Jones then e-mailed the editor the following advice.

My responses the other week were limited to just a few. I don't want you to take it as a formal review.

I don't see how any journal would ever contemplate publishing such a paper.

I hope you'll reconsider.

[2.12.2] Jones thus tried to persuade the editor to not publish my article—an article that implied strong criticism of work that he had published (Jones et al.). His attempts were clearly and repeatedly dishonourable, and were not based on the article's merits.

[2.12.3] The other reviewer, however, recommended accepting my article. Editor Peiser then sent my article to a third scientist for review; the third reviewer was also sent a copy of the exchange between Jones and me. (All of this is common practice.) The third reviewer recommended accepting my article for publication. The editor then made his decision: my article was to be published. (A copy is at <http://www.informath.org/pubs/EnE07a.pdf>.)

2.13 A further breach of trust

[2.13.1] When an article is sent to a scientist for review, the article is supposed to be kept confidential. It has since emerged from the leaked CRU e-mails that Jones did not abide by this, but instead sent copies of my article to several others. This is a breach of trust.

[2.13.2] Relevant e-mails that evidence this include the following: [1188412866](#), [1188478901](#), [1188508827](#), [1188557698](#), [1189515774](#), [1189536059](#).

3. Work for the Intergovernmental Panel on Climate Change

3.1 Jones and a chapter of the IPCC Assessment Report

[3.1.1] Every six years or so, the IPCC issues an Assessment Report. Those reports are widely considered to be the most authoritative assessment of the scientific understanding of climate change. For the 2007 report, there were two scientists with final responsibility for the chapter in the IPCC report on “surface and atmospheric

climate change” (here “surface” refers to the surface of the Earth, i.e. where people live). Those two were Jones and an American colleague, [Kevin Trenberth](#).

[3.1.2] The chapter on surface climate might be considered the most important chapter of the IPCC report. It cites Jones et al. (1990), but it does not cite Wang et al. (1990).

3.2 Citing research known to be based on false claims

[3.2.1] The principal statement in the report of Jones et al. that I alleged to be fabricated is this: “The stations were selected on the basis of station history: we chose those with few, if any, changes in instrumentation, location or observation times”. My article argues that at the time the report was published, Jones believed the statement was true, and the responsibility for the fabrication lay with Jones’ co-author Wang. The following paragraph from my article is relevant.

How much did Jones know about Wang’s fabrications? As discussed in my Report on Wang’s claims, it appears very likely that Jones knew nothing at the time (1990). In 2001, however, Jones co-authored a study, by Yan et al., which considered two meteorological stations in China (at Beijing and at Shanghai). This study correctly describes how the stations had undergone relocations, and it concludes that those relocations substantially affected the measured temperatures—in direct contradiction to the claims of Wang. Thus, by 2001, Jones must have known that the claims of Wang were not wholly true.

On 19 June 2007, I e-mailed Jones about this, saying “this proves that you knew there were serious problems with Wang’s claims back in 2001; yet some of your work since then has continued to rely on those claims, most notably in the latest report from the IPCC”. I politely requested an explanation. I have not received a reply.

[3.2.2] The study of Yan et al. is the following.

Yan Zhongwei, Yang Chi, Jones P. (2001), “[Influence of inhomogeneity on the estimation of mean and extreme temperature trends in Beijing and Shanghai](#)”, *Advances in Atmospheric Sciences*, 18: 309–321.

Note that Jones is one of the three authors. The study correctly describes how the Beijing station moved five times, over 41 km, as well as having changes in observation times. Shanghai also had a small move, as well as changes in observation times.

[3.2.3] The above block quote from my article implies that Jones committed fraud in his work on the latest report from the IPCC—i.e. citing work that he knew to be based on false claims. Note too that Jones must have read the quote when he was reviewing my article; yet none of the review comments from Jones address the quote, either explicitly or implicitly. Jones seems to be thereby effectively admitting the fraud.

[3.2.4] There is another aspect to this as well. In 1993, Jones was the second author of a report that examined temperature trends in China and certain other countries ([Karl et al., Bulletin of the American Meteorological Society, 1993](#)). The report states

(p.1014) that “Station histories from the PRC [China] do not reflect any changes in instrumentation, instrument heights, instrument shelters, or observing procedures ...”. That directly contradicts the claim of Jones et al. to have chosen stations “with few, if any, changes in instrumentation”. So again, Jones knew that the IPCC Assessment Report was citing work that was based on false claims.

3.3 Ignoring research that contradicts his own

[3.3.1] The reports of Jones et al. and Wang et al. analyze the same data, but come to substantially-different conclusions about that data. The IPCC (2007) Assessment Report, though, only cites Jones et al., not Wang et al. That seems to be contrary to how the IPCC is supposed to work. Consider, for example, the following quote from IPCC Chairman Rajendra Pachauri (quoted by the U.S. Environmental Protection Agency, *Endangerment and Cause or Contribute Findings for Greenhouse Gases Under Section 202(a) of the Clean Air Act: Response to Public Comments—Volume 11*, 2009).

IPCC relies entirely on peer reviewed literature in carrying out its assessment and follows a process that renders it unlikely that any peer reviewed piece of literature, however contrary to the views of any individual author, would be left out. ... There is ... no possibility of exclusion of any contrarian views, if they have been published in established journals or other publications which are peer reviewed.

[3.3.2] If there is some reason for ignoring Wang et al., it should be in the peer-reviewed literature. I searched that literature (using the [ISI Web of Knowledge](#)) for work that compared Jones et al. and Wang et al. I found only one such work, by Riches et al. (*Bulletin of the American Meteorological Society*, 1992). Riches et al., which is co-authored by Wang, state the following (p.588).

Jones et al. (1990) have assessed the urbanization effects in time series of surface air temperature over land areas in European parts of the CIS, eastern Australia, and eastern China. The results suggest that urbanization influence appears to be small. However, Wang et al. (1990) have performed a more detailed study on the urban heat island effect in China. The effects were found to have a seasonal dependency, which varied considerably across the country.

[3.3.3] Thus, the citing of Jones et al. and the non-citing of Wang et al. appear to contradict the claims made by Chairman Pachauri, and indeed obvious good practice in research. Jones, together with Trenberth, had final responsibility for that selective citation. In other words, there is evidence that Jones abused his position of responsibility for the IPCC chapter to cite his own research (in support of global warming) and ignore other research that contradicted his.

4. Discussion

[4.1] This submission to the Committee is limited to 3000 words. There are other topics that merit presentation, but cannot be treated in such space. In reality, Jones is more unscrupulous than the foregoing indicates. Simply put, Jones is an incompetent who has advanced himself by joining what is in effect a mutual benefit society.

[4.2] In addition, there are many other researchers, in other fields of science, who are at least as unscrupulous as Jones. Consider that there are tens of thousands of scientists in the UK, and yet none of those have been found guilty of scientific fraud during the past decade, to my knowledge. It is not credible that tens of thousands of people would all act with complete integrity in all of their actions for a decade.

[4.3] I once filed an allegation of scientific fraud against a researcher at the University of Reading. The university refused to investigate my allegation. I was told by telephone that the university had no procedures for investigating such allegations, because their professors always acted with integrity. With responses like that, it is easy to understand why there have been no convictions for scientific fraud.

[4.4] On February 2nd, a front-page article in *The Guardian* reported on some of my work and described me in part as a “researcher of scientific fraud”. Indeed, I have substantial experience with scientific fraud, in many fields, in several countries. If the Committee is interested in taking steps to address such fraud, I would be quite willing to provide further information.

Declaration of Interests: None.

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