A few comments on the Keenan paper submitted to Energy and Environment

1. Blog sites and their associated discussions are not valid references.

   This pertains only to footnotes 1, 2, and 8. For footnote 1, I will leave the decision to the editors. For 2, the blog is a valid reference, as explained in the next point (below). For 8, this is not actually a blog, but a page on my website; the page elaborates on a paper that I published in *Theoretical and Applied Climatology* and is important.

2. There are no peer-review publications that criticise Parker (2006).

   False. The two most prominent publications are Pielke et al. [*JGR*, 2007] and Walters et al. [*GRL*, 2007] (note that not only are these are in well-respected journals). There are also several other publications that are related.

   The reviewer here seems not to have considered footnote 2, which says the following (emphasis added).

   > 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*.

   Additionally, the previous point stated that footnote 2 was a valid reference, despite citing a blog. The reason for that should now be clear: ClimateAudit has the only response from Parker to date.

3. Discussion about grape harvests and hockey sticks are not relevant.

   The reviewer seems to be confused about the paper's topic, both here and below. The paper is about the fraud allegation against Wang. This is stated in the title, for example. Wang's work is about global warming (as is this issue of *Energy & Environment*). So some other work on global warming for which there is evidence of fraud is indeed relevant. Note, too, that my paper only mentioned the other work in a footnote.

4. The Tao *et al*. (1991) report was published after the two papers from 1990.

   True. Here is what my paper says.

   > 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.

   The reviewer must not have read that part of my paper.

5. Two networks (one of 60 and another of 205) were developed around 1990. The 60-station network contained data for 12 meteorological variables and information on the station histories, but the 205-station network contained mean temperatures and precipitation totals only, without station histories. This was because of a lack of resources at the time. 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) from the 60 and 205 station networks. In making her decision she did have access to the station histories and the site population values.

This issue is discussed in my paper 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". Did the reviewer read this part of my paper?

6. 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).

   Here is the relevant quote from Jones et al.

   > We assembled a network of 42 station pairs of rural and urban sites.... The stations were selected on the basis of station history: we chose those with few, if any, changes in instrumentation, location or observation times.

   The data on my web site shows that the quote is untrue. Again, my paper is about the fraud allegation.

   Having said that, the review here seems to be claiming that the fraud (if true) is unimportant for the analysis. This issue is discussed in point 6 of my paper, which the reviewer seems not to have considered: what matters is whether or not fraud was committed, and as far as the analysis goes, it appears to have reached an incorrect conclusion. I will try to elaborate on this more though.

7. 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.

   The paper is about the fraud allegation against Wang. Urban biases are discussed only insofar as they relate to the paper's topic. Moreover, analyzing such data is extremely difficult (frankly, I do not know how to, and my skills with statistical time series are better than that demonstrated in any climatological work I know of).

8. 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).

What does this have to do with Wang's alleged fraud? (Moreover, Brohan et al. do not properly consider the issues being raised at surfacestations.org, etc.)

9. 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.

   Here is a pertinent quote from my paper.

   > 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. Since Li et al. is cited by the IPCC Fourth Assessment Report, though, it might be better if my paper explicitly pointed out that it has been superseded. But the reviewer's claim to have found something inaccurate is untrue.

References not on Keenan's paper

Brohan, P., Kennedy, J., Harris, I., Tett, S.F.B. and Jones, P.D., 2006: Uncertainty estimates in regional and global observed temperature changes: a new dataset from 1850. *J. Geophys. Res.* **111**, D12106, doi:10.1029/2005JD006548.

Li, Q., Zhang, H., Liu, X. and Huang, J., 2004: Urban heat island effect on annual mean temperature during the last 50 years in China. *Theoretical and Applied Climatology*, **79**, 165-174.

To summarize, there are two main problems with the review. First, the reviewer has not understood the paper's topic: the paper is about the fraud allegation against Wang, and it treats urban warming only insofar as this relates to that; I see no reason why that was not clear. Second, the reviewer does not seem to have read some portions of the paper, despite the paper being of medium length and written for a general audience. The review has identified a couple issues where my paper could elaborate a little (see comments 6 and 9). It has found no valid errors or oversights.

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