Review of "Signal detection in global mean temperatures after "Paris": an uncertainty and sensitivity analysis

Visser et al., submitted to Clim. Past Discuss.

Visser et al perform a comprehensive timeseries analysis of Global Mean Surface Temperatures (commonly referred to as GMSTs although for some reason referred to by the authors as GMTs). The rationale given is to inform the COP negotiations, which is a laudable aim. There is a significant amount of analysis undertaken that yields potentially useful and actionable information. As such, scientifically, this constitutes a publishable work.

Major points

There is a question whether the paper content is in scope of the journal as it deals exclusively with the recent past and with instrumental records. The decision of in-scope or not is one for the assigned editor and the broader editorial team to take a view on. I merely flag it here.

I find the hook to pre-industrial tenuous given that the authors make no attempt to estimate a true pre-industrial based value. They would be better, in my view, to state that they are making an estimate relative to the late 19th Century / early global instrumental record. This would be a fairer reflection of what is actually done and consistent with e.g. IPCC AR5 which deliberately avoided in the published version implying that 1850-1900 constituted pre-industrial as noted in Hawkins et al. Indeed, the final plenary of the WG1 involved a long discussion that I was personally involved in around the topic whereby the parties agreed that pre-industrial was earlier than 1850. It would be unwise, in my view, for the authors to reopen this issue. I note in a couple of places that there are phrases which could imply IPCC used 1850-1900 as pre-industrial, and they did not. Such implications absolutely must be avoided outright in any resubmission.

The discussion linking their work to the pre-industrial era would be far better being given exclusively in the Discussion section and, to my view, the authors should remove allusions to providing an estimate relative to pre-industrial earlier than this. Bottom line: They either should estimate relative to true-pre-industrial or be honest with respect to what they are estimating relative to for the paper to be acceptable. As I see it there is no rigorous attempt to estimate changes since pre-industrial. Rather, there is a rigorous attempt to estimate it since 1880 which in itself is useful and valuable. The authors should be honest in this regard and not oversell their work by claiming its an estimate relative to pre-industrial when it demonstrably is not.

I also find the Section at the end of the paper alluding to RCPs and end of Century to be out of scope and a distraction. It should either form an integral part of the paper integrated throughout or be dropped. Given journal scope I would lean heavily toward its removal. The year 2100 is not in the past (at least yet)!

Finally, given the authors apparent desire to explore uncertainty I find the omission of the JMA observational analysis and the NOAA 20CR product odd. I could see a case for omission

of 20CR, but I see no logical case why the JMA analysis should be omitted here as it has the same non-peer-reviewed basis as e.g. the Berkeley global (not land, but global) estimate. JMA uses a fundamentally distinct set of SSTs and so would better span uncertainty that the authors lament in Section 2.1.

More minor points

I have a number of further comments, suggestions and requests which I refer to in the order they arise chronologically below:

1. Line 19 per above remove 'and what is 'pre-industrial'?' as you make no attempt to robustly address that question.

2. Line 47 GMTs have …. So following the 21st

3. Lines 53-56 do not reflect the IPCC approach. This was not an attempt to inform on post-pre-industrial changes and it did not involve expert judgement. Rather the stated range is the range of available estimates and their uncertainties after correcting for AR(1) and using OLS. The text here significantly overcomplicates both what was done and why. As the IPCC author who undertook the lead on this analysis I can assure the authors it was not as complicated as they imply here. This should be revised to reflect the actual process.

4. If retained (and note earlier major suggestion to move this to discussion) line 55 forwards should constitute the beginning of the paragraph currently starting line 57.

5. Line 73 or similar do not

6. Line 73. Reader will ask so what? You need to be explicit that the approach limitations matter in a period of rapid change.

7. Line 76 progression to specific (remove allusion to pre-industrial per major comment)

8. Line 88 the main one being

9. Lines 108 to 121 omit the by far largest overlap of all in that the NOAA and NASA products are based on identical underlying land and ocean datasets differing solely in the applied post-processing. This needs to be acknowledged for this discussion to be acceptable. More generally this discussion is incomplete. It needs to be expanded and may be better if supported by a table.

10. Feels odd not to discuss and cite Cowtan et al at lines 125-127

11. Lines 155-157. First please clarify whether the AR1 factors are calculated on the annual series. This is important information that is being omitted. Secondly, even at annual scales the AR(1) is primarily an artefact of variability and not forcing so the

assertion here is wrong as you note in lines 175-177. Your two cheek-to-jowl statements here cannot both be right. The AR arising from variability is the correct one here. Year-to-year autocorrelation does not arise mainly due to forcing.

12. Line 202 you should clarify what the implications of ignoring this are or, preferably, perform the extra work necessary for its inclusion. Presumably the impact would be artificially reduced uncertainty ranges? In which case is it really safe to ignore this issue? I'm not entirely convinced and would suggest that extra work leading to its inclusion is instead warranted. Even if it ends up showing no change it would make the piece more robust. As you yourselves state the effect is statistically significant, in which case it really should be included.

13. Lines 224-227. This is true a. for this particular period and b. this particular small (and non-independent as noted in Section 2.1) draw from the broad range of plausible means by which to estimate historical changes in GMT. Hence I believe this statement oversimplifies the issues and as a result is more confident than is, in reality, warranted. The findings do not have the universality implied here and may not even be true if we instead had a further draw from the sample of plausible approaches to estimating GMTs from observations. Here, JMA's inclusion may fundamentally alter this finding which would imply non-robustness.