International Journal of Climatology Decision Letter regarding McIntyre and McKitrick, "The Consistency of Modeled and Observed Temperature Trends in the Tropical Troposphere: A Comment on Santer et al (2008)", submitted August 15, 2009

31-Oct-2009

Dear Mr McIntyre

Manuscript # JOC-09-0286 entitled "The Consistency of Modeled and Observed Temperature Trends in the Tropical Troposphere: A Comment on Santer et al (2008)" which you submitted to the International Journal of Climatology, has been reviewed. The comments of the referee(s), all of whom are leading international experts in this field, are included at the bottom of this letter. If the reviewer submitted comments as an attachment this will only be visible via your Author Centre. It will not be attached to this email. Log in to ScholarOne Manuscripts, go to your Author Centre, find your manuscript in the "Manuscripts with Decisions" queue. Click on the Decision Letter link. Within the Decision letter is a further link to the reviewer attachment.

The referees all have significant expertise and experience in this field. As you will see from the comments, the referees have identified serious problems with the manuscript.

Ordinarily, the nature and extent of these problems would lead me to completely reject this paper for publication. However, the reviewers believe that the work contains some interesting elements, that with considerable effort and revision may have the potential for publication in the International Journal of Climatology.

Therefore, the paper is being rejected at this point, but with the option to resubmit a manuscript that has undergone a complete overhaul. Revisions in this category involve very extensive changes to the text, as well as likely recalculations or new analyses in addition to many minor clarifications and corrections.

Please note that submitting a revision of your manuscript does not guarantee eventual acceptance, and that your revision will be subject to review by current and/or new referee(s) before a decision is rendered.

You can upload your revised manuscript and submit it through your Author Centre. Log into http://mc.manuscriptcentral.com/joc and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions."

When submitting your revised manuscript, you must respond to the comments made by the referee(s). You must use the space provided or supply an uploaded file to document any and all changes you make to the original manuscript. Please include a point-by-point response to all of the comments made by each referee.

IMPORTANT: We have your original files. When submitting (uploading) your revised manuscript, please delete the file(s) that you wish to replace and then upload the revised file(s).

Once again, thank you for submitting your manuscript to the International Journal of Climatology and I look forward to receiving your revision.

Sincerely,

Prof. Glenn McGregor
Editor, International Journal of Climatology g.mcgregor@auckland.ac.nz

NOTE FROM EDITOR
I have pasted below the verbatum reports of the reviewers. Clearly the revised version is a great improvement over the original but issues with the analysis still remain. Referee 1 makes two extremely important points regarding the diagnostoc used for the comparison and the mismatch in time periods. Referee 2 also picks up on the time period mismatch as an issue of concern. I have chosen "reject and resubmit" as further work is required before the analysis and the conclusions can be considered robust enough for publication. Both reviewers have requested to see the revised version. I will be basing my final decision on their re-review of the paper so it is important that you respond in full to all of the reviewers' concerns (in particular use the radiosonde data used by Santer et al., calculate the suggested amplification factor and base analyses on this and critically analyse the start date sensitivity issue). I look forward to receiving your revised version. If you decide not to submit a revised paper please do let me know.

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Referee(s)' Comments to Author:

Referee: 1
Comments to the Author
I reviewed an earlier incarnation of this paper. This new version is greatly improved with much less snark and strawman arguing and therefore much much closer to publication. However, the lack of resolution to two of my previous comments means that I am forced to again recommend rejection. This is because these aspects are absolutely crucial to our understanding of and resolution to the issue. If the authors were to very directly and specifically address the following two aspects in a robust manner in any revision then I would recommend acceptance of the revision with suitable minor corrections (I do not append minor issues to rejections to save time):

1. It is the amplification factor, the ratio of T trop to T surf, that is the constrained aspect of tropical model behaviour. It is this diagnostic that should be the subject of their lapse rate test and not the straight difference between the surface and troposphere which is precisely meaningless for ascertaining the lapse rate behaviour constraints (non-)applicability. The authors should read Santer et al. 2005 and utilise this diagnostic. It is a pity that Douglass et al took us down this interesting cul-de-sac and that Santer et al 2008 did not address it but rather chose to perpetuate it. The authors could reverse this descent away to meaningless arguments very simply by noting that the constrained aspect within all of the models is the ratio of changes and that therefore it is this aspect of real-world behaviour that we should be investigating, and then performing the analysis based upon these ratios in the models and the observations. This gets around the issue that the multi-model average trend at the surface is greater than in the obs pre-conditioning rejection of a consistency test that is meaningless as it is based on non-physical constraint grounds.

2. They have not touched on the analysis period issue sufficiently to warrant publication. Clearly if this choice is critical we want to know how critical and that means undertaking analyses across a broad range of timescales rather than just three periods that could be accused of being cherry-picked. I would like to see some meaningful analysis of sensitivity to start date and end date across a range of choices so that this can be robustly ascertained by the reader. Ideally this would be through the use of radiosonde records in addition which would permit a longer term perspective and more long-term trend periods to be looked at. If not then at least all overlapping 10 year MSU period choices, 11 year MSU period choices ... 25 year MSU period choices etc. That then allows the three highlight periods chosen to be interpreted in a comprehensive manner. At the moment the available information is indsufficient.

I would also urge the authors to consider the radiosonde data used in Santer et al., 2008. Otherwise it looks like they are just ducking an issue. So in the end it would do harm to the value of their paper not to do so.
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Referee: 2
Comments to the Author
Report on paper "The consistency of modeled and observed temperature trends in the tropical troposphere: A comment on Santer et al (2008)", S. McIntyre and R. McKitrick

This paper is a revision of an earlier short paper by McIntyre, that I reviewed some months ago.

It would be good to know why the authors did not pursue some of the suggestions made in my earlier review, in particular, the one about alternative autocorrelation models and the possibility of a direct comparison of UAH and RSS trends. However on the latter point, the paper makes a lot less of an issue about the inconsistency between these two data series, so it's not so relevant they make a formal test.

My impression is that most members of the climate community outside Alabama regard the RSS series as the more reliable of the two constructions, and the present paper might be interpreted as reinforcing that point of view. If we do accept that, then only the results in Table 2, for lapse rates, show a statistically significant discrepancy between the observed and model data, and they are for different time periods (observational data for 1979-2008 compared with model data for 1979-1999). So the practical significance of the results is still debatable.

It should be made clear that two different time periods are being compared. The way the abstract is currently written, the reader might get the impression that both the observational and model data have been updated, which is not the case.

Two minor queries:

1. The paper refers to a "Supporting Information" but none was submitted with the manuscript, as far as I can tell.

2. Could the authors please clarify the calculation of a land trend (page 4)? The most obvious method would be to assume that the (reported) land+ocean trend is a linear combination of the (reported) ocean trend with the (unreported) land trend, the weights being proportional to the respective surface areas. Is this in fact what was done? Is there any correction for missing data?