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Interactive comment on "Intercomparison of temperature trends in IPCC CMIP5 simulations with observations, reanalyses and CMIP3 models" *by* J. Xu and A. M. Powell Jr.

Anonymous Referee #2

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First of all, apologies to the authors for the delay in providing this review.

In this paper, the authors investigate recent trends in atmospheric temperatures by comparing radiosonde data, re-analyses and historical runs provided by CMIP3 and CMIP5 models. The manuscript mostly consists in computing linear trends in 3D-fields of yearly temperatures over 1979-2005, and I have nothing to say about the methodology except that I regret the systematic absence of confidence intervals and/or statistical significance throughout the paper. This results in a lot of plots of all kinds of averaged trends, with redundant information between figures, showing some differences and similarities across the three types of datasets. These figures are highly described in the text, but no discussion or interpretation of the results are provided, so that all

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questions remain open at the end of the paper.

In fact, I think that the major problem of this manuscript is that the goal of the study is unclear, or, at least, ambiguous. I'm indeed not sure whether the authors aim to provide estimates of recent trends from "equiprobable" sources of data (obs + re-analyses + models) or if this study should be considered as an evaluation of climate models used in CMIP relative to both observations and re-analyses. It would have been great to address both questions (trend estimates + model evaluation), but instead, the reader is left with no response to any of these two questions.

However, I still think that such an analysis of temperature trends using multiple sources of data is interesting, but the inter-comparison has to be accompanied by (1) a discussion of the reliability of the different datasets, (2) an interpretation/explanation of similarities and differences, and (3) implications for the use of climate models for estimating climate sensitivity or providing reliable future projections. I believe that the manuscript can not be published before it includes these three points. In addition, the text is not well structured and the figures and tables could be better organized.

I would also have several minor comments on both text and figures, but as the paper will probably be strongly revised and reorganized, I only list the most important ones below:

1 - There are many redundancies throughout the paper, eg the first sentence of Section 2, the last para of Section 3.2, or the whole Section 3.3 have been said previously.

2 - There are also many redundancies in the figures and tables, and that they could be re-ordered in order to better structure the text. For instance I believe that figs 4 and 5 should be gathered as a new fig 1, that fig 1 must be bigger and placed as new fig 2, and that figs 2 and 3 and table 1 could be gathered as a new fig 3.

3 - Some details concerning the use of CMIP3 and CMIP5 outputs are missing: which runs are used, especially for modeling groups that provide several members for one

experiment? Why using only eight models (there is no reason why the authors should use the same number as for re-analyses)?

4 - Is latitudinal weighting accounted for in global averages?

5 - Statistical significance should be systematically included. I'm not sure to understand how the 2.5 significance threshold is defined in fig 1. The color scale should be centered on zero, and use white for non-significant values (also for fig 4).

Interactive comment on Geosci. Model Dev. Discuss., 5, 3621, 2012.