

Interactive comment on “Imprints of climate forcings in global gridded temperature data” by J. Mikšovský et al.

Anonymous Referee #2

Received and published: 8 January 2016

This is a review of the manuscript titled “Imprints of climate forcings in global gridded temperature data” by J. Mikšovský, E. Holtanová, and P. Pišoft. This work breaks down gridded temperature anomalies into their components attributed to climate variability using multiple linear regression. Overall, this paper is well written and a useful addition to the published literature on this technique and subject and I recommend publication after revision and some minor copyediting.

General Comments: 1) The decision not to include aerosol forcing in the model leaves this work somewhat lacking. The authors briefly discuss the reasoning for this (i.e. the complexities involved in decoupling the GHG emissions from the aerosol forcing). Can aerosol forcing, as calculated by one of the CMIP5 models be used for this purpose? The CO₂ equivalent GHG forcing used in this study needs to be described further

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including a justification for as to why this was used when the header information for these files state that CO₂ equivalent is for informational purposes and not a CMIP5 recommendation.

2) Prior studies have been criticized for using AMO as a climate driver and some critics are vehement that AMO is simply a response to volcanic activity (i.e. inclusion of the AMO is essentially “double counting” volcanic forcing). I think that further justification of the inclusion of AMO is warranted. Also, the AMO data set described by Enfield et al., 2001, considers Atlantic Ocean temperatures from 0-70N. Data near the equator will also be affected by El Niño causing some of that signal to leak into the AMO signal.

3) One way to determine possible co-linearities between predictor variables is the use of conditional regress analysis. I suggest choosing a sample model framework and run a conditional analysis to highlight possible co-linearities.

4) This manuscript does an excellent job of breaking down the model results but I feel the manuscript could go into more detail as to why there are differences between the simulations, observational temperature records, and the global impact of these results. For instance, can certain predictor variables explain observed floods or drought conditions in certain parts of the world?

5) There is no figure comparing the various temperature time series observations to the sum of the explanatory variables. I think this would be a very important contribution to the manuscript to 1) Allow readers to directly compare to similar studies and 2) To see how well the reconstructed temperature time series agrees with the data. This is especially important for the recent “temperature hiatus” time period (if it exists) which has received a lot of attention recently.

6) There is no discussion of climate feedbacks and how inclusion of climate feedbacks may influence the results presented here. If they're not to be included in them model framework then some justification is needed.

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Minor Comments:

P2342, line13. Should specify these are surface temperature data sets. Surface data sets show different long term trends than the satellite data which measure tropospheric temperatures.

Figures: In general, the figures are often very small and it is difficult to see finer details, especially figs 5 and 6. If there's any way to increase the size of the figures, perhaps by breaking them into separate panels, it would be useful.

Figure 3: In a printed version of the manuscript, it is difficult to distinguish between the colored lines used for land and land+ocean curves and within each category. The shades of blue for the land+ocean curves are hard to tell apart and same can be said for the land curves. It is easier to see these differences on an electronic version of the manuscript but a few people still like to read hard copies.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2339, 2015.