

Review of Attribution in the Presence of a Long-Memory Climate Response, esd-2015-43, by K. Rypdal

Sorry this review is being submitted after the end of the discussion period. Usually I am sent a series of reminders, but this seems not to have happened. Anyway, I shall keep this brief.

I think the paper is suitable for publication provided the following comments are addressed:

1. The paper cites Lean and Rind, 2008, 2009 & Foster and Rahmstorf, 2011 are prior studies that have been conducted using multiple linear regressions. Another prior study that builds on these three:

Canty, T., Mascioli, N. R., Smarte, M. D., and Salawitch, R. J.: An empirical model of global climate – Part 1: A critical evaluation of volcanic cooling, *Atmos. Chem. Phys.*, 13, 3997-4031, doi:10.5194/acp-13-3997-2013, 2013.

has somehow been overlooked. There are tremendous similarities between the submitted paper and the findings of Canty et al.; as such, this paper must be cited upon revision.

A lot has been written about the fingerprint of AMOC on climate; complete citation of these papers is not needed. But another very important paper that also should be cited, in addition to Canty et al., is:

DeSole, T., Tippet, M. K., and Shukla, J.: A Significant Component of Unforced Multidecadal Variability in the Recent Acceleration of Global Warming. *J. Climate*, 24, 909–926. doi: <http://dx.doi.org/10.1175/2010JCLI3659.1>, 2011.

2. Bottom of pages 1312 / top of page 1313 states:

Delay effects are generally not accounted for ...These are introduced for the sole purpose to improve the fit, they increase the number free parameters in the regression model, and they seem to have little physical justification.

The phrase “little physical justification” is bizarre. Canty et al., and many of the other papers, provide extensive details of the physical justification for the delays. The best paper I know of, documenting the physical origin of the delays, is:

Thompson, D. W. J., Wallace, J. M., Jones, P. D., and Kennedy, J. J.: Identifying signatures of natural climate variability in time series of global-mean surface temperature: methodology and insights, *J. Clim.*, 22, 6120–6141, doi:10.1175/2009JCLI3089.1, 2009.

This phrase should be changed to something attune to mainstream climate science, which certainly has included extensive discussion of the physical basis of these delays (albiet, not in the Lean & Rind and Foster & Ramhstorff papers). A citation to Canty et al. and Thompson et al. for the physical basis of the delays would be acceptable.

3. I’ve already mentioned the Canty et al. paper twice. On the bottom of page 1319, it is stated:

Similarly, the reduction of the volcanic footprint in the LRM 3P model by a factor of approximately 0.5 seems to give a much better fit to the short-time temperature around the large volcanic

eruptions and suggests that the volcanic forcing signal in the forcing data may have been exaggerated.

This is precisely the conclusion of Canty et al., so again, Canty et al. needs to be cited here!

Page 1320 states:

Another feature that appears impossible to explain with only forcing predictors is the low temperatures during the first decades of the nineteenth century and the high temperatures in the decades after World War-II. These anomalies may be compatible with an oscillation with period 60–70 years.

Again, this anomaly was extensively discussed by Canty et al.

5) I really do not understand what is attempting to be conveyed, on page 1320, where it is stated:

Based on this detection it seems reasonable to introduce the AMO index as a predictor variable in addition to the Niño3 index. One could object that inclusion of temperature observations as predictor variables is a self-fulfilling trick. But regression is not really about attributing causes but rather to attribute global temperature variability to a set of signatures (fingerprints). These may signify responses to forcing (causation), but also the global temperature footprint of observed climate signals like the North-Atlantic temperature or the temperature in a region in the tropical pacific.

Perhaps the author is referring to “double counting”, a criticism (highly unwarranted in my opinion) leveled on the ACPD discussion board against the Canty et al. paper. The author may want to have a look at:

Medhaug, I. and Furevik, T.: North Atlantic 20th century multi-decadal variability in coupled climate models: sea surface temperature and ocean overturning circulation, *Ocean Sci.*, 7, 389–404, doi:10.5194/os-7-389-2011, 2011.

and use this paper as evidence for the physical connection, at least in some GCMs, for the relation between North-Atlantic SST and the strength of the thermohaline circulation.

6) I fully agree with the first paragraph of the conclusion section. My group is working on a paper on the hiatus, and we reach a near identical conclusion. Of course we'll cite this paper.

Would be nice to see the work of Canty et al. working into the conclusion section, particularly for the summary of volcanic forcing.

Good luck!

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END OF REVIEW