

Interactive comment on “An empirical model of global climate – Part 1: Reduced impact of volcanoes upon consideration of ocean circulation” by T. Canty et al.

Anonymous Referee #2

Received and published: 11 January 2013

General comments

In this paper a multiple linear regression model is applied to global surface temperature records in order to assess the relative contributions of natural and anthropogenic external forcings and internal variability from ENSO, PDO and AMO. The results suggest that variations in the Atlantic meridional overturning circulation (AMOC) has been a significant factor in the decline of global temperatures following 20th century large tropical volcanic eruptions. By neglecting ocean circulation the paper suggest that volcanic cooling after recent volcanic eruptions might have be overestimated by a factor of 2.

Understanding the governing mechanisms behind multidecadal variability and a proper

C11435

assessment of the relative importance of internal variability and external factors (aerosols, volcanoes, sun) are key challenges in climate research. In particular a better assessment of the effects of volcanic eruptions on global temperatures are important, and the paper offers some interesting perspectives on that topic that is well worth publishing. However, I find several problems with the paper in its current form which must be addressed before any publication might be considered (detailed below). Some of my main concerns have already been mentioned in comments by D. Zanchettin and A. Robock. In view of this I therefore recommend major revision before publication.

Specific comments

1) My biggest concern regarding the paper is the rather uncritical use of AMO as a proxy for ocean circulation. The authors use the term AMO and AMOC interchangeably throughout the text. I have serious objections to this. Given the importance of this assumption for the conclusions of the paper this must be addressed in much more detail. As already mentioned by some of the comments to this paper; the AMOC and the AMO is not necessarily the same. A few points are worth mentioning in here:

• There is no observational based evidence to suggest that AMOC is related to AMO, or that there is an 50-70 year oscillation at all. There are some land-based paleo-based proxy data that might suggest that the recently observed multidecadal Atlantic multidecadal variability (AMV) might also have been a robust feature in the past (Grey et al, 2004). Paleo-records from the actual ocean are, however, scarce. The sortable silt record of Boessenkool et al. (2007) which the authors mention in their response to A. Robock seem to show a relatively robust multidecadal behaviour. However, it is not given that this overflow proxy necessarily reflects changes in the AMO, or even the AMOC. In the tropics, coral records show less evidence for an Atlantic multidecadal oscillation back in time (Saenger et al. 2009). • Some unforced (typical pre-industrial control) climate model simulations do typically find a strong in-phase relation between AMO and AMOC (e.g. Knight et al. 2005). However, the relationship is far from robust. Medhaug and Furevik (2011) looked at the AMO and AMOC in 24 IPCC models. While

C11436

individual models show potential for decadal prediction based on the relationship between the AMO and AMOC, the models generally strongly disagree both in phasing and strength of the covariability. It is therefore not correct to use this particular study as a reference in support of a strong in phase AMOC/AMO relationship (line 15, page 23848) – Some recent model studies have highlighted the potential important role of volcanoes and tropospheric aerosols for explaining recent Atlantic multidecadal variability (Evan et al. 2009; Otterå et al. 2010; Booth et al. 2012). Common for these studies is that they suggest that a large part of the observed Atlantic multidecadal SST variability is in fact radiatively forced, and not so much MOC driven. This is particularly true for the tropical Atlantic. These studies should at least be mentioned and discussed in the text. In fact, as pointed out by D. Zanchettin in the model study by Otterå et al. (2010) suggest a out-of-phase relationship between AMO and AMOC for much of the last 600 years. In fact quite a number of model studies have now suggested that volcanic forcing tend to strengthen AMOC rather than weakening it (e.g. Stenchikov et al 2009; Otterå et al. 2010; Zanchettin et al. 2011). Furthermore, Evan et al. (2009) suggest that as much as 70% of the tropical Atlantic SSTs are due to aerosol loadings during the satellite era (dust and volcanoes). Combine this with the recently published study by Booth et al (2012) suggesting that much of the 20th century AMV can be attributed to tropospheric aerosols there is growing evidence for a strong radiatively forced component of AMV. If so, the authors would in fact to a large degree be “double counting” the temperature response as pointed out by A. Robock in his comment. I therefore strongly advice the authors to change the title of the paper. Ocean circulation should not be mentioned, but rather you should refer to what you are actually using: basinwide Atlantic SSTs. This could of course mean some modifications in terms of interpreting your results. You are of course welcome to hypothesize a AMO/AMOC relationship, but then you also have to properly address the above mentioned points in the discussion. This is not done in the current manuscript.

2) Is a separate subsection on the geo-engineering in the discussion really necessary? It seems to me this could just as well be mentioned in a few sentences towards the

C11437

end. Geo-engineering is not mentioned in the abstract or the introduction so why spend so much time on it in the discussion? You should rather discuss more on the above mentioned issues regarding AMO/AMOC, which I believe should be your main focus.

I generally find the paper to be well written and the figures are nice and clear. If the authors can adequately address the issues raised here, and in particular the issues raised in the comment by A. Robock, I think the paper could be worth publishing in ACP.

References

Bossenkool, K. P., Hall, I. R., Elderfield, H. & Yashayaev, I. North Atlantic climate and deep-ocean flow speed changes during the last 230 years. *Geophys. Res. Lett.* 34, L13614 (2007).

Booth, B. B. B. et al. Aerosols implicated as a prime driver of twentieth-century North Atlantic climate variability, *Nature*, doi:10.1038/nature10946.

Evan, A. T. et al. The role of aerosols in the evolution of tropical North Atlantic Ocean temperature anomalies, *Science*, doi:10.1126/science.1167404 (2009).

Gray, S. T., Graumlich, L. J., Betancourt, J. L. & Pederson, G. T. A tree-ring based reconstruction of the Atlantic Multidecadal Oscillation since 1567 A.D. *Geophys. Res. Lett.* 31, L12205 (2004).

Knight, J. R., Allan, R. J., Folland C. K., Vellinga, M. & Mann M. E. A signature of persistent natural thermohaline circulation cycles in observed climate. *Geophys. Res. Lett.* 32, L20708 (2005).

Otterå, O. H., et al. External forcing as a metronome for Atlantic multidecadal variability. *Nature Geosci.*, 3, 688-694 (2010).

Saenger et al. Surface-temperature trends and variability in the low-latitude North Atlantic since 1552. *Nature Geosci.*, 2, 492-495 (2009).

C11438

Stenchikov, G. et al. Volcanic signals in oceans. *J. Geophys. Res.* 114, D16104 (2009).

Zanchettin, D. et al. Bi-decadal variability excited in the coupled ocean-atmosphere system by strong tropical volcanic eruptions, *Clim. Dyn.*, doi:10.1007/s00382-011-1167-1 (2011).

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 12, 23829, 2012.

C11439