Atmos. Chem. Phys. Discuss., 15, C10427–C10428, 2015
www.atmos-chem-phys-discuss.net/15/C10427/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of major volcanic eruptions on stratospheric water vapour" *by* M. Löffler et al.

Anonymous Referee #1

Received and published: 15 December 2015

This paper focuses on a question of interest (at least to me) and the results look reasonable. That said, the experimental set-up does not seem robust and I cannot recommend publication until that is fixed.

Let me expand on this main criticism. The authors have two runs of their model — one version has the volcanic cloud in it, while the other does not. The difference between these models is then interpreted as the impact of the eruption. Both models, however, are nudged to ECMWF-interim reanalysis, which has the impacts of the eruption in it. Because both versions of the model or being nudged towards a reanalysis that is perturbed by the volcano, it is not clear to me how to interpret the actual difference between these model runs.

To their credit, the authors recognize this limitation, but this seems much more severe

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

than they seem to recognize. For example, around line 25 of p. 34412, they state that the hydrological cycle is “free running”. That is very misleading. Stratospheric water vapor is determined by the cold point in the TTL — and temperature is nudged towards the reanalysis. So the key parameter they are investigating, stratospheric water vapor, is indeed impacted by the nudging.

In the end, I do not believe this paper should be published until the authors can better characterize the difference between the runs.

Other comments: 1) I found the paper difficult to read. The grammar was fine — I’m referring more to the overall style of writing and sentence structure. I don’t have any specific suggestions other than that the authors should spend some time crafting the text. 2) One recent paper on this subject not cited is Dessler et al., (2014), Variations in stratospheric water vapor over the past three decades, J. Geophys. Res., 119, 12,588–12,598, doi: 10.1002/2014JD021712. 3) Section 3.3 argues that tropospheric water vapor increased after the eruption. That neither makes any physical sense nor does it agree with previous research. Given that the eruption cools the troposphere, you would expect tropospheric humidity to decline, which has been seen in observations, e.g., by Soden et al. (2002), Global cooling after the eruption of Mount Pinatubo: A test of climate feedback by water vapor, Science, 296, 727-730. 4) In Section 3.2, the authors turn to the “stratospheric fountain” hypothesis to explain the volcanic impacts on the monsoon regions. That is a weird argument because (to the best of my knowledge) nobody views the stratospheric fountain as a legitimate way to think about troposphere-stratosphere exchange.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 34407, 2015.

ACPD

15, C10427–C10428, 2015