Atmos. Chem. Phys. Discuss., 15, C12636–C12638, 2016
www.atmos-chem-phys-discuss.net/15/C12636/2016/

Atmospheric Chemistry and Physics Discussions

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

T. Shepherd

theodore.shepherd@reading.ac.uk

Received and published: 21 February 2016

I have two major concerns with this paper.

1. There is no connection made with observations. If you are discussing real events, then it is incumbent on you to compare with observations. In your experimental set-up, you are comparing a simulation of the real world with the world that would have been (some kind of counter-factual) without aerosol loading, but assuming that everything else would have been the same. Your attribution of the effect of the aerosol loading would be more convincing if you could show that your real-world simulation agreed with observations. I don't believe there is any observational evidence for increased stratospheric water vapour following Pinatubo (see e.g. the latest historical time series

from Hegglin et al. 2014 Nature Geosci.), and — consistently with this — according to Randel et al. (2000 JGR) the tropical cold point tropopause was not significantly warmed following Pinatubo, though it was after El Chichon. This doesn't mean that the mechanism was inoperative following Pinatubo, only that it was masked by other factors. You can show that by demonstrating that your real-world simulation matches the available observations, and that your counter-factual simulation would have had lower CPTs and lower water vapour than observed. I appreciate that the water vapour observations are uncertain, and unavailable in the tropical lower stratosphere right after the aerosol injection, but there are observations at other latitudes and altitudes. And the temperature is also an important validation field since it is not constrained by your nudging and is key to the water vapour response.

2. I do have some concerns about the nudging. I appreciate that there is no perfect approach here, but by nudging the divergence field, it must be that the vertical motion is strongly constrained. Yet after an aerosol injection, basic dynamics tells us that the additional heating will lead both to warming of the atmosphere and to vertical motion, during the transient phase of the response. (This is the classic Eliassen 1950 response.) By suppressing any changes in the vertical motion, the heating must go entirely into warming and the warming will thus be too strong. This would not be a big issue if the transient phase was short, but in the tropics it could be the better part of a year because of the flywheel effect (Scott & Haynes 1998 QJRMS). By comparing your model temperature with the observed temperature, you could determine how much of an error you are thereby incurring.

A second issue with the nudging is that since you leave only the global-mean temperature free to respond, there will presumably be some artefact in the extratropics because the radiative imbalance from the aerosol loading is only in the tropics. In other words, the entire global mean has to adjust to the level of the tropical adjustment. Is it obvious that this would not affect your results concerning the influence of the monsoon, for example?

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