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Atmospheric Chemistry and Physics
Discussions

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

M. Löffler et al.

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We thank Theodore Shepherd for the very stimulating comments. Here are our replies:

- *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.*

  Reply: The model system has been evaluated (including comparison to observa-

  tions) several times (Jöckel et al., 2006, 2010, 2016). The tropical tape recorder and the different pathways of water vapour into the stratosphere in our model system have been analysed by Eichinger et al. (2015a[1], b[2]). A detailed analysis of stratospheric water vapour anomalies is on the way (Brinkop et al., 2015[3]). Therefore, we omitted a detailed inter-comparison here. Nevertheless, we see the necessity to compare the volcanically perturbed periods in more detail here. In the revised manuscript, we provide (in a new subsection) a comparison of cold-point temperature anomaly, water vapour anomaly, and upwelling anomaly compared to ERA-Interim and water vapour anomaly compared to the recently published data of Hegglin et al. (2014)[4].

- *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*

[1]Eichinger, R., Jöckel, P., Brinkop, S., Werner, M., & Lossow, S.: Simulation of the isotopic composition of stratospheric water vapour – Part 1: Description and evaluation of the EMAC model, Atmospheric Chemistry and Physics, 15, 5537–5555, doi: 10.5194/acp-15-5537-2015, URL http://www.atmos-chem-phys.net/15/5537/2015/ (2015a).

[2]Eichinger, R., Jöckel, P., & Lossow, S.: Simulation of the isotopic composition of stratospheric water vapour – Part 2: Investigation of HDO / H2O variations, Atmospheric Chemistry and Physics, 15, 7003–7015, doi: 10.5194/acp-15-7003-2015, URL http://www.atmos-chem-phys.net/15/7003/2015/ (2015b).

[3]Brinkop, S., Dameris, M., Jöckel, P., Garny, H., Lossow, S., & Stiller, G.: The millennium water vapour drop in chemistry-climate model simulations, Atmospheric Chemistry and Physics Discussions, 15, 24 909–24 953, doi: 10.5194/acpd-15-24909-2015, URL http://www.atmos-chem-phys-discuss.net/15/24909/2015/ (2015)

[4]Hegglin, M. I., D. A. Plummer, T. G. Shepherd, J. F. Scinocca, J. Anderson, L. Froidevaux, B. Funke, D. Hurst, A. Rozanov, J. Urban, T. von Clarmann, K. A. Walker, R. Wang, S. Tegtmeier, and K. Weigel, Vertical structure of stratospheric water vapour trends derived from merged satellite data, Nature Geoscience, 7, 768–776, (2014), doi:10.1038/ngeo2236

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.

Reply: We completely agree. In the revised manuscript we show and discuss the cold-point temperature anomalies of both simulations in comparison to ERA-Interim, as well as the water vapour anomalies at 80 hPa. The results confirm exactly your suggestion: The NOVOL simulation shows a lower cold-point temperature anomaly and consistently lower water vapour. The VOL simulation, in contrast, overestimates both, the cold-point temperature anomaly and correspondingly the water vapour anomaly.

From this, we conclude that the masking factor you mention (see also Fueglistaler et al., 2012[5]) is indeed captured correctly by your nudged model: We see an attenuated upwelling in the period right after the Mt. Pinatubo eruption, which cools the tropical tropopause, and which is (over)compensated by an (in our case) too strong volcanic heating.

- *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*

[5]Fueglistaler, S. (2012), Stepwise changes in stratospheric water vapor?, J. Geophys. Res., 117, D13302, doi:10.1029/2012JD017582.

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.

Reply: The nudging by Newtonian relaxation as we applied it, does neither “strongly constrain”, nor “suppress any changes” in the vertical motion. The model retains all its degrees of freedom, although possibly damped. Indeed, the vertical velocity between NOVOL and VOL does change, as we expect it. This is shown by an additional figure (in the Supplement of the revised manuscript) of the differences in upwelling (w^* calculated according to the TEM (Transformed Eulerian Mean) method as described by Holton, 2004[6]).

To show that the nudging indeed mostly increases the signal-to-noise ratio, if two different simulations are compared point-by-point, we performed an additional sensitivity study of two simulations, in which we only nudge the (logarithm of the) surface pressure. The resulting signals of the differences are very similar, though superimposed by “meteorological noise”. An additional section on sensitivity studies in the revised manuscript discusses this.

- *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?*

Reply: We think that this is a wrong view on what the nudging is doing. As we

[6]Holton, J. R.: An Introduction to Dynamic Meteorology, International Geophysics Series, 4th edn., Academic Press, San Diego, New York, USA, 2004.

explain above, the model keeps all its degrees of freedom, implying the possibility that local temperature perturbations can propagate and be converted to vertical motion. The relaxation towards the local temperature anomaly does not imply that the model response can only be by global adjustment. Only the signal propagation might be damped due to the continuous relaxation towards a defined (local) state.

As above, we refer to the additional sensitivity study presented in the revised manuscript. We find a similar monsoon signal also in a quasi-free running simulation.

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