Atmos. Chem. Phys. Discuss., 9, C990–C993, 2009
www.atmos-chem-phys-discuss.net/9/C990/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The climatic effects of the direct injection of water vapour into the stratosphere by large volcanic eruptions" *by* M. M. Joshi and G. S. Jones

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Received and published: 20 May 2009

Anonymous Referee 2 Major comments: We have clarified the mechanism in the present paper (see also answers to L. Glaze's comments). We have also added a schematic to further clarify what we mean. A fuller description of the model, with references to papers describing its performance as well as its previous use in stratospheric research, is now in the text. We also refer to the transport timescales of the water vapour anomaly being consistent with previous work in the results section. Polar stratospheric cloud formation is included in the model physics. While a model that fully resolves the stratosphere is desirable for such studies in future, such a model would have to be fully coupled to the ocean, and spun up, which is impractical. Given that, and the other uncertainties below, and HadGEM1s performance, the present model configuration is deemed adequate.

More description is put in the text about how well the model simulates other volcanoes including Pinatubo.

We have added more details of the experimental description, particularly the model initialisation and the length of the integrations.

RF: a) Water vapour is advected as a tracer in the stratosphere: we have made this clearer in the revised text.

RF: b) The method of estimated radiative forcing is clearly described in Forster and Taylor 2006. We believed that it would have been distracting from the main aims of this paper to put in detail that may not interest most readers. However we have included a short summary of the method.

Using 20 years to deduce the statistics: We do not agree that using the mean of the 20 years before the eruption to calculate the climate change is appropriate. We are interested in the immediate change following the volcanic eruption. As climate has variability on decadal timescales any variability in the 20 years before the eruption could contaminate the calculated cooling. In particular any forced (e.g. from other volcanoes or from solar variability) or unforced variability before 1880 could make the observed cooling. We note that the use of such short periods to take as a reference has been used in other studies (e.g. Hansen et al. Clim. Dyn. 2007).

We do not understand why the reviewer feels that the t-test we use is not sufficient. If it were not sufficient then we would not be getting the fairly significant values we have obtained. We are comparing the cooling of the V simulations with the cooling from the VSW simulations. We use a t-test to test the significance that the difference between the two cooling values is due to internal variability or chance. The p-value gives that probability, so the lower the number the more significant the difference. A p-value of 0.06 does mean that there is a 6% chance that the VSW is warmer than V by 0.1K due to internal variability. We have changed the text to try and explain this standard statistical method more clearly.

Magnitude reduced: We agree that the statement is too strong. The NH mean as a whole is not "particularly reduced in magnitude", however the high latitudes are, as can be seen in Figure 4 (figure 5 in the new version).

Other impacts: the effects of chemistry have not been included as this was a first attempt at looking at the climatic effects of a stratospheric water vapour pulse of this type. Future work should include looking at chemical effects and we say this in the revised text.

Impact minor: The inclusion of water vapour does have an impact by causing relative warming: we would have to disagree with the reviewer: we think that a mechanism that can cause a $\sim$28% change in the global cooling following an eruption (or even more if the stratospheric water vapour anomaly is larger) is significant Specific comments:

P5448 L10: we have changed this We have clarified the statement in that the warming refers to the tropospheric climate.

P5449 L30: The different timescales are due to the aerosol droplets falling out of the stratosphere, which is now mentioned.

P5453 L17: We have corrected this.

P5454 L27: Temperature measurements over the oceans have smaller uncertainties than over land in this period mainly because observations were taken over a much wider spatial area. This coverage uncertainty is one of the biggest uncertainties in the measurement of the temperature of a large area. We refer to the Brohan et al. paper in the references section if the reader is interested in more details.

Fig 1: (now Fig 2) we have changed the colour scheme to make values straddling zero

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white.

Fig 4: (now Fig 5) this is an error and has been removed.

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