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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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F. Prata (referee) comments

1. We did not suggest that a phreatomagmatic eruption was the main source of water. We thank the reviewer for pointing out that such explosive reactions between the hot magma and water may not have been a major part of the Krakatau eruption. In addition, we are not saying that seawater itself is in the eruption cloud; rather that enhanced evaporation from the ocean surrounding an island adds to the moisture entrained into the cloud. Indeed this is quantitatively unconstrained somewhat: future modelling work with a cloud-resolving numerical model (one with a horizontal resolu-

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tion of O(km)) might help to constrain this. Indeed, if significant amounts of seawater were in the eruptive cloud, then sea-salt aerosol would play an important role in the radiative budget of the atmosphere following the eruption.

We have made it clearer in the text what the origin of the water vapour could be in our hypothesis (evaporation of water from pyroclastic flows over the sea), and the uncertainty in the amount of water used in the simulations.

Noctilucent clouds: water vapour being transported from the troposphere to the mesosphere over a timescale of 2 years does require a new mechanism. However, if water vapour parcels were initially at a height of 30-35km, they would reach the height at which noctilucent clouds are observed if transported upwards with a mean upward velocity of $\sim$0.7 mm/s or 60 m/day. Interestingly, such average velocities are diagnosed between 40km and 80km. We now state this in the revised text, but we do stress that the evidence is circumstantial.

2. We have added extra information about the past estimates of how much water vapour could be injected into the stratosphere by volcanoes. We note that the comment by Bekki et al (1996) refers to a modest forcing from water vapour and ozone compared to the aerosol. They do not give a value for that, but another simulation estimated the aerosol forcing from a "supereruption" could be of the order of -60Wm-2 (Jones et al. Clim.Dyn. 2006), so potentially the forcing from water vapour and ozone they estimate could be of the order of 10% of the aerosol forcing: not too dissimilar to what we have discovered.

3. Yes- according to the Sato et al. dataset we use, the amount of stratospheric aerosol produced by Krakatau is similar to Pinatubo. We make this clearer in the text, but also add that there are large uncertainties for the former eruption.

4. We use observed temperature changes as measured by thermometers (meteorological stations on land and ship/buoy measurements at sea). The proxy measurements mentioned are related predominantly to land temperatures. There is a stronger cooling

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following the Krakatau eruption in observed land temperatures than there are at sea, but the uncertainties are much larger for land temperatures in the 19th century. There are large uncertainties associated with the proxy measurements mentioned, as can be seen if the interannual variability of the records is examined, so it is unlikely that they are more accurate in this period than the temperatures deduced from thermometers. We use a short reference period to examine the cooling following Krakatau to minimize any influence from variability (forced or unforced) before it. We are also interested in the immediate cooling following the eruption, so we are interested in the change in temperatures over the relatively short timescale. Such an approach is used elsewhere (e.g. Hansen et al. Clim. Dyn. 2007).

The amount of aerosol included in the model for Krakatau is similar to that following Pinatubo. Because we use an aerosol climatology (Sato et al. 1993), it is not important for the purposes of comparison that the aerosol came from a single eruption or multiple eruptions, or whether the petrological estimates of the amounts of SO_2 emitted are different. In fact for 1884 there is a bigger optical depth than for 1992, but there are unquantified uncertainties on the changes in optical depth.

5. The cooling following Krakatau does appear to be much smaller than the cooling from Pinatubo and that expected from models. This has been noted by other studies (as referenced in our paper). There are large observational uncertainties, which means that it cannot be ruled out that there was some cooling (this is also commented on in the paper), but equally there is no strong evidence there was a large cooling. We have already discussed in the paper whether ENSO variability may have had a part in the lack of cooling (unlikely) and the impact of NAO variations, which are much more important for regional/seasonal temperature changes and not global annual mean variations.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5447, 2009.