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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Microphysical, macrophysical and radiative signatures of volcanic aerosols in trade wind cumulus observed by the A-Train" *by* T. Yuan et al.

Anonymous Referee #1

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The manuscript by Yuan et al. examines the impact of a long-lasting non-explosive volcanic eruption on the microphysical and macrophysical properties of trade cumuli over the Pacific Ocean downwind of the largest of the Hawaii islands. The idea is brilliant and the manuscript is well written. The results show a significant impact of aerosols, presumably sulphate aerosols from emissions of sulphur dioxide, on the cloud microphysical properties (cloud droplet size and cloud optical depth) and the cloud amount. I think the manuscript has the potential to be an excellent article and it is for that reason only that I am calling for major revisions. I hope that the suggestions below will help to achieve this.

Correlation does not imply causality. So the usual way of proceeding to suggest there is indeed causality between the presence of volcanic aerosols and the cloud modification is to envisage all other possible reasons one can think of to explain the correlations, and try to rule them out one by one. The authors discuss meteorology and the island wake effect as two possible confounding factors. There are a few more, however small and unlikely they might be, that should be discussed as well.

1) The volcano is probably emitting other chemical species like CO_2 and water vapour H_2O. Could these trace species change the heating rates and moisture availability in the region with increased aerosol loading?

2) In a similar vein, the volcano may be a source of heat, could that change the thermodynamic of the clouds in the volcanic plume?

3) The presence of aerosols may contaminate the cloud retrievals. Can the authors make a convincing case that any contamination effect is much smaller than the aerosol effect on clouds?

4) Could it be that the SO_2 or CO_2 emitted by the volcano contaminates the cloud retrievals? For instance SO_2 has absorption bands at 6.7 and 8.3 μm, can the authors make sure that these wavelengths are not used in the retrievals of cloud droplet size and cloud optical depth?

Back-of-the-envelope calculations should be enough to rule out factors 1) and 2) above. Factor 3) might be more tricky to address, while factor 4) can certainly be looked at.

The authors are right to say that the volcanic emissions would not depend on the meteorology, however the atmospheric transport of the plume does, and it would be good to have a feeling for how much variability in the atmospheric circulation and cloudiness there is from year to year.

There are two ways to diagnose the impact of the aerosols on the cumulus clouds: one

can either i) compare the cloud properties inside and outside the plume or ii) look at the anomaly in cloud properties in 2008 compared to previous years. The authors are doing both: as I understand it, figures 1, 2, 3, 4, 7, 9A and 9C are about the former, while figures 5, 6 and 9B are about the latter. The authors should be more explicit about this. The impact of the island wake on the cloud properties can only be ruled out from method ii), not method i).

I am not convinced by the normalisation procedure that is described on page 6424. How much variability is there from year to year in the cloud properties averaged over the box of Figure 6? Also the region for normalisation (DL to 155W, 10N to 25N) is somewhat arbitrary and it would be good to know the sensitivity of the results to that choice.

By normalising the cloud cover or cloud optical depth, the authors eliminate any large-scale impact of aerosols on clouds (because these average to zero by construction). I suspect the areas of negative COD change on Figure 6 are the results of the normalisation procedure. This is something important to establish and discuss because it would be good to know if the aerosols have an impact on the cloud properties in the "far field". Is the increase in COD downwind of the volcano compensated or not by a decrease in COD further downwind of the volcano when aerosol levels are down but water availability is still high? This might be difficult to prove or disprove but worth investigating. Also it is difficult to figure out the magnitude of the change in cloud optical depth from Figure 6 (I estimate it to be 20% (from -10% to 10%) of 20 or about 4 units, but is this correct?).

It is important to consider the wake effect from the island as a possible confounding factor, however I am not sure why the authors restrict their analysis to an SST effect only. I also wonder if the combination of the wake and aerosol effects could result in a larger sensitivity to the microphysics than the aerosol effect would do if it was alone (pure speculation on my side).

Figure 2, and page 6422, line 15ff: this is not clear at all from the plots. I'm not sure where to look on Fig. 2d. Are these the best plots to make this point (which is certainly valid if I judge from the other plots)?

Minor comments:

page 6416, line 5: what buffering mechanisms?

page 6416, line 7: or should it be the other way around? Modifications in trade cumulus clouds are associated with aerosols.

References should be cited with the year in brackets only when they form part of a sentence, e.g. on page 6417, line 27: "Stevens and Feingold (2009) provide ...". There are many other occurrences in the text where this is not the case. Can the authors go through the text and change this?

page 6419, line 14: "... the diffusive nature of the mixing..."

page 6419, lines 25-26 contradict directly lines 17-18. This needs to be phrased better. See also my major comment above.

page 6419, line 5: note that the transport of the aerosol from the volcano is not independent from the meteorology.

page 6423, line 15: I don't know what is meant by 3~8 μm.

page 6425, line 10: "microphysical"

page 6426, line 19: make it clear that the recent observations are for clouds affected by ships not by a volcano as in this paper.

page 6427, line 29: not clear where the 4 Wm^{-2} comes from. Can the authors explain how they come to this number?

page 6438, line 3: "Eeffect" should be "Effect"

Figure 7 presents a longitudinal mean for each latitude, which assumes some latitudinal consistency. However it is clear from the previous figures that the mean aerosol transport and effect is not exactly east-west, but it is a bit slanted. Could values be averaged along such an axis on Figure 7?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 6415, 2011.