

Interactive comment on “Effect of volcanic emissions on clouds during the 2008 and 2018 Kilauea degassing events” by Katherine H. Breen et al.

Anonymous Referee #1

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This study used satellite observations of aerosol and cloud, and AGCM simulation to investigate aerosol-cloud interaction downwind of the Kilauea volcano during eruptive events of 2008 and 2018. Level-3 MODIS and CALIOP cloud retrievals are used to calculate anomalies in cloud properties vs. climatology during the two events, which are compared with the GEOS model simulations. In the 2018 event with stronger volcanic emissions, significant ($p < 0.3$) anomalies in liquid and ice cloud properties are observed from satellites and largely reproduced by the GEOS model, and the simulated anomalies are found to be attributable mostly to the AIE due to volcanic emissions other than meteorological effects. The authors then used the model to illustrate specific pathways how changes in microphysics alter liquid and ice cloud properties.

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The topic is within the scope of ACP, the methodology is scientifically sound, and the paper is clearly written and easy to follow. I also found some issues that need further confirmation, especially about how “similar” the two events are. I support the publication of this paper if the following concerns could be addressed.

Major points:

I am a little confused about the current description on changes in cloud properties during the 2008 event, and the “similarity” between the two events. If we look at the summary of MODIS observations in Table 2 and 4, 2 liquid cloud parameters (COD and Reff) show a p-value less than 0.3, whereas all the observed parameters have a $p < 0.3$ during the 2018 event. Simulated changes are also less significant than the simulation for the 2018 events. Interestingly, the authors themselves have conducted the 2008_5x simulation to illustrate that stronger and statistically significant anomalies (similar to the 2018 events) could be derived by enhancing the 2008 emissions, and states that “This and the similarities in spatial patterns for cloud anomalies in JJA 2008 (Figure 6) and MJJ 2018 (Figure 7) suggested a threshold response to overcome meteorological effects.”

Therefore, my understanding is that the 2008 event is DIFFERENT from the 2018 event. The observed anomalies are not as strong to overcome possible anomalies from meteorology in liquid cloud properties other than COD and Reff, and the plume cannot reach as high up to modify ice cloud properties. I do agree that this difference is possibly due to the weaker volcanic emissions, which makes the 2018 analysis unique and important. I suggest the authors to modify the description of “similarity” between the two events in the paper (I will also mention several places later), and emphasize their difference and the uniqueness of the 2018 event in the revision.

Minor points:

Page1, Line 8-9: Since the two events are not similar in all aspects, please specify the parameters that are “similarly changed”. For example, “Significant changes ($p < 0.3$) in

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cloud effective radius and cloud optical depth in both events suggested that. . .”

The presentation flow in the introduction (before Section 1.1) is a little confusing. Paragraph 3 (Page 2, Line 10-20) is detailed description of the AIE, which follows Paragraph 1 more closely. At the same time, Paragraph 2 (Page 2 Line 1-9) introduces AGCM and the difficulty to disentangle meteorology effects when interpreting observations, and I think Paragraph 4 (about the unique conditions Kilauea provides) follows closely from that point. Therefore, I suggest Paragraph 2 and 3 to be swapped.

Page 6, Line 13: In the other simulations where the emission height is changed (2008_PH2km and 2018_PH4km), how are the emissions distributed vertically?

Page 8, Line 10: Add “2008_PH2km” in the sensitivity simulations.

Page 8, Line 10-11: Add “and interpret their differences GEOS simulations.”

Page 12, Line 1: This statement only applies to the 2018 event.

Page 12, Line 5: Delete “The plume domain”

Page 12, Line 6: Add “and agrees in magnitudes with the 2018_1x.”

Page 17, Line 21: “25.8” disagrees with the number in Table 4 (20.59). Please double check consistency of numbers cited in the text and in the tables.

Page 17, Line 22-23: I am confused. Should this indicate microphysical control is weaker than meteorology for IWP?

Section 5.1, Figures 10-11: Does it make sense to also include the discussion of climatological profiles?

Figure 11: typo in the legend, should be “2018_PH4km”

Page 28, Line 18: suggested to be revised to “the simulated ACI signatures. . .”

Page 29, Line 10: add “and their discrepancies are largely attributable to different magnitudes of volcanic activities and aerosol loadings.”

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