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Interactive comment on "Systematic satellite observations of the impact of aerosols from passive volcanic degassing on local cloud properties" *by* S. K. Ebmeier et al.

Anonymous Referee #1

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Review of "Systematic satellite observations of the impact of aerosols from passive volcanic degassing on local cloud properties" by S. Ebmeier, A. Sayer, R. Grainger, T. Mather, and E. Carboni

General Comment: This paper builds on an active body of research that aims to understand and quantify the indirect effect of volcanic aerosol on downstream cloud properties. What sets this paper a part from other satellite-based studies is the considerably longer observation period, analysis of more active volcanic islands, and attempt to distinguish orography using multiple control islands. While the authors provide consistent results that strengthen conclusions made by previous studies additional steps could

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be made to forge new discoveries. By incorporating additional data (e.g., CERES and MACC discussed below) more could be stated about the potential implications of volcanic aerosol emissions on present day indirect radiative forcing. This paper could also benefit from improvements to the writing and clarification of some technical details.

Other Comments: Throughout the paper it is stated that isolated volcanic islands capture the aerosol indirect effects of the pre-industrial atmosphere. However, it is difficult to determine how important these volcanic islands are on the Earth's radiation budget when explicit indirect forcing estimates have not provided in this paper. Therefore, I suggest adopting the approach described in Yaun et al. (2011) in which they use CERES (Cloud and the Earth's Radiation Energy System) monthly mean observations to derive the top of atmosphere shortwave radiative flux. They find a 20 W/mˆ2 perturbation caused by the Kilauea plume. Similarly, this approach could be applied to the additional islands examined in this study to determine if the same sensitivity is observed at other locations. It might be beyond the scope of this study but it would be fascinating to examine volcanic plumes near major sources of anthropogenic activity (e.g., along coasts) to demonstrate (by comparing the responses) that, indeed, remote volcanic islands represent the “pre-industrial” atmosphere conditions. These additional steps would certainly make this paper more appealing to the scientific community.

It is mentioned briefly in the manuscript that averages (of cloud & aerosol properties) are constructed over 6-10 years of data (Pg2681 L6-9 & PG 2682 L28). How exactly is the sampling distributed over this time period? How does the sampling vary over the seasonal cycle? As I understand it, both cloud and clear-sky conditions are necessary so that both aerosol and cloud properties can be analyzed within the satellite field of view. How often does this condition fail? My concern is that these types of failures may be weighted more heavily during certain times of the year when metrological conditions produce either too much cloud or not enough in the lee of the island. Finally, the continuity of statistical sampling (over the seasonal cycle) needs to be discussed in greater detail.

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The sample size could also be biased due to atmospheric vortices caused by the wakes that islands generate. A prominent example of this is the two large quasi-steady counterrotating eddies that extend several hundred kilometers downwind of the big island of Hawaii (Smith and Grubisic, [1993], JAS). The sampling bias for Hawaii is probably small because this feature is quasi-stationary over the annual cycle, however the lower summit heights and substantial variation in thermodynamic conditions at other island locations may cause inconsistent wake patterns that could compromise the interpretation of the results in this study. Both changes in meteorology and aerosol concentration exert significant control on the properties of low-clouds. For example, as the lower troposphere stability decreases clouds become more convective thereby causing the effective radius to increase; similarly, a decrease in aerosol optical depth causes effective radius to increase (Lebsock et al. [2008], JGR). Thus, it is essential to examine the effects of meteorology on downstream cloud properties as a way to rule out mechanisms that could be mistakenly interpreted as an aerosol indirect effect. I suggest examining the response as a function of the Froude number.

Froude number is useful to diagnose whether the airflow will be forced up over the mountain causing trapped-lee waves or "blocked" and around causing lee-vortices downstream (Etling, [1989], Meteorl. Atmos. Phys.; Schar and Durran, [1997], JAS; Epifanio, 2003, Encyclopedia of the Atmospheric Sciences). The type of generated lee-waves may affect the sampling and properties of the retrieved aerosol and clouds. This should be investigated (as a function of Froude # or some other thermodynamic parameter) to strengthen the argument made in the paper that these orographic contributions to the mean cloud properties are indeed smaller than the influence from volcanic aerosol. Froude number can be calculated from ECMWF reanalysis data for the atmosphere below 700 hPa (Hughes et al. [2009], JAS provides a nice description of calculating the Froude #).

The manuscript is rather lengthy and could easily be shortened by 20% without losing any essential content. For instance the authors discuss, several times, methods in the

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paper that they considered to apply in their research however did not actually carry out. This type of information is extraneous. For example, pg2680 L15-20 discusses adopting a ship track finding algorithm to find volcano tracks but does not use it for the research here. Is this information necessary? Another example is the discussion about the use of trajectory analysis on Pg's 2697 to examine indirect effects near continents. I recommend removing both of these discussions or describing them in a separate paragraph aimed to establish future research initiatives.

Minor Comments: Pg. 2676: L1: I believe you are referring to the research of Schmidt et al. 2012, ACP. If so, you need to specify that the "significant source of uncertainty" comes from a global aerosol microphysics model.

Pg. 2676: L5: The line "Understanding the impact of volcanic emissions on indirect radiative forcing is important…" might be overstating its effect on climate. It is not clear how important it is because few studies have estimated the forcing (all of which used modeling data that is typically fraught with cloud property biases). Instead, the intended meaning could be changed appropriately by replacing the bold faced words "is important" with "provides a tool to study."

Pg. 2676: L9: Acronyms (MODIS & AATSR) should be spelled out when first defined.

Pg. 2676: L10-11: It is unclear what it means that the retrievals were rotated in this sentence. Specify that the retrievals were rotated about the volcanic vent to be parallel to the wind direction. Also, it's not clear in this sentence why signal to noise is improved without digging into the manuscript. Wasn't the rotation needed so that the upstream retrievals could be compared to the downstream retrievals?

Pg. 2676: L19-20: This sentence makes it sound like this is the first study to examine the volcanic indirect effect when it is not. This research does however present new information that should be highlighted here instead. What sets this paper a part from Gasso 2008 and Yaun et al. 2011 is that it provides a considerably longer observation period over multiple active and non-active volcanic island sites to estimate volcanic emissions on low-cloud properties.

Pg. 2677: L10-11: Evaporation of small droplets in polluted clouds can also be invigorated if the overlying air is sufficiently dry (Ackerman et al., 2004, Nature).

Pg. 2677: L11: Should specify that this is the change in the Earth's radiative balance "as CO2 levels rise." The aerosol indirect effect is minuscule compared to the incoming and outgoing radiative fluxes at the Earth's surface.

Pg. 2677: L23-24: I think the intended meaning of this sentence is to suggest that studies examining the "local effects of aerosols on clouds" is dominated by ship track studies. There are of course numerous studies that examine the indirect effect on global scales (e.g., Chalson et al. (1992), Science; Lebsock et al. (2008); JGR, Quaas et al. (2008),JGR Bellouin et al. (2013), ACP to name a few).

Pg. 2677: L27: The statement "the impact of the polluted clouds are otherwise identical in origin and thickness to clean clouds in the surrounding cloud deck" is not always true as pointed out by Christensen and Stephens, 2011, JGR where they observed that the polluted clouds are often elevated in height compared to the unpolluted clouds.

Pg. 2687: L17: Please quantify the words "high proportion" that degassing volcanic SO2 flux emissions make up in the atmosphere.

Pg. 2681: L3, L6; L12, L15, L20, and elsewhere; the word "we" is used repetitively and is distracting – consider adopting other writing styles to enrich the language in the manuscript.

Pg. 2682 L1: Don't you mean cloud top pressure > ("greater than") 440 mb?

Pg. 2682 L2: What do you mean by sampling rate of around 100-300 per bin? Is this the average number of samples you get in a 10 km grid box per day?

Pg. 2682 L8: How often does ECMWF output metrological fields? Are you using the 8-times daily product? Is this the Interim or ERA product? Please specify.

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Pg. 2682: Have you considered using the MACC (Monitoring Atmospheric Composition & Climate) aerosol ECMWF reanalysis product to examine the background composition? MACC is a framework developed by ECMWF Integrated Forecast System (IFS) which fully couples a numerical weather prediction model with data assimilation of satellite aerosol optical depth. The great advantage here is that aerosol optical depth retrievals can be obtained in regions with clouds! Incorporating this dataset would boost the number of samples for your study providing more robust statistics as well as offering an independent measurement of the aerosol indirect effect.

Pg. 2684: L22-23: Is the standard error larger for AATSR because the footprint size is larger? Please state the reason why smaller sample size occurs compared to MODIS.

Pg 2685: L1-5: I applaud the use of multiple independent measurements in the research; it strengthens many of these findings. However, more information needs to be provided about the retrieval. Please specify the wavelengths that are being analyzed for each satellite sensor. For example, MODIS retrieves effective radius at three different wavelengths (i.e., 1.6 um, 2.1 um, and 3.7 um). Which set of wavelengths are you using? I am also surprised to see such large absolute differences between MODIS and AATSR. For example, Chen et al., (2007), JAS, observe only slight variation ($\sim$ 1um) amongst the MODIS channels referenced above.

Pg: 2686: How does the number of cloud retrievals (upwind and downwind of the volcano) relate to the cloud cover fraction? Have you examined MODIS & AATSR cloud cover fraction variables? Does the volcanic plume increase the liquid phase cloud cover fraction? Fig 4. Indicates that there is considerably more cloud retrievals downstream of the Hawaii volcanic vent, it would be interesting to quantify this for each island! I'm sure the effect is big, Yaun et al. (2008) find a 10% increase.

Pg. 2691: L4: define 'a.s.l'

Pg. 2692: L4-6. You might find better wind-aerosol relationships using MACC aerosol data product. Separate species (sea salt, sulphate, ect. . .) of aerosol are reported in

MACC data. You could also directly estimate the Twomey effect aerosol and cloud properties would be retrieved in the same location.

Pg. 2694: L18 – 28. Please refer to Grandey et al. (2013), ACP for a complete description of aerosol optical depth retrieval artifacts under broken cloudy conditions. Also, these retrieval artifacts will be less affected using MACC data. It thus, may thus be worth considering this dataset for the analysis.

Pg. 2696: L8: What is a “volcanic ship track,” shouldn’t it just be “volcano track”

Pg. 296: L11-12: What do you mean by the words “particular days”? Were some of the days excluded from the analysis? This question leads back to my concern about how the data was sampled temporally and whether all seasons are being sampled equally.

Table 1: The column “summit height” is somewhat misleading because the emission altitude of the volcanic plume, as in the case of Kilauea, can be significantly lower in elevation than the summit of the island. Can you add the summit height (highest point on the island) and emission height in parenthesis?

Table 2: Why were the control islands omitted from this table?

Table 3: What does sigma represent? Is it the standard deviation, standard error, or something else? Also, is this table even needed, these values are already provided in Fig 6, are they not?

Fig1. I don’t understand the caption “using Dark Target and ocean datasets, with Deep Blue to fill in gaps over land?” Are you referring to some particular type of MODIS algorithm or the color scheme used to fill in gaps over the land? Please clarify.

Fig2. It is very difficult to read the words on this figure because the characters are very small. If you switch the order of the columns and rows and keep the range on the y-axis the same you would only need to show it once on the left most plot (e.g., show plots b, f, j, n, r, & v in one row; then cloud effective radius in the next row and so on). I cannot read anything inside the picture maps of each island. The valuable piece of information here is the wind rose. I would prefer seeing only the wind rose for each island than being distracted by everything else inside the map. These issues may also become resolved if the figure can be made larger.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 2675, 2014.