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Discussions

Interactive comment on "Analysis of properties of the 19 February 2018 volcanic eruption of Mount Sinabung in S5P/TROPOMI and Himawari satellite data" *by* Adrianus de Laat et al.

Anonymous Referee #1

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General comments

In this paper, de Laat et al. present a multi-platform, multi-sensor satellite remote sensing analysis of an eruption of Mount Sinabung (Indonesia) in 2018. In particular they focus on the 19 February 2018 paroxysmal eruption which produced a volcanic eruption cloud made up of ice-rich, ash-rich and SO2-rich components. The analysis focusses on a validation of three TROPOMI height retrieval algorithms (FRESCO, ROCINN and O22CLD) against a fortuitous CALIPSO lidar (CALIOP) pass that intersected the nascent volcanic cloud. Himawari-8 data is used to interpret the composition, evolution and height of the volcanic cloud and comparisons are made

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between the Himawari-8 ΔBT, TROPOMI AAI and SO2 retrievals. The analysis is suitable for NHESS. However, I have some major concerns about the interpretation of the data presented and the relationships drawn between the TROPOMI AAI and Himawari-8 ΔBT values. There are also numerous points of clarification needed throughout the manuscript. These concerns are expressed in the Specific comments and Technical corrections sections below. These comments should be addressed before the manuscript is considered for publication in NHESS.

Specific comments

My major concern is with the relationships drawn between the brightness temperature difference (ΔBT), Absorbing Aerosol Index (AAI) and SO2 total column amounts (i.e. Fig. 5). By eye, it looks like there is no correlation at all. However, it's difficult to tell as no statistical metrics are given. I suggest adding a statistical metric (perhaps a correlation coefficient if the relationship is expected to be linear) to demonstrate that there's a notable relationship (as the authors claim). Further comment on this is provided in the Technical corrections section.

Another concern is the reliance and interpretation of the 'VADUGS' algorithm. The authors refer to a conference talk, which in general is fine, but as some of the conclusions reached rely on an understanding of this algorithm and its uncertainties a reference to a published article describing it is necessary (in my opinion). If it is not published elsewhere, then a section describing the algorithm should be added if it is to be used in the comparison of the TROPOMI data.

Another issue is the interpretation of the CALIOP data. The CALIOP pass clearly showed a feature that reached 18 km (asl). This is not mentioned anywhere. In addition, the feature (on average) reached cloud-top heights of 16 km (the authors cite a height of 15 km). It is important to get this right as this paper could be a nice

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reference for the eruption height of the 19 February 2018 Sinabung cloud in the future.

In general, the authors use the terms ash plume, volcanic cloud, volcanic ash, volcanic ash plume, aerosols etc interchangeably to refer to the eruption cloud (and in some cases to refer to components of the volcanic cloud that were ice-rich). I suggest that the authors define these terms early on in the manuscript. This will avoid confusion, especially when discussing the microphysical make-up of the volcanic cloud. One suggestion could be to use the generic term 'volcanic cloud' to refer to a cloud of volcanic origin and use the terms ice-rich, ash-rich and SO2-rich to refer to regions of the volcanic cloud that exhibit these spectral signatures.

Technical corrections

Title: Please be consistent with the use of 'Himawari'. In the text and section headings, the authors use all capital letters in some cases (it is not an acronym). I suggest using 'Himawari-8' throughout the manuscript instead of just 'Himawari' as this is the platform that is used for the analysis (there is a Himawari-9 now, so the distinction is important).

P1L18: 'Evaluation of corresponding Himawari geostationary height retrievals based on InfraRed (IR) brightness temperature differences...' - This statement doesn't seem correct to me. It's the evaluation of the brightness temperature differences (not the height retrieval) that indicates whether the volcanic cloud contains ash or ice/water particles. I suggest changing to 'Evaluation of Himawari-8 geostationary InfraRed (IR) brightness temperature differences...'.

P2L35: 'global monitoring of volcanic eruptions..' - Volcanic Ash Advisory Centers began to do this in 1987 under the International Airways Volcano Watch (see Lechner et al., 2017). This could be mentioned here as well.

P2L57: Change ‘like’ to ‘such as’.

P3L71: Change ‘improved heights’ to ‘improved height retrievals’.

P3L78,L80: Please check the citation style for Smithsonian reports. I don’t know which report the authors are referencing. The citation styles used in these two lines are different and I only see one reference to the Smithsonian Institute in the References section. I suggest using their guidelines (i.e. ‘Cite this Report’ link) for referencing reports.

P3L84: Change ‘Calipso’ to ‘CALIPSO’.

P3L85: ‘13:30’ is this local time (LT) or UTC?

P3L88: Change ‘from combining’ to ‘by combining’.

P5L138: ‘column mass load’ - Please provide units. Also, to be consistent with the literature, the authors could use ‘column mass loading’.

P5L145: ‘attenuated backscatter imagery’ - Please be more specific. Is this the level 1 version 4, 532 nm total attenuated backscatter product (L1-Standard-V4-10)? There were several recent changes to the CALIPSO lidar calibration from version 3 to 4. Also an up-to-date reference could be added (a series of papers on the new version are published in AMT).

P5L153-155: What about wind shear? This is a common effect known to disperse volcanic ash at different altitudes and different directions. I think it would be worth adding an atmospheric sounding figure to help understand the role of vertical

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wind shear.

P5L158: 'local time of 06:25 UTC' - Is this local time or UTC?

P6L163: 'separation between aerosols and SO2' - I would say volcanic ash and SO2. As use of the term 'aerosols' could be interpreted as 'sulphate aerosols'.

P6L164: 'Cook et al. (2014)'. Could add references to Moxnes et al. (2014) and Prata et al. (2017), which both specifically investigate the separation mechanisms of volcanic ash and SO2.

P6L166-168: I would consider moving the VIIRS and NOAA/CIMSS volcanic ash retrieval Supplementary Figures into the main manuscript. The true colour VIIRS image is important for context and interpretation of the TROPOMI height retrievals (presented in Fig. 1). Also, use of NOAA/CIMSS retrievals (which are referred to for the cloud height in this sentence) should be stated with the correct references (i.e. Pavolonis et al. 2015a, b).

P6L171: 'Systematically higher' - This implies FRESCO cloud heights are always higher than the O22CLD heights. Based on Fig. 3, this looks to be the case from 3-5 degrees latitude. However, from 2-3 degrees latitude it looks like O22CLD is higher than FRESCO. So, I wouldn't call this systematic. Perhaps it would be simpler to state 'In general, FRESCO cloud heights are higher than the O22CLD heights'. Or something similar. A correlation plot could also be added to show the bias of FRESCO/ROCINN vs. O22CLD cloud heights.

P6L176: 'up to 15 km altitude' - Please provide a reference for this. Also, how strict is this limit? In Fig. 3, I see cloud heights higher than 15 km. Also, is this above sea level? Please make this clear in the text.

P6L183: Please provide a colour scale/legend with Fig. 2 to show which AAI values correspond to which colour.

P6L183: Interpretation of the CALIOP data. Based on Fig. 2, it looks like the main feature has cloud-top heights of around 16 km (15 km is stated in the manuscript). There is also a clear feature at 18 km (detected by the AAI). This is not mentioned at all and should be addressed in the manuscript.

P6L188-189: There is poor agreement between FRESCO and CALIOP from 3-4 degrees latitude, which should be stated here.

P7L193: CALIOP's feature mask - Please state which version of the feature mask is being interpreted. There were changes made from V3 to V4. I looked at the VFM V4 for this pass and I can see some small parts of the feature classified as dust aerosol but the majority is cloud.

P7L194: 'clearly the attenuation is not complete.' - I'm not sure it is that clear. This interpretation would be more justified if the VFM was plotted on the same scale as Fig. 2 and inserted as a second panel.

P7196: Comparison of FRESCO and ROCINN - is this only for AAI $>$ 0? Please clarify.

P7L200-201: 'and all data products increasing heights in the volcanic cloud going from south to north.' - This is simply not true. The heights increase from 2-4 degrees latitude and then decrease from 4-6 degrees latitude. Please clarify in the text.

P7L209-210: 'The eruption dynamics may thus have additional effects on the ash plume displacement, but this cannot be investigated based on the available satellite data.' - This statement requires further justification and clarification about why the available satellite data cannot be used to study the eruption dynamics. For example, Himawari-8 provides excellent observations (every 10 minutes) of the volcanic cloud's evolution and dynamics (as the authors discuss in Section 3.4).

P7L212: Change 'is' to 'was'.

P7L216: Positive ΔBTs are also indicative of clouds composed of water droplets (not just ice). Please clarify in the text.

P7L218-219: 'one associated also with a high cloud height, and another one further south with much lower cloud heights' - what cloud heights are being referred to here? The VADUGS algorithm in Fig. 4 only appears to show a high altitude cloud.

P7L221: 'dense high ice clouds' - What do the authors mean by 'dense' here? Optically thick ice clouds would show a near zero ΔBT, not a strongly positive ΔBT.

P7L221: 'purple region' - I actually see this as blue. Maybe call it an 'ice-rich cloud'?

P7L222-224:
Figure 5 - I found this figure difficult to interpret. At this line in the manuscript the authors refer to the 'HIMAWARI VADUGS ΔBTs'. How are VADUGS ΔBTs different to a simple 11-12 micron ΔBT? In Fig. 5 they just look like ΔBTs. The authors also state that 'When focusing on AAI and SO2 values, it appears that larger ΔBT values occur for smaller AAI values (< 2) and SO2 (< 20 DU)' - For the lower left plot in Fig. 5, I can see numerous data points that have positive ΔBT (0-10 K) for large (2-6) positive AAI values (contradictory to what the authors claim) and I find it very hard to interpret any relationship whatsoever in this panel. In Fig. 5 lower right panel, again, it's hard to see

any relationship because there are positive and negative ΔBT values that correspond to a whole range of SO2 values (5-100 DU).

There are several ways Fig. 5 could be improved:
First, I would only plot the data that falls within the contours plotted in the upper left panel of Fig. 5 as this clearly contains the volcanic cloud (what are these contours by the way? They are not mentioned in the Fig. 5 caption). This would remove the black dots (I assume?), which at the moment are distracting. Second, some kind of statistical metric could be used to indicate that there is indeed a relationship between AAI, ΔBT and SO2. If the relationship is not linear then maybe some kind of curve fit (exponential for lower right panel?) will help the reader interpret the relationships.

P7L224: 'The larger ΔBT are also associated with optically more dense clouds (see VIIRS imagery in the SI and comparison of TROPOMI with CALIPSO).' - This statement needs to be further clarified. It's not physically possible for an optically thick cloud to have a large ΔBT in the infrared. When clouds become optically thick they behave as grey bodies (little spectral variation across thermal infrared wavelengths) and so a difference in brightness temperature between 11 and 12 micron should be close to zero. However, I think what the authors are observing is a relationship between high reflectance at visible wavelengths (white clouds in VIIRS imagery) and large ΔBTs, but it's not clear in the way that it's stated.

P8L225-227: This could be due to scavenging of SO2 by ice (Rose et al., 2000). It could also be due to ice nucleation of volcanic ash particles (Durant et al., 2008). In terms of the conversion of SO2 to sulphate, is there a reference that could be added here? i.e. how long does it typically take for SO2 to convert to sulphate in the upper troposphere? And does this conversion rate make sense given the time of observation and time since eruption?

P8L245-248: ‘Comparison with geostationary IR volcanic ash height’ - Which retrieval is this statement referring to? Is this the VADUGS volcanic ash cloud height retrieval? It’s the comparison with CALIOP that demonstrates TROPOMI height algorithms may underestimate heights for semi-transparent ash clouds. Please clarify this.

P8L251-252: The ‘shielding’ effect - This is rather speculative and could be due to a number of different reasons (see previous comments on P8L225-227). Also, is this shielding of SO2 or ash or both? I think to substantiate this claim, evidence of SO2/ash existing underneath the cloud-top should be provided.

P9L257-258: ‘the retrieval algorithm’ - which retrieval algorithm is being referred to here? Please clarify.

P9L266-268: ‘TROPOMI cloud heights can be used for determining aerosol heights for AAI values greater than 4’ - How was this conclusion reached? What is the significance of AAI $>$ 4. As stated in the previous sentence, the TROPOMI cloud heights do not perform well for semi-transparent clouds regardless of their AAI value. This statement requires further clarification. Also ’column values $>$ 1 DU’ is TROPOMI’s signal-to-noise really this good? Please provide a reference.

P11L323-325: Please fix reference formatting here. Also link provided to Stein-Zweers (2016) results in a ’Page not found’ error.

P16L411: Check style for figure labels e.g. ‘A+E’ should be ‘(a) and (e)’.

P19L415: VADUGS cloud heights are on the right column of Fig. 4 not left and the ΔBTs are on the left.

P19L427: Change ‘derived’ to ‘shown’.

P19L427: What is the ΔBT bias correction? This needs to be explained and defined in the manuscript.

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