Review of the ACP manuscript acp-2025-924

"Balloon Observations Suggesting Sea Salt Injection into the Stratosphere from Hunga Tonga-Hunga Ha'apai"

By Hazel Vernier, Demilson Quintão, Bruno Biazon, Eduardo Landulfo, Giovanni Souza, V. Amanda Santos, J. S. Fabio Lopes, C.P. Alex Mendes, A.S. José da Matta, K. Pinheiro Damaris, Benoit Grosslin, P.M. P. Maria Jorge, Maria de Fátima Andrade, Neeraj Rastogi, Akhil Raj11, Hongyu Liu, Mahesh Kovilakam, Suvarna Fadnavis, Frank G. Wienhold, Mathieu Colombier, D. Chris Boone, Gwenael Berthet, Nicolas Dumelie, Lilian Joly, and Jean-Paul Vernier, 2025

The above manuscript describes a detailed analysis of measurement data derived for a quite unusual volcanic eruption. The authors use satellite, lidar and in situ balloon-borne measurement data for this analysis with a focus on optical and chemical plume particle properties. Even if the number of balloon flights is relatively small, the lack of in situ data points of this event make every single data point valuable. Consequently, the manuscript should be published, although I believe it needs some polishing and restructuring beforehand. The text seems rashly written (there are many mistakes) and the structure needs some improvement and streamlining. For instance information about the scientific background is given at several places in the manuscript (and not restricted to the introduction) and also instruments characteristics are described in different sections. Section headlines are not consistent and it is hard to follow the line of arguments, because it is not obvious, what will be discussed next. Additionally, the manuscript is quite lengthy and should be shortened to focus more on the main messages.

Even if this eruption would be the only event of its kind in a century, it is an interesting real-world experiment made by nature. Nevertheless, I miss the information about how frequently such events are expected in the introduction. As an atmospheric researcher I do not know this, but a volcanologist might be able to give an estimate or an upper limit.

Specific remarks:

p. 1, l. 32: Even if the measurements have an uncertainty, the SAOD was either lower compared to previous eruptions or it was not, but "appears" seems to me the wrong term.

p. 2., l. 64: "Paradigm" is a big word. This would fit if the occurrence of such eruptions is frequent (s. a.). "View" is probably the better term in this context.

p. 2., l. 77: Analyzing the plume is relevant, but what is to goal in doing this? Providing a better understanding or even a parametrization to the modelers? Understanding satellite data? It is not clear where the manuscript is heading.

p. 5., l. 125/126: What about the Sao Paulo measurements? Do they look similar?

Three kilometers difference in the top of the aerosol layer seems to be much, are there any other measurements available which support the zonal averaging theory?

Please explain more in detail the argument of no CALIPSO measurements over Brazil due to SAA, for a non-specialist this is not automatically clear.

p. 11., l. 249: This paragraph should be part of the introduction, but does not belong here.

p. 12., l. 271: The sentence containing "… both eruptions …" seems to be copied from somewhere as in Fig. 6 more than two eruptions are shown. This does not fit.

p. 15., Discussion on level Pos #2: It is not clear to me, why the discussion on these low level data is needed. This holds as well for Fig. 8. For the main topic of this manuscript, it is not needed. The only reason could be to validate the sample data versus ground station data to show that the sampler and the post-flight analysis works well. Otherwise this discussion should be deleted.

p. 17., Discussion on Fig. 9 and figure caption: The measurements were made 7-8 months after the eruption. Hence one would assume an already well mixed stratospheric aerosol. But the lower row of plots in Fig. 9 shows something different. What is this reason for this, for me unexpected and still large variability? And is this confirmed by, e.g. model results?

p. 18., l. 376: This paragraph is instrument characteristics description and does not belong here but should be moved to an earlier section.

p. 19., last paragraph: The discussion about the extinction profiles and also Fig. 10. As we are talking about a volcanic eruption can you rule out that there has not been any absorbing material in the stratosphere? How would the theoretical response functions look like if you assume e.g. 0.5 or 1% BC? And how would the comparison of the extinction in Fig. 11 be affected due to this?

p. 26., l. 564: "Standard temperature" is 273 K, hence 0°C, 20°C or 25 °C is "normal temperature", right?

p. 25., l. 555, the discussion about the sulfate concentration: The particles are sampled at -60°C and then experience +25°C in the sampler for at least 30 min, according to Fig. 5. How does this influence your sampled sulfate, there must be some evaporation?

p. 28., l 615 Tab. 7.: Please delete most of the digits in the numbers, three leading digits should be enough. Please provide the information on the campaign in a new column, as by the upper measure you saved space. What does "Volkilau" mean?

p. 29., l. 639: What does "sea salt relative to other combustion sources" mean?
Sea salt is not from a combustion source. And combustion sources have not been discussed beforehand.

p. 29., l. 646, The whole paragraph: The difference between the different sulfate measurements is discussed here once again. Please merge all sections with this topic and make it only one section, the reader gets lost otherwise.

p. 31., l. 689: The "presence of biomass burning" is mentioned for the first time in the conclusions. But this should not be the case, either biomass burning plays a role, then it should show-up already in the former sections, or it should be deleted in the conclusions.

p. 31., l. 708: The discussion in this paragraph, I again miss "evaporation" as potential reason.

p. 31., l. 710: You probably mean "sulfate particle mass formation" but not new particle formation.

Technical corrections:

p. 1, l. 46: The sentence reads strange, I assume that a “which” and a comma is missing before “are known …”, please check.

p. 1, l. 46: Referring to table 1 is misleading, as there is only one major (VEI = 6) volcanic eruption listed. I suggest to delete this reference here.

p. 1, l. 48: A paper reference to the statement about the 0.42 Tg SO_2 is missing.

p. 4, l. 102: “Therefore, …” is not correct, the above statement is not the reason, why HTHH did not produce a significantly larger SAOD (this is due to the kind of eruption), but why they look similar in the Fig. 1.

p. 5, l. 114: I did not know the city of Bauru, hence I looked at google maps and for me the city seems to be only 250 km west to Sao Paulo. Are you sure about the 400 km? Or is it 400 km distance pointing northwest?

p. 5, l. 126: Please start a new paragraph after “(SAA).” as the next sentence is about a different topic.

p. 6, l. 138: Please start the headline of section 3.1 with “Balloon-born instrument …”

p. 6, l. 140: A space is missing in “2 kg”

p. 6, l. 145: Any reference paper out on the POPC which could be cited in this paragraph?

p. 8, Fig. 4: The print quality of this figure is poor (pixels visible), please provide a better version.

p. 8, l. 190/193: Please write out the acronyms ECC and CFH when you use them the first time.

p. 9, l. 204: The first paragraph can be deleted as this information was already given on page 7 and is repeated later once again.

p. 10, l. 237: please exchange “, &” with “and”.

p. 10, l. 238: is the given ratio “12.8 ng/6ml” correct or is it “12.8 ng/ml”? in the former case a space would be missing.

p. 10, Fig. 5: As you write about the detection range of the POPC in Sec. 3.1.1 in terms of radii, it would be good to add at the legend here “particle diameter”, at least that is what I assume.

p. 11, l. 242: Please delete one of the two “((“.

p. 11, l. 242: The sentence “The analysis involved …” is a repetition of the short sentence in line 241.

p. 12, Fig. 6: Please add text to the color scale (what is displayed), otherwise the figure is hard to understand.

On the y-axis is SAOD or sSAOD displayed, as stated in the text?

p. 12, l. 272: The “Since” must be deleted, otherwise a half-sentence is missing.

p. 13, l. 282: I´m not a native speaker, but I believe “non-light” is not correct, what should this be, darkness? You mean not light absorbing, please check and rephrase.

p. 14, l. 329: Please delete the comma after “emissions”.

p. 16, l. 346: “AMSL” is not explained.

p. 17., l. 363: The reference to Fig. 10 seems to wrong, should be Fig. 11, as Fig. 10 shows something different (as correctly described on page 18).

p. 18, l. 382: Please exchange “H.(2022)” with “H., 2022)”

p. 18, l. 384: The “size of the POPC” hopefully stays constant, what you mean is the “particle size derived by the POPC”.

p. 19, l. 395: The unit for an hour should be “h” not “hr”, please exchange.

p. 19., l. 396: The name of the instrument “Compact Optical …” should be stated in Sec. 3.1.2, but not anymore in the results section.

p. 19, l. 399: The reference to the “Methods”, what is meant with this, an appendix? Or a former section of the manuscript?

p. 21, l. 448: The format of the Adachi and Buseck reference, there a two opening parentheses but only one closing parenthesis. Some of the references in this section use the “and” others the “&”. Please harmonize in the whole document.

p. 22, l. 458: Space missing after “eruption”.

p. 22, l. 461: The name of the volcano and its abbreviation was already introduced beforehand, hence use only HTHH here. Same line 600. Later in the manuscript many different versions of the name are used “Hunga” samples or plume, “Hunga Tonga” plume, “HungaTHH” etc., please harmonize.

p. 22, Tab. 4: Table caption, should be “Zhang”.
And in the first row should be “Pos# 4” and not “Pos3 4”.

p. 23, l. 493: In the reaction equation the charges on both sides are not equal, but this should be the case, right?

p. 23, l. 509: The “4” in $CaSO_4$ should be subscribed,. Please check the whole manuscript.

p. 23, l. 511: A space is missing in “5 Tg”.

p. 24, l. 520: Table 5 is of poor print quality, partly to small characters and overcrowded with information. How about extracting the essential information from the literature and include it in the newly made lower part of the table?

p. 27, l. 592: The space in “hy drated” must be deleted

p. 29, l. 633: Space missing before the “µm”.