Reply on RC1

Haris Rahadianto et al.

Author comment on "Long-Term Ash Dispersal Dataset of the Sakurajima Taisho Eruption for Ashfall Disaster Countermeasure" by Haris Rahadianto et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-42-AC1, 2022

We are grateful to the referee for the careful review of our paper. Please see each reply below addressing the general comments of the referee.

Comment #1: Since limiting the meteorological and atmospheric conditions of a specific day (in which the Taisho eruption occurred) to define the worst-case scenario, does not seem to have been given very reliable results. Nonetheless, the input parameters used for the modelling of the worst-case scenario (analysed within this paper), are too much specific, considering atmospheric conditions that are not common in Japan. Therefore, the scenario modelled, could happen, but with a very low certainty.

Response: We found that our description of the dataset is not good enough, and we will explain it more clearly in the paper. Actually, we did not limit the meteorological conditions in contrast to what the reviewer pointed out. Instead, we simulate ash dispersal of the Taisho eruption with diverse weather conditions for 64 years (1958-2021, 23,736 simulations) to obtain meteorologically-diverse ash deposits in the ground and airborne ash concentration. Given the size of the dataset, for example, it can be used to evaluate conditional ashfall risk in Japan when the Taisho eruption occurs in contemporary settings; estimating the volcanic ashfall risk by analysing both the dispersal and deposition pattern volcanic ashfall over a long period; developing hazard maps for the disaster risk management process; and seeking a better comprehension of the tendency from ashfall distribution from a large eruption over a large region. Following this, we would like to improve the dataset descriptions in the paper, specifically in the introduction section and Sect. 4.

Comment #2: In fact, the validation presented was not satisfactory, and the authors knows it...which was probably due to the fact that the authors pushed the model to a very narrow scenario, forcing the inputs, trying to recreate an historic eruption with sparse data.

Response: We found that we did not explain the validation mechanism in a well-structured manner and we plan to explain it better. For smaller-scale eruption cases, the validation of ash dispersal simulation using the PUFF model for Sakurajima eruptions has been done successfully before (e.g. Tanaka and Iguchi, 2019; Tanaka et al., 2020). However, the validation for the ash dispersal simulation from a large-scale eruption in Sakurajima is yet
to be done, and we assume a similar process should work for such a case. For our validation structure, we assume that if we could simulate the Taisho eruption using complete wind data at the time the eruption took place, we could produce a similar ash distribution for entire Japan. However, it is noted, in the paper, that the Taisho eruption occurred more than a century ago under extraordinary weather conditions. Moreover, we found that complete wind data at the eruption time are not entirely available for us to replicate the eruption. Therefore, to alleviate the issue, we tried to find the dates with similar weather to the time of the eruption, expecting it to have a similar wind pattern. Finally, we checked the simulation result of the selected dates with the available ground reports. Therefore, it is critical for us to highlight the extraordinary weather conditions when the Taisho eruption took place to find dates with similar wind patterns due to the same weather characteristics. In the paper, as we described in the Sect. 5.2, we use weather chart similarity to find such dates. However, we acknowledge that there may be other methods to do this. For example, global climate models may obtain more precise wind conditions during the eruption. Currently, we do not have access to such models, but we will try to incorporate them in the future.

As pointed out in the paper, using the method described there, we can only find a small number of dates that correspond with the weather of the eruption day. Therefore, we only compare the simulation results of the selected dates as a validation mechanism for our dataset. Nonetheless, the validation results of the simulation on selected dates show sufficient accuracy. Therefore, it is estimated that the dataset would also have reasonable accuracy with the ground truth, if available, for different weather conditions.

The validation mechanism we did above does not mean limiting the dataset to only contain specific condition. Following this, we would like to revise the validation structure of the dataset in the paper, specifically in Sect. 5.

Comment #3: The paper is well written, however, the objective of the paper should be reconsidered, along with the scope of the modelling.

Response: We acknowledge the lack of clarity on the objective of this paper that should cover a better dataset description as well as the significance of the dataset. If allowed by the editor, we would like to add a new subsection for illustrating the usage example of the dataset and the suggestion for the prospective users, specifically in subsection 4.2 within Sect. 4.

References:


We put our responses in the original document for the supplementary comments, directly replying to the comment in the pdf file. If it is difficult to follow, we also provide an alternative response document addressing each comment based on the order of the comments. Please see attached the pdf response files (zip).
Please also note the supplement to this comment: