nature portfolio

Peer Review File

Reconstructing the dispersal and magnitude of the Millennium Eruption of Paektu (Changbaishan) volcano

Web links to the author’s journal account have been redacted from the decision letters as indicated to maintain confidentiality

28th Jun 23

Dear Dr Costa,

Your manuscript titled "Reconstructing the dispersal and magnitude of the Millennium Eruption of Baekdu volcano (Changbaishan)" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In particular, please ensure that your revised manuscript meets the following editorial thresholds:

* Present a robust approach to estimate the magnitude of the Millennium Eruption which is validated by comparison with the caldera volume.

* Provide a thorough consideration of the uncertainties associated with using tephra deposits in lake and marine settings and incorporate these uncertainties into your conclsuions.

* Reconsider and justify the studies used to inform on the thickness and compositional and spatial variation in the tephra deposits, in response to the issues raised by Reviewer #2

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Joe Aslin

Senior Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to disciplinespecific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Reviewer comments

Manuscript: Reconstructing the dispersal and magnitude of the Millennium Eruption of Baekdu volcano (Changbaishan) by Costa et al.

Recommendation: Accept with minor revisions.

General comments:

The manuscript introduces a novel inversion approach used to determine the eruption source parameters (ESPs) (i.e., erupted mass, plume height and eruption duration) of the two eruptive phases of the Millennium Eruption of Mt. Baedku, and to revisit the evaluation of the magnitude of this eruption. The motivation for the study stems from the fact that the deposits of this eruption (found as far as Greenland, more than 7000 km away from the vent) suggest a large magnitude event with potential climatic impacts; however, previous studies are at odds reconciling the explosivity of this event and its effects on the climate. The authors propose here a novel inversion-based approach to provide new estimates for the eruption source parameters of this eruption with uncertainty quantification. This is an important and timely piece of work on an important challenge facing modern volcanology. That is refining our understanding of past explosive eruptions, their magnitudes, intensities, and impacts with the goal of improving future hazard assessment.

I would like to conclude that this manuscript is suitable for publication in Communications Earth and Environment, based on the relevance of the study (i.e., introducing a novel methodology to estimate the magnitudes of explosive eruptions and apply it to one high-profile eruption) and the broad appeal of this work to a larger audience. The manuscript is well organized and well written, and the results and model outputs are well presented. The methods section is described eloquently, and the authors made use of the relevant literature in the field, which is properly cited. Overall, I want to congratulate the authors for this great work, and I am looking forward to seeing the manuscript published in a timely manner.

I would like to address some minor issues to the authors, with the hope to improve and clarify some aspects of the study and the paper overall.

1. From the introduction the authors highlight a degree of uncertainty about this eruption and its impacts. The deposits indicate a rather large event, while the evidence for its climatic impacts is limited. The authors suggest that this may be resolved with a more accurate estimation of the eruption source parameters, with which I agree. The method proposed by the authors to estimate these ESPs relies on thickness measurements of the tephra deposit. However, I believe a discussion about the uncertainty in these measurements is mandatory, particularly given the fact that some of this thickness measurements are from sediment cores, both from lakes and from the sea. Previous studies have shown that for on-land deposits, post-depositional processes may affect the tephra deposits immediately after the eruption, and given the uncertainty stemming from this deposit's variability, erupted mass estimates may be highly corrupted. Moreover, how reliable are the thickness measurements of tephra from lake/marine core deposits? What is the uncertainty in these measurements? I am not familiar with how sedimentation occurs in water, so for readers like me, perhaps it's worth mentioning how reliable can tephra measurements in core deposit can be. Considering the 'standard' deposit thinning with distance from the vent and potential 'secondary maximum', do the thickness measurements from the sea reflect this trend? I imagine that sedimentation in small lakes may not influence too much the sinking tephra particles due to limited movements of water columns a different depth. But at sea, I'd imagine that tephra particles are sedimenting through a turbulent water column, so the thickness measurements in one core may not be representative and considered as a primary deposit. Even lapilli size particles can be carried away horizontally by water currents at considerable distances from where they landed.
Now I understand that the full deposit with 83 measurements is used in one inversion only, however, the inversion of the two phases also contains a small number of thickness measurements taken at sea. Did the authors attempt to conduct the inversion only on the on-land samples? How the results compare with the inversion conducted on all 23 samples?
Overall, I believe it is worth mentioning, in Discussion perhaps, how the uncertainty in both on-land and off-shore samples may affect the erupted mass estimates. And hence, how eruption magnitude classification may be affected by these uncertainties. Is it possible to take the 'best-fit' set of ESPs from the both eruptive phases and conduct the inversion only on the on-land samples? Next see if the uncertainty ranges overlap for both inversions, with and without the offshore samples. Considering these uncertainties, how the eruption magnitude can be affected, considering that what is given it is a rather precise M=6.7? Could this be a larger or smaller event?
This is just a suggestion, largely stemming from my lack of understanding of how tephra sedimentation occurs in a large, turbulent body of water. Overall, a small discussion of uncertainty related to tephra thickness measurements is required.

2. A second point I want to raise is related to the plume height and probably the dynamics of such large eruptions. The authors estimate a rather large range of plume heights. Are these heights given as the top of the plumes? What is the best-fit plume height for each of the two phases? Perhaps Table 2 can be updated to include the uncertainty ranges for all ESPs along with the best-fit set which was used to reconstruct the isopach maps in Figure 2. Currently, only the erupted mass has

the best-fit value. Also, how is the uncertainty range reported?

3. Following on the plume height. Recent studies using numerical models for tephra sedimentation and inversion algorithms showed that the plume height may be overestimated with inversion models showing wide ranges of plume heights giving more or less similar results for deposit fitting. These studies suggest that in fact, during such large-magnitude eruptions, tephra is transported by a laterally spreading umbrella cloud, formed at lower altitudes, rather than by a high-reaching vertical plume combined with strong winds. Such umbrella clouds have been seen forming at considerably lower altitudes than the top of the plume (see the recent Hunga Tonga eruption, or the Pinatubo event). Given the magnitude of this eruption, is it possible it also formed an umbrella cloud? Can the authors add a discussion about the role of the umbrella clouds during such events and how the estimated ESPs for this event may change if an umbrella cloud is considered? Could an umbrella cloud affect the classification of this event?

Overall, I believe the manuscript will benefit of small discussions regarding the major sources of uncertainty that may affect the estimated ESPs and eruption classification. Also, a bit more clarification about the reliability of marine core sediment measurements, the uncertainty associated with these measurements and their effect of erupted mass estimates.

Minor comments

Can the authors describe succinctly what is the ‘perturbation range’ in the inversion analysis? This may not be too straight-forward for people unfamiliar with this terminology.

Table 5 seems incomplete. Where are the rest of the simulated parameters? I apologize if I missed this information.

I see in Table 3 the ESPs ranges, but what are the ‘best-fit’ set of parameters for each inversion (phase 1, phase 2, total deposit) with their uncertainty range? Perhaps Table 2 can be updated? And if this set of ESPs is used to create the isopach maps in Figure 2, is this information the same with Table 5? I am a bit confused about Table 5 I guess.

If previous studies reached the same M=6.7 for this eruption with easier-to-use methods, what is the merit of this approach? Perhaps the authors may highlight why this approach is more suitable for ESP estimation.

I apologize to the authors if some of my queries here are found in the manuscript and I missed them. It is up to the authors to consider accepting or rejecting my comments and suggestions, but I believe the manuscript could benefit of some of the suggested changes.

Thank you for the great study and I look forward to seeing the paper published soon.

Reviewer #2 (Remarks to the Author):

Review of MS COMMSENV-23-0812-T entitled “Reconstructing the dispersal and magnitude of the

Millennium Eruption of Baekdu volcano (Changbaishan)" by Antonio Costa and coauthors.

General comments

In the present manuscript, Costa et al., modelled the ash dispersals from the Millennium eruption of Changbaishan volcano which is one of the largest eruptions over last 2000 years. They used the stratigraphic records from proximal to distal regions to model the volume of the two phases (comenditic and trachytic) from the Millennium eruption, which will be useful for future calculations on the volatile release and associated environmental impacts. Although I recommend this study, there some problems must be resolved before publication. Two major concerns on the manuscript (see below detailed information) should be considered, that is the output modelled figures (figure 2) and tephra data used to modelling (supplementary table 1).

Major concern 1: Figures 2

The reconstructed tephra dispersal and thickness for these three figures should be problematic based on your data, and thus there will be problems in the mass flow rates in the figure 4. Figure 2A is the comendite phase, but its distribution is more restricted than the Figure 2B trachyte phase (south direction) which is more widely distributed and its southwest limit can reach to east Korea. However, from stratgraphic and geochemical evidence, Chen et al., (2022) shows that the ME tephra recorded in the marine core from Ulleung basin exclusively is comenditic in composition while no trachytic glass shards in this tephra layer. In the Supplementary Table 1 which used to reconstruct the tephra dispersals, you cited the core of KH82-4-25 from Furuta et al. (1986) which may be used to reveal the comenditic phase to the southwest limit in the figure, however, Furuta did not analyzed the ME tephra from this core, and they only analyzed the tephras from the cores of Stn.6913, KH79-3-C2, P.130-2, KH69-2-23, Stn.6920 (See the figure 4a and Table legend). As for the ME tephra, there are another five cores to be used to check this tephra without geochemical determinations, but these cores also located in the middle to north Japan Sea. So how can you determine the southwest limit of trachytic phase by using Furuta's results or did you analyze the ME tephra from this core? If you used the Chen's result, why the current figure shows that the trachytic phase can reach to the eastern Korea, while the comenditic phase only cover the middle of Japan Sea. Even if you have analyzed the trachytic phase from Furuta's core, you should also clarify the difference between it and Chen's 2022 results. Additionally, you should explain why the modelled thickness and dispersal are not consistent with the tephra record by Chen et al., 2022. Therefore, the modelling is not consistent with stratigraphic records which must be resolved before publication.

Major concern 2: Supplementary Table 1

There are many mistakes on the matches between the references and locations, and please check them, such as P35, P36, P37. The tephra data in this table is very important for the tephra modelling of this manuscript, but five major problems should be considered carefully. Firtly, please careful to compare the thickness of ME tephra on the Tianwenfeng peak, such as there are two layers named as black GEMtr and light color GEMcom in the current reference of Pan et al., (2020), while the lower part of this site can be divided into two layers (upper is trachytic in composition and lower comenditic in composition) when considered the previous studies by Chen et al., (2016) and Sun et al., (2017) from the same site because the lower light color layer is not wholly comendite as stated by Pan et al., (2020). This geochemical composition difference is very important for the current manuscript because you would like to reveal the amount of tephra release from these two phases, however, if you use the current reference, the thickness of these two phases from this site will be not right. Secondly, for the thickness of ME tephra from Yuanchi site, I would like to recommend using the thickness of ME tephra recorded in the lake sediments of Lake Yuanchi where the sedimentary environment is more steady and less eroded by secondary processes. Thirdly, the proximal thickness of ME tephra, there are many such information as published by Liu et al., (1998), which may be more useful at here. Fourth, how can you determine the amount of the proximal PDCs in the total volume of the magma release? Finally, the thickness data in the table should be double checked and how can you determine the two phases thickness of such as P06 by Machida and Arai's (1983) result, additionally, for examples, the St.6920 recorded about 3 cm ME tephra and KH69-2-23 recorded about 25 cm ME tephra based on the Furuta's (1986) numerical result which is different from the current table numbers.

Specific points
Line 2: Changbaishan volcano is more common to be used in these days' literatures, the Baekdu/ Paekdu/Baitoushan/ Baekdusan are rare to be used, so, the Baekdu volcano should be revised to Changbaishan volcano and those through the text. I recommend that the Changbaishan volcano (also referred to as Baektu, Paektu and Baitoushan) in the main text of Line 44 or Line 62-63.
Line 63: DPRK to Korean?
Line 81-85: the eruptive history of Changbaishan volcano cited here is not ideal, and the eruptive stages divided by Zhang et al., 2018 is more proper to be used at here when considering the dating results and readable to readers.
Line 95: the number of reference 28 did not do analysis or compile the dating results on the Changbaishan Millennium eruption. Most of radiocarbon dates on the Changbaishan Millennium eruption have been sorted in the Sun et al., 2014.
Line 96: there are also many other well dated precise radio-carbon dates on the ME of Changbaishan volcano, like Yatsuzuka et al., 2010, Yin et al., 2012, Hakozaki et al., 2017, and Nakamura et al., 2007 also dated this eruption indirectly by dendrochronology.
Line 101: please check the age of 939-940 CE.
Line 130, 141, 160, 185, 204, 291 and following parts: the GME is highly confused and the ME which is easier to be accepted by readers, however, the GME is easier to make confusion. So, please revised all the GME to ME throughout the text to avoid using different names for the same event.
Line 135-136: such trachytic PDCs mainly founded in the valleys around the volcanic vent while not widely distributed like the comenditic PDCs, if so, why the early comenditic PDCs were not eroded?
Line 148: add reference.
Line 168: ……do not show noticeable changes following the Millennium eruption9, 10. The charcoal wood used in these two references are killed by the Millennium eruption, and the last ring represents the year of the ME. However, how can you determine the changes following the years after the ME because these woods did not record these rings after the ME?
Line 189: add reference to the ……both phasesreferecens, ….
Line 201: ……Sihailongwa, China, and Lake Kushu, Japan. Add references.
Line 204: 17, 21, 40 to references?

Reviewer #3 (Remarks to the Author):

Esteemed colleagues,

This manuscript proposes a reconstruction of the eruption dynamics of two phases of the millennium eruption of Peaktu in 946 CE. This eruption is a unique case study whereby despite its magnitude and gas yields, it is not associated with volcanic forcing (cooling) of climate. The

manuscript includes an extensive review of the existing literature on the physical volcanology and degassing budget estimates of the eruption. The paper presents a new strategy for the reconstruction of key eruptive parameters using dispersion models. It includes a large dataset over a wide distribution grid that is rare for past eruptions of this magnitude.
My expertise is limited to the reviewing of eruption dynamics and degassing budget estimates, which I am very familiar with. I am not qualified to assess the modelling and statistical approach used here, which are the core novelty of the manuscript. I enjoyed reading the manuscript, which has a clear structure and is well-written. As a volcanologist working with climate modellers to reconstruct the impact of past explosive eruptions on climate, I can attest that eruptive parameters key in the precise reconstruction of the atmospheric response to volcanic sulphur emissions. This work provides essential conclusions for physical volcanology and highlights the importance of tephrochronology, but also provides key information relevant to climate modellers and magma geochemists. In this view, this manuscript meets the criteria to be published in Communications Earth & Environment.
My overall assessment is that the paper contains robust publishable data and has some areas for improvement. Critical aspects to address are i) include a reconstruction of the caldera size and a comparison with erupted volumes estimates calculated here; ii) include a more thorough discussion about sulphur release within the process of the eruption associated with magma mixing and how the findings reflect ice core data and Toohey et al 2019 modelling that shows the stronger cooling followings winter NH eruptions; and iii) incorporate in the conclusion a comment on the wider significance of quantifying uncertainties in eruptive parameter reconstruction, which is the core of the paper.

A main concern I have which is trivial but very important is the spelling/naming of the volcano. My understanding is that Baektu is the South Korean name. Since the volcano is located at the Border between PRC and DPRK, in order to respect regional designations and honour diplomacy, I would suggest using the North Korean (Peaktu) and Chinese (Changbaishan) names and quoting the South Korean spelling in brackets right at the start of the manuscript. I also would suggest mentioning briefly at the start what is the significance of this volcano for local people. In DPRK at least it is considered as a sacred mountain in the regional folklore. More details on this are available in recent literature.

I would therefore encourage moderate revisions and would like to review a new version in the future.

Below I outline specific comments, where I have passionately kept my tone as encouraging as possible. I hope the suggestions below will help improve the manuscript.

Yours,

Céline Vidal

Specific comments:

Line 32. And large uncertainties

Line 35. As well as testing the accuracy of dispersal models

Line 43. Impact on climate.

Lines 43. Please consider replacing ‘we know’ with ‘we have good constraints on’ and ‘impact’ with ‘climate response’

Line 49. The 1815 eruption of Tambora, Indonesia

Line 55. What is the evidence for a hiatus of 3 months? I suggest providing more details here or omitting this in the teaser, and adding a reference.

Lines 81-82. ‘28 My ago’

Line 88. What is the published VEI of the millennium eruption?

Line 115. It would be great to have a figure with location of occurrence of the different types of deposits in order to follow the description. In the text, I also suggest adding directions (N,S,E,W) from the caldera to locations mentioned with a distance from the vent in order to help picture the geographical distribution of the deposits (it is important to understand plume dynamics).

Line 161. How does this volume compare with the size of the caldera? How deep is it? Has anyone reconstructed the caldera volume? I mention this because Peaktu caldera is slightly smaller than Tambora’s and Samalas’ and the volume for these two is ca.40-45 km3. So the upper DRE volume estimate for the millennium eruption seems high… see further comments on this point below.

Line 166-168. Records from where? Is that all NH JJA temperature reconstructions? We know that there can be great regional variability in tree ring reconstructions so it is important to precise.

Line 171-172. Please replace ‘considerably’ with an approximate factor. I would suggest using a NH eruption to compare rather than 1815 Tambora (e.g. at Katmai, in Kamchatka, Japan or Aleutian) because sulphur transport and deposition are not comparable with those of a tropical eruption.

Line 173. Tropospheric plume heights

Line 176. Was the thickness and composition data newly acquired for this study? I understood later that it is the case but this sentence can be confusing since there is already a lot of data in the literature.

Line 186. Please indicate a radius if possible, this would be more informative than ‘close’. Please also indicate how close to the caldera the sampling took place. With the very large scale of Fig 1, it is difficult to assess how proximal the deposits you sampled were.

Line 185-191. I don’t follow here. You mention in the context that the deposits are mingled. In this section, you suggest they are not visually distinct. Are they layered? Have you found any mingled deposits?
This is really important to clarify to constrain magma ascent/recharge and eruption trigger and directly affects degassing dynamics and at which point in the eruption it took place.

Line 196. How far is Lake Suigetsu from Peaktu?

Line 203. Please indicate how you 'know' the thickness if deposits are mixed or refer to the methods section.

Line 210. Figure 1. Please add country and ocean names, and the North arrow. Also report all location names mentioned in the text on Figure 1 to help the reader follow. Please also add an inset with the caldera and proximal deposit distribution, or this could be in a separate figure.

Lines 218-221. How does that time period reflect meteorological conditions for the year 946 - what is the argument for using this time frame? Are the atmospheric temperatures similar? Please add some details to justify the choice of meteorological conditions (apart from the winter timing)

Line 236-245. When you say ' all measurement points available' do you refer to the two phases cumulated? If that is the case, knowing that there was a short hiatus between them, what is the meaning of these parameters?

Line 327. I would like to see at least a qualitative comparison of these volumes with caldera volume to see how your volume ranges fit. I would be a bit cautious with the upper limit of 37.4 km3, knowing that this excludes PDC volume. Is there bathymetric data in the lake to assess the depth? With the shape of the caldera, it should be feasible to estimate qualitatively the missing volume using DEM, e.g. following Hutchison et al. 2016's approach (https://doi.org/10.1038/ncomms13192)

Lines 331-332. Please include some other examples of similar VEI because Pinatubo is smaller than the Millenium eruption according to your eruptive parameters.

Line 341. Where is the co-PDC fallout if any? It is not discussed in the manuscript but would be important to mention it somewhere, including if it wasn't found, because this would increase the uncertainty on the volume estimates.

Lines 343-365. In this section, the yields are confusing. Are you referring to previous yields based on previous volumes? Or have you rescaled the yields based on the new volume estimates? I would suggest not mentioning yields but rather concentrations (ppm) to avoid confusion.

Line 347. Please add in brackets the SO2 equivalent (since SO2 yields rather than S yields are mostly compared to other eruptions)

Lines 368-371. Rather than referring to Pinatubo here, it would be more important to briefly discuss the potential contribution of the trachytic magma to the comenditic volatile phase discussed extensively in the literature by M. Edmonds, P. Wallace and others, especially in regards to the apparently different magma mixing processes at stake revealed by deposit characteristics (mingled, layered?). This would influence when most of the sulphur was released, more likely by the volatile phase during the first phase (comendite) of the eruption and less through the syn-eruptive degassing of the trachytic magma in the second phase of the eruption. There are not enough details in the text to understand degassing processes. I know this has been covered partly by Iacovino et al., but it should be discussed here before scaling S emission to the new volume estimates.
The indicator that S made it into the stratosphere is the S in ice. Please also compare your new yields with estimates based on ice core records (Sigl et al. 2015, Sigl and Toohey 2022), for example after

line 380. What is found in the ice validates or not the results of this work.

Line 375. Released

Line 376. Add SO2 equivalent in brackets.

Line 383-384. Most importantly, there is a very poor understanding of halogen behaviour in plumes and their ability to reach the stratosphere because of rapid leaching in the plume (see e.g. Wade et al 2020 (no need to cite it but we discuss the limitations of our understanding there), Mather, 2015 etc).

Line 382. Toohey et al. 2019 (https://doi.org/10.1038/s41561-018-0286-2) discuss the stronger impact of high latitude NH eruptions occurring in the winter (rather than in summer) based on modelling. I would suggest discussing this in the light of your findings.

Line 393. Which fluids?

Line 397. You could also mention briefly local water poisoning by F and livestock deaths (e.g. 1783 Laki)

Line 414. Given the large uncertainties on the volume estimates, I would suggest referring to volume ranges rather than approximate values which are likely to be quoted in future work without an understanding of the uncertainties.

Line 423. It would be great to see a comment on the critical necessity of eruptive dynamics reconstructions and the assessment of uncertainties because these ripple on atmospheric circulation models that aim to reconstruct the impact of eruptions (see Marshall et al., 2022).

Methods

The Methods section does not explain how the tephrochronological and geochemical work was carried out. I understand now that this was published previously, is that right? If that is the case, further details on how phases were differentiated should be provided in the main text.

Dear Editor,

First, we apologize for the heavy delay to submit the revised version of the manuscript, mainly because during last August the supercomputer Mare Nostrum at Barcelona Supercomputing Center was under maintenance and that generated a very long queue of jobs that lasted up to a few days ago. We had to re-run all the simulations because reviewer #2 point out a few inconsistencies in the dataset reported in the Supplementary Table 1. After we corrected those errors, the results of the simulation remain quite stable and confirmed our previous main conclusions.

That said, we would like to warmly thank the reviewers for their criticism and comments that helped us to improve the manuscript. We are going to answer below the main points of each reviewer.

Best regards,

Antonio Costa on behalf of all authors

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Reviewer comments

Manuscript: Reconstructing the dispersal and magnitude of the Millennium Eruption of Baekdu volcano (Changbaishan) by Costa et al.

Recommendation: Accept with minor revisions.

General comments:

The manuscript introduces a novel inversion approach used to determine the eruption source parameters (ESPs) (i.e., erupted mass, plume height and eruption duration) of the two eruptive phases of the Millennium Eruption of Mt. Baedku, and to revisit the evaluation of the magnitude of this eruption. The motivation for the study stems from the fact that the deposits of this eruption (found as far as Greenland, more than 7000 km away from the vent) suggest a large magnitude event with potential climatic impacts; however, previous studies are at odds reconciling the explosivity of this event and its effects on the climate. The authors propose here a novel inversion-based approach to provide new estimates for the eruption source parameters of this eruption with uncertainty quantification. This is an important and timely piece of work on an important challenge facing modern volcanology. That is refining our understanding of past explosive eruptions, their magnitudes, intensities, and impacts with the goal of improving future hazard assessment.

I would like to conclude that this manuscript is suitable for publication in Communications Earth and Environment, based on the relevance of the study (i.e., introducing a novel methodology to estimate the magnitudes of explosive eruptions and apply it to one high-profile eruption) and the broad appeal of this work to a larger audience. The manuscript is well organized and well written, and the results and model outputs are well presented. The methods section is described eloquently, and the authors made use of the relevant literature in the field, which is properly cited. Overall, I want to congratulate the authors for this great work, and I am looking forward to seeing the manuscript published in a timely manner.

We greatly appreciate the reviewer's comments.

I would like to address some minor issues to the authors, with the hope to improve and clarify some aspects of the study and the paper overall.

1. From the introduction the authors highlight a degree of uncertainty about this eruption and its impacts. The deposits indicate a rather large event, while the evidence for its climatic impacts is limited. The authors suggest that this may be resolved with a more accurate estimation of the eruption source parameters, with which I agree. The method proposed by the authors to estimate these ESPs relies on thickness measurements of the tephra deposit. However, I believe a discussion about the uncertainty in these measurements is mandatory, particularly given the fact that some of this thickness measurements are from sediment cores, both from lakes and from the sea. Previous studies have shown that for on-land deposits, post-depositional processes may affect the tephra deposits immediately after the eruption, and given the uncertainty stemming from this deposit's variability, erupted mass estimates may be highly corrupted. Moreover, how reliable are the thickness measurements of tephra from lake/marine core deposits? What is the uncertainty in these measurements? I am not familiar with how sedimentation occurs in water, so for readers like me, perhaps it's worth mentioning how reliable can tephra measurements in core deposit can be. Considering the 'standard' deposit thinning with distance from the vent and potential 'secondary maximum', do the thickness measurements from the sea reflect this trend? I imagine that sedimentation in small lakes may not influence too much the sinking tephra particles due to limited movements of water columns a different depth. But at sea, I'd imagine that tephra particles are sedimenting through a turbulent water column, so the thickness measurements in one core may not be representative and considered as a primary deposit. Even lapilli size particles can be carried away horizontally by water currents at considerable distances from where they landed.
Now I understand that the full deposit with 83 measurements is used in one inversion only, however, the inversion of the two phases also contains a small number of thickness measurements taken at sea. Did the authors attempt to conduct the inversion only on the on-land samples? How the results compare with the inversion conducted on all 23 samples?
Overall, I believe it is worth mentioning, in Discussion perhaps, how the uncertainty in both on-land and off-shore samples may affect the erupted mass estimates. And hence, how eruption magnitude classification may be affected by these uncertainties. Is it possible to take the 'best-fit' set of ESPs from the both eruptive phases and conduct the inversion only on the on-land samples? Next see if the uncertainty ranges overlap for both inversions, with and without the offshore samples. Considering these uncertainties, how the eruption magnitude can be affected, considering that what is given it is a rather precise M=6.7? Could this be a larger or smaller event?
This is just a suggestion, largely stemming from my lack of understanding of how tephra sedimentation occurs in a large, turbulent body of water. Overall, a small discussion of uncertainty related to tephra thickness measurements is required.

We thank the reviewer for such comments on these valuable points.

Concerning the problem of sedimentation in lake/sea and use of core deposits, the reliability of data depends on the specific setting but in general core data are quite robust in terms of thickness and bulk granulometry and, on this topic, there is an extensive literature. For example, Engwell et al. (2014) analysed the problem for the Campanian Ignimbrite deposits and more recently Freundt et al. (2021) did an extensive review on this specific problem (see "Tephra layers in the marine

environment: a review of properties and emplacement processes by Freundt et al., in Di Capua et al., 2021). These studies are now cited in the Supplementary Material.

Although we did the inversion using a small subset of points where we know the partition between the two phases, as pointed out by the reviewer we have also validated the simulations considering 83 points where we know the total thickness. Carrying out the modelling using only the on-land data would not be reliable as there are too few points and most of the points would be very close to the source so do accurately reflect the dispersal. This problem has been described for example by Primerano et al. (2021) and more recently by Constantinescu et al. (2022).

Concerning the determination of the Eruption Magnitude, we expressed the uncertainty in terms of range of estimated volumes expressed as DRE from where one can calculate magnitude. M6.7 refers simply to the best estimate but since the Magnitude is expressed in logarithmic scale it does not change drastically. Moreover, the method is actually probabilistic and the output can be associated to different percentiles.

2. A second point I want to raise is related to the plume height and probably the dynamics of such large eruptions. The authors estimate a rather large range of plume heights. Are these heights given as the top of the plumes? What is the best-fit plume height for each of the two phases? Perhaps Table 2 can be updated to include the uncertainty ranges for all ESPs along with the best-fit set which was used to reconstruct the isopach maps in Figure 2. Currently, only the erupted mass has the best-fit value. Also, how is the uncertainty range reported?

3. Following on the plume height. Recent studies using numerical models for tephra sedimentation and inversion algorithms showed that the plume height may be overestimated with inversion models showing wide ranges of plume heights giving more or less similar results for deposit fitting. These studies suggest that in fact, during such large-magnitude eruptions, tephra is transported by a laterally spreading umbrella cloud, formed at lower altitudes, rather than by a high-reaching vertical plume combined with strong winds. Such umbrella clouds have been seen forming at considerably lower altitudes than the top of the plume (see the recent Hunga Tonga eruption, or the Pinatubo event). Given the magnitude of this eruption, is it possible it also formed an umbrella cloud? Can the authors add a discussion about the role of the umbrella clouds during such events and how the estimated ESPs for this event may change if an umbrella cloud is considered? Could an umbrella cloud affect the classification of this event?

Overall, I believe the manuscript will benefit of small discussions regarding the major sources of uncertainty that may affect the estimated ESPs and eruption classification. Also, a bit more clarification about the reliability of marine core sediment measurements, the uncertainty associated with these measurements and their effect of erupted mass estimates.

The heights inferred by the inversion procedure refer to the top height above the vent level and are consistent with the estimated MFR. Besides that, the method allows us to estimate the time evolution, so the entire variability of this quantity during the eruption phases. The obtained values are very feasible for Plinian eruptions and, as for the magnitude, we reported the best estimates. Since the method is probabilistic, we can provide any percentile related to their distribution. Concerning the umbrella cloud, as for most of Plinian columns, its formation has been considered in the simulations, following the method of Costa et al., 2013. We have now explicitly stated this point in the revised text.

Minor comments

Can the authors describe succinctly what is the 'perturbation range' in the inversion analysis? This may not be too straight-forward for people unfamiliar with this terminology.

It refers to the ranges we consider to create the ensemble, and this is now explicitly stated in the text.

Table 5 seems incomplete. Where are the rest of the simulated parameters? I apologize if I missed this information.

I see in Table 3 the ESPs ranges, but what are the 'best-fit' set of parameters for each inversion (phase 1, phase 2, total deposit) with their uncertainty range? Perhaps Table 2 can be updated? And if this set of ESPs is used to create the isopach maps in Figure 2, is this information the same with Table 5? I am a bit confused about Table 5 I guess.

Table 3 and Table 5 refer to the input parameters used in the two different steps. The first one for the classical inversion made with Parfit (that is an approach like that used with Hazmap or Tephra2) and Table 5 for the second step inversion made with Fall3D using the probabilistic ensemble method. This has been clarified in the headings and the text.

If previous studies reached the same M=6.7 for this eruption with easier-to-use methods, what is the merit of this approach? Perhaps the authors may highlight why this approach is more suitable for ESP estimation.

I apologize to the authors if some of my queries here are found in the manuscript and I missed them. It is up to the authors to consider accepting or rejecting my comments and suggestions, but I believe the manuscript could benefit of some of the suggested changes.

Thank you for the great study and I look forward to seeing the paper published soon.

Thank you again to the reviewer for the useful comments. We hope the revised version is clearer, and the advantages of the probabilistic method over the classical approach are evident.

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Engwell, S., Sparks, R.S.J. and Carey, S. 2014. Physical characteristics of tephra layers in the deep sea realm: the Campanian ignimbrite eruption. Geological Society, London, Special Publications, 398, 2–19, https://doi. org/10.1144/SP398.7

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Reviewer #2 (Remarks to the Author):

Review of MS COMMSENV-23-0812-T entitled "Reconstructing the dispersal and magnitude of the Millennium Eruption of Baekdu volcano (Changbaishan)" by Antonio Costa and coauthors.

General comments
In the present manuscript, Costa et al., modelled the ash dispersals from the Millennium eruption of Changbaishan volcano which is one of the largest eruptions over last 2000 years. They used the stratigraphic records from proximal to distal regions to model the volume of the two phases (comenditic and trachytic) from the Millennium eruption, which will be useful for future calculations on the volatile release and associated environmental impacts. Although I recommend this study, there some problems must be resolved before publication. Two major concerns on the manuscript (see below detailed information) should be considered, that is the output modelled figures (figure 2) and tephra data used to modelling (supplementary table 1).

We appreciate this thorough review. The reviewer made us aware of a few key things that we had missed, such as the discrepancy between Chen et al. (2016) and Sun et al. (2017), and Pan et al. (2020). We have updated these thicknesses in the dataset, and now also include the cryptotephra point in the Ulleung basin (from Chen et al., 2022). Given these changes to the dataset we reran the simulations and have updated the results accordingly. Although the new dispersal patterns are slightly different, these results are incredibly similar to those previously reported and do not affect the interpretation. The similar results also show that the modelled results are robust and not affected by small changes to individual points.

We have responded to the specific comments below.

The review did not provide full references for the articles mentioned in the review, and I wanted to highlight this as an issue. It was difficult to track down the papers that were being referred especially since the authors often have multiple papers each year, e.g., there are two Chen et al. (2022) papers. Also, the reviewer refers to some data (e.g. Furuta et al. (1986) thickness data) that we have not been able to find in the papers quoted so we either have not found the right paper or the citation is incorrect.

Major concern 1: Figures 2
The reconstructed tephra dispersal and thickness for these three figures should be problematic based on your data, and thus there will be problems in the mass flow rates in the figure 4. Figure 2A is the comendite phase, but its distribution is more restricted than the Figure 2B trachyte phase (south direction) which is more widely distributed and its southwest limit can reach to east Korea. However, from stratgraphic and geochemical evidence, Chen et al., (2022) shows that the ME tephra recorded in the marine core from Ulleung basin exclusively is comenditic in composition while no trachytic glass shards in this tephra layer.

Thanks for making us aware of this JGR paper. We had not seen it. This datapoint has now incorporated into the modelled dataset (see Supplementary Material) and cited.

In the Supplementary Table 1 which used to reconstruct the tephra dispersals, you cited the core of KH82-4-25 from Furuta et al. (1986) which may be used to reveal the comenditic phase to the southwest limit in the figure, however, Furuta did not analyzed the ME tephra from this core, and they only analyzed the tephras from the cores of Stn.6913, KH79-3-C2, P.130-2, KH69-2-23, Stn.6920 (See the figure 4a and Table legend). As for the ME tephra, there are another five cores to be used to check this tephra without geochemical determinations, but these cores also located in the middle to north Japan Sea. So how can you determine the southwest limit of trachytic phase by using Furuta's results or did you analyze the ME tephra from this core?

Thanks for highlighting the error. We now instead use the thickness of Chen et al. (2022) for that point.

Unfortunately, Furuta et al. (1986) do not record thickness information for the locations where they found the tephra so we do not have relevant data for the modelling. Their paper presents some compositional analyses showing that both phases are present directly E of the volcano, which is consistent with modelled results.

If you used the Chen's result, why the current figure shows that the trachytic phase can reach to the eastern Korea, while the comenditic phase only cover the middle of Japan Sea. Even if you have analyzed the trachytic phase from Furuta's core, you should also clarify the difference between it and Chen's 2022 results. Additionally, you should explain why the modelled thickness and dispersal are not consistent with the tephra record by Chen et al., 2022. Therefore, the modelling is not consistent with stratigraphic records which must be resolved before publication.

The results are the model output and there is uncertainty with these measurements (shown in Fig. 2). The modelled dispersal of both phases is a best fit, and shows the main dispersal from the plume, and not all locations covered by small amounts of ash. In the revised version we added also a new supplementary figure showing the second and third best modelled solutions in order to show that the dispersal patterns could be slightly different. Significantly more data points would be required to constrain them further.

Major concern 2: Supplementary Table 1
There are many mistakes on the matches between the references and locations, and please check them, such as P35, P36, P37.

Thanks for highlighting that the references were wrong for P35, P36 and P37. These references have been corrected. The location and thickness data for those points were correct.

The tephra data in this table is very important for the tephra modelling of this manuscript, but five major problems should be considered carefully. Firtly, please careful to compare the thickness of ME tephra on the Tianwenfeng peak, such as there are two layers named as black GEMtr and light color GEMcom in the current reference of Pan et al., (2020), while the lower part of this site can be divided into two layers (upper is trachytic in composition and lower comenditic in composition) when considered the previous studies by Chen et al., (2016) and Sun et al., (2017) from the same site because the lower light color layer is not wholly comendite as stated by Pan et al., (2020). This geochemical composition difference is very important for the current manuscript because you would like to reveal the amount of tephra release from these two phases, however, if you use the current reference, the thickness of these two phases from this site will be not right.

Thank you for highlighting this, we had not realised that there was an issue with the Pan et al. (2020) thicknesses. We have now revised the proximal thicknesses of the comendite and trachytic

phases using the compositional information reported in Chen et al., (2016) and Sun et al., (2017). The dataset was updated (Supplementary Table 1) and we reran the simulations.

Secondly, for the thickness of ME tephra from Yuanchi site, I would like to recommend using the thickness of ME tephra recorded in the lake sediments of Lake Yuanchi where the sedimentary environment is more steady and less eroded by secondary processes.

We are not sure where the Lake Yuanchi thickness is reported. We have checked all the references we have, those mentioned in this review, and did another online search and did not manage to find a reference that reports this thickness. We use the total thickness of Pan et al., (2020) but have used different thickness for the phases based on attribution in Chen et al., (2016) and Sun et al., (2017).

Thirdly, the proximal thickness of ME tephra, there are many such information as published by Liu et al., (1998), which may be more useful at here.

We assume the reviewer is referring to 'Liu, R. X., H. Q. Wei, and J. T. Li. "The Latest Eruptions from the Tianchi Volcano, Changbaishan." ***SciencePress, Beijing (in Chinese)*** **(1998)', which we are not able to access in print or online. Assuming Liu's locations are on the Chinese side of the border, the same side as Pan's locations, we think those of Pan et al., (2020) are likely to be representative. Additional data points are not required for the model, especially if they refer to proximal deposits. Moreover, the fact that we get similar results even after revising the dataset for other points it implies that the method and results are quite robust.**

Fourth, how can you determine the amount of the proximal PDCs in the total volume of the magma release?

We use the estimates of Zhao et al. (2020) for the PDC volumes (stated in line 158 and 343), and these are added to fallout volumes (estimated in this study) to provide constraints on the total volume erupted.

Finally, the thickness data in the table should be double checked and how can you determine the two phases thickness of such as P06 by Machida and Arai's (1983) result, additionally, for examples, the St.6920 recorded about 3 cm ME tephra and KH69-2-23 recorded about 25 cm ME tephra based on the Furuta's (1986) numerical result which is different from the current table numbers.

The ratio of comedite:trachyte of P06 was estimated using the glass compositions, as described in the paper. The glass compositions are reported in Machida and Arai (2003) and we now also cite this in the Supplementary Table 1.

The thickness measurements for P06 (KH69-2-23; 16 cm) and P03 (St.6920; 5 cm) are from Machida and Arai (1983) and we checked that they are correct. We are not sure what Furuta et al. (1986) numerical results are, and thickness is not reported in the Marine Geology paper. We also checked and did not find another Furuta et al. (1986) reference.

Specific points

Line 2: Changbaishan volcano is more common to be used in these days' literatures, the Baekdu/ Paekdu/Baitoushan/ Baekdusan are rare to be used, so, the Baekdu volcano should be revised to Changbaishan volcano and those through the text. I recommend that the Changbaishan volcano (also referred to as Baektu, Paektu and Baitoushan) in the main text of Line 44 or Line 62-63.

This has been changed.

Line 63: DPRK to Korean?

Changed.

Line 81-85: the eruptive history of Changbaishan volcano cited here is not ideal, and the eruptive stages divided by Zhang et al., 2018 is more proper to be used at here when considering the dating results and readable to readers.

We have now included some information from Zhang et al. (2018) and cited the paper.

Line 95: the number of reference 28 did not do analysis or compile the dating results on the Changbaishan Millennium eruption. Most of radiocarbon dates on the Changbaishan Millennium eruption have been sorted in the Sun et al., 2014.

We have removed this reference.

Line 96: there are also many other well dated precise radio-carbon dates on the ME of Changbaishan volcano, like Yatsuzuka et al., 2010, Yin et al., 2012, Hakozaki et al., 2017, and Nakamura et al., 2007 also dated this eruption indirectly by dendrochronology.

We understand but we cannot cite everything and cite the papers that provide the most constrained date for the eruption.

Line 101: please check the age of 939-940 CE.

It is correct. We explain the offset in the following sentence.

Line 130, 141, 160, 185, 204, 291 and following parts: the GME is highly confused and the ME which is easier to be accepted by readers, however, the GME is easier to make confusion. So, please revised all the GME to ME throughout the text to avoid using different names for the same event.

We now use ME throughout and explain in the text (line 130) that this corresponds to what Pan et al. (2020) describes as the GME.

Line 135-136: such trachytic PDCs mainly founded in the valleys around the volcanic vent while not widely distributed like the comenditic PDCs, if so, why the early comenditic PDCs were not eroded?

We do not comment on erosion of the comenditic PDCs as it has not been commented on in the literature. The comenditic PDCs underlie those of the trachyte and are partially welded so are likely to be considerably less eroded than those of the trachytic phase. The PDC deposits are not the focus of our work.

Line 148: add reference.

Done

Line 168: ……do not show noticeable changes following the Millennium eruption9, 10. The charcoal wood used in these two references are killed by the Millennium eruption, and the last ring represents the year of the ME. However, how can you determine the changes following the years after the ME because these woods did not record these rings after the ME?

Oppenheimer et al. (ref 10) looked at dendrochronology records elsewhere to look at the impact of the eruption. The proxy records Xu et al. (ref 9) used are temperature reconstructions based on ice core records, but we have now removed this reference as it was not necessary.

Line 189: add reference to the ……both phases referecens, ….

Machida and Arai (1983) is now cited here.

Line 201: ……Sihailongwa, China, and Lake Kushu, Japan. Add references.

We have rephrased the sentence and refer to the Supplementary Material.

Line 204: 17, 21, 40 to references?

Corrected

Reviewer #3 (Remarks to the Author):

Esteemed colleagues,

This manuscript proposes a reconstruction of the eruption dynamics of two phases of the millennium eruption of Peaktu in 946 CE. This eruption is a unique case study whereby despite its magnitude and gas yields, it is not associated with volcanic forcing (cooling) of climate. The manuscript includes an extensive review of the existing literature on the physical volcanology and degassing budget estimates of the eruption. The paper presents a new strategy for the reconstruction of key eruptive parameters using dispersion models. It includes a large dataset over a wide distribution grid that is rare for past eruptions of this magnitude.
My expertise is limited to the reviewing of eruption dynamics and degassing budget estimates, which I am very familiar with. I am not qualified to assess the modelling and statistical approach used here, which are the core novelty of the manuscript. I enjoyed reading the manuscript, which has a clear structure and is well-written. As a volcanologist working with climate modellers to reconstruct the impact of past explosive eruptions on climate, I can attest that eruptive parameters key in the precise reconstruction of the atmospheric response to volcanic sulphur emissions. This work provides essential conclusions for physical volcanology and highlights the importance of tephrochronology, but also provides key information relevant to climate modellers and magma geochemists. In this view, this manuscript meets the criteria to be published in Communications Earth & Environment.
My overall assessment is that the paper contains robust publishable data and has some areas for improvement. Critical aspects to address are i) include a reconstruction of the caldera size and a comparison with erupted volumes estimates calculated here; ii) include a more thorough discussion about sulphur release within the process of the eruption associated with magma mixing and how the findings reflect ice core data and Toohey et al 2019 modelling that shows the stronger cooling followings winter NH eruptions; and iii) incorporate in the conclusion a comment on the wider

significance of quantifying uncertainties in eruptive parameter reconstruction, which is the core of the paper.

A main concern I have which is trivial but very important is the spelling/naming of the volcano. My understanding is that Baektu is the South Korean name. Since the volcano is located at the Border between PRC and DPRK, in order to respect regional designations and honour diplomacy, I would suggest using the North Korean (Peaktu) and Chinese (Changbaishan) names and quoting the South Korean spelling in brackets right at the start of the manuscript. I also would suggest mentioning briefly at the start what is the significance of this volcano for local people. In DPRK at least it is considered as a sacred mountain in the regional folklore. More details on this are available in recent literature.

I would therefore encourage moderate revisions and would like to review a new version in the future.

Below I outline specific comments, where I have passionately kept my tone as encouraging as possible. I hope the suggestions below will help improve the manuscript.

Yours,

Céline Vidal

We greatly appreciate Celine's comments and we thank her for the suggestions that helped us to improve the clarity of the presentation. We have now added also a sentence on the consistency of our estimation of the erupted magma volumes with that obtained from simple consideration on the geometry and size of the caldera (added to the Volume and Eruption Parameters section that starts on line 532)

Specific comments:

Line 32. And large uncertainties

Done

Line 35. As well as testing the accuracy of dispersal models
Added

Line 43. Impact on climate.
Done

Lines 43. Please consider replacing 'we know' with 'we have good constraints on' and 'impact' with 'climate response'
Done

Line 49. The 1815 eruption of Tambora, Indonesia
Done

Line 55. What is the evidence for a hiatus of 3 months? I suggest providing more details here or omitting this in the teaser, and adding a reference.
This was based on the historic records that record events consistent with an eruption 3 months apart. We have removed it here and just comment elsewhere in the manuscript.

Lines 81-82. '28 My ago'
Done

Line 88. What is the published VEI of the millennium eruption?
It's VEI6 (Yang et al., 2021) but we prefer not to use VEI to consolidate the use of concepts like Eruption Magnitude and Eruption Intensity in accordance with Pyle (2015) (The Encyclopedia of Volcanoes http://dx.doi.org/10.1016/B978-0-12-385938-9.00013-4).

Line 115. It would be great to have a figure with location of occurrence of the different types of deposits in order to follow the description. In the text, I also suggest adding directions (N,S,E,W) from the caldera to locations mentioned with a distance from the vent in order to help picture the geographical distribution of the deposits (it is important to understand plume dynamics).

We now refer to direction in the text (lines 206 through to 217).

Line 161. How does this volume compare with the size of the caldera? How deep is it? Has anyone reconstructed the caldera volume? I mention this because Peaktu caldera is slightly smaller than Tambora's and Samalas' and the volume for these two is ca.40-45 km3. So the upper DRE volume estimate for the millennium eruption seems high… see further comments on this point below.

Thank you very much for this suggestion. We now compare our estimation with an independent one. In the revised version we added a few sentences into the Volume and Eruption Parameters section that starts on line 532.

Line 166-168. Records from where? Is that all NH JJA temperature reconstructions? We know that there can be great regional variability in tree ring reconstructions so it is important to precise.
They are combined Northern Hemisphere reconstructions, and we have now explained this in the text.

Line 171-172. Please replace 'considerably' with an approximate factor. I would suggest using a NH eruption to compare rather than 1815 Tambora (e.g. at Katmai, in Kamchatka, Japan or Aleutian) because sulphur transport and deposition are not comparable with those of a tropical eruption.
Done, and we have commented that the sulphate deposition will vary because the latitudes are very different. Other northern hemisphere extratropical eruptions are not good analogies as the dates are often not well enough constrained, the sulphate signals are very low, and they typically at least an order of magnitude smaller than the Millennium eruption.

Line 173. Tropospheric plume heights
Done.

Line 176. Was the thickness and composition data newly acquired for this study? I understood later

that it is the case but this sentence can be confusing since there is already a lot of data in the literature.
No, we use published thickness data. This has now been made more explicit in the text.

Line 186. Please indicate a radius if possible, this would be more informative than 'close'. Please also indicate how close to the caldera the sampling took place. With the very large scale of Fig 1, it is difficult to assess how proximal the deposits you sampled were.
Distances have been added to the text.

Line 185-191. I don't follow here. You mention in the context that the deposits are mingled. In this section, you suggest they are not visually distinct. Are they layered? Have you found any mingled deposits?
This is really important to clarify to constrain magma ascent/recharge and eruption trigger and directly affects degassing dynamics and at which point in the eruption it took place.
Mingling is noted in some publications and the layers are noted, and clearly observed in photographs, in other publications. It is true that these aspects are important to constrain magmatic processes but that is not the focus of this research.

Line 196. How far is Lake Suigetsu from Peaktu?
The distance has been added.

Line 203. Please indicate how you 'know' the thickness if deposits are mixed or refer to the methods section.
The explanation has been expanded in the 'Tracing the dispersal of the comenditic and trachytic phases' section.

Line 210. Figure 1. Please add country and ocean names, and the North arrow. Also report all location names mentioned in the text on Figure 1 to help the reader follow. Please also add an inset with the caldera and proximal deposit distribution, or this could be in a separate figure.
Fig. 1 has been updated accordingly.

Lines 218-221. How does that time period reflect meteorological conditions for the year 946 - what is the argument for using this time frame? Are the atmospheric temperatures similar? Please add some details to justify the choice of meteorological conditions (apart from the winter timing)

The models used a range of meteorological conditions and select those that best fit the observed dispersal. This methodology, originally proposed by Costa et al. (2012), assumes that the collection of modern winds fields can statistically represent those at the time of the ME.

Line 236-245. When you say ' all measurement points available' do you refer to the two phases cumulated? If that is the case, knowing that there was a short hiatus between them, what is the meaning of these parameters?

Technically, in a geological sense, they are the products of two phases of the same eruption. There is not enough evidence to suggest that there was a significant hiatus between the phases. The modelling approach does them both separately and together and the results are similar suggesting the approach is valid.

Line 327. I would like to see at least a qualitative comparison of these volumes with caldera volume to see how your volume ranges fit. I would be a bit cautious with the upper limit of 37.4 km3, knowing that this excludes PDC volume. Is there bathymetric data in the lake to assess the depth? With the shape of the caldera, it should be feasible to estimate qualitatively the missing volume using DEM, e.g. following Hutchison et al. 2016's approach (https://doi.org/10.1038/ncomms13192)
We have already answered to this point and added an independent estimation of the erupted volume based on the caldera sizes.

Lines 331-332. Please include some other examples of similar VEI because Pinatubo is smaller than the Millenium eruption according to your eruptive parameters.
The 1992 eruption of Pinatubo is the largest eruption for which the mass eruption rates have been calculated using observations, estimates for other eruptions are extrapolations.

Line 341. Where is the co-PDC fallout if any? It is not discussed in the manuscript but would be important to mention it somewhere, including if it wasn't found, because this would increase the uncertainty on the volume estimates.

We cannot distinguish between Plinian and co-PDC fallout as they share the same chemistry and at distances more than a few hundred kilometres from the vent, and they also cannot be distinguished on grain-size. The fallout is a combination of that associated with both the Plinian and PDC phases. We have made this more explicit in the text.

Lines 343-365. In this section, the yields are confusing. Are you referring to previous yields based on previous volumes? Or have you rescaled the yields based on the new volume estimates? I would suggest not mentioning yields but rather concentrations (ppm) to avoid confusion.
Sorry, we now realise that this section was extremely ambiguous. We have changed the whole section to address this comment and those below.

Line 347. Please add in brackets the SO2 equivalent (since SO2 yields rather than S yields are mostly compared to other eruptions)
Done

Lines 368-371. Rather than referring to Pinatubo here, it would be more important to briefly discuss the potential contribution of the trachytic magma to the comenditic volatile phase discussed extensively in the literature by M. Edmonds, P. Wallace and others, especially in regards to the apparently different magma mixing processes at stake revealed by deposit characteristics (mingled, layered?). This would influence when most of the sulphur was released, more likely by the volatile phase during the first phase (comendite) of the eruption and less through the syn-eruptive degassing of the trachytic magma in the second phase of the eruption. There are not enough details in the text to understand degassing processes. I know this has been covered partly by Iacovino et al., but it should be discussed here before scaling S emission to the new volume estimates.
The indicator that S made it into the stratosphere is the S in ice. Please also compare your new yields with estimates based on ice core records (Sigl et al. 2015, Sigl and Toohey 2022), for example after line 380. What is found in the ice validates or not the results of this work.
There is very little evidence for mingling of the magmas and essentially no hybridisation so the interaction of the two is likely to be extremely limited. Previous researchers have not even

considered the trachytic melt and we do not have any volatile data for that melt so we cannot comment on likely degassing processes.

We now state the calculated S load based on non-sea salt sulphur in the ice core from Sigl et al. (2022), and comment that it is consistent with the range that is estimated from scaling previous volatile estimates by Horn & Schmincke (2000) and Iacovino et al (2016).

Line 375. Released

Done

Line 376. Add SO2 equivalent in brackets.

Done

Line 383-384. Most importantly, there is a very poor understanding of halogen behaviour in plumes and their ability to reach the stratosphere because of rapid leaching in the plume (see e.g. Wade et al 2020 (no need to cite it but we discuss the limitations of our understanding there), Mather, 2015 etc).

We now comment on this (line 634) and cite Wade et al., 2020.

Line 382. Toohey et al. 2019 (https://doi.org/10.1038/s41561-018-0286-2) discuss the stronger impact of high latitude NH eruptions occurring in the winter (rather than in summer) based on modelling. I would suggest discussing this in the light of your findings.

We now mention and cite the work by Toohey et al. (2019)

Line 393. Which fluids?

Mainly hydrothermal fluids. This is now stated.

Line 397. You could also mention briefly local water poisoning by F and livestock deaths (e.g. 1783 Laki)

Done

Line 414. Given the large uncertainties on the volume estimates, I would suggest referring to volume ranges rather than approximate values which are likely to be quoted in future work without an understanding of the uncertainties.

We have now stressed this point and highlighted the ranges rather than the single value.

Line 423. It would be great to see a comment on the critical necessity of eruptive dynamics reconstructions and the assessment of uncertainties because these ripple on atmospheric circulation models that aim to reconstruct the impact of eruptions (see Marshall et al., 2022).

We now make such a comment (see line 697)

Methods

The Methods section does not explain how the tephrochronological and geochemical work was carried out. I understand now that this was published previously, is that right? If that is the case, further details on how phases were differentiated should be provided in the main text.

Yes, it is all published information. We have made this clearer in the section titled 'Tracing the dispersal of the comenditic and trachytic phases' that starts on line 226.

9th Nov 23

Dear Dr Costa,

Your REVISED manuscript titled "Reconstructing the dispersal and magnitude of the Millennium Eruption of Paektu (Changbaishan) volcano" has now been seen by our THREE ORIGINAL reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

Regarding reviewer 2's comment on the use of the volcano name, we find your current use clear; we leave it to you whether you would like to make any changes in light of these comments.

Please note that it may still be possible for your paper to be published before the end of 2023, but in order to do this we will need you to address these points as quickly as possible so that we can move forward with your paper.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Carolina Ortiz Guerrero
Associate Editor
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I wish to thank the authors for the answers they provided me and for considering my suggestions. My recommendation is to publish this article with no further revisions from my side. I am looking forward to see it published as soon as possible. Congratulation for this great and timely work!

Reviewer #2 (Remarks to the Author):

After having reading of the revised manuscript, I am pleased with the changes by the authors, and they have responded most of the concerns. However, there is still one major problem that may confused most of future readers is the name of the “Changbaishan volcano”.
In the current manuscript:
Line 64-65: ……eruption from Paektu volcano, also referred to as Baekdu (south Korean), Baegdusan (Japanese), or Changbaishan (Chinese).
Line 81: Paektu volcano
Line 82: Mt. Paektu
Line 203: Mt Paektu

Figure 1. Mt. Baekdu
Why so many expressions for the same volcano? As the authors statement, the Baektu is from south Korean. Then, where is the Paektu from, north Korean? South Korean is a country while not the language. Why the same language produces different names? I would suggest accepting the manuscript after changing all the highly confusing names of Paektu or Baekdu to Changbaishan for consistent.

Reviewer #3 (Remarks to the Author):

Dear authors,
Dear editor,

Thank you for the revised, improved manuscript. Authors have carefully addressed comments and questions from the reviewers and I am satisfied with their response and corrections.

I attach the manuscript file with some minor additional comments that should easily be addressed and will not impede the quick publication of the article. I would support the publication without further revisions.

Warm wishes,

Céline Vidal