nature portfolio

Peer Review File

Outgassing behaviour during highly explosive basaltic eruptions

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

Decision letter and referee reports: first round

9th Aug 23

Dear Dr Bamber,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "Outgassing behaviour during highly explosive basaltic eruptions". It has now been seen by 2 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.

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), a tracked-changes version of the manuscript (as a PDF file) 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/
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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
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article.pdf">Communications Earth & Environment formatting checklist

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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)
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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):

The manuscript by Bamber et al "Outgassing behaviour during highly explosive basaltic eruptions" investigates the role of ascent conditions, bubble characteristics and permeability development on eruptive style in basaltic magmas. The authors combine 3D textural analysis of basaltic lapilli erupted at three volcanoes and with a range of explosivity, with a numerical model of ascent and fragmentation. They show that explosivity is mostly controlled by bubble number density, friction coefficient but also by storage conditions in the cases of fast ascent.

I found this study very interesting and easy to read and understand. In my opinion, it contributes to a significant improvement of our understanding of highly explosive eruptions in basaltic magmas. I would recommend publication of this work after minor revisions. I provide my comments directly in the annotated PDF.

Kind regards,
Mathieu Colombier

Reviewer #2 (Remarks to the Author):

Dear Editor,

I have now read the manuscript "Outgassing behaviour during highly explosive basaltic eruptions". The manuscript contains a laboratory-based micro-CT analysis of samples from three different basaltic Plinian eruptions as well as two lava fountain samples (one from a large-scale fountain) for comparison. Furthermore, the authors present 1D numerical model simulations, which they use to investigate different conduit processes and how they relate to permeability and outgassing efficiency. Furthermore, the role of magma ascent rate is investigated as well as the impact of changes in initial conditions. The work is well-referenced throughout and provides a well-written, strong introduction to the field, a great methods section as well as several figures of good quality. The work is novel and ambitious in scope as it contains data from both samples and numerical simulations. I think it would be of great interest to the volcanological community, as it improves our understanding of eruption dynamics of highly explosive basaltic eruptions.

However, I have some reservations about the current format of the manuscript. I would recommend major revisions before I think it is suitable for publication.

My main concerns are:

1) Primary data/results are not included in manuscript, but only added as supplementary material. This is the case for A LOT of the data presented in the paper. Some is included in supplementary figures and some in supplementary data. This needs to be added as figures and/or tables to the manuscript itself. Supplementary data is in my opinion supposed to be that – supplementary – and the readers should be able to read the paper without consulting the supplementary files unless they opt to seek additional in-depth information. I have made annotated comments in the manuscript as to which data sources I think should be included in the manuscript.

2) Inconsistent and sometimes non-existent data descriptions and lack of reference to figures/tables. I find the quality of data descriptions in the Results and Discussion section highly variable. It lacks consistency and is difficult to navigate for the reader. Sometimes only some samples/scenarios are described and sometimes interpretations refer to data that is not presented anywhere. All datasets need to be described in a consistent and accessible manner throughout this section to help the reader understand and follow interpretations properly.

3) Lack of definitions of used parameters in the results section and with no reference to the methods section. I would like to see definitions of all parameters included before data of that parameter is presented, so the reader can follow along without jumping back and forth between sections. I also suggest moving the sample description from the methods section to the end of the introduction, so readers have been introduced to the samples before the data presentation and detailed discussion.

4) Interpretations are intertwined with data presentations and sometimes made without reference to specific data. This is very confusing and leads to repetitions throughout the Results and Discussion section. I suggest restructuring and rewriting this section with subsections of clear data presentations with reference to manuscript figures and definition of presented parameters and rewritten succinct interpretive subsections.

The structure could look like this:
a. descriptions of sample observations: crystallinity (needs to be added), vesicle morphology
b. Description of sample number densities (add a data presentation)
c. Description of vesicle size distributions and data fitting.

Please include supplementary figure 1 as you present primary data from this figure.
I suggest moving all the interpretations of these data types to a separate section with a subtitle, so it can be written more efficiently drawing on all the datasets without repetitions.

Then I suggest you keep the subtitle and add a sentence about permeability and what parameters are important for permeability (like you have an introduction and justification of the sensitivity analysis lines 230-235, which works really well), introduce the parameters like you did for connectivity in lines 191-194 and/or reference the equations in the methods section as appropriate, and then present the porosity, connectivity, ftb, and tortuosity factor data and include supplementary data tables in a table in the manuscript.
This can be followed by another section with a subtitle interpreting the presented data.

The sensitivity analysis of the four characteristic parameters works pretty well, though I would suggest again separating descriptions from interpretations in two different subsections with subtitles.
I think you should consider removing figure 7 as it is difficult to read, deals with non-explosive scenarios rather than explosive (the theme of the paper), and the interpretations are speculative. Instead, I would advocate that you include supplementary figures 5 and 6, as you cite these for primary data and the connect well with the ascent rate section.

The magma ascent rate section is a confusing section with a lot of repetition of both itself and the introduction. I think it needs substantial revision and shortening and again a clear definition of parameters and separation of results and interpretations. Please remember to tie interpretations to results and also draw on the whole dataset of the manuscript to avoid speculative interpretations.

The final section on initial conditions could also benefit from shortening and more efficient presentation and summary of your interpretations.

5) Not using previously presented sample data to support later interpretations of simulation outcomes. Sometimes is this done implicitly, but it would strengthen the manuscript if this was done consistently and explicitly.

6) Repeating statements previously outlined in the introduction in the modeling part of the Results and Discussion section. These should be rewritten to ease the flow of this section. There are several annotated comments in the manuscript about this.

I have annotated additional minor comments to the manuscript PDF as well.

I think with a thorough rewriting of the Results and Discussion and Conclusion and rearranging figures between manuscript and supplementary materials that this work would be a welcome contribution to a very interesting field of research.

We thank the reviewers for their careful and constructive review of our work and their comments and suggestions which have greatly improved our study. We have addressed each of the points raised on the manuscript PDF and below we provide a response to each individual comment (in italics). We have provided new line references in our reply which indicate where the corresponding revisions can be found in our revised manuscript. These revisions are colour coded in the manuscript and the response to the reviewer comments below. Revisions in response to the comments of reviewer 1 are highlighted in orange and revisions in response to the comments of reviewer 2 are highlighted in blue.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript by Bamber et al "Outgassing behaviour during highly explosive basaltic eruptions" investigates the role of ascent conditions, bubble characteristics and permeability development on eruptive style in basaltic magmas. The authors combine 3D textural analysis of basaltic lapilli erupted at three volcanoes and with a range of explosivity, with a numerical model of ascent and fragmentation. They show that explosivity is mostly controlled by bubble number density, friction coefficient but also by storage conditions in the cases of fast ascent.

I found this study very interesting and easy to read and understand. In my opinion, it contributes to a significant improvement of our understanding of highly explosive eruptions in basaltic magmas. I would recommend publication of this work after minor revisions. I provide my comments directly in the annotated PDF.

Kind regards,
Mathieu Colombier

COMMENTS ON PDF
Lines 56-57: This is a specific definition of outgassing associated with low viscosity melts only. Maybe specify this.
We agree with the reviewer and have updated the definition to refer to low viscosity melts (lines 58-59).

Line 58: Maybe nuance this sentence since even with decoupling and formation of large bubbles we can have explosive bursts
We have now revised the sentence to specify that magma fragmentation should occur to produce an explosive eruption in this case (lines 60-61).

Line 61: This is not the same outgassing process as having a separated/decoupled flow
We cannot exclude that there are other processes related to transitions from explosive to effusive activity, however, here we wanted to say that, in recent works, the explosive-effusive transition has been also related to increased permeability and outgassing efficiency.

Lines 88-89: Which is often not the case for lava fountaining
We agree with the reviewer and have now revised the sentence to clarify that we refer to clasts from explosive eruptions in this case, which should preserve evidence of degassing at the point of fragmentation (lines 88-91).

Lines 98-100: I agree these are advantages of micro-CT. But 2D studies are still very important in many cases where many vesicles are simply not resolved by CT and

decoalescence is not reliable in highly connected networks in 3D. Maybe just stress the advantages of 3D without stressing too much the flaws of 2D analysis.
We agree with the reviewer and have now modified this paragraph (lines 93-99) to discuss only how 3D techniques are more appropriate for our study as we are interested in quantifying properties which require 3D information, such as connectivity, tortuosity and the throat-pore size ratio.

Lines 117-119: You say that again later in this paragraph.
We have removed this sentence as it is repeated later in the paragraph (lines 115-117).

Line 120: Here and elsewhere, replace samples by pyroclasts, clasts or tephra.
We have replaced sample throughout the manuscript.

Line 122: at Etna volcano (Italy)
We have moved the reference to Etna (line 117).

Line 128: with this
We have updated the sentence (line 137).

Line 130: and style and intensity of basaltic eruptions
We agree with the reviewer and have clarified this point at the end of the sentence (lines 139-140).

Lines 135-136: FL and MTL are not defined before this
As we have now moved a section from the Methods section which introduces the sample suite analysed, also in response to a comment of reviewer 2, FL and MTL are now defined before the presentation of the data in the results section (lines 119-120).

Line 137: Why are these samples shown only in the supplementary?
These samples were shown in the supplementary as they have similar textures to clasts of the FL and Etna 122 BC eruption. However, we have now revised Figure 1 to include an orthoslice and 3D renderings of a clast of the MTL eruption so that this sample is present in the main text (lines 145-146).

Line 141: at large vesicle volumes
We have updated the sentence to better describe the power law distributions in these samples (lines 186-188).

Line 142: (Fig. 1)
We have added a reference to Figure 1 to this sentence (line 164).

Line 144: I did not find evidence of crystal-rich regions for FL samples in the supplementary
As our sample suite contains only clasts of the crystal-poor endmember of the FL eruption, we do not provide an images of crystal-rich regions in the supplementary. We have now revised the sentence in the main text (lines 175-176) to clarify that our samples all represent crystal-poor clasts of the FL eruption.

Line 147: I agree vesicles look more isolated and smooth for FL, and crystals may have enhanced coalescence for Etna 122 BC. But coalescence may have also occurred for FL, and then subsequently erased by bubble relaxation by surface tension to a smooth, isolated bubble. Melt film drainage may depend on variations in melt viscosity between FL and Etna (Nguyen et al., 2013)?
We agree with the reviewer and have added a sentence which also highlights the possibility that short interbubble film drainage times in lower viscosity, more crystal-poor basaltic magma (such as for the FL) may also allow coalescence of smaller bubbles and relaxation to

form larger bubbles with a smooth morphology (lines 181-183). We have added Nguyen et al. (2013) to the reference list.

Line 150: strikethrough

The sentence has been corrected (line 147).

Line 154: (Fig. 2)

A reference to figure 2 has been added (line 152).

Line 184: What about lava fountains at other volcanoes? Lava fountaining activity at Etna is quite intense compared to other locations and sometimes referred to as subplinian. Are these statements applicable to lava fountaining in general?

The high N_d observed for the Etna lava fountain and comparison with Plinian and paroxysmal samples here refers to the large-scale lava fountain type, which represents the high intensity fountain activity at Etna. In Figure 4 we also show that the N_d of large-scale fountain activity at Etna is comparable with the high N_d of high and intermediate fountains at Kīlauea, which have a comparable eruption rate and erupted volume to the large-scale lava fountains observed at Etna (lines 922-925). Therefore, these statements are applicable to lava fountains at other volcanoes also, but represent the large-scale lava fountain type, which have higher eruption rates and erupted volumes than more typical lava fountain activity, such as the Etna 2013 fountain, the 1959 Kīlauea Iki and the 1969 Mauna Ulu eruptions. We have added a reference to Figure 4a to highlight that high N_d are observed for both the large-scale lava fountain activity of Etna and the high and intermediate fountains of Kīlauea (line 230).

Line 190: Other clasts from lava fountains at Etna have much higher porosities, are your values representative of the whole pyroclast population associated with these events?

To investigate the variation of porosity during a lava fountain, the whole population of pyroclasts should be analysed, which is not the focus of our work. Rather we investigate the relationship between porosity and eruptive styles of increasing intensity represented by products of the 2001, 2013 fountains and 122 BC Plinian eruption at Etna. In this context, we think that our sampling may represent different porosities associated with these volcanic events.

Line 195: Please provide values

The range in connectivity for the MTL and Etna 122 BC samples has been added to the manuscript (lines 241-242).

Lines 203-209: Is there something else to say on this parameter? Isn't the throat size more relevant than this ratio? From the data, it seems that crystal-rich zones have a higher value of f_{tb} at a given porosity, is that correct?

We now indicate in the manuscript (lines 245-249) that the throat-pore size ratio is the ratio between the radius of the throat and the average of the radii of the two connected vescicles. Being an adimensional number, this value gives us general information on the pore network within the sample, which is independent from the size of the pores. As suggested by the reviewer, in terms of outgassing and permeability, the throat size is the relevant parameter to consider, which can be calculated as the mean throat-pore size ratio (f_{tb}) multiplied by the average bubble radius (r_b). Indeed, in the numerical model of magma ascent both permeabilities are calculated as function of the throat size $f_{tb} r_b$

$$k_v = \frac{(f_{tb} r_b)^2}{8} \alpha_g^m. \qquad k_i = \frac{f_{tb} r_b}{f_0} \alpha_g^{\frac{1+3m}{2}}$$

We now modified the text to emphasize that outgassing and permeability are controlled by the throat size, rather than directly by throat-pore size ratio. Furthermore, the above equations are reported in the Methods section. Crystal-rich clasts do show higher values of f_{tb} at a given porosity, such as comparing the crystal-rich clasts of the MTL and Etna 122 BC eruption with crystal-poor clasts of the FL eruption, however, they overlap when considering also the standard deviation. We have added a sentence to the manuscript which shows the difference in f_{tb} with crystallinity (lines 254-256).

Line 210: A general comment, why not combine supplementary data 2 and 3?
We have now combined supplementary data 2 and 3 (as Supplementary Data 2) (line 251). However, also in response to a comment from reviewer 2, part of supplementary data 2 is now also present in the main text as Table 1.

Lines 210-211: Tortuosity seems instead quite constant for a majority of samples with τ~1.5. Is there an explanation for this?
The tortuosity is comparable for the majority of the crystal-poor samples, such as those for the FL eruption. The similar tortuosity values may further suggest that crystallisation has considerable influence on the tortuosity of connections between bubbles, as bubbles will deform around rigid crystals which crystallise during magma ascent. Instead, the crystal-rich samples, such as those of the MTL and Etna 122 BC eruption show more variability in tortuosity. We have added a sentence to explain the range of tortuosity values calculated for the samples (lines 261-262). We have also modified the sentence (lines 257-258) to highlight that tortuosity and the tortuosity factor show greater variation across the sample suite, compared to f_{tb} and N_d.

Line 214: Is there really an influence of crystals seen on tortuosity from the present dataset? Could this be shown in a figure?
We suggest that crystallinity has an influence on tortuosity as it is only the crystal-rich clasts which reach the highest values of tortuosity ($m \geq 6$), and crystal-poor clasts such as those of the FL eruption and lava fountains have much lower values of m. As we present the ranges in crystal content measured for samples of these eruptions from the literature, we do not present a figure which illustrates crystal content and tortuosity. However, we have now added a reference to both Table 1 and supplementary data 2 to support this point (highlighted in blue due to a comment also from reviewer 2) at lines 295-296.

Line 219: But on the other hand you say that crystals promote coalescence and development of permeability, please nuance this sentence here to show that this is a balancing effect.
We agree with the reviewer and we have rewritten the sentence to highlight that it is the relative timescales of these processes which may influence the role of crystallisation in promoting or restricting the development of permeability (lines 301-303).

Line 235: I would not use the acronyms and simply say crystal-poor or rich
We have replaced the acronyms throughout the manuscript, figures and supplementary information.

Line 263: Is the rheological effect of crystals also taken into account in the model?
The conduit model accounts for the rheological contribution of syn-eruptive crystallisation during magma ascent, following Arzilli et al. (2019). A complete description of the conduit model, including the crystallisation model which accounts for this contribution can be found in the Supplementary Methods (equations 18 and 19).

Lines 267-269: It was said in the previous paragraph that such values may represent large-scale fountains. Maybe rephrase to stress that it is in the case where fragmentation occurs in the conduit?

We have revised the sentence to specify that here we present the data for explosive solutions and that these solutions are explosive as magma fragments within the conduit (lines 351-352).

Lines 285-287: I found this sentence not so clear

We have revised this sentence to separate the descriptions of explosive and non-explosive solutions and to better highlight the range in exit velocities observed for the two groups of non-explosive solutions (lines 373-376).

Line 397: What do you mean by rapid?

Here, we mean that due to the fast magma ascent rates estimated for basaltic Plinian eruptions, that a high N_d must nucleate over a short timescale. The decompression experiments of Le Gall and Pichavant (2016) on basaltic magmas use a decompression rate comparable to the rates estimated for the Plinian case and find a similar N_d to those observed in the basaltic Plinian clasts. The high N_d observed in clasts could nucleate over a short timescale as magma would ascend at a fast rate within the conduit. However, as this sentence has now been removed from the manuscript and this paragraph revised in response to a comment from reviewer 2, we have not updated this sentence in the revised version of the manuscript.

Reviewer #2 (Remarks to the Author):

Dear Editor,

I have now read the manuscript "Outgassing behaviour during highly explosive basaltic eruptions". The manuscript contains a laboratory-based micro-CT analysis of samples from three different basaltic Plinian eruptions as well as two lava fountain samples (one from a large-scale fountain) for comparison. Furthermore, the authors present 1D numerical model simulations, which they use to investigate different conduit processes and how they relate to permeability and outgassing efficiency. Furthermore, the role of magma ascent rate is investigated as well as the impact of changes in initial conditions. The work is well-referenced throughout and provides a well-written, strong introduction to the field, a great methods section as well as several figures of good quality. The work is novel and ambitious in scope as it contains data from both samples and numerical simulations. I think it would be of great interest to the volcanological community, as it improves our understanding of eruption dynamics of highly explosive basaltic eruptions.

However, I have some reservations about the current format of the manuscript. I would recommend major revisions before I think it is suitable for publication.

My main concerns are:

1) Primary data/results are not included in manuscript, but only added as supplementary material. This is the case for A LOT of the data presented in the paper. Some is included in supplementary figures and some in supplementary data. This needs to be added as figures and/or tables to the manuscript itself. Supplementary data is in my opinion supposed to be that – supplementary – and the readers should be able to read the paper without consulting the supplementary files unless they opt to seek additional in-depth information. I have made annotated comments in the manuscript as to which data sources I think should be included in the manuscript.

We have now significantly revised the data presentation in the revised manuscript by rearranging the figures between the main text and Supplementary Information. We have now included Table 1 in the main text, which includes the data which was originally present in Supplementary Data 1 and contains the data obtained from the X-ray microCT analysis for *all of the samples and the values for each Forchheimer parameter. Following also the annotated comments on the pdf, we have merged Figures 8 and 9 from the original submission (now included as Figure 8 in the revised manuscript) to provide space in the main text for Table 1. Also following these comments, we have moved Figure 7 to the Supplementary Information and replaced it with Supplementary figures 5 and 6 from the original submission. By doing so, the figure supporting the discussion of the effect of the Forchheimer parameters on conduit dynamics is now present in the main text and is consistent with other figures which present the results of the MTL and Etna 122 BC sensitivity analyses. We agree with the reviewer that the previous Figure 7 (now Supplementary Figure 4) is better placed in the Supplementary Information as the figure focuses on the results of non-explosive solutions, where the discussion and other figures in the main text discuss the results of explosive solutions.*

2) Inconsistent and sometimes non-existent data descriptions and lack of reference to figures/tables. I find the quality of data descriptions in the Results and Discussion section highly variable. It lacks consistency and is difficult to navigate for the reader. Sometimes only some samples/scenarios are described and sometimes interpretations refer to data that is not presented anywhere. All datasets need to be described in a consistent and accessible manner throughout this section to help the reader understand and follow interpretations properly.
We have now added a more detailed description of the results of the X-ray microCT analysis to the results and discussion section. As the results and discussion section has now also been reorganised (please also see the response to the comment below), the presentation of data such as vesicle morphology, size distributions and number densities are easier to find (lines 144-161). Specifically, further detail has been provided for vesicle size distributions and a paragraph has been added which presents the vesicle number densities for the samples (lines 156-161). Additional results from the sensitivity analysis have also been added (lines 340) and a supplementary figure (Supplementary Figure 2). Additional references have been added to figures and tables which show this data within the manuscript, particularly the new Table 1 which has been added to the revised manuscript and presents the results of the micro-CT analysis.

3) Lack of definitions of used parameters in the results section and with no reference to the methods section. I would like to see definitions of all parameters included before data of that parameter is presented, so the reader can follow along without jumping back and forth between sections. I also suggest moving the sample description from the methods section to the end of the introduction, so readers have been introduced to the samples before the data presentation and detailed discussion.

We have now included definitions of each of the parameters in the results and discussion section before the data is presented (lines 233-240 for connectivity; lines 244-249 for the throat-pore size ratio; lines 257-267 for tortuosity and tortuosity factor). References to the supplementary methods when further detail is provided have also been added. We have also moved the description of the samples to the end of the introduction (lines 118-131), so this is introduced before the description of the results.

4) Interpretations are intertwined with data presentations and sometimes made without reference to specific data. This is very confusing and leads to repetitions throughout the Results and Discussion section. I suggest restructuring and rewriting this section with subsections of clear data presentations with reference to manuscript figures and definition of presented parameters and rewritten succinct interpretive subsections.
We have restructured and rewritten the results and discussion section (also following the annotated comments on the PDF) to clarify our observations and interpretations and provide clear sections which present the results of the X-ray microCT analysis and sensitivity analysis. We have also provided references to manuscript figures (also following the

rearrangement of figures between the main text and supplementary information) and definitions of parameters.

The structure could look like this:
a. descriptions of sample observations: crystallinity (needs to be added), vesicle morphology
b. Description of sample number densities (add a data presentation)
c. Description of vesicle size distributions and data fitting.
Please include supplementary figure 1 as you present primary data from this figure.
I suggest moving all the interpretations of these data types to a separate section with a subtitle, so it can be written more efficiently drawing on all the datasets without repetitions.
We have revised this section to present three clear paragraphs which present the sample observations (lines 144-148), vesicle number densities (lines 156-161) and size distributions (lines 149-155). Figure 1 has also been revised to support the data presentation in this section. A separate section has been added after the presentation of the results which interprets the data obtained from the X-ray microCT analysis and how our results provide insight into bubble nucleation and growth processes (lines 163-230).

Then I suggest you keep the subtitle and add a sentence about permeability and what parameters are important for permeability (like you have an introduction and justification of the sensitivity analysis lines 230-235, which works really well), introduce the parameters like you did for connectivity in lines 191-194 and/or reference the equations in the methods section as appropriate, and then present the porosity, connectivity, ftb, and tortuosity factor data and include supplementary data tables in a table in the manuscript.
This can be followed by another section with a subtitle interpreting the presented data.
We have provided a definition of permeability at the start of this section (lines 233-234) also defined and introduced connectivity (lines 233-240), throat-pore size ratio (lines 244-249) and the tortuosity factor (lines 257-267) in the main text, with references to the Supplementary Methods when further detail is provided. Table 1 has now been added to the main text and is referenced in this section, which provides all the results from the X-ray microCT analysis that were originally present in Supplementary Data 1. A section has been added following the presentation of the 3D data which interprets these results and their influence on magma permeability (lines 272-309).

The sensitivity analysis of the four characteristic parameters works pretty well, though I would suggest again separating descriptions from interpretations in two different subsections with subtitles.
I think you should consider removing figure 7 as it is difficult to read, deals with non-explosive scenarios rather than explosive (the theme of the paper), and the interpretations are speculative. Instead, I would advocate that you include supplementary figures 5 and 6, as you cite these for primary data and the connect well with the ascent rate section.
We have separated the results of the sensitivity analysis (lines 311-377) and the interpretation of the role of the Forchheimer parameters on conduit dynamics (lines 379-412) into two sections in the results and discussion. Figure 7 has now been moved to the Supplementary Information (now Supplementary Figure 4) and has been replaced by Supplementary Figures 5 and 6, which have been revised to present the results of the explosive solutions.

The magma ascent rate section is a confusing section with a lot of repetition of both itself and the introduction. I think it needs substantial revision and shortening and again a clear definition of parameters and separation of results and interpretations. Please remember to tie interpretations to results and also draw on the whole dataset of the manuscript to avoid speculative interpretations.
This section has now been rewritten (also following annotated comments on the manuscript PDF) to remove repetitions and to make the interpretations more concise (lines 414-486). Several points of comparison between the X-ray microCT and sensitivity analysis results

have also been added to the results and discussion (lines 346-348; lines 367-368; lines 391-393) to better present the whole dataset and support the interpretations in this section.

The final section on initial conditions could also benefit from shortening and more efficient presentation and summary of your interpretations.
This section has also been rewritten and repetitions removed in order to focus on the data presented and our interpretations (lines 488-518). A summary of the X-ray microCT results has also been added to the conclusions (lines 521-525).

5) Not using previously presented sample data to support later interpretations of simulation outcomes. Sometimes is this done implicitly, but it would strengthen the manuscript if this was done consistently and explicitly.
We have now added direct comparisons between the results of the sensitivity analysis and the X-ray microCT analysis to show how the values calculated for the Forchheimer parameters from the natural samples correspond with the ranges from the model which would promote an explosive or non-explosive eruption. This comparison has been added to the sensitivity analysis section of the results and discussion (lines 346-348; lines 367-368; lines 391-393) and also includes references to Figure 5 where both the X-ray microCT and modelling results are presented together.

6) Repeating statements previously outlined in the introduction in the modeling part of the Results and Discussion section. These should be rewritten to ease the flow of this section. There are several annotated comments in the manuscript about this.
We have rewritten the results and discussion section and shortened the part of the discussion which presents the modelling results to remove repetitions (lines 415-486), following the annotated comments in the manuscript PDF.

I have annotated additional minor comments to the manuscript PDF as well.
Please see the responses below to individual comments on the manuscript PDF.

I think with a thorough rewriting of the Results and Discussion and Conclusion and rearranging figures between manuscript and supplementary materials that this work would be a welcome contribution to a very interesting field of research.

COMMENTS ON PDF
Lines 55-56: Perhaps lead with a sentence or two about why that is important to know.
An opening sentence has now been added to the manuscript which explains the connection between volcanic explosivity and the hazard (lines 55-57).

Line 133: You are permitted two levels of sub-headings. It might help the reader to orient themselves quickly if you add the second level indicating e.g. description of results of Plinian samples, interpretation of data or however you might subdivide this section of Results and Discussion.
We agree with the reviewer and the suggestions for restructuring the manuscript and have now rearranged the results and discussion sections. We have reorganised the subheadings so that results and interpretations are presented separately, and so that the descriptions of the samples can be found more easily. The first section of the results and discussion section presents the descriptions of the samples and the X-ray microCT data (lines 143-161), followed by a separate section on how this data provides insight into bubble nucleation and growth processes (lines 163-230). This approach has also been used for connectivity, the throat-pore size ratio and tortuosity (lines 232-270; lines 272-309) and for the results of the sensitivity analysis (lines 311-377; lines 379-412).

Line 133: It is difficult to read this section, because the descriptions of samples and data results are mixed in with the interpretations. I think it would make it easier to follow if your sample and data descriptions were kept separate from the interpretative sentences. Also, only some aspects of sample descriptions are reported for some samples, so that e.g. micro-textural observations are not reported for the Etna fountain samples. I also cannot find a description of the N_d data anywhere – only an interpretation. Please revise so descriptions of results are clear and consistent and add missing data descriptions.

The results and discussion section has now been reorganised, so that the results and sample descriptions are separated from the interpretations (please also see the response to the previous comments). A description of the N_d data has now been added as a clear, separate paragraph (lines 156-161) and further description of the Etna fountain samples at lines 176-178.

Line 134: I suggest you add crystallinity descriptions to this section as well.

Crystallinity descriptions have been added which describe the microtextures observed and ranges in crystal content measured in clasts of the eruptions studied (lines 165-172). As the clasts were examined using X-ray microCT in this study, it was not possible to resolve and study the microlites also present in highly crystallised samples of basaltic Plinian eruptions. Instead, we refer to previous studies of clasts from these eruptions which quantify crystal content to identify which clasts may be crystal-poor or crystal-rich. These clasts are from the same units and represent the textural heterogeneity of the deposit, and in the case of the FL and MTL eruptions, are from the same sampling locality. As we refer to the literature, we have moved this part of the description to the interpretation (the section added at lines 163-230) to clarify that this is not primary data provided by this study.

Line 135: What does FL samples mean? Since the Method section comes after this presentation, it would be helpful to move the sample description into the introduction section just above.

We agree with the reviewer and have now moved the section of the methods section where the FL, MTL and Etna samples are introduced, to the introduction (lines 118-131). As the eruptions studied are now introduced at the beginning of the manuscript, the abbreviations are now explained before they are presented in the results and discussion section.

Line 136: Please include in Figure 1 instead of as a supplementary figure as this is a primary dataset of the publication.

An orthoslice and 3D renderings of a sample of the MTL eruption has now been added to the revised Figure 1. The figure caption has also been updated (lines 893-895). Supplementary Figure 2 has now been integrated with Figure 1 to show how the matrix textures vary with vesicle morphology in further detail.

Line 139: Which?

The samples which show exponential distributions have now been referred to directly in the text (lines 152-153).

Line 141: of..

The sentence has been updated to clarify that it is the vesicle size distributions of the Fontana Lapilli and Etna samples that can be described using power law trends (lines 186-187).

Line 142: the significant micro-textural differences of their ..?

The sentence has been revised to indicate that it is the difference in vesicle morphology between the FL and Etna 122 BC which is discussed (lines 187-188).

Line 143: I think descriptions of crystallinity should be included as primary data and shown in a figure as you use this data for interpretation.

As we cannot resolve the small microlites using X-ray microCT in our clasts we refer to previous literature to categorise our clasts as crystal-poor or crystal-rich, which provide *crystal contents for all of the eruptions examined in this study (please also see the response to the comment above). To clarify this point, we have added crystallinity descriptions of the clasts, with reference to previous studies, in the interpretation section (lines 165-172). We use the descriptions of crystallinity to interpret the variation in vesicle morphology and tortuosity observed in the clasts and not as a primary dataset of this work.*

Line 144: As this is used as a substantial part of the interpretation, I think you need to include at least supplementary figure 1 data in the paper.
As Supplementary Figure 1 shows representative BSE images of clasts from the three eruptions and the textures observed in samples we have chosen to integrate Supplementary Figure 2 with Figure 1 and the main text as this figure shows orthoslices of the same clast and 3D rendering. As the BSE images are used to present representative textures of clasts of the eruption and interpret the causes of variability in vesicle morphology and tortuosity, we have chosen to keep this figure in the Supplementary Information and move all references to the figure and its discussion to the interpretation sections of the discussion. We have also updated the caption for Supplementary Figure 1 in the Supplementary Information to clarify that these are representative BSE images of the clasts from each eruption.

Line 146-147: Well, bubble nucleation and growth likely produced all the vesicles in the samples. In the crystal-poor samples this distribution was not modulated by crystals and coalescence. I suggest rephrasing to something like 'the observed VSD likely reflects bubble nucleation and growth processes' and moving and slightly rewriting the sentence in lines 142-145 to line 149 to avoid misunderstandings and keep descriptions of crystal-poor and crystal-rich clasts together.
We agree with the reviewer and have now updated the paragraph which discusses the VSD and vesicle morphology in the manuscript (lines 179-181).

Line 152: of the VSD
The sentence has been updated (line 151).

Line 152: What about the crystallinity of the Etna fountain samples and does that modify vesicle shapes?
In comparison, the Etna fountain samples have lower crystallinities and also exhibit vesicles with more regular, spherical-subspherical shapes, possibly as there is less of an influence of a crystalline network on vesicle morphology. A sentence has now been added to the manuscript to compare the Etna samples in terms of crystallinity and vesicle morphology (lines 176-178).

Line 155: A VSD
The sentence has been updated (line 189).

Line 162: This sentence is confusing, it is not the distribution types that require timescales, but the processes that generate those distributions. Please rewrite to indicate which processes generate exponential size distributions and how the transition to those requires timescales of hours to minutes.
We agree with the reviewer and have now updated the sentence to specify that the transition to equilibrium conditions, where vesicles are of similar size and distance from each other, takes hours to minutes in basaltic melts and that this transition will be reflected in a mixed power law-exponential VSD (lines 195-197). As most of the Plinian samples do not show mixed VSDs, it is therefore likely that bubble nucleation was rapid, continuous and proceeded under disequilibrium conditions. This information on the bubble nucleation process has also been added to lines 198-201.

Line 164: This also mixes the data fit with the volcanological process. They cannot be used interchangeably. Please rewrite to phrase as the transition to the conduit process and not the resultant data distribution.
We have revised the sentence to highlight that as most of the Plinian samples do not show mixed power law-exponential VSDs, rapid, continuous bubble nucleation likely occurred under disequilibrium conditions (lines 197-199).

Line 165: Also – like what? I can't find your description of those samples anywhere from your own dataset
A sentence has been added earlier in the results section (lines 152-155) which presents the mixed power law-exponential distributions observed in 2 of the samples and a reference to Supplementary Data 1 and Figure 2 where this data can be found.

Lines 166-168: This is what you just described for the power law only. How does that explain a mixed power law-exponential size distribution?
The paragraph presenting the interpretation of the VSD trends has now been revised to present the differences between power law, mixed power law-exponential and exponential VSDs and how they reflect different bubble nucleation and growth processes. A sentence has been added at lines 201-205 which provides an interpretation of mixed power law-exponential VSDs and how this relates to our samples and the interpretation of the VSDs of samples from the paroxysm at Stromboli.

Lines 171-172: Please add a description of the Nd data before you provide this interpretation.
The N_d data for each eruption has now been introduced and described at the beginning of the paragraph (lines 156-157), with reference to Table 1 which presents the N_d data.

Lines 171-172: You need to present the Nd data and how it related to these decompression rates.
This sentence has now been rewritten and expanded upon to discuss in further detail how the decompression rates used in experiments relate to the N_d measured in natural samples. Our calculated N_d are comparable to experimental run-products from decompression experiments performed at 0.078 MPa s^{-1} and are greater than run-products from experiments performed at lower decompression rates. The N_d of experimental run-products have now been discussed in further detail (lines 207-211) with a more detailed comparison with our calculated N_d from natural samples, as our N_d data is now presented at the beginning of this section (see response to comment above).

Line 174: Help the reader by adding an example of such a mechanism (such as ..)
A description of a homogeneous bubble nucleation mechanism has now been added to the manuscript (lines 213-214) and Shea (2017) added to the reference list.

Lines 175-176: How does that fit with your observations of VSD and vesicle morphology?
A sentence has now been added to this paragraph to also present how the VSDs and observations of vesicle morphology also suggest rapid bubble nucleation (lines 216-220).

Line 180: Samples! Samples cannot share similarities with fountain activity, paroxysms and Strombolian.. Please rewrite to indicate that you are comparing samples from different types of activity (eg. Lava fountain and paroxysm samples than samples from Strombolian activity).
We agree with the reviewer and have rewritten the sentence (lines 223-224).

Line 182: Samples!
We have rewritten this sentence (lines 228-230).

Lines 182-183: Is this consistent with vesicle morphology observations of your samples?
A sentence has been added which describes how the morphology of vesicles in our fountain samples suggests that bubble expansion was a more influential process for the lava fountain samples (lines 284-286).

Line 183: bubble
We have updated this sentence (line 286).

Lines 182-183: Again, please present the data and reference a figure before you make interpretations! I suggest leaving this interpretation to the next section, where porosity data is presented.
This sentence has now been moved to the next section where the porosity data is discussed (lines 277-280). The sentence is proceeded by a reference to the revised Figure 4 (previously Supplementary Figure 3) where the porosity data is now presented.

Lines 183-185: So what you are implying here is that large-scale fountains, a Strombolian paroxysm, and Plinian eruptions have similar eruption intensities. I don't think so as that would go against well-established eruption classifications. Please rewrite.
The comparison between N_d and eruption intensity here highlight that eruption styles such as large-scale fountains and Strombolian paroxysms are of higher intensity than more typical lava fountain and Strombolian eruptions. Samples of large-scale lava fountains and paroxysms also show a higher N_d than more typical lava fountain and Strombolian activity (as shown in Figure 4), which further supports the connection between N_d and eruption intensity. We have now rewritten the sentence to specify that we are comparing the higher N_d of large-scale lava fountain and Strombolian paroxysm products with samples of more typical lava fountain and Strombolian activity (lines 228-230).

Line 190: In line 182 you say that large-scale fountains (Etna 2013) tend to show higher porosities – which is it? Also, looking at figure 4, it looks to my eye like they are close to the average porosity of the Plinian samples or at least well within the Plinian range.
For the Etna 2013 large-scale lava fountain sample, it is the case that the porosity is comparable with the porosity of the Plinian samples. However, in general, large-scale lava fountain samples do tend to show larger porosities, which is the case for samples of the high and intermediate height fountains of Kīlauea where porosities can often exceed 0.8 and the maximum measured porosity of the Plinian samples (0.79) (Figure 4). We have now modified this sentence so it is clear that it is a more general statement on the trends in porosity and N_d with eruptive style, with a specific reference to Figure 4a where this data and that of the literature are presented (lines 277-280).

Line 190: Please reference a table/figure that show these values
A reference to Figure 4 and Table 1 has been provided (line 237).

Line 199: Samples! You can only compare porosity and connectivity between samples!
The sentence has been rewritten (line 273).

Line 199: Well… looking at supplementary figure 3 that is the case for the FL samples, but MTL and Etna samples shown no such trend. The literature Strombolian data almost show the opposite trend and literature fountain samples show a wide variability at 60% porosity and then a slight correlation with connectivity at higher porosities. I think you need to reassess this statement and I would encourage you to look into the different trends as they might tell an interesting story.
We have revised the interpretation of the connectivity data and provided further detail on the trends observed for the different eruptive styles (lines 276-281). We have added a sentence also which explains that it is likely the crystal content of the MTL and Etna 122 BC clasts which produces the high connectivity observed in the clasts (lines 276-277). The wide

variability in connectivity for fountain samples with similar porosity may reflect the different clast types erupted during fountain activity, which are then subject to different quench and cooling rates on ejection, leading to variable degrees of post-fragmentation vesiculation. Instead, the opposite trend observed for Strombolian clasts may reflect shrinkage of large bubbles and densification after eruption, decreasing porosity. Namiki et al. (2018) has been added to the reference list.

Line 199: Please add this to figure 4 and include it in the paper as you are using it as primary data in your study

We agree with the reviewer and have now added Supplementary Figure 3 to Figure 4 as a separate panel, in order to present the porosity and connectivity data in the main text. All references to Supplementary Figure 3 in the main text have been updated to Figure 4. The references for the data included in Supplementary Figure 3 (Kawabata et al. (2015) have also been added to the reference list for the main text. The figure 4 caption has also been updated (lines 935-938).

Lines 200-201: This comes back to the problem that you haven't really presented the crystallinity and vesicle morphology data in a systematic way. Please do so and reference figures accordingly here.

The descriptions of vesicle morphology (lines 144-148) and crystallinity (lines 164-172) have now been revised. We have also revised this sentence (lines 273-275) to provide a reference to Figure 1. As we do not provide crystallinity data and use previous literature to categorise the samples as crystal-poor and crystal-rich (please see the responses to previous comments), we use the crystallinity observations to interpret the variations in morphology and tortuosity when comparing the samples. We have therefore moved all the descriptions and discussion of crystallinity to the interpretation sections of the discussion to clarify this point.

Line 203: Please introduce the meaning of this parameter as you did with the connectivity and refer to the methods section for more information

We have moved the explanation of f_{tb} from the Methods to the main text where this parameter is introduced (lines 244-245) and added a reference to the Supplementary Methods section, where f_{tb} is discussed in further detail and the equation used to calculate f_{tb} is provided.

Line 204: This is not used as supplementary data, but as primary data of the study and should be included in the manuscript as a table or figure.

We agree with the reviewer and have now added Table 1 to the manuscript, which includes the data for all of the calculated parameters and will be presented in the main text, not in the Supplementary Information. Table 1 can be added to the manuscript without exceeding the maximum number of display items as Figures 8 and 9 of the original submission have now been merged as a single figure. Porosity, connectivity and bubble number density are also presented in Figure 4. References to Supplementary Data 2 have now been replaced with Table 1 throughout the manuscript and Supplementary Information.

Lines 208-209: What does that mean for the interpretation of your samples?

As a smaller value of f_{tb} would decrease magma permeability, the narrow range in f_{tb} measured across the Plinian and fountain eruptions is consistent with the relationship between magma permeability and explosivity. For the Plinian samples, the low value of f_{tb} would produce low magma permeability, consistent with the highly explosive intensity of these eruptions. The low f_{tb} also measured for the 2013 large-scale lava fountain sample is also consistent with the interpretation that large-scale fountains can result from increased gas-melt coupling. However, as the Etna 2001 lava fountain sample shows a similar f_{tb}, it is also possible that the f_{tb} may be a parameter which has less control on magma permeability and explosivity, compared to parameters such as N_d, which shows a clear relationship with *eruption intensity (Fig. 4). The relative influence of the Forchheimer parameters is also discussed further in the sensitivity analysis section. A sentence has now been added to this paragraph to relate the measured values of f_{tb} with the interpretation of our samples (lines 290-292).*

Line 210: Please define as you did for connectivity
We agree with the reviewer and have moved the definition of tortuosity from the Methods section to the section of the manuscript where the tortuosity data is introduced and described (lines 257-261).

Line 210: Please define as you did for connectivity
The explanation of and equation used to calculate the tortuosity factor has now been moved from the Supplementary Information to the main text (lines 262-267). The references Archie (1942) and Costa (2006) have been added to the reference list of the main text.

Line 210-211: than what?
Further detail has been provided to clarify that there is greater variation in the values of tortuosity for the sample set, compared to the throat-pore size ratio and bubble number density (lines 257-258).

Line 214: Reference to figure or table please. Also I can't find any crystallinity data on your samples anywhere. I think that should be included as a lot of your interpretation relies on it.
We have revised the sentence to refer to Table 1 and Supplementary Data 2. A more detailed description of sample crystallinity has been provided (lines 296-299). However, as we use previous literature to categorise our samples as crystal-poor and crystal-rich, we do not provide crystallinity data (please also see responses to comments above on sample crystallinity). All of the discussion on sample crystallinity and the effect on vesicle morphology and tortuosity has been moved to interpretation sections of the discussion to clarify this point.

Lines 218-219: Is this an interpretation of your samples or a statement from the literature?
The sentence has now been rewritten and the reference moved to clarify which parts of the sentence refer to previous work and which parts are the interpretation of our samples (lines 300-301). The rapid syn-eruptive crystallisation process for the MTL and Etna 122 BC eruptions was investigated by previous works (Arzilli et al., 2019; Bamber et al., 2020), whilst the effect of crystallisation on the development of tortuous pathways and outgassing is an interpretation of the samples presented in this study.

Lines 222-223: Kind of repeating, though expanding on lines 215-217. I suggest merging the two sentences.
We have now merged the two sentences describing the link between crystallisation and tortuosity and how this can reduce magma permeability (lines 296-299).

Line 225: But you just said in line 221 that m increases with explosivity?
We have now updated the sentence (line 307) as the Plinian samples do show higher values of m than less explosive samples.

Line 250: which one?
We have updated the sentence to specify that the model description can be found in the Supplementary Methods section of the Supplementary Information file (line 331).

Line 259: Where is this data shown?
The results of the MTL crystal-poor sensitivity analysis are now presented in Supplementary Figure 2 (line 340). As the <1% of the solutions are explosive, the figure presenting the

results for this sensitivity analysis is presented in the supplementary information and not the main text, as to not exceed the maximum number of display items.

Line 275: Please indicate how that is important for eruption dynamics.
When the magmatic mixture exits the vent, the pressure gradient with ambient pressure will produce a further expansion and acceleration of the magmatic mixture. The fact that for some explosive simulations, the exit pressure is equal to ambient pressure suggests that the gas-ash cloud will not experience a further acceleration, resulting in a lower ash column compared to the overpressurized gas-ash cloud. We have added this explanation to the manuscript at lines 359-361.

Line 290: Delete
'Also' has been deleted (line 381).

Lines 291-292: This your interpretation of the results and not actually shown in the analysis. Please rephrase to avoid confusion between results and interpretations.
We agree with the reviewer and have now separated the sentences to first present the results and then the interpretation (lines 381-383).

Lines 293-294: In a manuscript dealing with highly explosive eruptions, why is this figure then dedicated to the non-explosive solutions and the explosive solutions not shown?
By also investigating the role of the Forchheimer parameters on the occurrence of non-explosive solutions we can further examine the role of parameters such as N_d and f_0 on explosivity. For example, at low N_d and low f_0, non-explosive solutions are more likely, which indicates that low N_d and f_0 are more likely to promote a non-explosive solution than an explosive one, by facilitating gas-melt decoupling. The results of the explosive solutions are instead presented in Figure 5. This figure is also important for defining the two groups of non-explosive solutions present in the Etna 122 BC sensitivity analysis, characterised by different ranges in N_d, f_0 and f_{tb} which highlights the variation in the degree of coupling. However, Figure 7 has now been moved to the supplementary information, also in response to a later comment and is now Supplementary Figure 4. Instead, the figure presenting the Sobol Index has now been added to the main text to better support the discussion. The sentence has now been updated (lines 387-388) instead to present this figure in the supplementary information.

Lines 301-302: I think this interpretation would be a lot stronger if you tied it together with your data on the two fountain types. At least argue why you think this is the case.
We have now added a sentence which highlights the range in values for the Forchheimer parameters for the two fountain types and their differences, particularly for N_d (lines 391-393). A high N_d, low f_0 and f_{tb} would promote gas-melt coupling and high magma ascent rates, which is more consistent with large-scale lava fountain activity.

Lines 303-305: Based on?
Here, we can suggest that a high N_d, low f_0 and low f_{tb} is more likely to promote large-scale lava fountain activity due to the group of non-explosive solutions in Supplementary Figure 4d, which occur at high N_d, low f_0 and low f_{tb}. These parameters would result in a higher degree of gas-melt coupling. According also to the results of La Spina et al. (2021), large-scale lava fountain activity is more likely to occur when low viscosity magma ascends at high rates within the conduit and gas-melt coupling is maintained. We have now added a reference to Supplementary Figure 4 and citation of La Spina et al. (2021) to the sentence in the revised manuscript (lines 395-397) to support our interpretation.

Line 307: Again, you use this as a primary results and so the figure should be included in the manuscript as it does not provide supplementary information.

This Supplementary Figure has now been moved to the main text and replaces the original Figure 7 which was instead moved to the Supplementary Information. To maintain consistency throughout the manuscript and to not exceed the maximum number of display items, this figure has also been revised to present the results of the MTL (25 m) and Etna 122 BC sensitivity analyses for explosive solutions only. Instead, the non-explosive solutions are present in the Supplementary Information (Supplementary Figure 5), alongside the results for the MTL 5 m sensitivity analysis. This allows the main results for the explosive solutions to be presented in the main text, consistent with Figures 5 and 8, which show the explosive solutions for the MTL (25 m) and Etna sensitivity analyses. This paragraph has also been revised to present the results in the order which reflects the revised placement of the figures (lines 399-404 and lines 408-411). The figure caption has also been updated (lines 964-967).

Line 317: Looks like f_{tb} is almost as influential as f_0 – might be good to mention that.
A reference to the role of f_{tb} has also been added (line 408).

Lines 319-320: Why do you think that is?
We have removed this sentence from the manuscript as this comparison is not a main interpretation of our dataset.

Lines 323-335: I think this could be written more succinctly as the current version seems repetitive. It also repeats a lot of the points already made in the introduction section. Please rewrite to hone in on the new data you present in this section.
We have rewritten this paragraph to focus more on the effect of magma ascent rate on outgassing efficiency and also removed the points which are already present in the introduction (lines 415-426).

Lines 334-335: Highlighted
We have rewritten this paragraph to make it more concise (lines 415-426).

Lines 336-338: I think this sentence needs revision. This seems to contradict the statement above about low ascent rates for Strombolian eruptions. Please connect the dots more explicitly and explain how the overlap in porosity, connectivity etc. is a problem for explaining Plinian eruption style unless there is a high decompression and ascent rate out-competing the outgassing rate.
We have rewritten the sentence (lines 423-426) to clarify that the overlap in porosity and connectivity implies that clasts of Plinian and Strombolian eruptions record similar magma permeabilities. What may influence the Plinian case is the high ascent rates which occur during Plinian eruptions which may exceed outgassing rates and restrict outgassing.

Lines 343-345: This has already been presented in association with figure 5 and supplementary figure 5 (just outlined as the numbers of explosive eruptions).
We agree with the reviewer and have now removed this sentence.

Line 348-349: with decreasing r?
We have updated the sentence to clarify that the number of successful simulations decreases with decreasing conduit radius (r) (lines 433-435).

Lines 361-367: Highlighted
We have rewritten the sentence to make it more concise (lines 446-450).

Lines 366-367: Would the Forchheimer parameters be expected to be the same for a highly viscous magma and a low viscosity magma?
No, the Forchheimer parameters would vary between magmas with different viscosities as the Forchheimer parameters strongly depend on the properties of the magma and on how

vesicles nucleate, grow and coalesce during ascent. For example, for a low viscosity magma, once two bubbles coalesce, they are likely to recover the spherical shape quickly, since magma can deform much easier than a highly viscous magma. Therefore, it is more likely to find more tortuous pathways are more likely to be found in high viscosity magmas, rather than in low viscosity magmas. This is in agreement with our findings, i.e. that crystal rich samples (which means higher viscosity magma) show a higher mean tortuosity factor. La Spina et al. (2017) also suggested that the friction coefficient for a basaltic magma is expected to be much lower than that for a more silicic magma. Studies which use X-ray microCT to characterise N_d find that rhyolitic samples of the Kos Plateau Tuff (Degruyter et al., 2010) have a one order of magnitude lower N_d compared to our calculated N_d for clasts of basaltic Plinian eruptions, whilst the more alkaline phonolitic-trachytic Monte Nuovo eruption of Campi Flegrei (Liedl et al., 2019) shows a comparable N_d to our basaltic Plinian clasts. Therefore, it is possible that N_d may reach higher values in lower viscosity magmas.

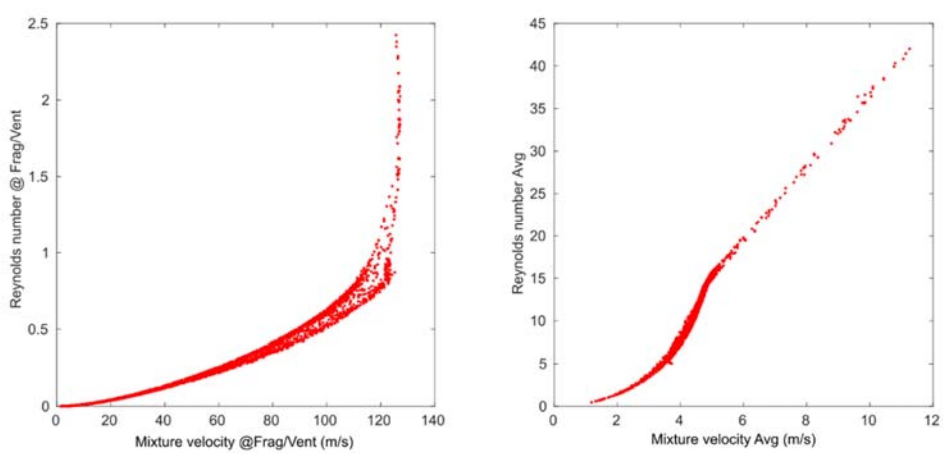
Lines 371-372: How?

Following Eq. 27 in the supplementary materials, the Reynolds number is calculated as

$$Re = \frac{\rho_l u_l 2r}{\mu_l}$$

where ρ_l is the density of the liquid phase (bubble-free magma), u_l is the velocity of the liquid, r is the radius of the conduit and μ_l is the viscosity of the liquid. Given that the density of the liquid is basically constant (bubble-free magma), the Reynolds number of ascent is directly dependent on magma ascent velocity (magma ascent rate) and inversely proportional to the viscosity of the liquid. However, the viscosity of the liquid also affects magma ascent velocity. Lower viscosity, results in a higher ascent velocity, and thus in a higher Reynolds number. With a similar argument we say that a higher viscosity, results in a lower ascent velocity, and thus in a lower Reynolds number.

In order to show the relation between the Reynolds number and the ascent velocity we plotted below the Reynolds number against the exit velocity at fragmentation (or at the vent, if fragmentation is not reached before the vent of the conduit) (left panel), and the average Reynolds number against the average mixture velocity (m/s) resulting from the Etna sensitivity analysis. Similar results are obtained for the MTL sensitivity analyses but over a smaller range in Re. As we can see there is a strong correlation between the Reynolds number and the ascent velocity. Thus, we can use the Reynolds number as a proxy for magma ascent rate. We have added this explanation to the Supplementary Information and a reference in the main text to where this explanation can be found at line 455.



Line 380: Should be expressed as your graph units like in line 378

The sentence has been updated, adding the units for outgassing efficiency (line 463).

Line 380: Please present what the ascent rate is

The ascent rate has been added (line 464).

Lines 380-381: This comes off as pure speculation. Please argue why they cannot represent Plinian or sub-Plinian eruptions.

These simulations most likely represent large-scale lava fountain activity as they exit the vent at high velocities, have higher values of Re and lower outgassing efficiency (low u_{rel}/u_{mix}*). These characteristics have been defined by La Spina et al. (2021) as more representative of large-scale lava fountain activity. Additionally, this group of simulations cannot represent Plinian or sub-Plinian eruptions as they do not fragment within the conduit during magma ascent. The sentence has now been updated to clarify that this group of simulations are non-explosive and do not fragment during magma ascent and are therefore most likely representative of large-scale lava fountain activity (lines 463-465). A reference to La Spina et al. (2021) has also been added.*

Line 384: Why do you not explicitly interpret all three groups in terms of eruption style?

We have interpreted the three groups of simulations (in terms of k_v*) with reference to the eruption style. At* $k_v < 10^{-11}$*, we always find explosive simulations, whereas at* k_v *between* 10^{-11} *and* 10^{-10} *most of the simulations are explosive, with a group of non-explosive simulations at higher outgassing efficiency. Instead at* $k_v > 10^{-10}$ *both explosive and non-explosive simulations occur, which are instead grouped according to the Re (or outgassing efficiency). The characterisation of the different groups of simulations in terms of eruptive style can be found at lines 472-478.*

Line 386: Why use 5m, when the rest has been done at 25 m radius? I think you should be consistent and use 25 m radius instead.

We have updated Figure 8 so now the results of the 25 m radius sensitivity analysis are now presented for consistency. The manuscript has also been updated (lines 469-472) to discuss the results of the 25 m sensitivity analysis. The results for the 5 m conduit radius sensitivity analysis have now been moved to the Supplementary Information and presented in Supplementary Figure 7, to illustrate that the trends are still observed also at a lower conduit radius.

Line 386: Based on what?

As we only observe non-explosive solutions at low ascent rates (low Re) and high outgassing efficiency (u_{rel}/u_{mix}*) for this sensitivity analysis, these solutions are not likely to be representative of large-scale lava fountain activity. We have added this sentence to the manuscript to clarify our interpretation (lines 469-472).*

Line 388: You cannot use Re and ascent rate interchangeably in this description. You need to describe the data distribution as a function of your modelling parameters and then make the interpretation of ascent rate.

As we showed in response to a previous comment, even though the Reynolds number and the ascent rate are not the same parameter, they are strongly correlated, therefore we can use the Reynolds number as a proxy for the ascent rate.

Line 390: What does that mean?

The results of the sensitivity analysis indicate that at high magma permeabilities, the eruptive style is not directly affected by the viscous and inertial permeability, but the ascent rate. This implies that the relative timescales of magma ascent and outgassing needs to be considered in relation to the eruptive style, as explosive solutions are still found at high magma permeabilities. This sentence has now been rewritten (lines 474-478) to clarify this point.

Lines 392-397: Highlighted

We have revised this paragraph to make it more concise (lines 479-486), please also see the response to the comment below.

Lines 398-401: Repeats a lot of the same statements you made in the first paragraphs of this section. Please revised to make more succinct and less repetitive.
We have revised this paragraph to remove repetitions (lines 479-486).

Line 410: reference please
A reference to Bamber et al. (2022) has been provided in the text (line 495).

Line 417: Which ones? I suggest you change the format there to describe how you summarize your interpretations in figure 10 and then describe what the figure shows rather than make statements and use figure 10 as a reference to support those.
The sentence has been revised to specify that it is the results of the sensitivity analysis which indicate that eruptive style is more sensitive to changes in magma storage conditions (lines 502-505), particularly as the MTL crystal-poor and crystal-rich sensitivity analyses show large differences in the frequency of explosive/non-explosive solutions for the same combinations of the Forchheimer parameters. The paragraph has also been revised to change how Figure 10 (now Figure 9) is presented in the main text (lines 505-506).

Line 420: Figure 10 does not actually show your results, but is an illustration of how you interpret the different eruption styles based on your results. Please don't use figure 10 as a reference for this statement.
The reference to figure 10 has been removed.

Lines 422-424: I don't see any arrows pointing from highly explosive eruptions towards the fountains in figure 10. Again the use of the figure reference seems odd for these statements.
The reference to figure 10 has been removed.

Line 433: I find this conclusion section could be improved. You have presented a lot of quantitative data in your study and it would be nice if you could summarize that along with those general conclusions you provide.
We have now rewritten part of the conclusions section to also summarize the data obtained from X-ray microCT and the observations of the 3D pore network for basaltic Plinian clasts (lines 521-525).

Line 436: You actually don't really combine them in the current version of the manuscript. You don't refer to the data of the samples in the numerical modelling section. However, I would really like to see that done and you have a strong dataset!
We agree with the reviewer and have now added a comparison between the results of the sensitivity analysis and the constraints provided by the X-ray microCT analysis of natural samples in the numerical modelling section of the discussion. We have added several points of comparison which show that the values for the Forchheimer parameters measured from natural samples of basaltic Plinian eruptions correspond with the results of the sensitivity analysis (lines 346-348; lines 367-368) and also for lava fountain samples (lines 391-393). We have also added references to specific panels in Figure 5 which combine the results of the sensitivity analysis and the X-ray microCT analysis.

Line 443: But you just said in line 439 that outgassing was inhibited independent of magma permeability because of high ascent rates. Please be more specific.
Here we highlight the role of magma storage conditions on eruptive style and how pre-eruptive temperature and crystallinity can influence syn-eruptive crystallisation and bubble nucleation and consequently form permeable pore networks. We have now revised the sentence (lines 531-533) to specify that magma storage conditions have considerable influence on eruptive style.

Lines 452-453: Where are the FL samples from?

The FL samples are from the same locality as described in Bamber et al. (2022). The sentence has been revised to make it clear that the reference provided in the text refers also to the sampling locality of the FL eruption (lines 539-540).

Lines 461-467: I suggest moving this to the introduction as the Results and Discussion section is meaningless without this information.
We have now moved this paragraph to the introduction (lines 118-131).

Lines 495-500: This should be part of the results presentation as these parameters are meaningless without the definitions.
The explanations of both the tortuosity and throat-pore size ratio have now been moved to the main text where the parameters are first introduced and the results are discussed. The explanation of tortuosity can be found at lines 257-267 and the throat-pore size ratio at lines 244-249.

Lines 510-511: which part of it?
We have added a reference to the Supplementary Methods section of the Supplementary Information where the complete description of the model can be found (lines 580-581).

Line 806: Sometimes figures are shown with free-standing letters and sometimes with letters in parentheses. It's not hugely important, but consistency would be nice.
The figures have now been updated so all figures have free-standing letters and are consistent.

Lines 807-816: This is a really nice figure caption, which also describes the data shown in the figure. It would improve the other captions in the manuscript if you used a similar approach.
The figure captions have now been updated to provide a more detailed description of each figure with more specific references to the separate panels which present this data.

Line 817: Please include a description of the plotted parameters like Nd.
A definition of N_d and how N_d was calculated has been added to the figure caption (lines 905-906).

Line 830: As indicated by interpretive arrows on the plot
We have now clarified the arrows in the figure caption (lines 921-922).

Lines 845-846: I don't understand this sentence. Please elaborate a little more to help those less familiar with this type of graph.
We have added further detail to the caption for Figure 5 (lines 941-943).

Line 874: How can a figure based solely on non-explosive solutions show the relative importance on explosivity? I think you are too ambitious in the number of parameters you are trying to combine here – at least I don't understand how to read this figure. I encourage you to rethink this and perhaps put it in the supplementary data instead, as it doesn't seem essential to your interpretations (if it is – please change the format and description of the figure).
We agree with the reviewer that Figure 7 is better placed in the supplementary information. This figure can now be found in the supplementary information as Supplementary Figure 4. By also presenting the results of the non-explosive solutions, we can also examine the role of N_d and f_0 on the occurrence of non-explosive solutions. This provides further information on which parameters are important controls on explosivity as low N_d and f_0 promote non-explosive solutions, not explosive solutions. Therefore, it is less likely that explosive solutions would be promoted at low N_d and low f_0.

Line 877: Of what?
We have now updated the sentence to clarify that u_{rel} represents the relative velocity between the gas and melt phases and u_{mix} the velocity of the mixture (lines 970-972).

Line 882: I suggest merging with figure 8.
We agree with the reviewer and have now merged Figures 8 and 9 and updated all of the references to these figures throughout the manuscript. In response to a previous comment also on figure 9 (see comment at line 386 and response), panels 8a-d now present the results of the MTL simulations with a conduit radius of 25 m. Panels 8e-h present the results of the Etna 122 BC simulations.

Figure 1: Please add a scale bar for all images
A scale bar has been added for each rendering.

Figure 7: This is a very ambitious figure, and sadly too ambitious for me to read it. The sizes are hard to tell because of all the overlap between points, and you have lost me on how this is supposed to show any relative importance between the parameters. I strongly suggest you find a different way to graph or express this or alternatively delete figure.
This figure shows how the frequency of non-explosive solutions varies with the different input parameters. As there is still a large variation in m and f_{tb} for each panel, the effect of the f_{tb} and m is less important in controlling the outcome of the solution compared to N_d and f_0. Instead, for N_d and f_0, no non-explosive solutions are found above a specific value, for example $N_d = 10^{13}$ m^{-3} for the MTL examples and f_0 exceeding 10^{-1} for the Etna case, so all of the results are explosive. This implies that high N_d and f_0 will result in an explosive solution and thereby there is a strong control of these parameters on explosivity. In response to a previous comment on this figure, this figure has now been moved to the Supplementary Information and Figure 7 replaced with the Sobol Index, so it is no longer present in the main text in the revised manuscript.

Decision letter and referee reports: second round

21st Nov 23

Dear Dr Bamber,

Please allow us to apologise for the delay in sending a decision on your manuscript titled "Outgassing behaviour during highly explosive basaltic eruptions". It has now been seen again by Reviewer #1, whose comments appear below. Reviewer #2 was unfortunately unable to send a second report. In light of the advice we have received, 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 edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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,

Joe Aslin

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

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear Editor and authors,

I am satisfied by the response to comments and changes made by the authors, and recommend this work for publication. Thank you for the opportunity to review this work.

Kind regards,
Mathieu Colombier

Author Responses: second round

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear Editor and authors,

I am satisfied by the response to comments and changes made by the authors, and recommend this work for publication. Thank you for the opportunity to review this work.

Kind regards,
Mathieu Colombier

We would like to thank the reviewer for their thorough and constructive review of our study. The suggested revisions have greatly improved our work.