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Peer Review File

Phreatic eruptions at Whakaari Volcano driven by hydrothermal mineralisation and magmatic gas input

Corresponding Author: Dr Sophie Pearson-Grant

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr Pearson-Grant,

Your manuscript titled "Phreatic eruptions at Whakaari Volcano driven by hydrothermal mineralisation and magmatic gas input" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns. Specifically, a revised manuscript must fully discuss the assumptions adopted for the numerical model by taking into account the actual hydrology at Whakaari volcano with respect to previous conceptual models.

We hope you will find the reviewers' comments useful as you decide how to proceed. If additional work allows you to either incorporate or refute these criticisms, we will be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

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Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Domenico M. Doronzo, PhD
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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of “Phreatic Eruptions at Whakaari Volcano Driven by Hydrothermal Mineralisation and Magmatic Gas Input” by Pearson-Grant et al.

This manuscript addresses important questions regarding the mechanisms driving phreatic eruptions at Whakaari volcano. The authors integrate numerical modeling of multi-phase flow in porous rock with field-based constraints on rock properties (permeability, rock strength) to assess how dynamic changes in magmatic fluid input affect near-surface fluid pressurization and rock failure. The primary conclusion is that significant pressurization in excess of rock failure criteria near the surface (5 MPa, corresponding to the tensile strength of the rock) requires the conduit to be sealed on the sides by rock with a permeability <10-14 m2. Lower seal permeability (on the top or sides of the conduit) or higher rates of magmatic fluid input promotes more rapid pressurization and rock failure.

The author's approach to integrating numerical modeling with field-based constraints on rock properties is commendable, and I agree that this approach presents a promising pathway to understand the dynamics of these systems. However, I am concerned that the model, as presented, fails to account for fundamental aspects of the hydrology of the Whakaari system, and starkly disagrees with previous conceptual models of the system. On a general level, the model (at least in it's starting state prior to enhanced magmatic degassing) appears to more closely resemble a near-hydrostatically pressured liquid-dominated geothermal system, rather than a magma-hydrothermal system rapidly transporting magmatic gas within a volcanic conduit to the near-surface, with variable interactions with meteoric fluids close to the surface. This aspect of the model, along with several other points, require clarification or reconsideration to strengthen the scientific rigor and realism of the results.

1. Model Hydrology

The authors use Waiwera to simulate multi-phase flow in the Whakaari volcanic system. This software represents the “state-of-the-art” for modeling geothermal reservoirs, but is less commonly used to model magma-hydrothermal interactions in volcanic systems due to current temperature restrictions mentioned by the authors in the Methods section (<350 °C).

To work within the limitations of Waiwera, the authors specify an input fluid enthalpy of 1600 kJ/kg at the base of the conduit at 2 km depth (“Conduit mass enthalpy”, Extended Data Table 2). This influx acts as the primary heat source for the system, and the mass input rate at the bottom boundary is varied between 5-500 kg/s, generating the pressure changes in the shallower part of the system. Throughout the manuscript, this input fluid is referred to "magmatic gas". However, in my opinion, this is a problematic aspect of this study. For the simulated near-hydrostatic pressures at the base of the model near 2 km depth (~20 MPa, Fig. 1b), this enthalpy corresponds to vapor-saturated or slightly sub-cooled liquid water at ~300-350°C; this is clearly a liquid, not a gas. Real magmatic gases likely have much higher enthalpies (>3000 kJ/kg), reflecting magmatic temperatures. The rapid transport of these gases to the surface along with minimal/variable interaction with meteoric fluids can result in superheated fumaroles with temperatures up to 800 °C as observed in Whakaari (e.g., Giggenbach, 1987).

This fundamental discrepancy between the liquid-dominated fluid state inside of the conduit simulated by the authors and previous conceptual models of Whakaari that imply a single-phase vapor conditions inside of the conduit is concerning. Although this discrepancy is not addressed in the manuscript, the presence of liquid-dominated conditions in the model is reflected by the initial fluid pressure (Fig. 1b) and temperature distribution (Fig. 1c, seemingly close to boiling point with depth), as well as an initial vapor saturation of ~0.3 below the top seal (Figure 3c) for the model conditions presented in Figure 3. The latter represents a relatively low vapor fraction on the order of a few percent by mass; this does not correspond to vapor-dominated conditions. Rather than magmatic gas rapidly ascending towards the surface within the conduit, I expect that this vapor fraction is generated by near-isenthalpic decompression boiling of the input fluid with an enthalpy of 1600 kJ/kg.

In addition to the presence of fumaroles with temperatures far in excess of the boiling point, evidence for single-phase vapor conditions throughout most of the central conduit at Whakaari includes geochemical signatures at the surface consistent with magmatic degassing, such as significant SO2 emissions (Werner et al., 2008) and the geochemistry of the crater lake being consistent with magmatic degassing followed by variable scrubbing of the magma-derived acid volatiles (Christenson et al., 2017). If there was truly a 2 km thick column of liquid water that the magmatic gas had to pass through before reaching the surface, these signatures would be relatively muted at the surface compared to what is observed. Instead, most conceptual models for Whakaari suggest that magmatic degassing generates a predominantly single-phase vapor conduit, and that this vapor variably interacts with meteoric water in the upper ~100 m below the surface (Christenson et al., 2017; Kilgour et al., 2021). While the vapor conduit is surrounded by a liquid-dominated system, within the conduit there is vapor that is partially isolated from the surrounding liquid-dominated system by low permeability rock (the “side seal” considered by the authors).

This fundamental difference in fluid state within the conduit likely has implications for overpressure generation mechanisms and timescales, which is an important aspect of this study. Due to differences in phase relations and compressibility, a single-phase vapor conduit will likely respond in a fundamentally different manner in comparison to a boiling liquid-dominated system (see point 2 below).

I can appreciate that there are restrictions imposed by numerical simulation software, as well as the general difficulty of modeling single phase vapor conditions inside of volcanic conduits. However, this fundamental conceptual difference between the presented model and previous studies of Whakaari deserves much more attention in the manuscript. In addition to the bottom boundary condition that results in a liquid-dominated state within the conduit, the top boundary condition of a fixed temperature (15 °C) likely promotes very strong cooling and liquid condensation near the surface, with possible implications for the assumed mechanism of overpressure generation.

Christenson, B.W., White, S., Britten, K., Scott, B.J., 2017. Hydrological evolution and chemical structure of a hyper-acidic spring-lake system on Whakaari/White Island, NZ. Journal of Volcanology and Geothermal Research 346, 180–211. https://doi.org/10.1016/j.jvolgeores.2017.06.017

Giggenbach, W.F., 1987. Redox processes governing the chemistry of fumarolic gas discharges from White Island, New Zealand. Applied Geochemistry 2, 143–161. https://doi.org/10.1016/0883-2927(87)90030-8
Kilgour, G., Kennedy, B., Scott, B., Christenson, B., Jolly, A., Asher, C., Rosenberg, M., Saunders, K., 2021. Whakaari/White Island: a review of New Zealand’s most active volcano. New Zealand Journal of Geology and Geophysics 64, 273–295. https://doi.org/10.1080/00288306.2021.1918186

Werner, C., Hurst, T., Scott, B., Sherburn, S., Christenson, B.W., Britten, K., Cole-Baker, J., Mullan, B., 2008. Variability of passive gas emissions, seismicity, and deformation during crater lake growth at White Island Volcano, New Zealand, 2002–2006. J. Geophys. Res. 113, 2007JB005094. https://doi.org/10.1029/2007JB005094

2. Dynamic Condensation and Overpressure Generation

While the condensation-induced overpressure mechanism seems conceptually possible, its dynamics are likely different if the initial condition is superheated/supercritical vapor rather than boiling water. Starting from a vapor-dominated state could substantially change the rate and spatial distribution of overpressure generation, which merits detailed examination.

Several critical issues result if the conduit is mainly occupied by single-phase vapor, rather than a two-phase vapor-liquid:
- Pressure changes resulting from variations in fluid input are partially governed by fluid compressibility. There is a strong compressibility contrast between single-phase liquid, single-phase vapor, and two-phase liquid/vapor (boiling water). While vapor is more compressible than liquid, the compressibility of boiling water is enhanced by several (>2x) orders of magnitude compared to single-phase liquid or vapor, due to the possibility of phase changes in response to pressure variations (liquid->vapor in response to pressure decrease or vapor->liquid in response to pressure increase). Due to the several orders of magnitude higher compressibility of boiling water compared to vapor, pressure pulses and overpressure

generation due to enhanced fluid input would behave fundamentally differently. In particular, in boiling systems, pressure changes propagate more slowly and with greater damping, while a conduit occupied by single-phase vapor may transmit pressure pulses more rapidly with less damping. While this requires further study to fully understand, the basic point is that the authors' overpressure buildup rates and spatial patterns may not be representative of the actual Whakaari system, if the conduit is occupied by vapor rather than a two-phase fluid. Moreover, liquid condensation may not be as important to overpressure generation.
- If condensation-induced overpressure is important as the authors propose, it is difficult to envision how liquid would condense out of a superheated vapor with enthalpy >3000 kJ/kg solely in response to pressurization resulting from enhanced magmatic degassing. Liquid condensation would seem to require some degree of temperature decrease (cooling). Lateral heat loss from the conduit to surrounding rocks, caused by permeability enhancement and meteoric water circulation, could cause rapid liquid condensation in the conduit. This mechanism was not considered by the authors but might be worth considering.
- The role of non-condensable gases (CO_2, SO_2) in overpressure generation should also be considered, as these would be more difficult to condense, particularly if starting from a very high temperature vapor. Fluids with significant concentrations of these components, such as magmatic gas, may buffer pressure changes differently than pure water.

3. Permeability Sensitivity Analysis

The sensitivity analysis presented in the manuscript focuses predominantly on static seal permeabilities and mass flux rates at the bottom boundary. The conclusion that low permeability is necessary for significant pressurization is consistent with previous numerical and conceptual models but provides limited novel insight. The finding that the conduit must be sealed by relatively low permeability rock ($<10^{-14}$ m^2) for substantial pressurization is relatively predictable given established understanding of advective flow processes.

In my opinion, it would be valuable to explore dynamic permeability changes, although this may create a “chicken and egg” relationship when looking at interplay between pressurization, rock failure and permeability. For example, the authors do not consider the potential for sudden permeability increases at depth due to deeper failure events. If overpressure buildup causes rock failure and fracturing at depth, this could create new high-permeability pathways that enhance transport of magmatic gas and change fluid conditions in the conduit. Similarly, permeability enhancement at the surface may rapidly depressurize the system. This might represent a feedback mechanism of sorts, where the overpressure generation process may be self-limiting through permeability enhancement; in general, the relationship between pressure buildup and permeability evolution deserves a closer look, as this could have implications for eruption timing and magnitude.

4. Definition and Use of "Overpressure"

The term "overpressure" is used ambiguously throughout the manuscript. Typically, overpressure is defined relative to a baseline hydrostatic (or vapor-static) equilibrium, i.e. overpressure = actual fluid pressure – hydrostatic fluid pressure. Figure 2a (left column) seems to indicate zero overpressure during initial conditions, which could be misleading since constant upward fluid flux necessitates at least a small positive vertical pressure gradient above hydrostatic. Given the authors' chosen terminology and model setup, the manuscript should clarify precisely how overpressure is computed and its reference state.

5. Failure Criterion, Connection to Brittle Shear Failure and Integration of Rock Properties

The failure criterion used (fluid pressure exceeding tensile strength by 5 MPa) should be explicitly linked to standard brittle shear failure frameworks (e.g., Cox, 2010) and the assumption of a critically-stressed crust. Although detailed studies of rock properties have been performed at Whakaari, as cited in the manuscript, the level at which this data is integrated into the model is limited. The authors should better clarify what is the advantage of collecting and measuring all of this detailed site-specific rock property data, when it appears that relatively rough approximations of these properties are sufficient for the purposes of these kinds of numerical models.

Cox, S.F., 2010. The application of failure mode diagrams for exploring the roles of fluid pressure and stress states in controlling styles of fracture-controlled permeability enhancement in faults and shear zones. Geofluids 10, 217–233. https://doi.org/10.1111/j.1468-8123.2010.00281.x

In conclusion, while this study has the potential to make a valuable contribution to understanding phreatic eruption mechanisms, addressing the fundamental disconnect between the modeled liquid-dominated hydrology and conceptual models (and field evidence) implying vapor conditions inside the conduit is essential. As a result, this manuscript requires substantial revision to address the identified concerns.

Reviewer #2 (Remarks to the Author):

My comments and suggestions are in an attached document.

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Version 1:

Decision Letter:

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Dear Dr Pearson-Grant,

Your revised manuscript titled "Phreatic eruptions at Whakaari Volcano driven by hydrothermal mineralisation and magmatic gas input" has now been seen by our 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.

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.

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

Best regards,

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Alireza Bahadori, PhD
Senior Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have substantially revised the paper in ways that address the major conceptual issues from the first round. Most importantly, they re-ran the models with a supercritical version of Waiwera to represent a vapor-dominated volcanic conduit. While there have been several other studies that have simulated vapor-dominated geothermal systems at temperatures <300 C (e.g. Ingebritsen and Sorey, 1988, JGR Solid Earth; Raharjo et al. 2016, Geothermics) to my knowledge this is the first study to simulate a volcanic conduit containing superheated vapor, which is a substantial accomplishment.

As a result of the changed model set-up, they removed the condensation-induced overpressure mechanism, which was previously a major part of the initial submission. The core message now appears to be that low-permeability seals can trap rising magmatic gases and reach a tensile-failure overpressure within weeks or faster if fluxes of magmatic gases are high. This is a clear and useful scientific message of substantial value to the broader volcanological community.

Below are a few specific suggestions to further tighten the manuscript:

- As you note in the manuscript and rebuttal letter, you assume that failure occurs when fluid pressure exceeds the confining pressure + tensile strength of an intact seal. Please state explicitly that this is a tensile failure criterion and not a shear failure criterion for critically stressed faults. Critically stressed rock with pre-existing faults/fractures can fail by shear reactivation at fluid pressures well below the confining pressure; is there evidence of rock with pre-existing fractures at Whakaari that could fail in this manner? A one- or two-sentence contrast will help readers better understand how this assumed failure mechanism contrasts with the failure of critically stressed rock.

- Regarding the conclusion that "boiling is not necessary" to generate phreatic eruptions (lines 169-173), this may be an important, generalizable point: in a vapor-dominated conduit, you can reach failure without boiling/flashing. I think it would be worthwhile to emphasize why - low vapor compressibility? Also, is this a Whakaari-specific observation, or could you delineate under what conditions boiling might be more important?

- In scaling magmatic gas fluxes from SO_2 measurements, you report gas composition ~59% water, 28% CO_2, 10% SO_2. It should be clearly stated that these percentages are by mass (please double check) - it is common in the volcanic gas community to report gas composition in terms of mole %, not mass %.

- In the abstract, you mention "increased magmatic gas flux", but "increased" is always relative. I suggest to add "relative to background fluxes".

- It might be worthwhile to spell out the practical monitoring implications. Is it true, that most of the time, partial openness of either the top or side seals keeps overpressure below failure? In that case, short lead-time failures would require "temporarily" tight top and side seals or elevated magmatic gas input? Something like this might help communicate if or why (under what conditions) low permeability zones are critical, and why many volcanoes such as Whakaari can pass long time intervals without eruptions.

Overall, I enjoyed reading the revision and look forward to seeing the published text after minor revisions.

Reviewer #2 (Remarks to the Author):

I think that the authors did a fine job responding to the comments and suggestions provided by both reviewers to the original manuscript. The revised manuscript address many of the comments, but I think that more clarifications are needed to make the conclusions more robust.

** Responding to reviewer #1, the authors write “We reduced the vertical model dimension to 1 km rather than 2 km to reduce the occurrence of supercritical conditions that Waiwera could not simulate.” However, the existence of SO2 rather than H2S in the emitted gas (described in the paragraph starting in line 86) suggests temperatures that are most likely above the critical temperature of pure water. This is inconsistent with the statements in the new assumptions section (lines 275-278) “In most of the conduit, magmatic gases and steam would be superheated, although at the base of the model CO2 and SO2 would be supercritical but steam would not.” And then “we believe that using steam as a proxy for the combined hydrothermal fluids is a reasonable assumption.” This discrepancy and better descriptions of thermodynamic properties of high-temperature aqueous-rich fluids should be at least mentioned.

** The terms “steam” and gas seem to be used interchangeably in the title (“magmatic gas input”) and the text. The paragraph starting in line 86 describes fluxes of noncondensable gases (CO2 and SO2). The text should probably minimize the use of the word “gases” as other than water they are not incorporated in the simulations. For example, in lines 141-142 “As heated groundwater and magmatic gases rise from below, overpressure builds near the surface (Figure 2).” This is misleading or at least confusing because groundwater and gases suggest to me that gases other than water are also modeled, but they are not.

** Following the sentence in lines 33-34 “highlighting the importance of monitoring hydrothermal systems and understanding the underlying physical processes”, I suggest citing the paper by Poland et al. (GRL 2025) which provides one of the first examples of a successful detection of an explosion based on monitoring of hydrothermal activity.

** The assumption described in lines 35-36 and depicted in Figure 1 is that only the conduit is sealed. That is, a small narrow area within the hydrothermal system is sealed rather than a broader hydrothermal reservoir as depicted in most models of hydrothermal explosions/phreatic eruptions. For example, in Browne & Lawless (ESR 2001) or more recently in Harrison et al. (Eos 2025) - https://eos.org/features/hydrothermal-hazards-on-display-in-yellowstone-national-park. Based on the volume of ejected ballistics in past explosions, is it reasonable to assume that the seal was confined to the conduit? In any case, a sentence or two explaining this aspect of the conceptual model would be helpful.

** I don’t think that the range of plausible controlling parameters was explored enough (see below) and suggest looking into section 6 in Ingebritsen et al (RoG 2010) for a more in-depth discussion on common assumptions and simplifications in such models of hydrothermal flow. I also think that logically, it makes more sense to describe the major assumptions before describing simulation results.

** Several studies have shown that the effects of permeability anisotropy, especially in volcanic terrain could be significant (section 6.10 in Ingebritsen et al., RoG 2010). Starting in line 241 “In the anisotropic permeability case, permeability in the conduit was reduced to one tenth in the lateral directions, and host-rock permeability was one tenth in the vertical direction. In these models, time to eruption is typically less (Figure 6) and maximum overpressure is greater.” There is really no justification for choosing these arbitrary anisotropy values except for citing another modeling paper. I suggest expanding the sensitivity analysis of isotropic permeability. For example, using a value of 10^-15 m^2, examine how do the final overpressure and time to failure change where when horizontal permeability is 1 and 2 orders of magnitude larger or smaller than the vertical permeability for the host rock and the conduit? Results from this analysis could be an extension to Figure 4. My assumption is that the effect might be quite significant.

** In the methods section there is a description of capillary pressure and relative permeability functions and values but no sensitivity of the actual functon(Corey vs. Linear relative permeability, for example) is presented, despite the relatively large effects. I suggest looking at sections 6.5 & 6.6 in Ingebritsen et al (RoG 2010) for a more in-depth discussion on relative permeability and capillary pressure in hydrothermal flow models.

** Following the description of the “seal” in lines 215-216 It would be useful to provide a short description of the mineral modality of the breccia. How much alunite is in the rock and is this volume enough to be the “weak link”? In the rebuttal, the authors write “There are few, if any, studies on rates of alunite precipitation in volcanic environments, but recent unpublished experiments suggest it can be very fast” and then “seal formation timescales may only minimally impact our timescales for pressurization”. This seems to contradict the statement in lines 206-207 “timescales are days to weeks depending on the permeabilities of the seals.” I suggest better describing the relation between alunite precipitation rates, time-dependent permeability changes, and overpressure timescales. Also, conceptually, what is “time zero” in which there is no alunite in the conduit?

** The word “monitoring is mentioned many times throughout the text. Lines 303-306 state “Comparing model outputs to monitoring data can test and refine 303 qualitative interpretations of subsurface activity from data; temperatures and gas fluxes can be modelled directly, while deformation, seismicity, and gravity signals can be calculated from modelled pore pressures.” This entire section starting in line 297 suggests that modeling results can inform monitoring, but it seems that the association is vague. I think that more specific assertions are needed. For example, from this study’s simulation results, what kind of specific recommendations could be made to improve the monitoring system in Whakaari/White Island? New types of monitoring instruments? More dense seismometers? An improved geodetic network? Additional or more frequent satellite-based observations?

** I think that the statement in lines 330-332 “Modelling the hydrothermal system at Whakaari has allowed us to establish a clear relationship between hydrothermal mineralisation, magmatic degassing rates, and the occurrence and timing of phreatic eruptions” is exaggerated. I suggest something like “based on a range of simplifying assumptions, model results suggest that relation between hydrothermal mineralization and magmatic degassing rates have a strong control on the occurrence and timing of phreatic eruptions."

** I suggest removing the word "believe" which appears in lines 252 & 277 as it should not be used in a scientific paper. Rather words such as conclude, infer, or a similar word could be used.

I hope the authors find the suggestions useful. I am looking forward to seeing the revised manuscript published.

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Dear Dr Pearson-Grant,

Your manuscript titled "Phreatic eruptions at Whakaari Volcano driven by hydrothermal mineralisation and magmatic gas input" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns. Specifically, a revised manuscript must fully discuss the assumptions adopted for the numerical model by taking into account the actual hydrology at Whakaari volcano with respect to previous conceptual models.

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When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

Best regards,

Domenico M. Doronzo, PhD
Editorial Board Member
Communications Earth & Environment

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

Dear Dr. Doronzo and Dr. Bahadori,

We'd first like to thank you and the reviewers for providing comments that have helped us to improve our manuscript. The comments of both reviewers are provided in our rebuttal letter (in grey italics) and our responses are provided below each comment (in black). We have made substantial changes to our manuscript, including re-running all models and providing new text that discusses the model assumptions. We believe that the manuscript is now much stronger as a result, and we thank the reviewers for their valuable suggestions. We hope that you will consider our substantially revised and improved manuscript for publication in Communications Earth & Environment.

Yours sincerely,

Dr. Sophie Pearson-Grant (on behalf of all the authors)

Reviewer 1 (anonymous)

We thank Reviewer 1 for their insightful and thoughtful review. We acknowledge that our models did not replicate the conceptual models of a hydrothermal system fed by magmatic gases, and have rerun all the models with higher temperatures and pressure to address this. This concern and the others raised by the reviewer are discussed in more detail below. We believe that we have addressed all the concerns, and that the manuscript is much stronger because of it.

1. Model Hydrology

The authors use Waiwera to simulate multi-phase flow in the Whakaari volcanic system. This software represents the "state-of-the-art" for modeling geothermal reservoirs, but is less commonly used to model magma-hydrothermal interactions in volcanic systems due to current temperature restrictions mentioned by the authors in the Methods section (<350 °C).

To work within the limitations of Waiwera, the authors specify an input fluid enthalpy of 1600 kJ/kg at the base of the conduit at 2 km depth ("Conduit mass enthalpy", Extended Data Table 2). This influx acts as the primary heat source for the system, and the mass input rate at the bottom boundary is varied between 5-500 kg/s, generating the pressure changes in the shallower part of the system. Throughout the manuscript, this input fluid is referred to "magmatic gas". However, in my opinion, this is a problematic aspect of this study. For the simulated near-hydrostatic pressures at the base of the model near 2 km depth (~20 MPa, Fig. 1b), this enthalpy corresponds to vapor-saturated or slightly sub-cooled liquid water at ~300-350°C; this is clearly a liquid, not a gas. Real magmatic gases likely have much higher enthalpies (>3000 kJ/kg), reflecting magmatic temperatures. The rapid transport of these gases to the surface along with minimal/variable interaction with meteoric fluids can result in superheated fumaroles with temperatures up to 800 °C as observed in Whakaari (e.g., Giggenbach, 1987).

This fundamental discrepancy between the liquid-dominated fluid state inside of the conduit simulated by the authors and previous conceptual models of Whakaari that imply a single-phase vapor conditions inside of the conduit is concerning. Although this discrepancy is not addressed in the manuscript, the presence of liquid-dominated conditions in the model is reflected by the initial fluid pressure (Fig. 1b) and temperature distribution (Fig. 1c, seemingly close to boiling point with depth), as well as an initial vapor saturation of ~0.3 below the top seal (Figure 3c) for the model conditions presented in Figure 3. The latter represents a relatively low vapor fraction on the order of a few percent by mass; this does not correspond to vapor-dominated conditions. Rather than magmatic gas rapidly ascending towards the surface within the conduit, I expect that this vapor fraction is generated by near-isenthalpic decompression boiling of the input fluid with an enthalpy of 1600 kJ/kg.

In addition to the presence of fumaroles with temperatures far in excess of the boiling point, evidence for single-phase vapor conditions throughout most of the central conduit at Whakaari includes geochemical signatures at the surface consistent with magmatic degassing, such as significant SO2 emissions (Werner et al., 2008) and the geochemistry of the crater lake being consistent with magmatic degassing followed by variable scrubbing of the magma-derived acid volatiles (Christenson et al., 2017). If there was truly a 2 km thick column of liquid water that the magmatic gas had to pass through before reaching the surface, these signatures would be relatively muted at the surface compared to what is observed. Instead, most conceptual models for Whakaari suggest that magmatic degassing generates a predominantly single-phase vapor conduit, and that this vapor variably interacts with meteoric water in the upper ~100 m below the surface (Christenson et al., 2017; Kilgour et al., 2021). While the vapor conduit is surrounded by a liquid-dominated system, within the conduit there is vapor that is partially isolated from the surrounding liquid-dominated system by low permeability rock (the "side seal" considered by the authors).

This fundamental difference in fluid state within the conduit likely has implications for overpressure generation mechanisms and timescales, which is an important aspect of this study. Due to differences in phase relations and compressibility, a single-phase vapor conduit will likely respond in a *fundamentally different manner in comparison to a boiling liquid-dominated system (see point 2 below).*

I can appreciate that there are restrictions imposed by numerical simulation software, as well as the general difficulty of modeling single phase vapor conditions inside of volcanic conduits. However, this fundamental conceptual difference between the presented model and previous studies of Whakaari deserves much more attention in the manuscript. In addition to the bottom boundary condition that results in a liquid-dominated state within the conduit, the top boundary condition of a fixed temperature (15 °C) likely promotes very strong cooling and liquid condensation near the surface, with possible implications for the assumed mechanism of overpressure generation.

We thank the reviewer for pointing out that the enthalpy we were using in our models corresponds to 350 °C liquid water rather than steam as observed at Whakaari. We reduced the vertical model dimension to 1 km rather than 2 km to reduce the occurrence of supercritical conditions that Waiwera could not simulate. However, temperatures were still too low near the surface and fluids in the conduit were not vapour dominated. We therefore transitioned the model to the new supercritical version of Waiwera that will be publicly released soon. This allowed us to have temperatures of up to 800 °C and a single-phase vapour conduit (see Figure 3 and Extended Data Figure 4-2) which is much more representative of the hydrothermal system at Whakaari. We have updated all figure results and the text where appropriate to reflect these new models and their results.

Regarding the top boundary condition of 15 °C, this is within atmosphere blocks with very large volume and so there is minimal fluid exchange unless driven by pressure gradients in the blocks below. In the hot areas within the conduit, pressure is driving the fluids upwards from the rocks to the atmosphere and so there is minimal cooling and condensation in the subsurface. This is a widely implemented boundary condition for hydrothermal models (Pruess et al. 1991, Croucher et al. 2020).

2. Dynamic Condensation and Overpressure Generation

While the condensation-induced overpressure mechanism seems conceptually possible, its dynamics are likely different if the initial condition is superheated/supercritical vapor rather than boiling water. Starting from a vapor-dominated state could substantially change the rate and spatial distribution of overpressure generation, which merits detailed examination.

Several critical issues result if the conduit is mainly occupied by single-phase vapor, rather than a two-phase vapor-liquid:

- Pressure changes resulting from variations in fluid input are partially governed by fluid compressibility. There is a strong compressibility contrast between single-phase liquid, single-phase vapor, and two-phase liquid/vapor (boiling water). While vapor is more compressible than liquid, the compressibility of boiling water is enhanced by several (>2x) orders of magnitude compared to single-phase liquid or vapor, due to the possibility of phase changes in response to pressure variations (liquid->vapor in response to pressure decrease or vapor->liquid in response to pressure increase). Due to the several orders of magnitude higher compressibility of boiling water compared to vapor, pressure pulses and overpressure generation due to enhanced fluid input would behave fundamentally differently. In particular, in boiling systems, pressure changes propagate more slowly and with greater damping, while a conduit occupied by single-phase vapor may transmit pressure pulses more rapidly with less damping. While this requires further study to fully understand, the basic point is that the authors' overpressure buildup rates and spatial patterns may not be representative of the actual Whakaari system, if the conduit is occupied by vapor rather than a two-phase fluid. Moreover, liquid condensation may not be as important to overpressure generation.

- If condensation-induced overpressure is important as the authors propose, it is difficult to envision how liquid would condense out of a superheated vapor with enthalpy >3000 kJ/kg solely in response to pressurization resulting from enhanced magmatic degassing. Liquid condensation would seem to

require some degree of temperature decrease (cooling). Lateral heat loss from the conduit to surrounding rocks, caused by permeability enhancement and meteoric water circulation, could cause rapid liquid condensation in the conduit. This mechanism was not considered by the authors but might be worth considering.

- The role of non-condensable gases (CO_2, SO_2) in overpressure generation should also be considered, as these would be more difficult to condense, particularly if starting from a very high temperature vapor. Fluids with significant concentrations of these components, such as magmatic gas, may buffer pressure changes differently than pure water.

Given our revised vapour-dominated models, the proposed mechanism of dynamic condensation to generate increased pressure is no longer relevant as the reviewer points out. We have therefore removed reference to it in our manuscript.

3. Permeability Sensitivity Analysis

The sensitivity analysis presented in the manuscript focuses predominantly on static seal permeabilities and mass flux rates at the bottom boundary. The conclusion that low permeability is necessary for significant pressurization is consistent with previous numerical and conceptual models but provides limited novel insight. The finding that the conduit must be sealed by relatively low permeability rock (<10^-14 m2) for substantial pressurization is relatively predictable given established understanding of advective flow processes.

In my opinion, it would be valuable to explore dynamic permeability changes, although this may create a "chicken and egg" relationship when looking at interplay between pressurization, rock failure and permeability. For example, the authors do not consider the potential for sudden permeability increases at depth due to deeper failure events. If overpressure buildup causes rock failure and fracturing at depth, this could create new high-permeability pathways that enhance transport of magmatic gas and change fluid conditions in the conduit. Similarly, permeability enhancement at the surface may rapidly depressurize the system. This might represent a feedback mechanism of sorts, where the overpressure generation process may be self-limiting through permeability enhancement; in general, the relationship between pressure buildup and permeability evolution deserves a closer look, as this could have implications for eruption timing and magnitude.

Exploring the effects of dynamic permeability would be a valuable and interesting direction for future work, but needs coupled thermal-hydraulic-mechanical modelling which is beyond the scope of this paper. Whilst we acknowledge that our results are broadly consistent with previous work, this work is the first attempt to model and quantify the processes leading to phreatic eruptions that we are aware of. We argue that our work provides valuable insights such as the fact that fairly minor reductions in permeability can result in pressure increasing to the failure threshold, and that increased magmatic gas input from depth is not necessary if the system is sealed at the top and sides.

4. Definition and Use of "Overpressure"

The term "overpressure" is used ambiguously throughout the manuscript. Typically, overpressure is defined relative to a baseline hydrostatic (or vapor-static) equilibrium, i.e. overpressure = actual fluid pressure – hydrostatic fluid pressure. Figure 2a (left column) seems to indicate zero overpressure during initial conditions, which could be misleading since constant upward fluid flux necessitates at least a small positive vertical pressure gradient above hydrostatic. Given the authors' chosen terminology and model setup, the manuscript should clarify precisely how overpressure is computed and its reference state.

We have clarified in the methods and main body of the text that we define overpressure as the difference between pore fluid pressure and confining pressure of overlying gas-saturated rock. As clarified in the methods, we have taken the reference state for confining pressure from the initial conditions as calculated by Waiwera, so that temperature- and pressuredependence of fluid density within confining rock is accounted for. The small overpressure driving upward gas movement through the open conduit is therefore neglected but would be on the order of kPa rather than the MPa we are interested in.

5. Failure Criterion, Connection to Brittle Shear Failure and Integration of Rock Properties

The failure criterion used (fluid pressure exceeding tensile strength by 5 MPa) should be explicitly linked to standard brittle shear failure frameworks (e.g., Cox, 2010) and the assumption of a critically-stressed crust. Although detailed studies of rock properties have been performed at Whakaari, as cited in the manuscript, the level at which this data is integrated into the model is limited. The authors should better clarify what is the advantage of collecting and measuring all of this detailed site-specific rock property data, when it appears that relatively rough approximations of these properties are sufficient for the purposes of these kinds of numerical models.

This is an interesting comment. In our models, we envisage the tensile (extension) failure of an intact seal. In the new assumptions and their implications section suggested by Reviewer 2, we have added 'For failure to occur, we envisage the tensile failure of an intact seal when pore pressure is greater than the combined confining pressure of overlying gas-saturated rock and tensile strength of the sealing rock. Progressive failure or brittle shear failure (Cox, 2010) could result in a longer time to eruption.'

Site-specific data are extremely useful, as properties such as tensile strength can vary from near zero to almost 35-40 MPa (Heap and Violay, 2021). The tensile strength we used was based on experimental data, and a discussion of the implications of using a different value has been added to the model assumptions section. Rock property data was also used to inform the range of permeabilities and porosities that were modelled. In the new assumptions section we compare results from our simplest models, which minimise assumptions, with ones with anisotropic permeability or heterogeneous porosity as suggested by field and experimental data. We show that they do not affect our overall conclusions. However, to use these models to help quantitatively interpret monitoring data, which is a future goal, more refined rock property distributions will be necessary in our models which will greatly benefit from detailed site-specific data.

Reviewer 2 (Shaul Hurwitz)

We are grateful to Shaul Hurwitz for his thorough and encouraging review. We have addressed his suggestions and comments within the manuscript and detail our responses below.

Assumptions and simplifications

There are obviously quite a few of them included in the simulations (as with every numerical simulation) and some of them are mentioned. I'm aware that there are length limitations but suggest finding space for a paragraph that lists the most significant of these assumptions and simplifications. Some obvious ones include using isotropic permeabilities (in volcanoes most rock units are highly anisotropic), scaling of the rock properties from the laboratory experiments, the use of the Van Genuchten function and Corey curves (both critical for timescale determinations), and quite a few more. The assumptions and approximations all seem to be valid, but obviously other functions, geometries, and parameter values are also plausible.

As pointed out, numerical simulations require a range of assumptions. We had tried to cover these in the relevant sections of the manuscript, but acknowledge that they were not as clear or comprehensive as they could be. We have therefore added a new section 'Model assumptions and their implications' to the main body of the manuscript which addresses the assumptions mentioned here and others. We thank the reviewer for prompting this addition, which has improved our manuscript.

Data availability

Where will the input files and Python scripts reside? I do not see information on this, yet it is important for reproducability.

The input files and Python scripts to modify them are in a github repository which will be made public when the paper is published.

The effect of dissolved gases on fluid properties

In line 12 "Key factors contributing to these eruptions include the intensity of the heat source,…". One other key parameter is the effect of dissolved gases on the thermodynamic properties of the pressurized fluid. See for example, Hurwitz et al. (Geology 2016). The comprehensive dataset presented in Christenson et al. (JVGR 2017) might suggest that small changes in fluid composition could have large effects on phase distribution and consequently, pressure.

We have added a discussion of the effect of dissolved gases on fluid properties to the new 'Model assumptions' section. Our revised models, which represent the conceptual model at Whakaari more closely, have a vapour-filled conduit that is primarily superheated gas and therefore pressure differences because of changes in fluid composition would be very small.

Seal

The term "seal" and sealing are mentioned many times throughout the text and are a major component of the presented model. Yet, the composition of the proposed seal is not mentioned. Seals that can hold pressure are usually predominantly of silica polymorphs (or in an amorphous form) and the timescales to deposit them (which are defined by solubility in the fluid and kinetics) could limit the timescale of pressurization (line 84 "Little is known about the timescales of hydrothermal sealing processes"). In contrast, clay minerals are of low tensile strength and would not form effective seals. Lines 88-90 "At Whakaari, mineralised zones (alunite, anhydrite) may open/close rapidly, in seconds to hours…" Precipitation of minerals in pore space is followed by crystallization which (depending on the mineral) impact the tensile strength of the rock. So, perhaps maybe mention what is known on the composition(s) of the seal(s) and the consequences for the model, especially for the timescales.

This comment is very interesting. Kennedy et al. (2020) found that sealing material at Whakaari contained alunite, silica polymorphs, and clay minerals. Comparisons with other volcanoes worldwide (e.g. at Turrialba in Costa Rica; Mick et al. 2021) and laboratory experiments (Kilgour, personal communication 2025) suggest that alunite is most prevalent and acts as a cement that binds the rock fragments together, forming the seal. There are few, if any, studies on rates of alunite precipitation in volcanic environments, but recent unpublished experiments suggest it can be very fast (Kilgour, personal communication, 2025). Therefore, seal formation timescales may only minimally impact our timescales for pressurisation. We have added discussion of these to the manuscript.

Nomenclature

In their 2001 review paper, Browne & Lowless (Ref. #8) provided a glossary for terms associated with these events. They define "Phreatic eruption: An eruption which is caused by heating and flashing of water produced when magma comes into contact with water but only country rock or overburden is ejected (i.e. no juvenile magmatic material.) If there is unequivocal evidence for the ejection of any juvenile igneous material, it should be classed as a phreatomagmatic eruption." I think that the term "hydrothermal explosion" as was defined in the classic paper by Muffler and White (GSAB 1971) which is surprisingly not cited in this paper is preferred. The idea is that the process originated in the hydrothermal system (as in the second part of the title), and "explosion" rather than "eruption" because these are impulsive, short duration energy releasing events, whereas "eruptions" (from geysers or volcanoes) describes a continuous release of energy over time. This is just a suggestion to consider and not a comment that must be addressed. In the paper, the two terms seem to be interchangeable; for example, lines 41-42 "Observations have been limited following the 2019 eruption due to restricted access, but steam explosions and eruptive activity…"

Montanaro et al. (Bulletin of Volcanology, 2022) discuss the terminology of phreatic versus hydrothermal eruptions in some detail. Our gas flux rates are based on the 2016 and 2019 eruptions of Whakaari volcano, which Montanaro et al. (2022) classify as phreatic eruptions. We therefore believe that this is the most appropriate term, as well as being most consistent with the wider literature. Steam explosions referred to in lines 41-42 were much smaller magnitude and duration events as described in the Global Volcanism Program report – our terminology for explosions and eruptions is consistent with their reports.

The sentence in lines 22-24 "Changes in precursory signals such as seismic activity, ground deformation, and increased degassing and heat flux can indicate subsurface heat and fluid movement leading to a phreatic eruption" contradicts the sentence in lines 13-14 "However, phreatic eruptions are typically unpredictable…" To date, no hydrothermal explosion was captured by monitoring data prior to the event (no short-term warning was ever issued). Very few explosions were shown a priori to indicate changes leading to the explosion.

We have rephrased lines 13-14 to "Changes in monitoring signals such as seismic activity, ground deformation, and increased degassing and heat flux can indicate subsurface heat and fluid movement potentially associated with pre-eruptive processes" to clarify.

Line 52 Replace "dissolved rock components" with "solutes dissolved from rocks".

Done.

Line 60 – "they often experience different shallow subsurface conditions" - this is a bit vague and would benefit from a bit more detail.

Changed to 'fluid pathways and flow rates are somewhat independent in the shallow subsurface'.

Lines 72-73 – "Geochemical analysis suggests that SO2 is approximately 10% of the total mass degassed at the surface". There is also plenty of CO2 emitted (Christenson et al., JVGR 2017, Table 3) which could have a significant effect on the composition of the pressurized fluid at depth.

We have changed the sentence to 'Geochemical analysis suggests that degassing at the surface is approximately 10% SO_2, 28% CO_2, and 59% water/steam.' We have also added a comment on the effects of different gas compositions to the new assumptions section.

Line 994-95 – I don't understand the sentence and how lithostatic and hydrostatic pressures are distributed throughout the model domain.

Waiwera calculates pore fluid pressure based on hydrostatic pressure plus variations due to fluid flow. We have therefore removed the reference to lithostatic pressure because the system is assumed to be at geostatic equilibrium.

Paragraph starting in line 100 - Was the domain assumed to be completely rigid, that is no elastic moduli defined? This could be important because some of the overpressure could be absorbed if the rocks can deform elastically (Hurwitz et al., JGR 2007; Fournier & Chardot, JGR 2012). Perhaps this is another simplification of the model?

Waiwera is a hydrothermal modelling package, and therefore does not account for elastic deformation of the rocks. We have added a discussion of this to the new assumptions and their implications section of the paper.

Figures 1 & 2 – the unsaturated (vadose) zone is not shown bin oth figures. What was the depth to water saturation? Was it constant throughout the domain. I also don't see it in Extended Data Figures 2 & 3 or described in the paragraph starting in line 408.

The unsaturated zone was calculated within the models and therefore varied across the domain. We have added the following sentence at the end of the paragraph starting at line 408: 'The elevation of the water table was calculated within the models and was around sea level across the model domain.' The unsaturated zone can be seen in Extended Data Figures 4-2 and 4-3, as we have now explained in the caption 'Vapour saturation is the proportion of air + steam as a fraction of total fluid: above sea level any vapour is air corresponding to the unsaturated zone; below sea level vapour is predominantly steam.'

Line 140 – add the word "results" – "Numerical modelling results show that…"

Done.

Paragraph starting in line 148 – "the shallow hydrothermal system at Whakaari is often already vapour dominated..". The compressibility of a two-phase mixture is significantly higher than that of a single-phase fluid (liquid or gas) (Grant and Sorey, WRR 1979). Thus, to generate large overpressures, multiphase fluids are likely required.

As reviewer 1 pointed out, geochemistry and conceptual models suggest that the conduit at Whakaari contains single-phase steam. We have revised our models to replicate this and rewritten the paragraph accordingly.

Figure 3 – Why not limit the x-axis to 1 month? There are almost no changes after the first few weeks, so by shortening the time presented, the actual changes can be better seen.

Done.

Section starting in Line 158 – the timescale of the simulated time to explosion is highly dependent on the initial conditions. Could 1-2 sentences be added to address this issue which is quite significant? How would different (and yet plausible) initial conditions affect the calculated timescales? Line 455 "We also ran models to explore sensitivity of results to initial conditions…" but the results from these tests and their implications are not described.

The results from these tests and their implications were at the end of the Methods section and in Extended Data Table 1 and Extended Data Figure 4. We have moved the text and figure to the new 'Model assumptions and their implications' section in the main body of the manuscript and expanded the discussion.

Lines 159-160 – important to note that the inferences were made after the explosions.

Have changed the sentence to 'Timescales of hydrothermal changes before phreatic eruptions have been inferred after the event in many previous studies globally'.

Lines 164-165 “Our results indicate that these timescales are a direct result of the geological and hydraulic conditions.” What other options are there other than geologic and hydraulic? This is a very generic statement and can be a bit more specific.

Changed to ‘Our results indicate that these timescales are a direct result of hydrothermal mineralisation and magmatic gas input rates.’

Paragraph starting in line 194 has a title of “Implications for future monitoring”. I don’t really see implications in the paragraph (for example, possible suggestions for monitoring) but I think that a title like “new insights on processes leading up to hydrothermal explosions” better describes the section. Hopefully, studies like this will one day help in the design of a monitoring system that could detect such explosions.

Changed to ‘Implications for interpreting monitoring data’. We also hope that studies like this will help with monitoring one day.

Line 195 – I don’t think the words “most common” is correct in this context.

Removed.

Line 231 – should be “magmatic degassing rates”.

Changed.

Lines 405-407 “If multiple mass components are present” – is this dissolved solids and/or dissolved gas? I suggest providing some explanation.

Have added ‘such as water and air or CO_2’ to the sentence for clarity.

Extended Data Figure 2 – in the legend, maybe add conductive heat near the yellow rectangle (rather than just “heat”) to distinguish from the advective heat source (orange rectangle).

In our legend we have heat + mass and then heat to distinguish between advective and conductive sources. This is consistent with Waiwera terminology and avoids confusion because the orange rectangle has both conductive and advective sources.

Extended Data Figure 3 – in the two bottom plots showing vapor saturation there is an area showing saturation of ~0.5 to the left of the vent and above the fully saturated area. Is this air or water vapor ascending from depth? Should maybe add a sentence with an explanation.

We have added ‘Vapour saturation is the proportion of air + steam as a fraction of total fluid: above sea level any vapour is air corresponding to the unsaturated zone; below sea level vapour is predominantly steam.’

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have substantially revised the paper in ways that address the major conceptual issues from the first round. Most importantly, they re-ran the models with a supercritical version of Waiwera to represent a vapor-dominated volcanic conduit. While there have been several other studies that have simulated vapor-dominated geothermal systems at temperatures <300 C (e.g. Ingebritsen and Sorey, 1988, JGR Solid Earth; Raharjo et al. 2016, Geothermics) to my knowledge this is the first study to simulate a volcanic conduit containing superheated vapor, which is a substantial accomplishment.

As a result of the changed model set-up, they removed the condensation-induced overpressure mechanism, which was previously a major part of the initial submission. The core message now appears to be that low-permeability seals can trap rising magmatic gases and reach a tensile-failure overpressure within weeks or faster if fluxes of magmatic gases are high. This is a clear and useful scientific message of substantial value to the broader volcanological community.

Below are a few specific suggestions to further tighten the manuscript:

- *As you note in the manuscript and rebuttal letter, you assume that failure occurs when fluid pressure exceeds the confining pressure + tensile strength of an intact seal. Please state explicitly that this is a tensile failure criterion and not a shear failure criterion for critically stressed faults. Critically stressed rock with pre-existing faults/fractures can fail by shear reactivation at fluid pressures well below the confining pressure; is there evidence of rock with pre-existing fractures at Whakaari that could fail in this manner? A one- or two-sentence contrast will help readers better understand how this assumed failure mechanism contrasts with the failure of critically stressed rock.*

In the assumptions section, we have added the sentence ‘We consider only tensile failure, and do not consider reactivation of local shear fractures/faults that could reduce time to eruption.’ We do not know of any evidence for shear faults, healed or otherwise, within the shallow volume of the conduit that we are focussing on at Whakaari.

- *Regarding the conclusion that "boiling is not necessary" to generate phreatic eruptions (lines 169-173), this may be an important, generalizable point: in a vapor-dominated conduit, you can reach failure without boiling/flashing. I think it would be worthwhile to emphasize why - low vapor compressibility? Also, is this a Whakaari-specific observation, or could you delineate under what conditions boiling might be more important?*

Thank you for highlighting this important point. We have added ‘Fluids stay in vapour phase throughout (Figure 3c) and boiling is not necessary because the increasing mass of steam raises the pressure, while the resulting higher pressure reduces the steam compressibility. This mechanism is likely applicable at other vapour-dominated hydrothermal systems where gas accumulation beneath a shallow seal occurs. In contrast, in liquid-dominated systems, boiling and phase changes probably play a more critical role in generating overpressure and initiating phreatic eruptions.’

- *In scaling magmatic gas fluxes from SO_2 measurements, you report gas composition ~59% water, 28% CO_2, 10% SO_2. It should be clearly stated that these percentages are by mass (please double check) - it is common in the volcanic gas community to report gas composition in terms of mole %, not mass %.*

Added ‘by mass’ to the end of the sentence.

- In the abstract, you mention "increased magmatic gas flux", but "increased" is always relative. I suggest to add "relative to background fluxes".

Done.

- It might be worthwhile to spell out the practical monitoring implications. Is it true, that most of the time, partial openness of either the top or side seals keeps overpressure below failure? In that case, short lead-time failures would require "temporarily" tight top and side seals or elevated magmatic gas input? Something like this might help communicate if or why (under what conditions) low permeability zones are critical, and why many volcanoes such as Whakaari can pass long time intervals without eruptions.

This is a good point. Figure 4 shows that partial openness keeps overpressure below failure unless fluid input is elevated, and that tight top and side seals are required for rapid failure. We have expanded on this in the monitoring implications section as suggested.

Overall, I enjoyed reading the revision and look forward to seeing the published text after minor revisions.

Reviewer #2 (Remarks to the Author):

I think that the authors did a fine job responding to the comments and suggestions provided by both reviewers to the original manuscript. The revised manuscript address many of the comments, but I think that more clarifications are needed to make the conclusions more robust.

*** Responding to reviewer #1, the authors write "We reduced the vertical model dimension to 1 km rather than 2 km to reduce the occurrence of supercritical conditions that Waiwera could not simulate." However, the existence of SO2 rather than H2S in the emitted gas (described in the paragraph starting in line 86) suggests temperatures that are most likely above the critical temperature of pure water. This is inconsistent with the statements in the new assumptions section (lines 275-278) "In most of the conduit, magmatic gases and steam would be superheated, although at the base of the model CO2 and SO2 would be supercritical but steam would not." And then "we believe that using steam as a proxy for the combined hydrothermal fluids is a reasonable assumption." This discrepancy and better descriptions of thermodynamic properties of high-temperature aqueous-rich fluids should be at least mentioned.*

We have expanded the discussion section to clarify this: 'Model temperatures within the conduit are above the critical temperature for the predominant gases: steam 374 °C; CO_2 31 °C; and SO_2 157 °C. At the base of the model, pressure exceeds 8 MPa when the system is sealed and has increased fluid input (Figure 2), which is above the critical pressure for CO_2 (7.38 MPa) and SO_2 (7.88 MPa) but not steam (22 MPa). However, the critical pressure is not exceeded in most of the conduit, and all three major fluids would be in a superheated state with comparable fluid properties. We are focused on shallower depths below the seal, and therefore steam is a reasonable first approximation for the combined hydrothermal fluids.'

*** The terms "steam" and gas seem to be used interchangeably in the title ("magmatic gas input") and the text. The paragraph starting in line 86 describes fluxes of noncondensable gases (CO2 and SO2). The text should probably minimize the use of the word "gases" as other than water they are not incorporated in the simulations. For example, in lines 141-142 "As heated groundwater and magmatic gases rise from below, overpressure builds near the surface (Figure 2)." This is misleading or at least confusing because groundwater and gases suggest to me that gases other than water are also modeled, but they are not.*

We have changed 'magmatic gases' to 'fluids' when referring to simulated gases. We have also expanded the discussion of using steam to represent magmatic gases as well as steam in the models, as suggested above.

*** Following the sentence in lines 33-34 "highlighting the importance of monitoring hydrothermal systems and understanding the underlying physical processes", I suggest citing the paper by Poland et al. (GRL 2025) which provides one of the first examples of a successful detection of an explosion based on monitoring of hydrothermal activity.*

Lines 33-34 are talking about phreatic eruptions at Mount Ontake and Whakaari, which are a different setting and scale to the hydrothermal explosions at Yellowstone monitored by Poland et al. (2025). Also, the latter work detected explosions as they occurred but did not identify immediate precursors and is, therefore, looking at a different timescale to our study.

*** The assumption described in lines 35-36 and depicted in Figure 1 is that only the conduit is sealed. That is, a small narrow area within the hydrothermal system is sealed rather than a broader hydrothermal reservoir as depicted in most models of hydrothermal explosions/phreatic eruptions. For example, in Browne & Lawless (ESR 2001) or more recently in Harrison et al. (Eos 2025) - https://eos.org/features/hydrothermal-hazards-on-display-in-yellowstone-national-park. Based on the volume of ejected ballistics in past explosions, is it reasonable to assume that the seal was confined to the conduit? In any case, a sentence or two explaining this aspect of the conceptual model would be helpful*.

We have modified line 222 to 'Thicker or more extensive seals could result in faster pressurisation' to reflect the fact that rock adjacent to the conduit would likely also suffer a reduction in permeability. There is not enough information from ejected ballistics to know the extents of sealing unfortunately. We are basing our numerical models on conceptual models of Whakaari, where phreatic eruptions are thought to originate from the main conduit as described in the hydrothermal activity section of the manuscript (Christenson et al. 2017, Kilgour et al. 2021). In addition, vertical seals in our models will limit the influence of a more laterally extensive top seal.

*** I don't think that the range of plausible controlling parameters was explored enough (see below) and suggest looking into section 6 in Ingebritsen et al (RoG 2010) for a more in-depth discussion on common assumptions and simplifications in such models of hydrothermal flow. I also think that logically, it makes more sense to describe the major assumptions before describing simulation results.*

To further address assumptions we have added the sentence 'There are several assumptions implicit in the modelling software, such as the applicability of modified Darcy's Law to simulate fluid flow, the discretisation of rock properties into finite elementary volumes, the assumption that rocks are in local thermal equilibrium with fluids, and the absence of reactive fluid flow[40,47].' We looked at moving the assumptions section to before the results, but it did not make sense to present results that explored these assumptions (e.g. Figure 5) before the main results.

*** Several studies have shown that the effects of permeability anisotropy, especially in volcanic terrain could be significant (section 6.10 in Ingebritsen et al., RoG 2010). Starting in line 241 "In the anisotropic permeability case, permeability in the conduit was reduced to one tenth in the lateral directions, and host-rock permeability was one tenth in the vertical direction. In these models, time to eruption is typically less (Figure 6) and maximum overpressure is greater." There is really no justification for choosing these arbitrary anisotropy values except for citing another modeling paper. I suggest expanding the sensitivity analysis of isotropic permeability. For example, using a value of 10^-15 m^2, examine how do the final overpressure and time to failure change where when horizontal permeability is 1 and 2 orders of magnitude larger or smaller than the vertical permeability for the host rock and the conduit? Results from this analysis could be an extension to Figure 4. My assumption is that the effect might be quite significant.*

We have added citations that discuss permeability anisotropy in the laboratory to this section of the manuscript. Models with an order of magnitude decrease in permeability vertically or horizontally were presented in Figure 5 – we have rephrased from one tenth to an order of magnitude to make this clearer. To reflect the inherent uncertainty in permeability values, we have also run another set of models with reduction by two orders of magnitude perpendicular to the direction of flow. These have been added to Figure 5. In host rock, 10^{-15} m^2 is represented in models with one order of magnitude reduction of permeability. 10^{-15} m^2 would be very low for the conduit, but 10^{-14} m^2 is represented in the added models with a reduction of 2 orders of magnitude.

*** In the methods section there is a description of capillary pressure and relative permeability functions and values but no sensitivity of the actual functon(Corey vs. Linear relative permeability, for example) is presented, despite the relatively large effects. I suggest looking at sections 6.5 & 6.6 in Ingebritsen et al (RoG 2010) for a more in-depth discussion on relative permeability and capillary pressure in hydrothermal flow models.*

We have expanded our discussion of what relative permeability and capillary pressure are and how important they can be. We have added the sentences 'We tried all functions available within Waiwera for relative permeability and capillary pressure, but other than model instabilities with the van Genuchten function, found minimal differences in results. This is likely due to the predominantly single-phase steam in the conduit in the models (Figure 3).'

*** Following the description of the "seal" in lines 215-216 It would be useful to provide a short description of the mineral modality of the breccia. How much alunite is in the rock and is this volume enough to be the "weak link"? In the rebuttal, the authors write "There are few, if any, studies on rates of alunite precipitation in volcanic environments, but recent unpublished experiments suggest it can be very fast" and then "seal formation timescales may only minimally impact our timescales for pressurization". This seems to contradict the statement in lines 206-207 "timescales are days to weeks depending on the permeabilities of the seals." I suggest better describing the relation between alunite precipitation rates, time-dependent permeability changes, and overpressure timescales. Also, conceptually, what is "time zero" in which there is no alunite in the conduit?*

We have rephrased lines 206-207 to 'pressurisation timescales are days to weeks depending on the permeabilities of the side and top seals' to clarify that pressure is changing over time dependent on static permeabilities in the top and side seals, rather than permeability changing over time. Time zero is represented in our initial conditions, and precipitation is assumed to be instantaneous at the beginning of the 3-month model runs. The modality of the breccia is poorly constrained because the bombs measured by Kennedy et al (2020) are unlikely to represent the whole seal, and they did not report numbers. We therefore do not know how much alunite is present or required, but have added 'If alunite precipitation was slow, our modelled timescales would underestimate the time required to pressurise the system'.

*** The word "monitoring is mentioned many times throughout the text. Lines 303-306 state "Comparing model outputs to monitoring data can test and refine 303 qualitative interpretations of subsurface activity from data; temperatures and gas fluxes can be modelled directly, while deformation, seismicity, and gravity signals can be calculated from modelled pore pressures." This entire section starting in line 297 suggests that modeling results can inform monitoring, but it seems that the association is vague. I think that more specific assertions are needed. For example, from this study's simulation results, what kind of specific recommendations could be made to improve the monitoring system in Whakaari/White Island? New types of monitoring instruments? More dense seismometers? An improved geodetic network? Additional or more frequent satellite-based observations?*

We have added '… can help to inform monitoring networks and field campaigns', but more detailed recommendations would benefit from more targeted modelling. We have also added

‘Our models suggest that rapid pressurisation requires temporarily tight permeability and/or increased magmatic gas flux. Monitoring data can indicate these changes but are often ambiguous[29]. For example, decreasing surface gas flux measurements could result from waning magmatic hydrothermal activity or tightening seals, which have very different implications for likelihood of a phreatic eruption. However, increasing pressurisation from tightening seals may also be reflected in changing seismicity or deformation, helping to distinguish between these scenarios[21,54].’

*** I think that the statement in lines 330-332 “Modelling the hydrothermal system at Whakaari has allowed us to establish a clear relationship between hydrothermal mineralisation, magmatic degassing rates, and the occurrence and timing of phreatic eruptions” is exaggerated. I suggest something like “based on a range of simplifying assumptions, model results suggest that relation between hydrothermal mineralization and magmatic degassing rates have a strong control on the occurrence and timing of phreatic eruptions.”*

Done.

*** I suggest removing the word “believe” which appears in lines 252 & 277 as it should not be used in a scientific paper. Rather words such as conclude, infer, or a similar word could be used.*

Changed to conclude in both instances.

I hope the authors find the suggestions useful. I am looking forward to seeing the revised manuscript published.

Review of " *Phreatic eruptions at Whakaari Volcano driven by hydrothermal mineralisation and magmatic gas input*" by Sophie Pearson-Grant, Michael Heap, and Adrian Croucher submitted to Communications Earth & Environment.

The paper describes results from multiphase groundwater flow and heat transport simulations of hydrothermal activity leading to phreatic eruptions (hydrothermal explosions) at Whakaari Volcano (White Island) in New Zealand. Based on simulation results, the authors provide new inferences on processes and their timescales leading up to these dramatic events. The topic of the paper is significant because these explosions could be lethal, yet not much is known about the complex processes associated with their occurrence. An improved conceptual and quantitative understanding of these processes is needed for designing monitoring systems that would eventually detect precursory signals.

I found the paper to be interesting, well written, and easy to follow. This is not always the case with papers presenting numerical simulations, especially in short-format papers. It is constructed logically, and the hypotheses, methods, results and interpretations are mostly explained. I do however have some suggestions and some comments for the authors to consider:

Assumptions and simplifications - there are obviously quite a few of them included in the simulations (as with every numerical simulation) and some of them are mentioned. I'm aware that there are length limitations but suggest finding space for a paragraph that lists the most significant of these assumptions and simplifications. Some obvious ones include using isotropic permeabilities (in volcanoes most rock units are highly anisotropic), scaling of the rock properties from the laboratory experiments, the use of the Van Genuchten function and Corey curves (both critical for timescale determinations), and quite a few more. The assumptions and approximations all seem to be valid, but obviously other functions, geometries, and parameter values are also plausible.

Data availability - where will the input files and Python scripts reside? I do not see information on this, yet it is important for reproducability.

The effect of dissolved gases on fluid properties – in line 12 "*Key factors contributing to these eruptions include the intensity of the heat source*,…". One other key parameter is the effect of dissolved gases on the thermodynamic properties of the pressurized fluid. See for example, Hurwitz et al. (Geology 2016). The comprehensive dataset presented in Christenson et al. (JVGR 2017) might suggest that small changes in fluid composition could have large effects on phase distribution and consequently, pressure.

Seal – the term "seal" and sealing are mentioned many times throughout the text and are a major component of the presented model. Yet, the composition of the proposed seal is not mentioned. Seals that can hold pressure are usually predominantly of silica polymorphs (or in an amorphous form) and the timescales to deposit them (which are defined by solubility in the fluid and kinetics) could limit the timescale of pressurization (line 84 "*Little is known about the timescales of hydrothermal sealing processes*"). In contrast, clay minerals are of low tensile strength and would not form effective seals. Lines 88-90 "*At Whakaari, mineralised zones (alunite, anhydrite) may open/close rapidly, in seconds to hours*…" Precipitation of minerals in pore space is followed by crystallization which (depending on the mineral) impact the tensile strength of the rock. So, perhaps maybe mention what is known on the composition(s) of the seal(s) and the consequences for the model, especially for the timescales.

Nomenclature - In their 2001 review paper, Browne & Lowless (Ref. #8) provided a glossary for terms associated with these events. They define "***Phreatic eruption****: An eruption which is caused by heating and flashing of water produced when magma comes into contact with water but only country rock or overburden is ejected (i.e. no juvenile magmatic material.) If there is unequivocal evidence for the ejection of any juvenile igneous material, it should be classed as a* ***phreatomagmatic eruption***." I think that the term "hydrothermal explosion" as was defined in the classic paper by Muffler and White (GSAB 1971) which is surprisingly not cited in this paper is preferred. The idea is that the process originated in the hydrothermal system (as in the second part of the title), and "explosion" rather than "eruption" because these are impulsive, short duration energy releasing events, whereas "eruptions" (from geysers or volcanoes) describes a continuous release of energy over time. This is just a suggestion to consider and not a comment that must be addressed. In the paper, the two terms seem to be interchangeable; for example, lines 41-42 "*Observations have been limited following the 2019* ***eruption*** *due to restricted access, but steam* ***explosions*** *and* ***eruptive*** *activity*…"

The sentence in lines 22-24 "*Changes in precursory signals such as seismic activity, ground deformation, and increased degassing and heat flux can indicate subsurface heat and fluid movement leading to a phreatic eruption*" contradicts the sentence in lines 13-14 "*However, phreatic eruptions are typically unpredictable*…" To date, no hydrothermal explosion was captured by monitoring data prior to the event (no short-term warning was ever issued). Very few explosions were shown *a priori* to indicate changes leading to the explosion.

Line 52 Replace "*dissolved rock components*" with "solutes dissolved from rocks".

Line 60 – "*they often experience different shallow subsurface conditions*" - this is a bit vague and would benefit from a bit more detail.

Lines 72-73 – "*Geochemical analysis suggests that SO2 is approximately 10% of the total mass degassed at the surface*". There is also plenty of CO2 emitted (Christenson et al., JVGR 2017, Table 3) which could have a significant effect on the composition of the pressurized fluid at depth.

Line 994-95 – I don't understand the sentence and how lithostatic and hydrostatic pressures are distributed throughout the model domain.

Paragraph starting in line 100 - Was the domain assumed to be completely rigid, that is no elastic moduli defined? This could be important because some of the overpressure could be absorbed if the rocks can deform elastically (Hurwitz et al., JGR 2007; Fournier & Chardot, JGR 2012). Perhaps this is another simplification of the model?

Figures 1 & 2 – the unsaturated (vadose) zone is not shown bin oth figures. What was the depth to water saturation? Was it constant throughout the domain. I also don't see it in Extended Data Figures 2 & 3 or described in the paragraph starting in line 408.

Line 140 – add the word "results" – "Numerical modelling **results** show that…"

Paragraph starting in line 148 – "*the shallow hydrothermal system at Whakaari is often already vapour dominated*..". The compressibility of a two-phase mixture is significantly higher than that of a single-phase fluid (liquid or gas) (Grant and Sorey, WRR 1979). Thus, to generate large overpressures, multiphase fluids are likely required.

Figure 3 – Why not limit the x-axis to 1 month? There are almost no changes after the first few weeks, so by shortening the time presented, the actual changes can be better seen.

Section starting in Line 158 – the timescale of the simulated time to explosion is highly dependent on the initial conditions. Could 1-2 sentences be added to address this issue which is quite significant? How would different (and yet plausible) initial conditions affect the calculated timescales? Line 455 "*We also ran models to explore sensitivity of results to initial conditions*…" but the results from these tests and their implications are not described.

Lines 159-160 – important to note that the inferences were made after the explosions.

Lines 164-165 "*Our results indicate that these timescales are a direct result of the geological and hydraulic conditions*." What other options are there other than geologic and hydraulic? This is a very generic statement and can be a bit more specific.

Paragraph starting in line 194 has a title of "*Implications for future monitoring*". I don't really see implications in the paragraph (for example, possible suggestions for monitoring) but I think that a title like "new insights on processes leading up to hydrothermal explosions" better describes the section. Hopefully, studies like this will one day help in the design of a monitoring system that could detect such explosions.

Line 195 – I don't think the words "*most common*" is correct in this context.

Line 231 – should be "magmatic degassing **rates**".

Lines 405-407 "*If multiple mass components are present*" – is this dissolved solids and/or dissolved gas? I suggest providing some explanation.

Extended Data Figure 2 – in the legend, maybe add **conductive** heat near the yellow rectangle (rather than just "heat") to distinguish from the advective heat source (orange rectangle).

Extended Data Figure 3 – in the two bottom plots showing vapor saturation there is an area showing saturation of ~0.5 to the left of the vent and above the fully saturated area. Is this air or water vapor ascending from depth? Should maybe add a sentence with an explanation.

Looking forward to reading the published paper.

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