nature portfolio

Peer Review File

Seawater-magma interactions sustained the high column during the 2021 phreatomagmatic eruption of Fukutoku-Oka-no-Ba

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

11th April 22

Dear Dr Maeno,

I hope you are well?

Please allow me to apologise for the delay in sending a decision on your manuscript titled "First timeseries record of a large-scale silicic shallow-sea phreatomagmatic eruption". It has now been seen by 3 reviewers, whose detailed 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. In addition to the reviewer's comments, we would like you to consider the following editorial thresholds when revising your manuscript:

- Define clear, science-focused research questions or hypotheses that are tested with your observations and present a conceptual advance in our understanding of phreatomagmatic eruptions

- Discuss your interpretations fully in the context of published literature on other submarine/phreatomagmatic eruptions, and particularly the existing literature available for the Fukutoku-Oka-no-Ba eruption.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would 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.

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

If the revision process takes significantly longer than three months, we will 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.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not 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 reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any 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 **

Please do not hesitate to contact me 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,

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EDITORIAL POLICIES AND FORMAT

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This paper is a clear description of an interesting eruption. Eruptions in submarine settings are not often well described. In this case the geophysical and satellite data are a strength, but there is little local observation of the actual eruption events during the main phase and the deposits were not accessible. Hence the manuscript makes a convincing argument, but much of it is based on theory and generalised interpretation. There are several places, however in the manuscript, where there are alternatives to the statements and conclusions. In particular, a lot is made of the differences of this event to “typical” surtseyan or phreatomagmatic events, such as the high Si content and the volume. However, this is a trachytic magma with a very high alkali content, so in terms of its physical properties, particularly its rheology, it is likely behaving more like a lower silica-content (basaltic andesite) in a calc-alkaline setting (albeit erupted at a lower temperature). Further, the volume estimations are quite approximate, and not enormous in comparison to past surtseyan eruptions. The lack of significant ash in the plume and the formation of a local tuff ring, could also be argued as being a typical energetic surtseyan event.

The paper body is well written, but it is a little “dry” and descriptive. It provides a chronology based report of the eruption – but it doesn’t pose any major questions or hypotheses at the outset that are tested with these observations.

The abstract could more clearly link statements to build the story – it describes essentially a timeline of events and a few key summary facts. I believe that the scientific contribution of this work is greater than just a description of events – this comment (i.e., going beyond a descriptive volcanic event report) needs to be considered throughout the paper – why is the “timeseries” important (e.g.., the title is based on this “report” style also), what are the key questions that this study seeks to answer? Why is this eruption relevant, beyond its rarity in composition? This is currently written as an eruption report, of an albeit unusual eruption, but to have a greater impact, it should be framed around discovering or elucidating new insights into submarine volcanic phenomena via this event…

Interesting contributions include why the eruption produced so little ash in the plume (steam/water dominated), yet a large pumice raft and tuff ring. This appears to be a "normal" phreatomagmatic eruption despite the unusual trachytic composition

Line 13 – not all voluminous silicic eruptions have a high eruptive rate?

Line 14
"such eruptions mostly appear in geological records" meaning is unclear
Replace with "such eruptions have rarely been witnessed or described in historical records"

Line 38 – 41 – discussion of "small" volume vs "large" volume eruptions seems a little weak. What fundamentally should be the difference – is there a research question or hypothesis in this to be tested? There have been many submarine or lake-sourced surtseyan and phreatomagmatic and sub-glacial eruptions witnessed around the world and described in the scientific literature. These range from basalt (e.g., Ambae) to basaltic andesite (e.g., Eiyafjallajokul) to andesite (e.g., Ruapehu), just to name three of many examples.

Also, was this event truly that different in volume to other surtseyan or phreatomagmatic events? Perhaps it is larger, but likely by less than an order of magnitude?

The size or volume seems not as relevant to process as eruption rate(s) and the steadiness of mass ejection rate… perhaps that is where this paper should concentrate

Line 60-62 – the sustained high plume is perhaps the most interesting thing about this eruption – how many hours did it last? Phreatomagmatic events are typically unsteady and involve many explosions with pauses between, such as in Surtseyan or vulcanian styles. Even the Eiya eruption produced a distinct, long-lived plume, but it was being produced by loosely spaced but staccato eruptions. The event described here is different in the steadiness.

Line 67-69
The trachydacitic composition has a high silica content, but it also has a very high alkali content, so that the magma is actually quite similar in viscosity to the basaltic andesites that typify most other "smaller" phreatomagmatic eruptions. Hence the "silicic" difference may be less important than implied in the text

Line 76-83 – what does steady with fluctuations mean? How steady was it? There is mention of pulses but periods of sustained plumes – was the MER steady for periods – how long were they?

Co- eruptive generation of the pumice plume is interesting and this process could be a good focus…

Line 91-106 The two periods of sustained columns are brief, 1 hr and 20 min. To what extent are the columns reflective of sustained eruption mass ejection rates? i.e., was the eruption pulsing during this time – like typical phreatomagmatic events or was it truly steady? Columns are reflective of the convection above an eruption site, even if it is unsteady – this is especially true if the plume was mainly steam.

Line 150 – the models of a sustained plume are assuming that there is a substantive jet region – whereas this could have been generated from steam rising from vigourous surtseyan pulsing eruptions.

Line 171-83 – the high eruption plume was mainly steam – how much of this was a convective thermal condition rather than an explosive one? Especially when there is so little ash, it is hard to justify that this was an especially vigorous eruptions, beyond the scale of a large surtseyan event. For example the Hunga volcano (referred to in the introduction) produced a series of surtseyan eruptions in 2024/15 that also produced plumes to similar heights.

Line 184-192 suggests that the decoupling of the plume into the pyroclasts rich and steam rich portions – this is interesting, but is it the only mechanism to produce a pumice raft and steam plume? Could phase 1 not just represent the phase of a growing tephra ring on the ocean floor before it broached the surface and jetting is more visible?

Line 216-223 the terminology questions seem premature if the mechanisms of this eruption remain quite speculative.

Line 353 Volume estimation – the error ranges in the depth equivalent for the pumice raft are very large, is there any independent verification of this raft thickness? Could it not have been only one or two particles thick?

Plume modelling - the observation of little or no ash within the plume makes it difficult to directly relate the steam plume to a magma ejection rate, although the authors make a convincing attempt at working on this

Composition – the high alkali contents of this composition likely make the viscosity very low – similar to basaltic andesites in calc-alkaline settings. Hence, the silica content in of itself should not be a major difference to other surtseyan cases, even if the temperature of the magma is lower.

Reviewer #2 (Remarks to the Author):

Review comments for Maeno et al.
General comments:
The paper presents a compilation of remote sensing and infrasound data for the shallow submarine 2021 Fukutoku Ok-an-oba eruption. The data is used to divide the eruption into four phases with distinct characteristics. The paper also includes geochemical data which is utilised to estimate eruption volume from volatile differencing, but this method and interpretation has its caveats. The paper is well written with clear figures, however, there seems to be a lack of exact direction or what specifics processes are in question. Some terminology needs revising to better fit with more conventional terms used for identifying and classifying eruptive styles. More could be made of coupling the available infrasound and plume data to explain the eruptive sequence, and there are several important comparisons with other shallow submarine eruptions that could be made. The discussion of some data in the extended figures is missing in the text that would be key to include. The timing of this review also gives the authors the opportunity to integrate their study with a recent published study and another pre-print using similar data regarding this eruption. The authors will want to make sure their work and dataset interpretations are standalone from these other studies. However, there are important aspects from these studies that should be referenced and discussed. I would recommend this paper requires major revisions for more attention to specific questions and processes, and incorporating recent literature on the eruption.

Specific comments:
Title. I do not think the word “first” is helpful to include in the title, as there are others that could put this claim into question.
10. This opening line would be better suited identifying the hazardous nature of these eruptions instead of their frequency.
13-15. This reads strangely, why are they “therefore poorly understood” if they “mostly appear in geologic records”? I’m unsure of this sentence’s intent.
20. This may be an underestimate based on new submarine edifice volume (points later), but worth adding in briefly that this matches from SO2 estimates and physical topographic and raft area calculations
24. What specific “processes”? This persists throughout the text. I am unsure what exact processes or dynamics the study is attempting to look at. Conduit, eruptive, style transitions, fragmentation? More specificity would be helpful in several places.
Intro. The paper needs to incorporate findings and data from the recently published Yoshida et al. (2022) geochemical/textural study of the FOB 2021 raft products. This study does not really

overlap with your findings, but the geochemical and mineral data should be utilised into your interpretations and discussion of different eruptive styles and eruption dynamics. The other study is Fauria et al. (pre-print with ESSOAR) which similarly uses satellite and aerial observations to look at raft and plume dynamics. Some of the interpretations, volume estimates, are similar. But for this paper to be accepted, your study will need to standalone from this study – better use of your infrasound data, discussion on mass discharge rates, transitions in eruption style. Your paper presents an opportunity to include the interpretations of the Yoshida and Fauria studies for a more comprehensive understanding of the FOB 2021 eruption as a whole.
38. Dry as in subaerial or anhydrous magma? Clarify.
49. With more time passed since the Hunga Tonga-Hunga Ha'apai (HT-HH) eruption, I think this study would be served well with a little more comparison to this eruption, and other shallow/emergent eruption with good observations/deposits records – Bogoslof 2016-2017, the 2009 and 2014-15 HT-HH events (Brenna et al. 2022; Colombier et al., 2018), the 1650AD Kolumbo eruption, Santorini (Cantner et al., 2014; Fuller et al., 2018). The 2022 HT-HH event interestingly has a reverse phase behaviour to the FOB 2021 eruption, where eruptive activity started as emergent Surtseyan intermittent explosions, followed the larger explosion and plume production on Jan 14 and 15. One of our key tools in understanding submarine eruptions, is making sure we make detailed comparisons to similar (and different) behaviours at similar depths – particularly for the very shallow hazardous systems. More comparisons could be made in this paper, highlighting what similarities can tell us, but also recognising the uniqueness of individual submarine eruptions.
50. Note here, the lack of ground-based observations, or proximal data collection, at this stage. Your selling point is the high-resolution time series analysis, make sure this is fully apparent in the introduction.
53. Again more specificity needed for what "processes" you are looking at.
54. "most" missing?
55. Odd use of the word "process". Eruption dynamics, styles?
64. Will need to reference Yoshida et al. (2022) and Fauria et al., in several cases here.
66. Your geochemical data needs to be compared with the variety observed and analysed in Yoshida et al. (2022). This data needs adding onto the TAS plot in the extended data. I think it is important to state your geochemical data better in the main text results and not within the later methods section. I saw no figure of table for your melt inclusion and matrix glass S data. This needs including somewhere to be available to readers and reviewers.
69. I would disagree with this statement, reading it as it is. There are others (intermediate to silicic) that have been documented/recorded in the last few decades, just not as directly observed in real time via high resolution satellite imagery and aerial imagery. This needs stating. Other eruptions in the Tofua Arc (Volcano F 2019 – andesite-dacite), the 1962 Protector Shoal seamount eruption (rhyolite), Bogoslof 2016-17 (high SiO2 trachyandesite, just a little less SiO2 than FOB).
75-87. This section of phase descriptions would be better described using more familiar and translatable terms of sustained/unsustained and steady/unsteady behaviour.
77-78. Pulsating how? In mass discharge, plume size, as well as infrasound? I would make more use of the infrasound data if possible. This is not included int the other FOB 2021 studies, so it is a useful dataset to make more use of here, particularly relating different eruptive styles. I do appreciate that this source is a little distant for smaller signal resolution. Some more direct coupling of the plume behaviour and infrasound data would work well.
81. How long does this decay last until the eruption is considered over? The Phase 4 plume diameter data (fig 2) also needs explaining briefly.
87. Do your observations add anything new to the observations and time series from Fauria et al.?
115. How does this thermal anomaly compare with other shallow/emergent eruptions? Is lack of a thermal anomaly a common feature in large submarine eruptions? If so, its is an important observation to note for understanding heat transfer, dynamics of incorporating seawater etc. (Note, this was the case, even for an eruption as large as the 2021 Havre eruption – just one thermal MODIS pixel).
127. At what stage is this number from? Is the volume estimate before volume removal in the center of the cone? When in your figure 2 timeline is the estimate?
138. Similar conclusion to the Fauria et al. study, good to note that.
156-166. This is where more of your geochemical data needs including. How does your data sit within the data from Yoshida et al. (2022)? The volume estimate from the SO2 data is a good find. How does this compare with the TROPOMI satellite SO2 estimates in Fauria et al.?

189. Unsure about that. The increasing temperature of vaporization and heat capacity of seawater within increasing pressure make this process quite different much deeper down, even more at depths >29 MPa where vapor becomes supercritical. Worth looking at some detailed modelling of submarine plume behaviour by Cahalan and Dufek (2020), in particular, the ingestion of seawater and residence of vapor within the plume relative to depth. Murch et al. (2021) also presented some modelling of gas expansion behaviour and seawater heating in deep to shallow submarine volcanism.
192. Modelling has shown that this flash effect may be very limited in comparison to the size of the plume itself . It is worth noting that not much of the seawater is "flashing" to steam, or it makes it sound as if this is the source of the whole subaerial plume.
194. Lighter?
196. How much does this match with experimental plume and current modelling e.g. Gilchrist et al. (2021) and Newland et al. (2022)?
199. So how much material might we expect in the submarine portions of the edifice and proximally settled deposits? Does the SO2 calculation of 0.11km3 in fact underestimate total DRE? How is SO2 injection vs total eruptive volume reflected in other eruptions? My understanding is that there is sometimes a deficit of SO2 output relative to volumes estimated from deposits. The caveats of the SO2 calculation will need discussing in the methods.
204. An interesting observation to note!
207. Your fig 3. makes an interesting depiction of Phase 2 and 3 as a vent system closed by the new cone deposits. If you think the build-up of the cone to an emergent edifice was key in controlling eruptive transition behaviour, it needs stating more clearly. Your figure shows this very nicely! It would be a key observation if Surtseyan eruptions are controlled by the availability of external water vs. shallow eruption directly into the open water fuelling sustained eruptive subaerial plumes. Again, I am thinking about the reverse change during the Hunga Tonga-Hunga Ha'apai event.
208. I think you can sell more the discussion of eruptive transitions during this shallow-emergent eruption. The changing behaviour from more Plinian-style column to Surtseyan activity… This was also observed partly for the Kolumbo 1650 AD eruption (Cantner et al. 2014) and was observed reversely for the HT-HH eruption in January.
213. References for this? Are you referring to specific other examples with these criteria? From your Plumeria modelling, there also no discussion of the gas velocity calculations. Do you need to include them in your extended data if you don't discuss them?
217. Missing "style"?
219. How do textural features observed in the Yoshida (2022) pumice reflect potential magmatic processes? They noted a significant array of textures and colours of clasts. I see in your extended data you observed the same, but there is no mention of this.
222. Different how?
223. Use of the term "large-scale" needs some relative comparison at some point – quite early on ideally. This event was certainly not large scale in comparison to the HT-HH event.
228. State for what purpose here. What specifics do we need to measure on the seafloor for better understanding? Mainly I am thinking volume and extent of new submarine deposits.
230. I was left a little unsure of the main takeaways from your analysis here. You could also explicitly state the value added from your data in comparison to the other recent literature on the eruption (Yoshida and Fauria).
361. What is the justification of the 1250 kg/m3 value in comparison to the pumice value? Based on ash deposits? How do theses value compare with Yoshida et al. textural data from the raft?
395-406. This data needs presenting in the results or more in the extended figures, not here in the methods.
405. What about accounting for S uptake during crystallisation? Is the assumption that all S loss from XRF to matrix glass is through degassing at the vent? Would some S quickly dissociate in the seawater?

Figures:
459. Images i) to v) need marking more clearly on the b) data. A thin red line at each time would work well.
469. The idea that the vent become closed with the growth of the cone needs discussing more, and also labelled as a key feature to help explain later behaviour.
526. Could you include some discussion of the textural variety and the similarities to Yoshida et

al.?
529. Include the Yoshida et al. data on this plot, and the rest of the geochemical data needs presenting either in plots or tables.

Reviewer #3 (Remarks to the Author):

Review comments: [formatted version also submitted]

First timeseries record of a large-scale silicic shallow-sea phreatomagmatic eruption
by F Maeno, T Kaneko, M Ichihara, YJ Suzuki, A Yasuda, K Nishida and T Ohminato

This manuscript analyses the 2021 eruption of Fukutoku-Oka-no-Ba (FOB) volcano, which produced a tuff cone, pumice raft, and relatively high, 16 km, eruption plume. Good use is made of data from the Himawari-8 satellite and remote infrasound measurements to assess plume behavior and eruption timing.
The observational information is of high quality and deserves to be published.
I find some of the interpretations to be less than well-supported, and also am troubled by the description of this as a "large-scale silicic" eruption, simply because the volume erupted is small, on the order of a tenth of a cubic km. Even eruptions such as Mt St Helens, of about one cubic km, are not generally considered "large-scale", which seems more appropriate for something like Pinatubo.
Specific comments below are keyed to line numbers. I have also returned a revision-tracked manuscript with suggested wording improvements and some additional comments.
100 The whiteness of the eruptive plume indicates lots of water, but not specifically seawater. A separate argument needs to be made for this.
133 Can the factor of two range in estimated tuff cone volume, from 0.04 to 0.07 km3, be improved?
147 Same comment on the factor of 3 range in pumice raft volume.
150 Not all water in an eruption column is vapor – plume is water-rich, not specifically steam rich.
153 External and magmatic water are subject to phase changes.
157 The "fraction" in question here has a huge range, depending on the previous two ranges for the pumice raft and the tuff cone – this should be made explicit
188 FOB's eruption plume reached the tropopause. So did the plume from Surtsey, on multiple occasions. Reaching the tropopause is probably a more significant measure than absolute height of the plume.
200 This was a very shallowly submerged vent – please present evidence that fragmentation and formation of an eruption plume preceded interaction with water. Saying that it might work like a deep-water eruption seems odd – surely the satellite information, and formation of a subaerial tuff cone, implies very different behavior from a fully submerged, deep-water, eruption.
207 To me the discussion here is quite confusing. When do particles transfer their heat, and is it being used to warm air to make a buoyant column, or to drive fragmentation through thermal interaction that makes water vapour that subsequently condenses in the eruption column, reducing its buoyancy?

Please take care to address separately interactions involving the erupting jet, vs. those taking place in the convective column.
219 A "slurry gushing from the submarine vent" is not consistent with water-surface breaching and generation of a subaerial jet and plume.
241 This eruption did not produce a large volume of magma. It produced a tuff cone, suggesting limited transport for the main mass of ejecta. There is little about the inferred eruptive processes that seems similar to Surtsey, at least from what's presented here, so it seems odd to name the eruption as an "ultra" version of Surtsey.

Response to Reviewers' comments

We thank for reviewers carefully reading our manuscript and giving insightful comments and suggestions, which helped to improve the manuscript. Point-by-point response to reviewers' comments is listed below (Black is reviewer's comments; Red is our reply).

Response to Reviewer #1

A lot is made of the differences of this event to "typical" Surtseyan or phreatomagmatic events, such as the high Si content and the volume. However, this is a trachytic magma with a very high alkali content, so in terms of its physical properties, particularly its rheology, it is likely behaving more like a lower silica-content (basaltic andesite) in a calc-alkaline setting (albeit erupted at a lower temperature).

'Silicic' was used to characterise the FOB eruption in the previous version of the manuscript; however, as pointed out by the reviewers, the use of 'silicic' may be inappropriate in the context of this paper. Therefore, we will not use 'silicic' and emphasise that the FOB eruption was a substantial explosive phreatomagmatic eruption, the rare detailly recorded in modern history.

The volume estimations are quite approximate, and not enormous in comparison to past surtseyan eruptions. The lack of significant ash in the plume and the formation of a local tuff ring, could also be argued as being a typical energetic surtseyan event.

We will not use the term 'large-scale' in the revised manuscript because of the uncertainty of the volume estimation, as pointed out. Some aspects of the FOB eruption may be similar to a typical Surtseyan event; however, the sustained feature of the plume and the relationship between plume behaviour and infrasound during the most intense period during Phase 1 doesn't indicate the typical Surtseyan eruption, and it is thought that the mass discharge rate was higher than those of the typical Surtseyans. So, we focus on this point rather than the erupted volume.

It doesn't pose any major questions or hypotheses at the outset that are tested with these observations.

The scientific questions of this study are how magma interacts with seawater in the shallow water environment, the relationship between magma discharge rate (particularly in high-discharge cases) and plume height in such conditions, and their transition in real space and time. We addressed this in the middle of the introduction section.

(Why the eruption produced so little ash in the plume, yet a large pumice raft and tuff ring)

Phase 1 of the FOB eruption was a phreatomagmatic eruption with a higher magma discharge rate than the observed examples; however, it was not as energetic as to cause intense fragmentation and disperse a large amount of pyroclasts into the atmosphere. Therefore, the major portion of the eruptive products accumulated near the vent and was not dispersed on a large scale. This was mentioned in the discussion section.

“normal” phreatomagmatic eruption despite the unusual trachytic composition.

We recognised several examples of relatively large-scale phreatomagmatic explosions with various rock types in modern history. Therefore, we cited the relevant papers and are not going to mention ‘large-scale’ and ‘silicic’ but addressed that the FOB eruption is a rare example of an eruption with a high magma discharge rate and detailly monitored and analysed.

Line 13 – not all voluminous silicic eruptions have a high eruptive rate?

We emphasise not ‘voluminous silicic’ but a high magma discharge rate. This sentence was fundamentally changed.

Line 14

“such eruptions mostly appear in geological records” meaning is unclear

Replace with “such eruptions have rarely been witnessed or described in historical records”

This sentence was deleted to concise the abstract. Instead, this is mentioned in the introduction section.

Line 38 – 41 – discussion of “small” volume vs “large” volume eruptions seems a little weak. What fundamentally should be the difference – is there a research question or hypothesis in this to be tested? There have been many submarine or lake-sourced surtseyan and phreatomagmatic and sub-glacial eruptions witnessed around the world and described in the scientific literature. These range from basalt (e.g., Ambae) to basaltic andesite (e.g., Eiyafjallajokul) to andesite (e.g., Ruapehu), just to name three of many examples.

The key questions of this study are how magma interacts with seawater in the shallow water environment and the relationship between mass discharge rate (particularly in high-discharge cases) and plume height in such conditions. We focus on the importance of the magma discharge rate and do not discuss small volume vs large volume.

Also, was this event truly that different in volume to other surtseyan or phreatomagmatic events? Perhaps it is larger, but likely by less than an order of magnitude?

Although the FOB-type phreatomagmatic eruption with a relatively high magma discharge rate and with sustained water-rich plume is rarely observed, and the detailed eruptive sequence has never been summarised, it may be challenging to say that this eruption was truly a much larger event (in terms of total volume) than other Surtseyan and phreatomagmatic events. Therefore, we avoid using ‘larger’ or ‘large-scale’ to describe this eruption.

The size or volume seems not as relevant to process as eruption rate(s) and the steadiness of mass ejection rate… perhaps that is where this paper should concentrate

We agree with this comment and focus on the importance of the mass discharge rate and its steadiness rather than size or volume. We changed the relevant phrases in the introduction section.

Line 60-62 – the sustained high plume is perhaps the most interesting thing about this eruption – how many hours did it last? Phreatomagmatic events are typically unsteady and involve many explosions with pauses between, such as in Surtseyan or vulcanian styles. Even the Eiya eruption produced a distinct, long-lived plume, but it was being produced by loosely spaced but staccato eruptions. The event described here is different in the steadiness.

The high eruption plume was maintained for at least 9 hours. We mentioned this. We understand that the unsteady pulsate explosions can generate a long-lived continuous plume. In the case of the FOB eruption, such pulsate explosions were observed in early Phase 1 and after Phase 1, in which explosions are characterised by strong infrasound, as indicated in Figure 2. However, infrasound was not so strong but continuous during the steady eruption plume period, especially after 15:00, even though the considerable eruption plume developed. This observation for Phase 1 is different from the typical Surtseyan explosions. We explain this in the result section.

Line 67-69

The trachydacitic composition has a high silica content, but it also has a very high alkali content, so that the magma is actually quite similar in viscosity to the basaltic andesites that typify most other “smaller” phreatomagmatic eruptions. Hence the “silicic” difference may be less important than implied in the text

We are not going to use ‘silicic large-scale’ because it may not capture the feature of the FOB eruption appropriately. We rephrased the last part of the abstract and introduction to avoid qualitative descriptions.

Line 76-83 – what does steady with fluctuations mean? How steady was it? There is mention of pulses but periods of sustained plumes – was the MER steady for periods – how long were they?

‘Fluctuations’ mean the fluctuations of plume diameter and infrasound signal. We mentioned this and added more explanations of Phase 1: In early Phase 1, strong infrasound signals were repeatedly observed, although the plume diameter was still small. After 12:00 JST, the plume became larger; contrarily, infrasound became relatively weak, excepting at 14:30–15:00, and continuous.

Line 91-106 The two periods of sustained columns are brief, 1 hr and 20 min. To what extent are the columns reflective of sustained eruption mass ejection rates? i.e., was the eruption pulsing during this time – like typical phreatomagmatic events or was it truly steady? Columns are reflective of the convection above an eruption site, even if it is unsteady – this is especially true if the plume was mainly steam.

The most vigorous eruption columns developed for 1 hr and 20 min. Strong infrasound signals, indicating intense explosions, were also detected during the same period. Although it is difficult to say if the magma discharge was entirely sustained at a high level, the development of the umbrella shape and the overshooting portion of the eruption column indicates the eruption column was sustained with a relatively high magma discharge rate in a certain period. Furthermore, after the most vigorous eruption columns (after 15:00), the large diameter of the eruption plume was also sustained without strong infrasound signals. This observation suggests that the eruption plume was not caused by the intense pulsating explosions such as typical Surtseyan but by the more sustained type of the eruption.

Line 150 – the models of a sustained plume are assuming that there is a substantive jet region – whereas this could have been generated from steam rising from vigourous surtseyan pulsing eruptions.

The satellite and infrasound data suggest that the initial of Phase 1 was characterised by increases in plume diameter and strong infrasound signals; however, during the most intense period, the eruption plume was sustained at a large diameter with continuous infrasound, but intense pulsating explosions were mostly not associated with the sustained plume after 15:00. We think that the assumption of the sustained plume (with the substantive jet region) is valid based on this observation.

Line 171-83 – the high eruption plume was mainly steam – how much of this was a convective thermal condition rather than an explosive one? Especially when there is so little ash, it is hard to justify that this was an especially vigorous eruptions, beyond the scale of a large surtseyan event. For example the Hunga volcano (referred to in the introduction) produced a series of surtseyan eruptions in 2024/15 that also produced plumes to similar heights.
Plume shapes such as an umbrella cloud at the tropopause, overshooting, and sustained features suggest that the plume was vigorous and convective, differing from the short-period explosive (thermal) types. The recent Hunga eruption (before the most explosive phase) produced similar-sized eruption plumes but different in shapes: the umbrella and overshooting portions were not developed like the FOB eruption. Our understanding is that such plumes were unsustained and caused by discrete, pulsating explosive events.

Line 184-192 suggests that the decoupling of the plume into the pyroclasts rich and steam rich portions – this is interesting, but is it the only mechanism to produce a pumice raft and steam plume? Could phase 1 not just represent the phase of a growing tephra ring on the ocean floor before it broached the surface and jetting is more visible?
We think that, during Phase 1, decoupled dense parts of the eruption jet formed submarine density currents and resulted in the formation of the foundation of the tuff ring on the ocean floor. This is mentioned in the second paragraph of the discussion and summary.

Line 216-223 the terminology questions seem premature if the mechanisms of this eruption remain quite speculative.
We think it is meaningful that the FOB-type eruption is compared with other typically observed eruption styles; however, we agree that it is not easy to propose a new classification term such as ‘Ultra-Surtsey’ without deposit data at this stage. Therefore, we only compare the FOB eruption with typical eruption styles.

Line 353 Volume estimation – the error ranges in the depth equivalent for the pumice raft are very large, is there any independent verification of this raft thickness? Could it not have been only one or two particles thick?

It is challenging to know the depth equivalent for the pumice raft because of no direct measurement data near the source. In the distal area such as Okinawa Islands, the thickness of the raft was from a couple of particles thick to ~10 cm thick in some cases. Still, this thickness reduction is probably due to disaggregation of the pumice raft during long drifting. We thought it is better to use the data of other known examples, such as Haver 2012 near the source. Brandl et al. (2020) also used similar values (0.1-0.5 m) to estimate pumice raft volume for the 2019 eruption of Volcano F, Tonga.

Plume modelling - the observation of little or no ash within the plume makes it difficult to directly relate the steam plume to a magma ejection rate, although the authors make a convincing attempt at working on this

It is difficult to discuss the amount of ash within the plume precisely; however, the heat balance between rising magma, water-rich eruption plume, and pumice raft will be an essential constraint for the nature of the water-rich eruption plume in the FOB eruption. Therefore, we mainly focus on the heat balance based on plume modelling. We think this way is the best and most helpful to understand the nature of the FOB eruption plume.

Composition – the high alkali contents of this composition likely make the viscosity very low – similar to basaltic andesites in calc-alkaline settings. Hence, the silica content in of itself should not be a major difference to other surtseyan cases, even if the temperature of the magma is lower.

We agree that the high alkali contents may show a similar feature to basaltic andesite in calc-alkaline settings. We are not going to emphasise the chemical composition of this eruption and its volume. Instead, we will focus on the eruption being a rare phreatomagmatic eruption with a high discharge rate in the relatively shallow water environment.

Response to Reviewer #2

There seems to be a lack of exact direction or what specifics processes are in question.
The specific processes in this study are how the magma with a high discharge rate interacts with ambient water in the shallow water environment and the relationship between mass discharge rate, particularly in high-discharge cases, and plume height in such conditions. We included the critical question in the Introduction section.

Some terminology needs revising to better fit with more conventional terms used for identifying and classifying eruptive styles.
'Silicic' and 'large-scale' were used to characterise the FOB eruption in the previous version of the manuscript; however, as pointed out by the reviewers, these terms may be inappropriate in the context of this paper. Therefore, we will not use them. Also, we will not use 'Ultra-Surtsey' because the deposit characteristics remain speculative.

More could be made of coupling the available infrasound and plume data to explain the eruptive sequence, and there are several important comparisons with other shallow submarine eruptions that could be made.
Throughout the manuscript, we added some explanations of the relationship between plume behaviours and infrasound. In the Result section, we added the following sentence: Infrasound became stronger corresponding to the development of the large eruption plume at ~14:30; however, it became weak but continuous during the eruption plume being steady at 15:00–20:00 JST on 13 August. In the result and discussion sections, we added comparisons with other representative shallow submarine eruptions such as Anak Krakatau, Bogoslof, HT-HH, and Kolumbo.

The discussion of some data in the extended figures is missing in the text that would be key to include.
We changed to include the figures of geochemistry and a vital result of the estimation of mass discharge rate.

The timing of this review also gives the authors the opportunity to integrate their study with a recent published study and another pre-print using similar data regarding this eruption.
Yoshida et al. and Fauria et al. papers are cited. We added the following sentence in the introduction: Although the outline of the 2021 FOB eruption has been studied from the

points of view of geochemistry (Yoshida et al.) and satellite observations (Fauria et al.), our high-resolution time-series records and quantitative analyses of eruption parameters such as mass discharge rate will provide a more comprehensive understanding of this explosive shallow-sea phreatomagmatic eruption.

There are important aspects from the other studies that should be referenced and discussed. We recognised several examples of phreatomagmatic explosions with various rock types in modern history. Therefore, we cited the relevant papers.

Specific comments:

Title. I do not think the word “first” is helpful to include in the title, as there are others that could put this claim into question.

We changed the title.

10. This opening line would be better suited identifying the hazardous nature of these eruptions instead of their frequency.

We changed ‘the most common styles’ to ‘the most hazardous types.’

13-15. This reads strangely, why are they “therefore poorly understood” if they “mostly appear in geologic records”? I’m unsure of this sentence’s intent.

We intended that ‘such eruptions have rarely been witnessed or described in historical records and the nature of this type of eruption is therefore poorly understood’. This sentence was deleted to concise the abstract. Instead, this is mentioned in the introduction section.

20. This may be an underestimate based on new submarine edifice volume (points later), but worth adding in briefly that this matches from SO2 estimates and physical topographic and raft area calculations.

We added that the magma volume, ~0.1 km^3, including the tuff cone and the pumice raft, is consistent with the SO_2 emission estimated from the satellite observation and geochemistry in the abstract.

24. What specific “processes”? This persists throughout the text. I am unsure what exact processes or dynamics the study is attempting to look at. Conduit, eruptive, style transitions, fragmentation? More specificity would be helpful in several places.

We specify the scientific question of this study: how the magma with a high discharge rate interacts with ambient water in a shallow water environment and its eruptive transition. We included this statement in several places in the abstract and introduction.

Intro. The paper needs to incorporate findings and data from the recently published Yoshida et al. (2022) geochemical/textural study of the FOB 2021 raft products. This study does not really overlap with your findings, but the geochemical and mineral data should be utilised into your interpretations and discussion of different eruptive styles and eruption dynamics. The other study is Fauria et al. (pre-print with ESSOAR) which similarly uses satellite and aerial observations to look at raft and plume dynamics. Some of the interpretations, volume estimates, are similar. But for this paper to be accepted, your study will need to standalone from this study – better use of your infrasound data, discussion on mass discharge rates, transitions in eruption style. Your paper presents an opportunity to include the interpretations of the Yoshida and Fauria studies for a more comprehensive understanding of the FOB 2021 eruption as a whole.

We briefly mentioned the Yoshida and Fauria studies in the introduction and emphasised the difference between our study and theirs. As pointed out here, our paper includes the infrasound data and discussions on mass discharge rates and transition in eruption style. We addressed this point throughout the manuscript.

38. Dry as in subaerial or anhydrous magma? Clarify.

This means subaerial conditions. We clarified this.

49. With more time passed since the Hunga Tonga-Hunga Ha'apai (HT-HH) eruption, I think this study would be served well with a little more comparison to this eruption, and other shallow/emergent eruption with good observations/deposits records – Bogoslof 2016-2017, the 2009 and 2014-15 HT-HH events (Brenna et al. 2022; Colombier et al., 2018), the 1650AD Kolumbo eruption, Santorini (Cantner et al., 2014; Fuller et al., 2018). The 2022 HT-HH event interestingly has a reverse phase behaviour to the FOB 2021 eruption, where eruptive activity started as emergent Surtseyan intermittent explosions, followed the larger explosion and plume production on Jan 14 and 15. One of our key tools in understanding submarine eruptions, is making sure we make detailed comparisons to similar (and different) behaviours at similar depths – particularly for the very shallow hazardous systems. More comparisons could be made in this paper, highlighting what similarities can tell us, but also recognising the uniqueness of individual submarine eruptions.

In the discussion section, we added a paragraph comparing the FOB eruption with other shallow submarine eruptions. We think that in the case of the 2022 HT-HH event, the deepening of the submarine vent in the early stage might make the rising magma more explosively interact with seawater in a high magma discharge rate and cause the powerful phreatomagmatic explosion in the later climactic stage. We emphasised this point and the importance of balancing the availability of external water, represented by eruption depth and magma discharge rate. Since the detailed process and mechanism of the 2022 HTHH event remain unclear, we would only compare the approximate sequences of these eruptions. We also cited relevant papers in the introduction and discussion sections.

50. Note here, the lack of ground-based observations, or proximal data collection, at this stage. Your selling point is the high-resolution time series analysis, make sure this is fully apparent in the introduction.
We add this point in the last part of the first paragraph of the introduction.

53. Again more specificity needed for what "processes" you are looking at.
We are looking at the process of explosive interaction between the magma with a high discharge rate and ambient seawater involved with phreatomagmatic explosions in a shallow sea environment. We specify this point in the introduction.

54. "most" missing?
We added 'most' before 'poorly'.

55. Odd use of the word "process". Eruption dynamics, styles?
We amended this sentence. We analyse the surface phenomena and their transitions of this eruption using satellite imagery, aerial photos, infrasound data, plume modelling, and geochemistry, and discuss the interaction between magma and ambient seawater, which caused a water-rich sustained plume.

64. Will need to reference Yoshida et al. (2022) and Fauria et al., in several cases here.
We cited these papers in this section.

66. Your geochemical data needs to be compared with the variety observed and analysed in Yoshida et al. (2022). This data needs adding onto the TAS plot in the extended data. I think it is important to state your geochemical data better in the main text results and not within the later methods section. I saw no figure of table for your melt inclusion and

matrix glass S data. This needs including somewhere to be available to readers and reviewers.

We revised the figures related to geochemistry to include the results of Yoshida et al. and explained this in the section on chemical composition. The section title was changed to 'Chemical composition of magma and SO_2 emission'.

69. I would disagree with this statement, reading it as it is. There are others (intermediate to silicic) that have been documented/recorded in the last few decades, just not as directly observed in real time via high resolution satellite imagery and aerial imagery. This needs stating. Other eruptions in the Tofua Arc (Volcano F 2019 – andesite-dacite), the 1962 Protector Shoal seamount eruption (rhyolite), Bogoslof 2016-17 (high SiO2 trachyandesite, just a little less SiO2 than FOB).

We changed the sentence to include these examples in the introduction section, and don't use 'first silicic'.

75-87. This section of phase descriptions would be better described using more familiar and translatable terms of sustained/unsustained and steady/unsteady behaviour.

We changed to use more familiar and translatable terms in the section on phase descriptions as requested.

77-78. Pulsating how? In mass discharge, plume size, as well as infrasound? I would make more use of the infrasound data if possible. This is not included int the other FOB 2021 studies, so it is a useful dataset to make more use of here, particularly relating different eruptive styles. I do appreciate that this source is a little distant for smaller signal resolution. Some more direct coupling of the plume behaviour and infrasound data would work well.

Phase 2 was a pulsating unsteady phase characterised by frequent strong infrasound signals and smaller eruption plumes. We clarified this in the text. We also added the relationship between plume behaviour and infrasound, particularly in Phase 1. We found that strong infrasound signals accompany the development of the eruption plume; however, during the period the plume is sustained over hours after 15:00 on 13 August, infrasound signals were relatively weak but continuous, indicating the eruption is not the same as typical Surtseyan eruptions which are characterised by discrete explosions with strong infrasound signals as seen in Phase 2 and 3.

81. How long does this decay last until the eruption is considered over? The Phase 4 plume diameter data (fig 2) also needs explaining briefly.

We added a brief explanation as to the following. In Phase 4, the activity decayed with the decrease of plume radius to less than a few km and mostly ceased in 15 hours.

87. Do your observations add anything new to the observations and time series from Fauria et al.?

Fauria et al. used satellite data to construct the chronology of the FOB eruption but couldn't divide eruptive phases. In contrast, we used satellite and infrasound data and revealed that the FOB eruption could be divided into phases 1 to 4, showing different infrasound characteristics. We also clarified the relationship between plume behaviours and infrasound. Furthermore, Fauria et al. focused on the generation and development of pumice rafts. They concluded that the pumice raft was primarily created by ballistic delivery of clasts to the ocean surface from only Surtseyan explosions. Contrarily, we proposed that some eruption phases were sustained not only by tephra jets such as Surtseyan explosions but also by the development of a sustained eruption plume. We also suggested that pumice clasts were directly caused by the submarine vent and transported by gravity currents. Our study also has a unique discussion of the volume using the sulfur data. These points are different from Fauria et al.

115. How does this thermal anomaly compare with other shallow/emergent eruptions? Is lack of a thermal anomaly a common feature in large submarine eruptions? If so, its is an important observation to note for understanding heat transfer, dynamics of incorporating seawater etc. (Note, this was the case, even for an eruption as large as the 2021 Havre eruption – just one thermal MODIS pixel).

A thermal anomaly was undetected during the most intense phase of the shallow marine eruption at Anak Krakatau in 2018. This was probably because of cooled pyroclastic materials and the eruption site being covered by a relatively thick water-rich plume, even if there was a small hot spot. These effects of cooling and water-rich plume are similar to the FOB eruption. For Plinian eruptions, immediately before or after developing a strong eruption column, thermal anomalies can be often detected by satellite. Although we cannot say if the features of thermal anomalies can be clues for quantitatively understanding the dynamics of the submarine eruption or not, listing similar examples will be meaningful. We added the example of Anak Krakatau in the section on Thermal anomaly.

127. At what stage is this number from? Is the volume estimate before volume removal in the center of the cone? When in your figure 2 timeline is the estimate?

The volume estimate was carried out using the aerial photos taken after the eruption. Therefore, the volume of tuff cone 0.04–0.07 km^3 is the value in estimation for the timing at the end of the eruption. We clarified this in the text.

138. Similar conclusion to the Fauria et al. study, good to note that.

The timing of observation of pumice raft is almost the same as Fauria et al. We noted their volume estimation: 'Fauria et al. also estimated the tephra volume as ~0.1 km^3 for this eruption'.

156-166. This is where more of your geochemical data needs including. How does your data sit within the data from Yoshida et al. (2022)? The volume estimate from the SO2 data is a good find. How does this compare with the TROPOMI satellite SO2 estimates in Fauria et al.?

We briefly explained chemical compositions in this section and cited Yoshida et al. work. Fauria et al. showed satellite SO_2 observation data; however, they didn't estimate the total mass of SO_2 emission but only mentioned the amount per area using Dobson Unit. Therefore, it is difficult to compare our data with them directly.

189. Unsure about that. The increasing temperature of vaporization and heat capacity of seawater within increasing pressure make this process quite different much deeper down, even more at depths >29 MPa where vapor becomes supercritical. Worth looking at some detailed modelling of submarine plume behaviour by Cahalan and Dufek (2020), in particular, the ingestion of seawater and residence of vapor within the plume relative to depth. Murch et al. (2021) also presented some modelling of gas expansion behaviour and seawater heating in deep to shallow submarine volcanism.

We agree that the increasing temperature of vaporisation and heat capacity of seawater within increasing pressure make this process quite different. We think that the effect of sea depth is limited in the case of the FOB eruption because the depth of the initial vent was <100 m, which means that the water pressure is less than 1 MPa. We modified the sentence as the followings: the gas–pyroclast mixture above the submarine vent (50–100 m b.s.l.) may penetrate the seawater and reach the atmosphere if the magma discharge rate is relatively high ~10^5–10^6 kg/s because the effects of water pressure and density change of the jet centre due to mixing with ambient water are limited in such situations.

We cited Cahalan and Dufek (2020) here. They addressed that if the water depth is less than 200 m and the mass discharge rate is like subplinian eruptions, the eruption plume can breach the sea surface. We are also not going to mention the similarity to deep submarine conditions.

192. Modelling has shown that this flash effect may be very limited in comparison to the size of the plume itself. It is worth noting that not much of the seawater is “flashing” to steam, or it makes it sound as if this is the source of the whole subaerial plume.
The flash effect may be limited but contributes to the rapid acceleration of the jet. We modified the relevant sentence: a breaching part into the atmosphere caused by the upward motion of a gas-particle jet, where the particles transfer heat rapidly to seawater that changes to steam and contributes to the rapid acceleration.

194. Lighter?
We changed ‘the mixture’ to ‘the lighter mixture’.

196. How much does this match with experimental plume and current modelling e.g. Gilchrist et al. (2021) and Newland et al. (2022)?
These papers are dealing the development of buoyant plumes in stratified fluids with different densities. There are some similarities but differences in the situation with strong density contrast between water and atmosphere. We think no appropriate previous works deal the jet behaviour in a shallow water environment, as seen in the FOB eruption.

199. So how much material might we expect in the submarine portions of the edifice and proximally settled deposits? Does the SO2 calculation of 0.11km3 in fact underestimate total DRE? How is SO2 injection vs total eruptive volume reflected in other eruptions? My understanding is that there is sometimes a deficit of SO2 output relative to volumes estimated from deposits. The caveats of the SO2 calculation will need discussing in the methods.
We estimated the volume of submarine portions of the edifice and proximally settled deposits as 0.03-0.06 km^3 based on the bathymetry map before the eruption and the shape of the new islets.
In many arc volcanoes, the remotely measured SO_2 mass is larger than estimated based on geology and petrology that use the tephra volume and the difference between melt inclusion and groundmass glass S concentrations. This discrepancy is generally called ‘excess sulfur’ or ‘excess degassing’ (e.g., Shinohara 2008). In the case of the FOB

eruption, a portion of S exsolved from melt may have been dissolved into the seawater. Although the satellite-based estimation and the deposit-based estimation agree in our study, the dissolution of S might have affected this balancing. We discussed this issue in the subsection '*Errors in SO_2 estimation*'. Also, other processes may affect the SO_2 calculations: S uptake during crystallisation and the error in petrological estimation, S dissolution in seawater, and the effect of mafic minerals. These are explained in answer to the later comment and mentioned in the method section.

207. Your fig 3. makes an interesting depiction of Phase 2 and 3 as a vent system closed by the new cone deposits. If you think the build-up of the cone to an emergent edifice was key in controlling eruptive transition behaviour, it needs stating more clearly. Your figure shows this very nicely! It would be a key observation if Surtseyan eruptions are controlled by the availability of external water vs. shallow eruption directly into the open water fuelling sustained eruptive subaerial plumes. Again, I am thinking about the reverse change during the Hunga Tonga-Hunga Ha'apai event.
We agree with this comment and add the following in the discussion section. The build-up of the cone to an emergent edifice was probably a key in controlling eruptive transition behaviour from Phase 1 to Phases 2 and 3. Surtseyan eruptions in the later phase might be governed by the availability of external water vs shallow eruptions in the open water fuelling sustained subaerial plumes.

208. I think you can sell more the discussion of eruptive transitions during this shallow-emergent eruption. The changing behaviour from more Plinian-style column to Surtseyan activity… This was also observed partly for the Kolumbo 1650 AD eruption (Cantner et al. 2014) and was observed reversely for the HT-HH eruption in January.
We added the comparisons of eruption transitions with other shallow submarine eruptions in the third paragraph of the discussion section.

213. References for this? Are you referring to specific other examples with these criteria? From your Plumeria modelling, there also no discussion of the gas velocity calculations. Do you need to include them in your extended data if you don't discuss them?
After this sentence, we refer to some examples of the eruption transition in the shallow sea environment, probably due to changes in eruption depth and magma discharge rates. In Plumeria modelling, we need to give 'exit velocity' as indicated in Figure 4, Supplementary Figure 5, and Supplementary Note 1. The range of the possible exit velocity to achieve the mass discharge rate explaining the observed plume height is

approximately 100-120 m/s for the vent of 100 m diameter. We added the exit velocity estimated from the modelling in the text.

217. Missing “style”?

We added ‘style’ between ‘eruption’ and ‘into.’

219. How do textural features observed in the Yoshida (2022) pumice reflect potential magmatic processes? They noted a significant array of textures and colours of clasts. I see in your extended data you observed the same, but there is no mention of this.

We also found textural variations in pumice clasts, as Yoshida et al. indicated; however, quantitative textural analyses such as bubble size distribution and population density have not been done yet; therefore, it is difficult to discuss the magma ascent process at this stage. This kind of work should be summarised in other papers. On the other hand, we identified the similarity of our data on textures and colours of pumice to those of Yoshida et al. data. This point is mentioned in the section on sampling and chemical analysis.

222. Different how?

We changed the sentence to be more apparent. We have no evidence that a large amount of fine-grained ash was generated in the FOB eruption. The inferred dominance of near-vent depositional processes strongly suggests that widespread dispersal of ejecta was minimal.

223. Use of the term “large-scale” needs some relative comparison at some point – quite early on ideally. This event was certainly not large scale in comparison to the HT-HH event.

We will not use ‘large-scale’ because of its qualitative description.

228. State for what purpose here. What specifics do we need to measure on the seafloor for better understanding? Mainly I am thinking volume and extent of new submarine deposits.

We added the sentence: Tephra stratigraphy, thickness variation, and sedimentary structure of the submarine deposits will be crucial to unveiling the detailed chronology and dynamics of the eruption.

230. I was left a little unsure of the main takeaways from your analysis here. You could

also explicitly state the value added from your data in comparison to the other recent literature on the eruption (Yoshida and Fauria).

We added our achievements in the last paragraph: High-resolution time-series records of infrasound and satellites and quantitative analyses of eruption parameters such as volumes and mass discharge rates enabled us to comprehensively advance our understanding of the 2021 FOB eruption. Our key finding is that even if the major portion of the pyroclastic materials ejected from the submarine vent is not released into the atmosphere, it can cause the eruption plume to grow significantly when the magmatic heat is efficiently consumed to vaporise the seawater in the shallow sea environment. Therefore, the relationship between the erupted mass and the plume height in the explosive marine eruptions is not straightforward as proposed for the on-land eruptions.

361. What is the justification of the 1250 kg/m3 value in comparison to the pumice value? Based on ash deposits? How do theses value compare with Yoshida et al. textural data from the raft?

The deposit density of the tuff cone should have been assumed to have a specific range. In the revised version of the manuscript, the density as 1000-1500 kg/m^3 was considered for tuff cone based on some previous studies for pyroclastic flow deposits. For example, Scott et al. (1996) adapted the value of 1000-1300 kg/m^3 for the bulk density of the Pinatubo pyroclastic flow deposits based on their laboratory measurements. We think that the comparison with Yoshida et al. textural data from the raft is not so important because here, the density of tuff cone is dealt with. The density of 500-800 kg/m^3 for pumice raft is reasonable based on our preliminary pumice density data.

395-406. This data needs presenting in the results or more in the extended figures, not here in the methods.

We moved the chemical composition part in the method section to the result section and added relevant figures.

405. What about accounting for S uptake during crystallisation? Is the assumption that all S loss from XRF to matrix glass is through degassing at the vent? Would some S quickly dissociate in the seawater?

The errors in SO_2 estimation are caused by some magmatic and eruptive processes and are evaluated below. 1) *Effects of uptake during crystallisation and analytical errors*: For melt inclusions, we assumed it is unnecessary to consider the change of S concentration during crystallisation because there is no evidence of post-entrapment crystallisation of host minerals, such as systematic compositional change near the melt-crystal contact at least for the analysed crystals. On the other hand, the average value of the S concentration of melt inclusions (C_{s_mi}) may underestimate the initial S concentration in the magma reservoir because melt inclusions can experience gas leakage and may not reflect the original concentration. For groundmass, the average value of the S concentration of groundmass glass (C_{s_gm}) may overestimate the residual sulfur after the eruption because only glassy parts were analysed; crystallised parts where sulfur was thoroughly degassed cannot be analysed. Therefore, our petrological estimation of the degassed sulfur ($\Delta C_s = C_{s\_mi} - C_{s\_gm}$) indicates a minimal case. If ΔC_s is larger, the volume of erupted magma will be smaller. 2) *Effect of mafic minerals*: Mafic minerals such as pyroxenes and olivine contain glassy inclusions (52–54 wt.% SiO_2) with higher sulfur concentrations than those of plagioclase shown in Fig. 3. However, the mafic minerals are not major components of the erupted magma, and their contribution to the degassed sulfur is very limited. Even if we consider the effect of mafic minerals, the result is not changed.

Based on the above considerations, we assumed that the S concentration in the magma reservoir just before the eruption is represented by the S concentration of melt inclusions in plagioclase.

Effect of dissociation into the seawater: Although a portion of S exsolved from groundmass glass during magma ascent might have been dissolved into the seawater, the eruption site is a shallow water environment, and the detailed mixing process between the magma with a high discharge rate and surrounding water cannot be evaluated. At this stage, it is difficult to quantitatively estimate the amount of S loss due to dissolution from erupted products into the seawater. This study assumes the S loss occurs only during magma ascent.

The above is included in the new subsection '*Errors in SO_2 estimation*' in the method section.

Figures:

459. Images i) to v) need marking more clearly on the b) data. A thin red line at each time would work well.

We used thin red lines to connect images in (a) and the positions in (b) data.

469. The idea that the vent become closed with the growth of the cone needs discussing more, and also labelled as a key feature to help explain later behaviour.

We added a brief explanation in the figure caption. Surtseyan eruptions in the later intermittent phase might have been controlled by the availability of external water vs. shallow eruptions, due to the build-up of the cone.

526. Could you include some discussion of the textural variety and the similarities to Yoshida et al.?

We added some explanation on pumice variation as the following. Although there are textural and colour variations, whole-rock major element compositions are not varied. Most of the pumice clasts (more than 90%) are white-grey, as shown in No. 2a, 2b, and 3 and FKT211008-2 and 3. Black-coloured portions (FKT211004-7) and individual black pumice (FKT211008-4a, b, and c) are minorly present. The black colour has been interpreted as reflecting higher groundmass crystallinity due to nano-scale crystallization (Yoshida et al. 2022).

529. Include the Yoshida et al. data on this plot, and the rest of the geochemical data needs presenting either in plots or tables.

The figure was revised. We included the Yoshida et al. data and our glass composition data on this plot. This was shown as the main figure (Fig. 3) together with SO_3 data. Other relevant geochemical data were included in the appendix tables.

Response to Reviewer #3

Comments:

Troubled by the description of this as a "large-scale silicic" eruption, simply because the volume erupted is small, on the order of a tenth of a cubic km. Even eruptions such as Mt St Helens, of about one cubic km, are not generally considered "large-scale", which seems more appropriate for something like Pinatubo.

We agree that the description of “large-scale” is qualitative and causes trouble if we compare it with other examples. We avoid the use of “large-scale” for the FOB eruption. Instead, we use ‘sizeable’ or ‘substantial’.

100 The whiteness of the eruptive plume indicates lots of water, but not specifically seawater. A separate argument needs to be made for this.

We use ‘water-rich’ eruption, which is more appropriate to describe the eruption, rather than “steam-rich”.

133 Can the factor of two range in estimated tuff cone volume, from 0.04 to 0.07 km3, be improved?

At this stage, it is difficult to estimate a more precise volume with a smaller range. We would remain this volume because it doesn’t affect the total volume so much.

147 Same comment on the factor of 3 range in pumice raft volume.

Unfortunately, we have no data on the direct measurement of the thickness of the pumice raft. We can only refer to the past similar events like the 2012 Havre eruption.

150 Not all water in an eruption column is vapor – plume is water-rich, not specifically steam rich.

We change “steam-rich” to “water-rich”.

153 External and magmatic water are subject to phase changes.

We agree and add magmatic water as well.

157 The "fraction" in question here has a huge range, depending on the previous two ranges for the pumice raft and the tuff cone – this should be made explicit

The results indicate that a magma discharge rate of 3–6 × 10^5 kg/s is sufficient to explain the observed plume height if a 5-50% fraction of erupted magma (0.3–3 × 10^5 kg/s) goes into the plume. We mentioned this in the text.

188 FOB's eruption plume reached the tropopause. So did the plume from Surtsey, on multiple occasions. Reaching the tropopause is probably a more significant measure than absolute height of the plume.

We agree with this comment and emphasise ‘tropopause’ rather than the fact reaching 10 km at several places.

200 This was a very shallowly submerged vent – please present evidence that fragmentation and formation of an eruption plume preceded interaction with water. Saying that it might work like a deep-water eruption seems odd – surely the satellite information, and formation of a subaerial tuff cone, implies very different behavior from a fully submerged, deep-water, eruption.

We should have used ‘jet’ rather than ‘plume’ for the gas-pyroclast mixture just after the eruption to avoid misunderstanding. The description of the similarity to deep submarine eruption also causes misunderstanding; therefore, we avoid it.

207 To me the discussion here is quite confusing. When do particles transfer their heat, and is it being used to warm air to make a buoyant column, or to drive fragmentation through thermal interaction that makes water vapour that subsequently condenses in the eruption column, reducing its buoyancy?

We agree that there were inappropriate descriptions in the previous manuscript. We modified it to ‘a breaching part into the atmosphere caused by the upward motion of a gas-particle jet, where the particles transfer heat rapidly to seawater that changes to steam and contributes to the rapid acceleration of the jet.’

Please take care to address separately interactions involving the erupting jet, vs. those taking place in the convective column.

We carefully addressed separately phenomena involving the erupting jet and those in the plume or column. We use ‘jet’ for the gas-pyroclast mixture above the vent in a shallow water environment and used the plume or column for the subaerial part.

219 A "slurry gushing from the submarine vent" is not consistent with water-surface breaching and generation of a subaerial jet and plume.

Our description was poor. We should use ‘jet margin’ and modify the sentence to “A large amount of floating pumice might result from sedimentation from subaerial PDCs and/or directly from the jet margins.

241 This eruption did not produce a large volume of magma. It produced a tuff cone, suggesting limited transport for the main mass of ejecta. There is little about the inferred eruptive processes that seems similar to Surtsey, at least from what's presented here, so it seems odd to name the eruption as an "ultra" version of Surtsey.

We are not going to use ‘Ultra-Surtsey’ in the revised version, because the deposit characteristics remain unknown.

2nd Aug 22

Dear Dr Maeno,

I hope you are well?

Please allow me to apologise for the delay in sending a decision on your manuscript titled "First timeseries record of a large-scale silicic shallow-sea phreatomagmatic eruption". It has now been seen by 3 reviewers, whose detailed 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. In addition to the reviewer's comments, we would like you to consider the following editorial thresholds when revising your manuscript:

- Define clear, science-focused research questions or hypotheses that are tested with your observations and present a conceptual advance in our understanding of phreatomagmatic eruptions

- Discuss your interpretations fully in the context of published literature on other submarine/phreatomagmatic eruptions, and particularly the existing literature available for the Fukutoku-Oka-no-Ba eruption.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would 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.

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

If the revision process takes significantly longer than three months, we will 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.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not 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 reviewers’ comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any 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 **

Please do not hesitate to contact me 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,

Emma Liu, PhD
Editorial Board Member
Communications Earth & Environment
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Joe Aslin
Senior Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This paper is a clear description of an interesting eruption. Eruptions in submarine settings are not often well described. In this case the geophysical and satellite data are a strength, but there is little local observation of the actual eruption events during the main phase and the deposits were not accessible. Hence the manuscript makes a convincing argument, but much of it is based on theory and generalised interpretation. There are several places, however in the manuscript, where there are alternatives to the statements and conclusions. In particular, a lot is made of the differences of this event to "typical" surtseyan or phreatomagmatic events, such as the high Si content and the volume. However, this is a trachytic magma with a very high alkali content, so in terms of its physical properties, particularly its rheology, it is likely behaving more like a lower silica-content (basaltic andesite) in a calc-alkaline setting (albeit erupted at a lower temperature). Further, the volume estimations are quite approximate, and not enormous in comparison to past surtseyan eruptions. The lack of significant ash in the plume and the formation of a local tuff ring, could also be argued as being a typical energetic surtseyan event.

The paper body is well written, but it is a little "dry" and descriptive. It provides a chronology based report of the eruption – but it doesn't pose any major questions or hypotheses at the outset that are tested with these observations.

The abstract could more clearly link statements to build the story – it describes essentially a timeline of events and a few key summary facts. I believe that the scientific contribution of this work is greater than just a description of events – this comment (i.e., going beyond a descriptive volcanic event report) needs to be considered throughout the paper – why is the "timeseries" important (e.g.., the title is based on this "report" style also), what are the key questions that this study seeks to answer? Why is this eruption relevant, beyond its rarity in composition? This is currently written as an eruption report, of an albeit unusual eruption, but to have a greater impact, it should be framed around discovering or elucidating new insights into submarine volcanic phenomena via this event…

Interesting contributions include why the eruption produced so little ash in the plume (steam/water dominated), yet a large pumice raft and tuff ring. This appears to be a "normal" phreatomagmatic eruption despite the unusual trachytic composition

Line 13 – not all voluminous silicic eruptions have a high eruptive rate?

Line 14
"such eruptions mostly appear in geological records" meaning is unclear
Replace with "such eruptions have rarely been witnessed or described in historical records"

Line 38 – 41 – discussion of "small" volume vs "large" volume eruptions seems a little weak. What fundamentally should be the difference – is there a research question or hypothesis in this to be tested? There have been many submarine or lake-sourced surtseyan and phreatomagmatic and sub-glacial eruptions witnessed around the world and described in the scientific literature. These range from basalt (e.g., Ambae) to basaltic andesite (e.g., Eiyafjallajokul) to andesite (e.g., Ruapehu), just to name three of many examples.

Also, was this event truly that different in volume to other surtseyan or phreatomagmatic events? Perhaps it is larger, but likely by less than an order of magnitude?

The size or volume seems not as relevant to process as eruption rate(s) and the steadiness of mass ejection rate… perhaps that is where this paper should concentrate

Line 60-62 – the sustained high plume is perhaps the most interesting thing about this eruption – how many hours did it last? Phreatomagmatic events are typically unsteady and involve many explosions with pauses between, such as in Surtseyan or vulcanian styles. Even the Eiya eruption produced a distinct, long-lived plume, but it was being produced by loosely spaced but staccato eruptions. The event described here is different in the steadiness.

Line 67-69
The trachydacitic composition has a high silica content, but it also has a very high alkali content, so that the magma is actually quite similar in viscosity to the basaltic andesites that typify most other "smaller" phreatomagmatic eruptions. Hence the "silicic" difference may be less important than implied in the text

Line 76-83 – what does steady with fluctuations mean? How steady was it? There is mention of pulses but periods of sustained plumes – was the MER steady for periods – how long were they?

Co- eruptive generation of the pumice plume is interesting and this process could be a good focus…

Line 91-106 The two periods of sustained columns are brief, 1 hr and 20 min. To what extent are the columns reflective of sustained eruption mass ejection rates? i.e., was the eruption pulsing during this time – like typical phreatomagmatic events or was it truly steady? Columns are reflective of the convection above an eruption site, even if it is unsteady – this is especially true if the plume was mainly steam.

Line 150 – the models of a sustained plume are assuming that there is a substantive jet region – whereas this could have been generated from steam rising from vigourous surtseyan pulsing eruptions.

Line 171-83 – the high eruption plume was mainly steam – how much of this was a convective thermal condition rather than an explosive one? Especially when there is so little ash, it is hard to justify that this was an especially vigorous eruptions, beyond the scale of a large surtseyan event. For example the Hunga volcano (referred to in the introduction) produced a series of surtseyan eruptions in 2024/15 that also produced plumes to similar heights.

Line 184-192 suggests that the decoupling of the plume into the pyroclasts rich and steam rich portions – this is interesting, but is it the only mechanism to produce a pumice raft and steam plume? Could phase 1 not just represent the phase of a growing tephra ring on the ocean floor before it broached the surface and jetting is more visible?

Line 216-223 the terminology questions seem premature if the mechanisms of this eruption remain quite speculative.

Line 353 Volume estimation – the error ranges in the depth equivalent for the pumice raft are very large, is there any independent verification of this raft thickness? Could it not have been only one or two particles thick?

Plume modelling - the observation of little or no ash within the plume makes it difficult to directly relate the steam plume to a magma ejection rate, although the authors make a convincing attempt at working on this

Composition – the high alkali contents of this composition likely make the viscosity very low – similar to basaltic andesites in calc-alkaline settings. Hence, the silica content in of itself should not be a major difference to other surtseyan cases, even if the temperature of the magma is lower.

Reviewer #2 (Remarks to the Author):

Review comments for Maeno et al.
General comments:
The paper presents a compilation of remote sensing and infrasound data for the shallow submarine 2021 Fukutoku Ok-an-oba eruption. The data is used to divide the eruption into four phases with distinct characteristics. The paper also includes geochemical data which is utilised to estimate eruption volume from volatile differencing, but this method and interpretation has its caveats. The paper is well written with clear figures, however, there seems to be a lack of exact direction or what specifics processes are in question. Some terminology needs revising to better fit with more conventional terms used for identifying and classifying eruptive styles. More could be made of coupling the available infrasound and plume data to explain the eruptive sequence, and there are several important comparisons with other shallow submarine eruptions that could be made. The discussion of some data in the extended figures is missing in the text that would be key to include. The timing of this review also gives the authors the opportunity to integrate their study with a recent published study and another pre-print using similar data regarding this eruption. The authors will want to make sure their work and dataset interpretations are standalone from these other studies. However, there are important aspects from these studies that should be referenced and discussed. I would recommend this paper requires major revisions for more attention to specific questions and processes, and incorporating recent literature on the eruption.

Specific comments:
Title. I do not think the word "first" is helpful to include in the title, as there are others that could put this claim into question.
10. This opening line would be better suited identifying the hazardous nature of these eruptions instead of their frequency.
13-15. This reads strangely, why are they "therefore poorly understood" if they "mostly appear in geologic records"? I'm unsure of this sentence's intent.
20. This may be an underestimate based on new submarine edifice volume (points later), but worth adding in briefly that this matches from SO2 estimates and physical topographic and raft area calculations
24. What specific "processes"? This persists throughout the text. I am unsure what exact processes or dynamics the study is attempting to look at. Conduit, eruptive, style transitions, fragmentation? More specificity would be helpful in several places.
Intro. The paper needs to incorporate findings and data from the recently published Yoshida et al. (2022) geochemical/textural study of the FOB 2021 raft products. This study does not really

overlap with your findings, but the geochemical and mineral data should be utilised into your interpretations and discussion of different eruptive styles and eruption dynamics. The other study is Fauria et al. (pre-print with ESSOAR) which similarly uses satellite and aerial observations to look at raft and plume dynamics. Some of the interpretations, volume estimates, are similar. But for this paper to be accepted, your study will need to standalone from this study – better use of your infrasound data, discussion on mass discharge rates, transitions in eruption style. Your paper presents an opportunity to include the interpretations of the Yoshida and Fauria studies for a more comprehensive understanding of the FOB 2021 eruption as a whole.
38. Dry as in subaerial or anhydrous magma? Clarify.
49. With more time passed since the Hunga Tonga-Hunga Ha'apai (HT-HH) eruption, I think this study would be served well with a little more comparison to this eruption, and other shallow/emergent eruption with good observations/deposits records – Bogoslof 2016-2017, the 2009 and 2014-15 HT-HH events (Brenna et al. 2022; Colombier et al., 2018), the 1650AD Kolumbo eruption, Santorini (Cantner et al., 2014; Fuller et al., 2018). The 2022 HT-HH event interestingly has a reverse phase behaviour to the FOB 2021 eruption, where eruptive activity started as emergent Surtseyan intermittent explosions, followed the larger explosion and plume production on Jan 14 and 15. One of our key tools in understanding submarine eruptions, is making sure we make detailed comparisons to similar (and different) behaviours at similar depths – particularly for the very shallow hazardous systems. More comparisons could be made in this paper, highlighting what similarities can tell us, but also recognising the uniqueness of individual submarine eruptions.
50. Note here, the lack of ground-based observations, or proximal data collection, at this stage. Your selling point is the high-resolution time series analysis, make sure this is fully apparent in the introduction.
53. Again more specificity needed for what "processes" you are looking at.
54. "most" missing?
55. Odd use of the word "process". Eruption dynamics, styles?
64. Will need to reference Yoshida et al. (2022) and Fauria et al., in several cases here.
66. Your geochemical data needs to be compared with the variety observed and analysed in Yoshida et al. (2022). This data needs adding onto the TAS plot in the extended data. I think it is important to state your geochemical data better in the main text results and not within the later methods section. I saw no figure of table for your melt inclusion and matrix glass S data. This needs including somewhere to be available to readers and reviewers.
69. I would disagree with this statement, reading it as it is. There are others (intermediate to silicic) that have been documented/recorded in the last few decades, just not as directly observed in real time via high resolution satellite imagery and aerial imagery. This needs stating. Other eruptions in the Tofua Arc (Volcano F 2019 – andesite-dacite), the 1962 Protector Shoal seamount eruption (rhyolite), Bogoslof 2016-17 (high SiO2 trachyandesite, just a little less SiO2 than FOB).
75-87. This section of phase descriptions would be better described using more familiar and translatable terms of sustained/unsustained and steady/unsteady behaviour.
77-78. Pulsating how? In mass discharge, plume size, as well as infrasound? I would make more use of the infrasound data if possible. This is not included int the other FOB 2021 studies, so it is a useful dataset to make more use of here, particularly relating different eruptive styles. I do appreciate that this source is a little distant for smaller signal resolution. Some more direct coupling of the plume behaviour and infrasound data would work well.
81. How long does this decay last until the eruption is considered over? The Phase 4 plume diameter data (fig 2) also needs explaining briefly.
87. Do your observations add anything new to the observations and time series from Fauria et al.?
115. How does this thermal anomaly compare with other shallow/emergent eruptions? Is lack of a thermal anomaly a common feature in large submarine eruptions? If so, its is an important observation to note for understanding heat transfer, dynamics of incorporating seawater etc. (Note, this was the case, even for an eruption as large as the 2021 Havre eruption – just one thermal MODIS pixel).
127. At what stage is this number from? Is the volume estimate before volume removal in the center of the cone? When in your figure 2 timeline is the estimate?
138. Similar conclusion to the Fauria et al. study, good to note that.
156-166. This is where more of your geochemical data needs including. How does your data sit within the data from Yoshida et al. (2022)? The volume estimate from the SO2 data is a good find. How does this compare with the TROPOMI satellite SO2 estimates in Fauria et al.?

189. Unsure about that. The increasing temperature of vaporization and heat capacity of seawater within increasing pressure make this process quite different much deeper down, even more at depths >29 MPa where vapor becomes supercritical. Worth looking at some detailed modelling of submarine plume behaviour by Cahalan and Dufek (2020), in particular, the ingestion of seawater and residence of vapor within the plume relative to depth. Murch et al. (2021) also presented some modelling of gas expansion behaviour and seawater heating in deep to shallow submarine volcanism.
192. Modelling has shown that this flash effect may be very limited in comparison to the size of the plume itself . It is worth noting that not much of the seawater is "flashing" to steam, or it makes it sound as if this is the source of the whole subaerial plume.
194. Lighter?
196. How much does this match with experimental plume and current modelling e.g. Gilchrist et al. (2021) and Newland et al. (2022)?
199. So how much material might we expect in the submarine portions of the edifice and proximally settled deposits? Does the SO2 calculation of 0.11km3 in fact underestimate total DRE? How is SO2 injection vs total eruptive volume reflected in other eruptions? My understanding is that there is sometimes a deficit of SO2 output relative to volumes estimated from deposits. The caveats of the SO2 calculation will need discussing in the methods.
204. An interesting observation to note!
207. Your fig 3. makes an interesting depiction of Phase 2 and 3 as a vent system closed by the new cone deposits. If you think the build-up of the cone to an emergent edifice was key in controlling eruptive transition behaviour, it needs stating more clearly. Your figure shows this very nicely! It would be a key observation if Surtseyan eruptions are controlled by the availability of external water vs. shallow eruption directly into the open water fuelling sustained eruptive subaerial plumes. Again, I am thinking about the reverse change during the Hunga Tonga-Hunga Ha'apai event.
208. I think you can sell more the discussion of eruptive transitions during this shallow-emergent eruption. The changing behaviour from more Plinian-style column to Surtseyan activity… This was also observed partly for the Kolumbo 1650 AD eruption (Cantner et al. 2014) and was observed reversely for the HT-HH eruption in January.
213. References for this? Are you referring to specific other examples with these criteria? From your Plumeria modelling, there also no discussion of the gas velocity calculations. Do you need to include them in your extended data if you don't discuss them?
217. Missing "style"?
219. How do textural features observed in the Yoshida (2022) pumice reflect potential magmatic processes? They noted a significant array of textures and colours of clasts. I see in your extended data you observed the same, but there is no mention of this.
222. Different how?
223. Use of the term "large-scale" needs some relative comparison at some point – quite early on ideally. This event was certainly not large scale in comparison to the HT-HH event.
228. State for what purpose here. What specifics do we need to measure on the seafloor for better understanding? Mainly I am thinking volume and extent of new submarine deposits.
230. I was left a little unsure of the main takeaways from your analysis here. You could also explicitly state the value added from your data in comparison to the other recent literature on the eruption (Yoshida and Fauria).
361. What is the justification of the 1250 kg/m3 value in comparison to the pumice value? Based on ash deposits? How do theses value compare with Yoshida et al. textural data from the raft?
395-406. This data needs presenting in the results or more in the extended figures, not here in the methods.
405. What about accounting for S uptake during crystallisation? Is the assumption that all S loss from XRF to matrix glass is through degassing at the vent? Would some S quickly dissociate in the seawater?

Figures:
459. Images i) to v) need marking more clearly on the b) data. A thin red line at each time would work well.
469. The idea that the vent become closed with the growth of the cone needs discussing more, and also labelled as a key feature to help explain later behaviour.
526. Could you include some discussion of the textural variety and the similarities to Yoshida et

al.?
529. Include the Yoshida et al. data on this plot, and the rest of the geochemical data needs presenting either in plots or tables.

Reviewer #3 (Remarks to the Author):

Review comments: [formatted version also submitted]

First timeseries record of a large-scale silicic shallow-sea phreatomagmatic eruption
by F Maeno, T Kaneko, M Ichihara, YJ Suzuki, A Yasuda, K Nishida and T Ohminato

This manuscript analyses the 2021 eruption of Fukutoku-Oka-no-Ba (FOB) volcano, which produced a tuff cone, pumice raft, and relatively high, 16 km, eruption plume. Good use is made of data from the Himawari-8 satellite and remote infrasound measurements to assess plume behavior and eruption timing.
The observational information is of high quality and deserves to be published.
I find some of the interpretations to be less than well-supported, and also am troubled by the description of this as a "large-scale silicic" eruption, simply because the volume erupted is small, on the order of a tenth of a cubic km. Even eruptions such as Mt St Helens, of about one cubic km, are not generally considered "large-scale", which seems more appropriate for something like Pinatubo.
Specific comments below are keyed to line numbers. I have also returned a revision-tracked manuscript with suggested wording improvements and some additional comments.
100 The whiteness of the eruptive plume indicates lots of water, but not specifically seawater. A separate argument needs to be made for this.
133 Can the factor of two range in estimated tuff cone volume, from 0.04 to 0.07 km3, be improved?
147 Same comment on the factor of 3 range in pumice raft volume.
150 Not all water in an eruption column is vapor – plume is water-rich, not specifically steam rich.
153 External and magmatic water are subject to phase changes.
157 The "fraction" in question here has a huge range, depending on the previous two ranges for the pumice raft and the tuff cone – this should be made explicit
188 FOB's eruption plume reached the tropopause. So did the plume from Surtsey, on multiple occasions. Reaching the tropopause is probably a more significant measure than absolute height of the plume.
200 This was a very shallowly submerged vent – please present evidence that fragmentation and formation of an eruption plume preceded interaction with water. Saying that it might work like a deep-water eruption seems odd – surely the satellite information, and formation of a subaerial tuff cone, implies very different behavior from a fully submerged, deep-water, eruption.
207 To me the discussion here is quite confusing. When do particles transfer their heat, and is it being used to warm air to make a buoyant column, or to drive fragmentation through thermal interaction that makes water vapour that subsequently condenses in the eruption column, reducing its buoyancy?

Please take care to address separately interactions involving the erupting jet, vs. those taking place in the convective column.
219 A "slurry gushing from the submarine vent" is not consistent with water-surface breaching and generation of a subaerial jet and plume.
241 This eruption did not produce a large volume of magma. It produced a tuff cone, suggesting limited transport for the main mass of ejecta. There is little about the inferred eruptive processes that seems similar to Surtsey, at least from what's presented here, so it seems odd to name the eruption as an "ultra" version of Surtsey.

Response to Editor and Reviewers' comments

We thank for an editor and reviewers carefully reading our manuscript again and giving helpful comments and suggestions. Point-by-point response to reviewers' comments is listed below (Black is reviewer's comments; Red is our reply).

Response to Editor

In addition to the reviewer comments, please ensure that you propagate an uncertainty associated with the calculated erupted mass (Line 174-185).

(A) The mean S compositions come with a large variance (which is reported, and is large: for example, 334ppm +/- 336 (1 sigma) for the ground mass), therefore this propagates as uncertainty on the petrological estimate of degassed S concentration.

We added some comments on the errors in SO_2 estimates and their propagations at relevant places in methods section. In the chemical analysis subsection, we mentioned the errors in SO_2 estimates (+409.1 ppm / -73.3 ppm if 1σ error is considered). Also, in the subsection of errors in SO_2 estimation, we mentioned the effect on volume estimates. In the case of the largest error with $\Delta C_s = 409.1$ ppm, the volume of erupted magma will be reduced to 1/5–1/6; however, this estimate would be inconsistent with ones from eruptive deposits. Although our estimates from observations and analyses (eruptive products, satellite observation of SO_2, petrology) contain relatively large errors, the estimated volume ~0.1 km^3 can be explained consistently by the mean and representative values considered in this study. We emphasise this point.

(B) The satellite TROPOMI measurement of emitted SO2 mass will also have an uncertainty attached, related to both the retrieval algorithm and the injection altitude. Please also include details in the methods of how you determined SO2 flux in kg/h from daily TROPOMI mass loadings. All assumptions made should be described fully.

We used the near real-time SO_2 estimate data (max. SO_2 column and SO_2 mass loading) by Support to Aviation Control Service (SACS, Royal Belgian Institute for Space Aeronomy; https://sacs.aeronomie.be). The notification was issued at 4:15 (UTC, 13:15 JST) on 13 August 2021, 7 hours from the onset of the eruption. The fundamental of the methodology by SACS is summarized in their website and papers (https://sacs.aeronomie.be/TROPOMIalert/2021/08/alertsTROPOMI_20210813_04h11_210.php?alert=20210813_052436_210, Brenot et al., 2014 NHESS, etc.); therefore, instead of going into detail here, we think that citing their website and papers would be sufficient. We noted the data source and how we used their data in the method section.

Response to Reviewer #2:

L1 - Suggestion: "Infrasound and satellite time series..." or "High-resolution time series..."

The title was changed to ‘Seawater-magma interactions sustained the high column during the 2021 phreatomagmatic eruption of Fukutoku-Oka-no-Ba’.

L40 - …and inhibited vesiculation of magma.

We added this phrase.

L47 - Check for a REF for this, even if an early pre-print on satellite SO2/ash estimates.

Yuen et al. 2022 was added here.

L62 – What phenomena?

‘such as plume shape’ was added.

L67 – “for at least”... otherwise it sounds like it took 9 hours to reach the tropopause

We agree this comment. ‘in at least’ was changed to ‘for at least’.

L72 – expand on these impacts e.g.: “ports and harbours in Okinawa”.

We added ‘such as ports and harbours in Okinawa’ after ‘coastal infrastructure’.

L72 – A Chemical analyses...

‘analysis’ was changed to ‘analyses’.

L94 – 15 hours since the onset of Phase 4?

We added ‘since the onset of this phase’ after ‘15 hours’.

L106 - … measured, local tropopause…

This phrase was added before ‘tropopause’.

L130 – Refer to Sup. Fig. 1. In general, look through the manuscript to find more places where you can refer the reader to the main figures and supplementary material. Introduce figures as soon as they can be.

You are right. Supplement Fig. 1 was added here.

L139 – Refer to Fig, 1 and Sup Fig 2.

Fig. 1 and Sup Fig. 2 were added here.

L143 – This could be labelled clearer in Sup Fig. 2

Sup Fig. 2 was added here.

L148 – When do you think PDC/ column collapses occurred? Which phase?

Mainly during the later Phase 1. We explained this.

L159 – …"non-proximal" formation of the pumice raft?

Yes. We clarified this sentence as 'In the downwind direction, not pumice raft was seen to form, indicating that the eruption plume did not carry a large amount of pumice clasts to be deposited into the pumice raft in the downwind direction'.

L163 - state right here based on thickness estimates of 0.3-1.0m. Readers will want to see it right there.

We added 'based on thickness estimates of 0.3–1.0 m' as requested.

L169 – Refer to Fig. 3 sooner (here).

We referred Fig. 3.

L200 - Slight confusion, is your "magma" incorporation the same as the thermal component released via pumice entrainment?

Yes, 'magma' can be replaced as the 'thermal component'. We added a phrase 'and contributes as the thermal component released via pumice entrainment' after 'goes into the plume'.

L213 – Initiation or initial [phase/episode]?

We decided to use 'Phase 1a' for the initial part of Phase 1.

L215 - Looking at the data and your discussion, it does seem as though there are quite distinct parts of Phase 1, almost separate phases. Would an introduction of 1a and 1b to better characterize the maximum intensity phase (those 7 hours) of the eruption be worthwhile? If so, include Phase 1 b in your figures and talking points.

We agree this comment and divided Phase 1 into Phase 1a and 1b. These phases were used in description of observations and discussion.

L252 - Also check comparison with Brandl et al. (2019) and Yeo et al. (2022) for the 2019 Volcano F eruption.

We checked these eruptions, but it was difficult to obtain information on the ratio of the pumice raft volume to the total erupted volume.

L258 – …external water (with the saturated emergent cone)?

In fact, the cone may not have been perfectly symmetric in shape and may have been opened on the north and south sides. There are several possible processes that external water could have been involved, but we will not go into the details here, but will concentrate on summarising only the points. We would like to keep the original sentence.

L259 - What about the effects of cone flank collapses as a mechanism for fresh availability of external water to generate later, more discrete explosions? These temporary cones are very unconsolidated, unstable and can be brief (as soon in many cases).

We agree this comment. But as explained for the previous comment, there are several possible processes that external water could have been involved in this situation. We would like to keep summarising only the points.

L280 – "this eruption" or "this eruption style"?

We will use 'phreatoplinian' instead of 'this eruption'.

L298 and 301 – Oddly phrased "the marine eruption", be consistent with use of shallow-sea or submarine.

Thanks. We changed it to submarine.

L299 - Could add here that applications of VEI scale relative to varying eruptive volume and plume height may not hold. This is something that is frequently and quickly tried to determine, but something we should be cautious of in these more understudied eruption styles and mechanisms.

We agree this comment. We added the sentence ‘Applications of Volcano Explosivity Index (VEI) scale relative to varying eruptive volume and plume height may not hold’ and referred Newhall and Self (1982).

L474 – FOB eruption.
‘eruption’ was added.

L480 – 15-hours? I only saw 14 hours and 7 hours used for Phases 1 and 2.
The intense phase consists of 14 hours of the entire Phase 1 and 1 hour of intense period of Phase 2. We added this comment.

L490 – Also note that there is a significant caveat in this raft thickness because there are little/no validated groundtruthed measurements for a raft over time.
This is true. We added a sentence ‘Note that there is a large uncertainty in this raft thickness because little or no validated groundtruthed measurements for a raft over time’.

L499 - …but unlikely during highest intensity phase of such a shallow eruption
It is really hard to say how much of the impact of the dissolution of S was actually, because there are no useful data to constrain this process. Considering this, we discuss the error.

L515 – A good addition to the model… Out of curiosity (any maybe note it), what difference does not including the thermal component of the pumice make to final Me calculations?
More mass discharge rate will be required to establish the same eruption column height. This is just a problem of enthalpy balance.

L518 – T of magma or ambient?
T of external water. We explained this.

L575 - and the early phases are high intensity enough that the timescale of S through water would be << dissociation timescales of SO2 into SO4
We added the sentence ‘In the early stages, the eruption may have been so intense that the timescale for the S migration into seawater was shorter than the timescale for dissociation of SO_2 into SO_4.’

L664 – What exactly do you mean by “decay”? I’m still unsure what the Phase 4 plume diameter data is showing? Seems chaotic relative to Phase 3? Noise?

Phase 4 is characterised by weak infrasound and little or no plume development from the source, with only ambient clouds being captured. We added this explanation.

L689 – vs shallow eruptions into open water. Add phase numbers onto these panels.

Phase numbers were added onto the panels in Fig. 5.

L731 – These images could be annotated to look at the different layers

We added the sentence ‘It appears to consist of two major layers’ in the caption.

L735 – Define symbols again in table caption.

We defined symbols in table.

Response to Reviewer #3

I still have some questions about exactly what you consider to be happening in terms of "balance" of mass flux versus water. I don't see the balance -- Phase 1 has more mass flux and more water (the vent has no barriers at all to enclosing seawater); other phases have less mass flux and less water (the island/vent provides some partial barrier to water -- not much of a barrier, but access to water is less than in open seawater during Phase 1).

Thanks for this comment. Probably, the use of the term ‘balance’ is confusing. As you pointed out, we think the relationship between mass flux and water can be organized as follows: when the mass discharge rate is high and a large amount of external water is supplied, plume growth is enhanced, and when the mass discharge rate is low and the supply of external water is reduced, the eruption style becomes intermittent like the Surtsey. Instead of using ‘balance’, we have changed the wording to tell that both the mass discharge rate and availability of external water are important.

This is related to questions about development of the jet, which is fed directly from the vent and exists under water as well as subaerially. The text is not yet clear about this -- I have offered suggestions.

We agree that the jet must already be present at the stage of entraining the large amount of external water. We changed the text according to this suggestion.

Also, we considered the annotated manuscript by the reviewer and amended many places as requested.