nature portfolio

Peer Review File

Massive earthquake swarm driven by magmatic intrusion at the Bransfield Strait, Antarctica

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

15th Nov 21

Dear Dr Cesca,

Your manuscript titled "Massive earthquake swarm driven by magmatic intrusion at the Bransfield Strait, Antarctica" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns. In particular, please ensure that the revised manuscript meets the following editorial thresholds:

** Ensure your interpretations and conclusions on the role of magmatic intrusion driving the seismic swarm at the Bransfield Strait are fully supported by the evidence you present, or tone these down where necessary **

** Clarify the data-processing and procedure and quantify the uncertainties of your data and models **

** Please reorganize the main text to improve the reading the paper, following the recommendations of Reviewer #2 **

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,

Luca Dal Zilio,
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):

Thank you for providing me with the opportunity to review the manuscript titled “Massive earthquake swarm driven by magmatic intrusion at the Bransfield Strait, Antarctica” by Dr. Cesca et al. for consideration for publication in Communications Earth & Environment. This study shows that the remarkable seismic activity at the Bransfield Strait since last year is due to magmatic dike intrusion. The Bransfield Strait is a place where steady seismic activity exists, and its focal mechanisms and locations are estimated from moment tensor inversions of seismic waveforms obtained from the global network. These were moderate earthquakes (Mw 5 class). Although it is not an area with high seismicity associated with sporadic earthquake swarms, the earthquake swarm was observed off the southeastern coast of King George Island for the first time in the area, continuing as of August 2021 when the manuscript was written. Although the global seismic catalog obtained by routine analysis from global seismological data shows only earthquakes larger than M=4, this study applied several modern seismological methods using waveforms to detect a significant number of earthquakes and determine their hypocenters to track earthquakes with much smaller magnitudes. In addition, moment tensor solutions from more than 80 events were obtained, revealing the existence of two types of seismicity: deeper strike-slip events and shallower normal faulting events. From the time-series data of GNSS at two sites and the displacements along the coastline of King George Island obtained from InSAR data, the point deflation source indicating the outflow of magma and the sub-vertical dike inflation source shows the estimated magma inflow. Although the seismicity and deformation rate decreased at the time of the largest event on November 6, 2020 (Mw=6.0), the timing and existence of submarine eruptions have not been verified. However, there was a large magma intrusion of the order of 10⁸ m³, as obtained by deformation modeling, and the seismicity was accompanied by moderate earthquakes. It is worth obtaining a detailed picture of such a large-scale magma intrusion activity near a submarine volcano. Therefore, this manuscript is appropriate for Communications Earth and Environment. I recommend that the manuscript should be accepted after minor revisions. My comments and suggestions for improving the manuscript are as follows:

Key comments:

1. The deeper location of the strike-slip type earthquakes and the shallower location of the normal fault-type earthquakes have not been considered or interpreted. By comparing the time evolution of the sub-vertical inflation dike estimated by the deformation data with the seismicity of the strike-slip and normal fault types and by discussing the spatial relationship between the dike and the hypocenters of the two types of earthquakes, the meaning of the deeper location of the strike-slip type earthquakes and the shallower location of the normal fault earthquakes should be clarified.

2. In the moment tensor solutions described in Supplementary Fig. 5, non-DC, CLVD, and isotropic components exist in considerable proportions; however, their meanings have not been sufficiently explained. For example, lines 196–199 only point out that “substantial and consistent volumetric and tensile moment tensor components further support the magmatic origin of the swarm” and no further discussion has been made. As shown in the Hudson diagram in Supplementary Fig. 5, the moment tensor is widely distributed from the DC to the dipole or CLVD. Please comment on the mechanisms of earthquake occurrence in which such non-DC components are present. For example, it will be necessary to carry out crack modeling, as the authors were involved in the past research on the earthquakes of Mayotte⁴³.

3. It is interpreted that the occurrence of the largest earthquake on November 6, 2020 (Mw=6.0) at the beginning of phase 4 after the completion of phase 3 decreased the pressure in the dike and inhibited further dike evolution, as described in lines 179–180 and 225–227. This interpretation should be supported, for example, by calculating the stress change in the dike due to the occurrence of the largest earthquake.

4. Regarding the “Geodetic modeling of the assumed magmatic source” explained in lines 388–425 and “Insights into dike extent from InSAR observations” explained in lines 427–448, the aseismic process can be emphasized by modeling the assumed magmatic source with the differential displacement obtained by subtracting the displacement obtained as a result of geodetic modeling of co-seismic displacement from the observed displacement as data. The authors can model the magmatic source accurately.

Individual comments:

5. Line 25, Abstract: It is necessary to explain the mechanism by which the internal pressure of the magmatic dike is lowered by the effect of slipping of a large fault.

6. Lines 106–107: Though it is described that shear and opening tensile failure are indicated as an interpretation of the isotropic component, at least one reference is necessary. Shimizu et al. (1987) is the first one in the world to show that the combination of tensile crack and shear faulting are the mechanisms of volcanic earthquakes.

Shimizu, H., Ueki, S., and Koyama, J. (1987): A tensile-shear crack model for the mechanism of volcanic earthquakes. Tectonophysics 144, 1-3, 287-300, doi:10.1016/0040-1951(87)90023-0.

7. Line 302: 'seismo-vulcanic' should be "seismo-volcanic".

8. Line 168: Although the influence of different host rock densities is considered the reason why the intrusion stops by approaching volcanic edifice, appropriate citations should be provided.

9. Line 130: It is quoted as Suppl. Mat. Please specifically show which part of the Supplementary Material it refers to or which figure it is.

10. Lines 723–734, Fig. 4: Similar to Fig. 3, please comment on t₁ t₂, t₃, and t₄.

11. Lines 123–124: The static displacements are calculated from the results of the moment tensor analysis of the largest earthquakes, and it is explained that the accumulated displacements are only a few percent of the observed displacements. It is easier for the readers to understand this if there is a supplementary figure showing the static displacement field. Moreover, is the spatial distribution pattern of the displacement estimated from the largest earthquakes consistent with the spatial pattern of the displacement obtained from InSAR and GNSS data? Although there is an additional explanation in lines 123–124 and 377–380, a figure would help readers understand this.

12. Lines 158–161: A fast upward migration of the magmatic dike is suggested based on the occurrence of the shallower normal faulting events immediately after the deeper strike-slip events. As it is difficult to understand a fast upward migration of the magma dike only by the seismicity pattern, is it possible to discuss, for example, changes in the time-series after 29 August 2020 of GNSS stations DAL5 and UYBA shown in Supplementary Fig. 12 that suggest upward migration of the dike? According to Supplementary Fig. 12, the changes in stations DAL5 and UYBA were the same from 29 August 2020 to the end of September 2020. Does this suggest upward migration of the dike?

13. Lines 166–168: At the end of phase 2, the seismicity slowly decreases; which figure should be referenced to understand this? This cannot be understood by referring to Figs. 3ef; however, it can be understood by referring to lines 166–168 and Supplementary Fig. 8b; I suggest this should be referenced.

14. Lines 174–176: In phase 3, seismicity migration and strike-slip events were predominant, which is interpreted to be caused by unloading caused by ice melting in King George Island. It is necessary to explain why seismicity migration occurs and strike-slip events are dominant after the unloading.

15. Lines 179–180: Same as Key comments #3.

16. Lines 190–191: As it is stated in lines 123–124 that the co-seismic displacement is only a small percent of the observed displacement, it is better to refer to this explanation.

17. Lines 196–199: Same as Key comments #2.

18. Lines 126–128: Although a high correlation between the cumulative number of normal faulting events and observed displacement of the GNSS data in time series seems apparent, it is not possible to explain that the normal faulting events are only caused by a sub-vertical magmatic dike. Is it possible to cause normal faulting events with a sub-horizontal dike? Hill (1977): The cited literature shows a conceptual model of volcanic earthquakes, and Passarelli et al. (2015) modeled the magma intrusion event that occurred beneath Miyake-Kozu Islands in 2000. Therefore, this is weak as a basis on which to assert these claims. The author’s claim can be understood because the sub-vertical dike is estimated from the modeling of the displacements from InSAR and GNSS data from the description after this sentence.

19. Lines 225–227: Same as Key comments #3.

20. Lines 138–140: Although the manuscript states, “confined below the normal faulting earthquakes,” Fig. 4a, which shows the depth distribution of the hypocenters, shows that normal faulting events are distributed from 2.5 km depth to 10 km depth. On the other hand, the result shown in Supplementary Table 2 indicates the depth of the dike of 13 km. However, the spatial relationship between the normal faulting events and sub-vertical dike differs depending on whether this depth indicates the center, top, or bottom of the sub-vertical dike. Please clarify this.

21. Lines 713–722, Figure 3; the figure caption needs to explain what t₁ t₂, t₃, and t₄ are. Also, in phase 1 and the first half of phase 2, the direction of the azimuth changed considerably, as shown in Fig. 3h. Please comment on this in the main text.

Reviewer #2 (Remarks to the Author):

The paper is a study of an energetic earthquake swarm in Bransfield Strait, Antarctica, that took place from August 2020 to February 2021. The earthquake swarm is very interesting, but taking place in a remote region with limited geophysical instrumentation. There are only two GNSS stations and two seismic stations, located at the Carlini and Artigas Antarctic bases, making for less than ideal network to study the events. The paper brings together impressive team of researchers that try to build a picture of what took place, augmenting the stations at the Antarctic bases with InSAR data along the coast and global seismic stations.

I was a bit disappointed when reading the paper. The paper feels very rushed and disorganized. I had to look up other papers to get a better overview picture to understand what the authors were talking about at times (e.g. there needs to be a better overview picture that showes e.g. the whole subduction zone, how it has been reduced to 1000 km slither along the Phenix Microplate). There is limited relevant background information but statements like
“A comparably massive swarm has never been observed” (what has been observed? What is the history of recorded swarms for that region, what about large earthquakes? Is this a region with high tectonic stress which would produce lots of seismic activity if disturbed by magma (like present activity at Reykjanes Peninsula).

“is considered a unique case of an active back-arc basin” (why is it unique? I can think of a few back-arc basins, tell me what is unique about it).
And the use of “massive” without giving context (how do you define massive, how does the seismic swarm e.g. compare to other recent event, e.g. Bardarbunga 2014? It isn’t until line 206 that you give the total moment of the seismic release (comparable to Mw6.3), I would expect that information to be earlier in the paper given the title of the paper).

The authors show little attempt to evaluate uncertainties of data and model parameters which is dissapointing to say the least.

Lastly the final section doesn’t rhyme well with the rest of the paper, the authors start to look for evidence of an eruption by searching for floating pumice, which is not something I would expect if said eruptions was a result of a dike propagating up from the mantle (basalt) – which is what the paper seemed to be inferring.

There are a lot of long sentences (if 4+lines, please cut – becomes very hard to read and often the authors are cramping in a lot of information in a long sentence so it is hard to follow where they are going). Also, a minor point, but authors often use words that are not overly familiar to me which could easily be replaced by more common language so that the reader does not get lost. As and example.
(line 76) “here we recollect … “ why not “here we analyse” ?
(line 90) “Our results antedate “ (not even sure this is the correct use of this word, I am assuming this means your results suggests the unrest started as early as 10 August).
(line 115) “… is depicted both at GNSS stations “ (again not sure this is even the correct use of this word, but why not say “GNSS and InSAR data show transient surface deformation from August 2020 and persisting over months”.
(line 302) “seismo-vulcanic”. This should be volcanic
But also, line 61, “magma-volcanic”, supposed to be seismo-volcanic? Or volcano-tectonic? All three versions currently used in the paper.

In addition to wording and long confusing sentences, the authors sometimes talk about the events in the past and sometimes in the present, as if they weren’t sure if the unrest was still ongoing or not, or weren’t sure when the study period should end. Abstract says Swarm of 85k earthquakes, giving the first impression that the event is over. The none starting Introduction (surely there is a missing sentence at the beginning of it) says, “continued over months … “. But line 79-80 then says “at the time of the writing (August 2021) seismicity is still ongoing) – but it had “faded away” already in line 32! Lines 184 to 187 then jump to a M7 event (close to Elephant Island) which occurs after the swarm is over. You leave the reader confused. Elephant Island is not even marked on Fig 1 (even though it is on the map).

I also have a problem with how the data are presented.
I don’t see data uncertainties in this paper. Figure 1 has vectors without uncertainty ellipses. Same for Figure 2 (and reported ~11 cm etc, line 119-20). Colour scale for InSAR data in Figure 2 (yellow to green? Blue?) is such that the figure doesn’t really provide any info on the InSAR deformation (supplement figures are better, but there are more colours available to the authors that would help the reader). Figure 3 is rather squished and again no uncertainties for GNSS data. This is really not a nice way to present real data. Would be fine if it was a model, these are datapoints. It is really hard to judge the quality of processing and data from this figure (and not sure panel h adds any info – if

azimuth was changing it would be argument for magma migration right – but the focus on this graph automatically becomes the pre deformation azimuth as it is just looking at random noise).
Same is true for Suppl figure 12 and 13. It would have been super helpful to show datapoints (with uncertainties) in Suppl Fig13 rather than lines. That would make it a really good figure. I might have missed it, but I could not find any info on the actual number of SAR images used for this study. And it wasn't clear to me either what the reference point is for the InSAR images. Isn't it a bit problematic to remove a ramp in a deformation field such as this one, where you only have scatter of points around the coast – could you not expect a ramp naturally in the deformation field? I understand that there isn't a whole lot of GNSS data to help constrain this but that weakness might need to be pointed out.
The depth of the earthquakes, how well are they determined? I thought that you had to have a good network of stations close to the activity to get good depth estimates. Isn't the depth range given possibly shifted up or down (so relative to each other the depth is good, but not absolute)? I understand that the strike slip earthquakes are deeper than normal but the average depth (given as 9.2+/-2.4 km) could be shifted up or down right?
Moho (crust-mantle boundary). You give it as 15 km (e.g. Figure 4). Then in line 131 you put a volumetric source at 20 km depth. Is this needed? Why would you get strike slip earthquakes in the mantle? This is confusing me. And do you really need this source for your modelling of Geodetic data? It is deep and your data are limited. Do the results change much if you remove it? From Method section you seem to say it wasn't improving anything so why have it here.
InSAR data. There is ice during winter on the island that then melts (Supp Fig 15). I would expect a large seasonal signal corresponding to the melting of the ice. Probably rapid uplift starting around November. What do previous years show? Did you look at InSAR data during quiet periods? This should also show up in the GNSS data. According to the text only linear background trend was removed from the data. The time series from the Nevada page show that there clearly is a significant seasonal signal affecting the vertical data (http://geodesy.unr.edu/NGLStationPages/stations/UYBA.sta). Has there been any attempt to remove this signal from the data?

Introduction.
The paper needs to start with a better key first sentence, in the very least it should say August 2020 somewhere.
I would have started it by a) setting the stage, where is it, b) defining the time period of the study, b) putting the "massive" in scientific defenition (how much seismic moment was released). That would give the paper the constraints it currently badly needs:
"In 2020 an intense seismic swarm took place in Bransfield Strait, Antarctica, with XX number of earthquakes exceeding Mw. The swarm peaked with two large earthquakes on 2 October (Mw 5.9) and 6 November (Mw 6.0), before tapering off. The estimated seismic moment released from August to February 2021 XXX.
This way the paper has structure from then on.

Line 37, I would say "recorded" rather than "observed".

Line 39. Here Figure 1 really needs to help by showing bigger region (the whole of Phoenix Block if possible (block not plate)).

Line 47. Is there really no other, more recent, study of spreading rate for the region?
I had a quick look at the Nevada page, UYBA has been operating on and off since 2015 and FREI has

some long time series too (even though there are large data gaps). Would not have expect it to be too hard for the authors to estimate background rates for these sites (would assume you already did this for UYBA when you remove background).

Line 59. “The development of … including”. Presumably you are listing them in chronological order, no need for the word “including”.

Line 61. “Ongoing magma volcanic activity”. This is confusing. I know you mean the area is active (long time scale) but since the paper is on volcanic unrest this causes confusion.
I would start this with “Seismic swarms at … show that Bransfield Strait is volcanologically active”

Line 67. I am not clear on how ocean bottom deployment can provide evidence for magma accumulation at shallow depth. I check the citation and it was not clear from that paper either that that’s what they were saying.

Line 97-8, shorten text by removing “one with a” and “another with”

Line 124-5 “Given the HISTORY OF volcanic activity IN the area”

Line 101-103, as mentioned before, I am missing some discussion on how good vertical – and horizontal, determinations are. They are a bit of a key in the discussion of magma migration to follow.

Line 119-121, no uncertainty given, just approximate numbers. Not sure 1 cm uplift is even significant.

Line 129. Again, it would be great if depth range of earthquakes (absolute) came with some uncertainty.

And again, line 131. Is this 20 km depth source really supported/needed in the modelling of this limited dataset? Seems from method section that it cannot provide better fit (line 417-8) so why included here?

Line 138. Maybe use km instead of m? (0.56 km^3). There is no indication given of how well this volume is constrained by data. What volumes fit the data within 2 sigma of the data? In Supplement table 2 there are different models given, showing volume from 0.26 km^3. Can you do some sort of monte carlo or something, using uncertainty of data etc to give some sort of bounds on this value and the length and width of the dike? These are big numbers.

The estimated volume is comparable volume to the Bardarbunga dike intrusion which was also seismologically very intense but much better instrumented. I find it curious that there is no reference to that sequence in this paper. That 2014-5 rifiting event in Iceland showed mostly strike slip earthquakes and the dike was modelled to be above the observed seismicity (e.g., Agustsdottir et al., 2016, Sigmundsson et al., 2015).

Line 159. Not sure earthquake locations can be used like this. Normal fault earthquakes are occurring in the volume above, strike slip presumably below? How are you getting 3-4 km/day? (which would mean that you go through the whole crust in just 4-5 days). Again would be helpful if uncertainty on absolute and relative earthquake locations was presented in the paper.

Line 175. I find it curious that the authors talk about the unloading of ice as affecting seismicity migration, but do not consider it as a source of deformation signal in GNSS and InSAR data.

Line 196, Bardarbunga 2014-5 should be added as an example of magma migration and seismicity, really well documented example.

Line 213, again, please some sort of confidence regions on these model numbers.

Line 450-70. Why would you expect floating pumice? This is a rifting basin where you are creating ocean floor (according to text). In line 64 you talk about basalts. You have a fast moving dike from (well paper claims) 20 km mantle depth. Should this not be a basaltic dike and thus basaltic eruption? I am just very confused. I think this section works against the paper. It makes absolutely no sense. What is the water depth in this region? Are there any tremor signals suggesting eruption at the JUBA seismic station? Perhaps the most recent volcanic eruption in Iceland should be an example for discussions here as leading up to the eruption both seismicity and deformation slowed down, the eruption started aseismically with no significant deformation (there might be a abstract or a paper somewhere by now you could cite, not sure).

Lastly, when reviewing the paper I had to google the activity to get some better information to better understand the paper. I came across a paper Olivet, Bettucci et al, “A seismic swarm at the Bransfield Rift, Antarctica”. The authors should cite that paper. (https://doi.org/10.1016/j.jsames.2021.103412)

Rebuttal letter

Dear reviewers,

we are thankful for a careful reading and constructive comments. In this rebuttal letter, we provide a point to point answer to all raised comments, reporting the original comment in black and our reply in blue.

Simone Cesca (on behalf of all authors)

Reviewer #1 (Remarks to the Author):

Thank you for providing me with the opportunity to review the manuscript titled “Massive earthquake swarm driven by magmatic intrusion at the Bransfield Strait, Antarctica” by Dr. Cesca et al. for consideration for publication in Communications Earth & Environment. This study shows that the remarkable seismic activity at the Bransfield Strait since last year is due to magmatic dike intrusion. The Bransfield Strait is a place where steady seismic activity exists, and its focal mechanisms and locations are estimated from moment tensor inversions of seismic waveforms obtained from the global network. These were moderate earthquakes (Mw 5 class). Although it is not an area with high seismicity associated with sporadic earthquake swarms, the earthquake swarm was observed off the southeastern coast of King George Island for the first time in the area, continuing as of August 2021 when the manuscript was written. Although the global seismic catalog obtained by routine analysis from global seismological data shows only earthquakes larger than M=4, this study applied several modern seismological methods using waveforms to detect a significant number of earthquakes and determine their hypocenters to track earthquakes with much smaller magnitudes. In addition, moment tensor solutions from more than 80 events were obtained, revealing the existence of two types of seismicity: deeper strike-slip events and shallower normal faulting events. From the time-series data of GNSS at two sites and the displacements along the coastline of King George Island obtained from InSAR data, the point deflation source indicating the outflow of magma and the sub-vertical dike inflation source shows the estimated magma inflow. Although the seismicity and deformation rate decreased at the time of the largest event on November 6, 2020 (Mw=6.0), the timing and existence of submarine eruptions have not been verified. However, there was a large magma intrusion of the order of 10^8 m^3, as obtained by deformation modeling, and the seismicity was accompanied by moderate earthquakes. It is worth obtaining a detailed picture of such a large-scale magma intrusion activity near a submarine volcano. Therefore, this manuscript is appropriate for Communications Earth and Environment. I recommend that the manuscript should be accepted after minor revisions. My comments and suggestions for improving the manuscript are as follows:
We are thankful to the reviewer for the appreciation to our work and carefully considered the reviewer’s constructive suggestions.

Key comments:
1. The deeper location of the strike-slip type earthquakes and the shallower location of the normal fault-type earthquakes have not been considered or interpreted. By comparing the time evolution of

the sub-vertical inflation dike estimated by the deformation data with the seismicity of the strike-slip and normal fault types and by discussing the spatial relationship between the dike and the hypocenters of the two types of earthquakes, the meaning of the deeper location of the strike-slip type earthquakes and the shallower location of the normal fault earthquakes should be clarified. We improved the manuscript to discuss better the implications of having these two families of earthquakes, their location and depth and the spatiotemporal evolution of the sequence. While showing different rupture styles, both types of focal mechanisms are coherent with the background tectonic settings: they share a common NW-SE oriented tension axis, perpendicular to the ridge axis. However, we consider they occur as different processes in response to magma propagation at different depths. We included the following paragraph:

"The tensile source resolved by geodetic data, the presence on positive non-DC terms in focal mechanisms, and the alignment and extent of normal faulting all consistently give evidence for a vertical dike beneath these events: normal faulting earthquakes are triggered on top of a vertical dike and their spatial extent can provide an indirect evidence for the lateral growth of the shallow dike (Hill 1977, Passarelli et al. 2015). Deeper strike-slip events are likely the response to magma intrusion deeper in the crust during the initial opening of the feeder dike and later magma inflow."

2. In the moment tensor solutions described in Supplementary Fig. 5, non-DC, CLVD, and isotropic components exist in considerable proportions; however, their meanings have not been sufficiently explained. For example, lines 196–199 only point out that "substantial and consistent volumetric and tensile moment tensor components further support the magmatic origin of the swarm" and no further discussion has been made. As shown in the Hudson diagram in Supplementary Fig. 5, the moment tensor is widely distributed from the DC to the dipole or CLVD. Please comment on the mechanisms of earthquake occurrence in which such non-DC components are present. For example, it will be necessary to carry out crack modeling, as the authors were involved in the past research on the earthquakes of Mayotte.

We extended the discussion on the non-DC component of our MT solutions. The bootstrap-based MT solutions of the single earthquakes display some uncertainty in the non-DC component, which can for example be seen in Suppl. Fig. 5. However, the very best solutions of all earthquakes, which have the smallest misfits, show a similar decomposition into DC, CLVD and isotropic components, which we consider as an indication for a robust result. We resolved a predominance of positive isotropic and CLVD source components, even accounting for uncertainties (see new Suppl. Fig. 7), which provide evidence for opening tensile components. As for the last comment, at Mayotte we used an oscillating source model to fit VLPs observations, which are not present here. For VTs, we followed indeed the same approach used here. Our results indicate a combination of shear and tensile faulting. While significant non-DC components are present in our results, DC components remain dominant for the studied VTs, so that neither a pure DC nor a pure tensile crack model can explain well the seismological observations. We improved the text accordingly.

3. It is interpreted that the occurrence of the largest earthquake on November 6, 2020 (Mw=6.0) at the beginning of phase 4 after the completion of phase 3 decreased the pressure in the dike and inhibited further dike evolution, as described in lines 179–180 and 225–227. This interpretation

should be supported, for example, by calculating the stress change in the dike due to the occurrence of the largest earthquake.

We agree that this statement may look unsupported. The quantitative results of the suggested calculation are very sensitive to the dike and the fault's exact geometries (exact locations, dips, strikes). Given that these are not very well constrained, a stress interaction calculation would be affected by large uncertainties and probably end up being not very meaningful. We prefer to motivate the statement on the basis of previous studies and on theoretical grounds (the revised manuscript text has been updated accordingly).

4. Regarding the "Geodetic modeling of the assumed magmatic source" explained in lines 388–425 and "Insights into dike extent from InSAR observations" explained in lines 427–448, the aseismic process can be emphasized by modeling the assumed magmatic source with the differential displacement obtained by subtracting the displacement obtained as a result of geodetic modeling of co-seismic displacement from the observed displacement as data. The authors can model the magmatic source accurately.

We indeed modeled the deformation data after removing the static displacement attributed to the seismicity (this was estimated as the cumulative deformation produced by each earthquake, accounting for its location, depth, magnitude and moment tensor, overall accounting for the large majority of the seismic contribution). We added Suppl. Fig. 15 to provide a comparison of the observed deformation and the one modeled accounting for seismicity only to clarify this point.

Individual comments:
5. Line 25, Abstract: It is necessary to explain the mechanism by which the internal pressure of the magmatic dike is lowered by the effect of slipping of a large fault.
We cannot add this information in the abstract, due to limit in the number of words, but we included an explanation later in the text (see reply to comment #3).

6. Lines 106–107: Though it is described that shear and opening tensile failure are indicated as an interpretation of the isotropic component, at least one reference is necessary. Shimizu et al. (1987) is the first one in the world to show that the combination of tensile crack and shear faulting are the mechanisms of volcanic earthquakes.
Shimizu, H., Ueki, S., and Koyama, J. (1987): A tensile-shear crack model for the mechanism of volcanic earthquakes. Tectonophysics 144, 1-3, 287-300, doi:10.1016/0040-1951(87)90023-0.
Thanks. We are glad to include this good, early reference

7. Line 302: 'seismo-vulcanic' should be "seismo-volcanic".
Corrected

8. Line 168: Although the influence of different host rock densities is considered the reason why the intrusion stops by approaching volcanic edifice, appropriate citations should be provided.
We modified the text and added references as: "possibly inhibited by different host rock density (e.g. Taisne et al., 2011) or by the load of the volcanic edifice (Kervyn et al., 2009)"

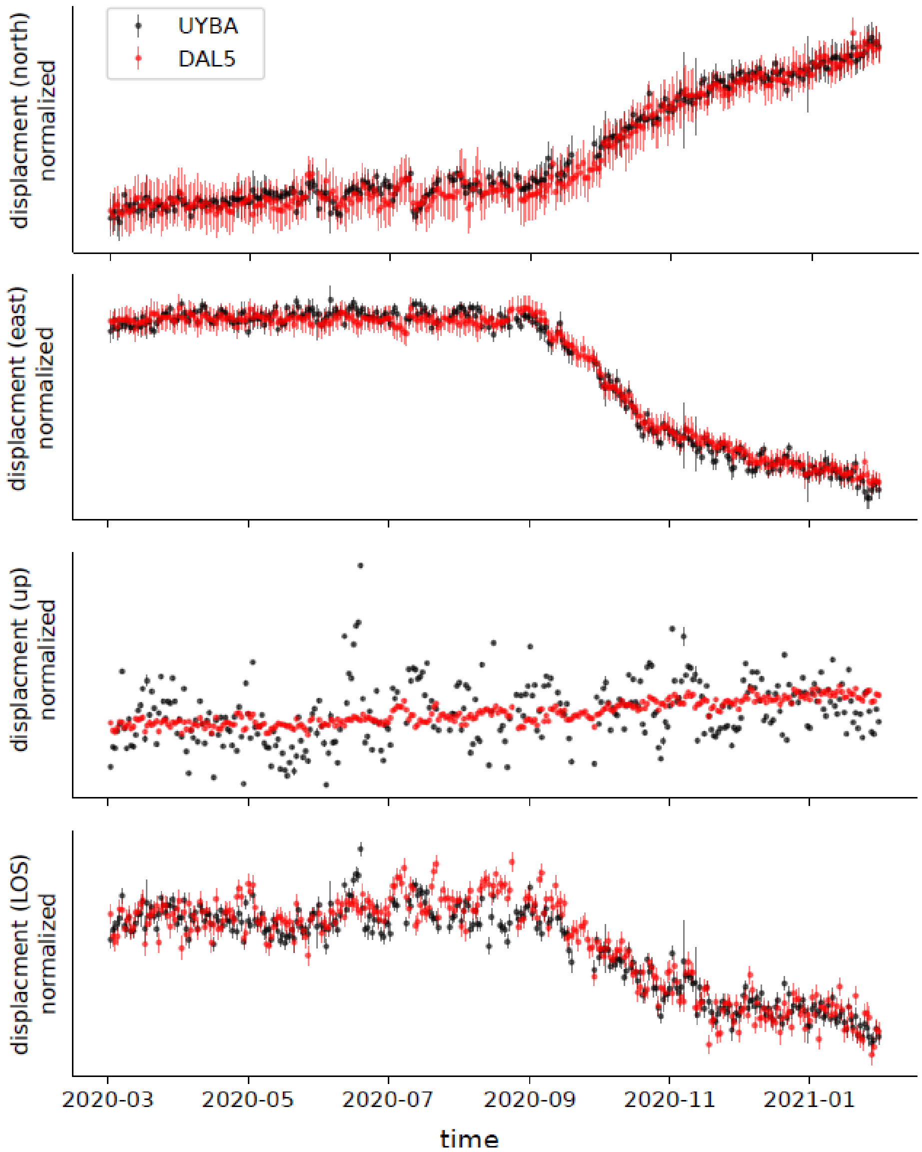
9. Line 130: It is quoted as Suppl. Mat. Please specifically show which part of the Supplementary Material it refers to or which figure it is.
Corrected (Suppl. Tabs. 2 and 3)

10. Lines 723–734, Fig. 4: Similar to Fig. 3, please comment on t1 t2, t3, and t4.
We improved captions of Figs. 3 and 4 accordingly.

11. Lines 123–124: The static displacements are calculated from the results of the moment tensor analysis of the largest earthquakes, and it is explained that the accumulated displacements are only a few percent of the observed displacements. It is easier for the readers to understand this if there is a supplementary figure showing the static displacement field. Moreover, is the spatial distribution pattern of the displacement estimated from the largest earthquakes consistent with the spatial pattern of the displacement obtained from InSAR and GNSS data? Although there is an additional explanation in lines 123–124 and 377–380, a figure would help readers understand this.

We elaborated an additional figure (Suppl. Fig. 14), which compares the coseismic and the modeled displacement components N, E, Z and an approximative LOS estimate.

12. Lines 158–161: A fast upward migration of the magmatic dike is suggested based on the occurrence of the shallower normal faulting events immediately after the deeper strike-slip events. As it is difficult to understand a fast upward migration of the magma dike only by the seismicity pattern, is it possible to discuss, for example, changes in the time-series after 29 August 2020 of GNSS stations DAL5 and UYBA shown in Supplementary Fig. 12 that suggest upward migration of the dike? According to Supplementary Fig. 12, the changes in stations DAL5 and UYBA were the same from 29 August 2020 to the end of September 2020. Does this suggest upward migration of the dike?
There is no additional evidence for the fast upward migration apart from the seismological data. Consequently, we smoothed some statements in this part of the text, only hypothesizing the rapid magmatic ascent based on the seismicity timeline, early appearance of shallow normal faulting and their following lateral migration. We plotted a comparison of (normalized) deformation patterns at DAL5 and UYBA, as suggested (see below). In fact, DAL5 and UYBA show coherent changes during the whole observation time, not only in the beginning.



13. Lines 166–168: At the end of phase 2, the seismicity slowly decreases; which figure should be referenced to understand this? This cannot be understood by referring to Figs. 3ef; however, it can be understood by referring to lines 166–168 and Supplementary Fig. 8b; I suggest this should be referenced.

We improved the reference to this observation, as suggested, and slightly rephrased the sentence.

14. Lines 174–176: In phase 3, seismicity migration and strike-slip events were predominant, which is interpreted to be caused by unloading caused by ice melting in King George Island. It is necessary to explain why seismicity migration occurs and strike-slip events are dominant after the unloading.

We rephrased the text and added a proper reference.

15. Lines 179–180: Same as Key comments #3.

We extended the text and better elaborated on this point (see reply to comment #3).

16. Lines 190–191: As it is stated in lines 123–124 that the co-seismic displacement is only a small percent of the observed displacement, it is better to refer to this explanation.
We modified the sentence as suggested.

17. Lines 196–199: Same as Key comments #2.
We improved the text. See reply to comment #2

18. Lines 126–128: Although a high correlation between the cumulative number of normal faulting events and observed displacement of the GNSS data in time series seems apparent, it is not possible to explain that the normal faulting events are only caused by a sub-vertical magmatic dike. Is it possible to cause normal faulting events with a sub-horizontal dike? Hill (1977): The cited literature shows a conceptual model of volcanic earthquakes, and Passarelli et al. (2015) modeled the magma intrusion event that occurred beneath Miyake-Kozu Islands in 2000. Therefore, this is weak as a basis on which to assert these claims. The author's claim can be understood because the sub-vertical dike is estimated from the modeling of the displacements from InSAR and GNSS data from the description after this sentence.
We thanks the reviewer for the comment and rearranged this part of the text as follows:

"The high correlation among the deformation time series and the cumulative number of normal faulting events (Fig. 3) suggests that both seismicity and deformation are controlled by the same process, which we hypothesize to be a sub-vertical dike intrusion. The observed deformation is well explained by the opening of a sub-vertical dike. This can be modelled by means of a tensile dislocation source[REF] (Suppl. Tabs. 2-3), whose location and orientation can be constrained by the normal faulting activity above it. The observation of aligned normal faulting earthquakes, which are favored above a sub-vertical dike[REFS], further supports our interpretation."

19. Lines 225–227: Same as Key comments #3.
See reply to comment #3. The sentence should be now clear, given that we have provided further information in the previous text.

20. Lines 138–140: Although the manuscript states, "confined below the normal faulting earthquakes," Fig. 4a, which shows the depth distribution of the hypocenters, shows that normal faulting events are distributed from 2.5 km depth to 10 km depth. On the other hand, the result shown in Supplementary Table 2 indicates the depth of the dike of 13 km. However, the spatial relationship between the normal faulting events and sub-vertical dike differs depending on whether this depth indicates the center, top, or bottom of the sub-vertical dike. Please clarify this.
We harmonized the text as follows:

"The lower extent of the dike is hardly constrained by the seismic activity, but its top should be at ~5-10 km depth, confined below the normal faulting earthquakes (Fig.4), which have an average depth of 7±2 km. Consistently, the dike geometry constrained by the geodetic modeling, with a slightly deeper average depth (13 km) and a width of 12 km (Suppl. Tab. 2), suggests the dike top at 7±3 km depth."

21. Lines 713–722, Figure 3; the figure caption needs to explain what t1 t2, t3, and t4 are. Also, in phase 1 and the first half of phase 2, the direction of the azimuth changed considerably, as shown in Fig. 3h. Please comment on this in the main text.

We extended the caption of Fig. 3 (and later Fig. 4) as suggested. Fig. 3h has been removed after comments by reviewer #3. The azimuth variation in the former panel was simply due to the lack of a coherent deformation signal during phase 1 and part of phase 2, and thus the azimuth was simply changing due to random (small amplitude) noise in the deformation data.

Reviewer #2 (Remarks to the Author):

The paper is a study of an energetic earthquake swarm in Bransfield Strait, Antarctica, that took place from August 2020 to February 2021. The earthquake swarm is very interesting, but taking place in a remote region with limited geophysical instrumentation. There are only two GNSS stations and two seismic stations, located at the Carlini and Artigas Antarctic bases, making for less than ideal network to study the events. The paper brings together impressive team of researchers that try to build a picture of what took place, augmenting the stations at the Antarctic bases with InSAR data along the coast and global seismic stations.

I was a bit disappointed when reading the paper. The paper feels very rushed and disorganized. I had to look up other papers to get a better overview picture to understand what the authors were talking about at times (e.g. there needs to be a better overview picture that showes e.g. the whole subduction zone, how it has been reduced to 1000 km slither along the Phenix Microplate).

We improved and restructured Figure 1, adding one panel to show a larger region for the overview and preserving the layout of former panels, which are needed to illustrate local structures.

(1) There is limited relevant background information but statements like

“A comparably massive swarm has never been observed” (what has been observed? What is the history of recorded swarms for that region, what about large earthquakes? Is this a region with high tectonic stress which would produce lots of seismic activity if disturbed by magma (like present activity at Reykjanes Peninsula).

We improve the text to clarify this issue: “Due to region remoteness, seismic instrumentation scarcely covers the Bransfield Basin. Temporary experiments, including ocean-bottom sensors, were carried on recovering seismicity features (Robertson et al.). There was no evidence of similar volcanic swarms in the past experiments: even if the off-shore volcanism and extensional tectonics could sometimes increase the seismic rate in the back-arc basin, a comparably energetic swarm has never been recorded in the region (Fig. 1).”

(2) “is considered a unique case of an active back-arc basin” (why is it unique? I can think of a few back-arc basins, tell me what is unique about it).

We slightly extended the introduction text to clarify this point:

“Bransfield basin is not a “normal” back-arc basin (Lawver et al., 1996). It lies in a unique tectonic environment. Although active subduction occurred during most of the past 200 m.y., it slowed dramatically at about 4 Ma when the Phoenix-Antarctic spreading center was abandoned offshore, leaving a small remnant of the former Phoenix plate incorporated in the Antarctic plate. Even though geochemical data indicate that unaltered basalt dredged from the Bransfield basin is like midocean ridge basalt, there is no clear evidence for normal seafloor spreading."

adding the reference to:

Lawver, L.A., Sloan, B.J., Barker, D.H.N. Ghidella, M., Von Hertzen, R.P., Keller, R.A., Klinkhammer, G.P., Chin, C.S., 1996. Distributed, Active Extension in Bransfield Basin, Antarctic Peninsula: Evidence from Multibeam Bathymetry, GSA Today, 6, 11, 1-6.

(3) And the use of "massive" without giving context (how do you define massive, how does the seismic swarm e.g. compare to other recent event, e.g. Bardarbunga 2014? It isn't until line 206 that you give the total moment of the seismic release (comparable to Mw6.3), I would expect that information to be earlier in the paper given the title of the paper).

We followed the reviewer suggestions and rephrased the text. We mention the cumulative moment earlier in the text, referring there to the estimate based on global catalogs. We also included additional references to suggested case studies (e.g. Bardarbunga 2014). We changed the wording "massive" in the text, being more specific. We use it now only in the title, where we consider it adequate, given that we discuss a swarm of at least 85,000 events. As a comparison, at Bardarbunga, where seismicity was very well studied and the monitoring conditions were much better, more than 30,000 events were reported (Agustdottir et al. 2016).

(4) The authors show little attempt to evaluate uncertainties of data and model parameters which is dissapointing to say the least.

We apologise, as the original manuscript was not clear in this respect. We estimated uncertainties on many seismological parameters as those affecting geodetic data, which are now quantitatively discussed and shown in the paper. Most specifically, uncertainties on significant earthquake source parameters (e.g. depth, magnitude, non-DC terms) are now illustrated in new Suppl. Fig. 7. As for GNSS and InSAR data, these are shown in the updated Suppl. Fig. 13 and 15.

(5) Lastly the final section doesn't rhyme well with the rest of the paper, the authors start to look for evidence of an eruption by searching for floating pumice, which is not something I would expect if said eruptions was a result of a dike propagating up from the mantle (basalt) – which is what the paper seemed to be inferring.

Our attempts to look for sea surface anomalies using remote sensing was indeed not successful, but justified by a number of other similar observations, e.g. at El Hierro or other cited submarine volcanoes. Indeed, we only report it in a single sentence in the main text (plus some more discussion only in the method and supplement). We shortened the sentence and moved it to the list of observations, providing one evidence more, on which we base our overall interpretation.

(6) There are a lot of long sentences (if 4+lines, please cut – becomes very hard to read and often

the authors are cramping in a lot of information in a long sentence so it is hard to follow where they are going).

We carefully revised and improved the text as suggested.

(7) Also, a minor point, but authors often use words that are not overly familiar to me which could easily be replaced by more common language so that the reader does not get lost. As and example.

(line 76) “here we recollect … “ why not “here we analyse” ?
(line 90) “Our results antedate “ (not even sure this is the correct use of this word, I am assuming this means your results suggests the unrest started as early as 10 August).
(line 115) “… is depicted both at GNSS stations “ (again not sure this is even the correct use of this word, but why not say “GNSS and InSAR data show transient surface deformation from August 2020 and persisting over months”.
(line 302) “seismo-vulcanic”. This should be volcanic
We accepted all suggestions, except for ‘antedate’ (line 90): the word may be unusual but our usage of it is perfectly correct, as confirmed by native speakers.

But also, line 61, “magma-volcanic”, supposed to be seismo-volcanic? Or volcano-tectonic? All three versions currently used in the paper.
We harmonized the manuscript text and the supplementary material. ‘Volcano-tectonic’ is now used when discussing earthquakes only (in order to distinguish from other type of seismic events in volcanic environments, such as LPs) and ‘seismo-volcanic’ in reference to the whole investigated process, causing deformation and seismicity.

(8) In addition to wording and long confusing sentences, the authors sometimes talk about the events in the past and sometimes in the present, as if they weren’t sure if the unrest was still ongoing or not, or weren’t sure when the study period should end. Abstract says Swarm of 85k earthquakes, giving the first impression that the event is over. The none starting Introduction (surely there is a missing sentence at the beginning of it) says, “continued over months … “. But line 79-80 then says “at the time of the writing (August 2021) seismicity is still ongoing) – but it had “faded away” already in line 32! Lines 184 to 187 then jump to a M7 event (close to Elephant Island) which occurs after the swarm is over. You leave the reader confused. Elephant Island is not even marked on Fig 1 (even though it is on the map).
We improved the text to avoid any confusion on the timeline. A label for Elephant Island has been included in Fig. 1.

(9) I also have a problem with how the data are presented.
I don’t see data uncertainties in this paper. Figure 1 has vectors without uncertainty ellipses. Same for Figure 2 (and reported ~11 cm etc, line 119-20). Colour scale for InSAR data in Figure 2 (yellow to green? Blue?) is such that the figure doesn’t really provide any info on the InSAR deformation (supplement figures are better, but there are more colours available to the authors that would help the reader). Figure 3 is rather squished and again no uncertainties for GNSS data. This is really not a nice way to present real data. Would be fine if it was a model, these are datapoints. It is really hard to judge the quality of processing and data from this figure (and not sure panel h adds any info – if azimuth was changing it would be argument for magma migration right – but the focus

on this graph automatically becomes the pre deformation azimuth as it is just looking at random noise).

Here the concern is mostly about GNSS data. We improved the layout of Figs. 1 and 2 to include uncertainty ellipses. We modified the extent of the viridis color scale (the color scale is perceptually uniform both in color and black-white and designed to be perceived by viewers with common forms of coulour blindness) used for InSAR in Fig. 2, using the same range as in supplementary figures, as suggested. As for Fig. 3 we plot now GNSS data as data points and remove panel h, as suggested. We do have GNSS uncertainties and we now plot them in Suppl. Figs. 13 and 15. Plotting them also in Fig. 3 seems redundant and the figure results overcrowded. Therefore, we prefer not to include them there; we could still do it, if the reviewer is convinced this would be extremely beneficial.

(10) Same is true for Suppl figure 12 and 13. It would have been super helpful to show datapoints (with uncertainties) in Suppl Fig13 rather than lines. That would make it a really good figure.

We modified the corresponding figures, now Suppl. Figs. 13 and 15 of the revised manuscript, as suggested.

(11) I might have missed it, but I could not find any info on the actual number of SAR images used for this study.

The actual number of SLC data is 46 images, with time interval of 12 days and spanning time started from November 2019 till May 2021. We included this information in the supplement.

And it wasn't clear to me either what the reference point is for the InSAR images.

As we have 2 GPS stations covered our study area, we consider one of the GPS stations located farther from the deformation source as a reference point and use the other GPS station (DAL5) to validate the InSAR time series results. We improved the supplement to provide this information.

Isn't it a bit problematic to remove a ramp in a deformation field such as this one, where you only have scatter of points around the coast – could you not expect a ramp naturally in the deformation field? I understand that there isn't a whole lot of GNSS data to help constrain this but that weakness might need to be pointed out.

What we did as a ramp removal was to reduce a planer surface (defined by a 1-degree polynomial) from each interferogram (not point scatters). Ramp effect might be caused by unwrapping or orbital errors. We separately remove the ramp from each interferogram. Each interferogram is a grid image covered a large extend around the study area. We did not remove the ramp from the point scatters. Once the ramp removal process is done for each interferogram we perform time series processing. While running time series analysis number of points selected as a PS (Permanent Scatter) points that construct the scatter of point shown as the final time series results. Lack of enough GPS stations does not let us estimate the ramp directly using the true observation. What we did instead was to first, fit a 1-degree planer surface over each interferogram, second remove the ramp if it has a noticeable correlation with the phase map (or interferogram), we finally use the GPS observation (considered as a reference) to remove the bias.

(12) The depth of the earthquakes, how well are they determined? I thought that you had to have a good network of stations close to the activity to get good depth estimates. Isn't the depth range

given possibly shifted up or down (so relative to each other the depth is good, but not absolute)? I understand that the strike slip earthquakes are deeper than normal but the average depth (given as 9.2±2.4 km) could be shifted up or down right?

The average absolute depth uncertainty is 2 km. We included uncertainties on various source parameters, including centroid depths (they are plotted in new Suppl. Fig. 7). Uncertainties are reported on absolute depth estimates.

(13) Moho (crust-mantle boundary). You give it as 15 km (e.g. Figure 4). Then in line 131 you put a volumetric source at 20 km depth. Is this needed? Why would you get strike slip earthquakes in the mantle? This is confusing me. And do you really need this source for your modelling of Geodetic data? It is deep and your data are limited. Do the results change much if you remove it? From Method section you seem to say it wasn't improving anything so why have it here.

The Moho depth is poorly known. Recent studies suggest ~25 km below King George Island, but likely thinner in the Bransfield Strait. Therefore, 15-20 km are plausible values. The volumetric source is set to 20 km, to be below the centroid depths (which uncertainties are now discussed). Depths of strike-slip earthquakes, as in general for all observed seismicity, hardly exceeds 15 km, even accounting for uncertainties (new Suppl. Fig. 7b), so they are within the crust.

We rephrased the paragraph to make clear that by adding the depleting source we can better model the GNSS observation (see Suppl. Tab. 2, rms without depleting source: 0.25; rms of preferred model with depleting source: 0.14). Given the limited geodetic dataset, it is difficult to resolve the reservoir features accurately. Here, we instead wish to prove that the additional presence of a deeper depleting source (required in a model of a deeper reservoir feeding the shallower dike) explains the data quite well. We restructured the text and, instead of a single value, we report now the range of volumes (either modeling only the dike or the dike plus a deeper source).

(14) InSAR data. There is ice during winter on the island that then melts (Supp Fig 15). I would expect a large seasonal signal corresponding to the melting of the ice. Probably rapid uplift starting around November. What do previous years show? Did you look at InSAR data during quiet periods? This should also show up in the GNSS data. According to the text only linear background trend was removed from the data. The time series from the Nevada page show that there clearly is a significant seasonal signal affecting the vertical data (http://geodesy.unr.edu/NGLStationPages/stations/UYBA.sta). Has there been any attempt to remove this signal from the data?

We checked for seasonal variations potentially affecting GNSS data. This cannot be verified for station DAL5, for which the available timeline is too short. At UYBA, the estimated seasonal variations have small amplitude compared to the observed deformation transient (i.e. below 2 and 4 mm on horizontal and vertical components). We mention these values in the supplement. We prefer to avoid any attempt to remove such effects, given that we could process at most one station only.

(15) Introduction.
The paper needs to start with a better key first sentence, in the very least it should say August 2020 somewhere.
I would have started it by a) setting the stage, where is it, b) defining the time period of the study, b)

putting the “massive” in scientific defenition (how much seismic moment was released). That would give the paper the constraints it currently badly needs:
“In 2020 an intense seismic swarm took place in Bransfield Strait, Antarctica, with XX number of earthquakes exceeding Mw. The swarm peaked with two large earthquakes on 2 October (Mw 5.9) and 6 November (Mw 6.0), before tapering off. The estimated seismic moment released from August to February 2021 XXX.
This way the paper has structure from then on.
We modified the introduction’s incipit, following the suggestions.

(16) Line 37, I would say “recorded” rather than “observed”.
Corrected

(17) Line 39. Here Figure 1 really needs to help by showing bigger region (the whole of Phoenix Block if possible (block not plate)).
We improved the figure layout.

(18) Line 47. Is there really no other, more recent, study of spreading rate for the region?
I had a quick look at the Nevada page, UYBA has been operating on and off since 2015 and FREI has some long time series too (even though there are large data gaps). Would not have expect it to be too hard for the authors to estimate background rates for these sites (would assume you already did this for UYBA when you remove background).
Our estimate at UYBA (~2.1 cm/y) is comparable to cited values. At this point of the introduction, where our work is not yet set out, we prefer to cite these usual references for spreading rate in the region. Instead, we have included mention to our estimate later in the text.

(19) Line 59. “The development of … including”. Presumably you are listing them in chronological order, no need for the word “including”.
Line 61. “Ongoing magma volcanic activity”. This is confusing. I know you mean the area is active (long time scale) but since the paper is on volcanic unrest this causes confusion.
I would start this with “Seismic swarms at … show that Bransfield Strait is volcanologically active”
Both corrected

(20) Line 67. I am not clear on how ocean bottom deployment can provide evidence for magma accumulation at shallow depth. I check the citation and it was not clear from that paper either that that’s what they were saying.

Citing from the reference: “… Figure 15 shows an example of the tomography data recorded in the vicinity of Orca volcano (see Figure 6). […] clear arrivals are largely absent at distances of 4-6 km from the profile center (12-14 km range), likely due to the presence of a highly attenuating region near the summit of Orca volcano which may be linked to the presence of magma or highly altered and fractured crust.”

We modified the sentence to be more specific.

(21) Line 97-8, shorten text by removing “one with a” and “another with”
Line 124-5 “Given the HISTORY OF volcanic activity IN the area”
Both corrected

(22) Line 101-103, as mentioned before, I am missing some discussion on how good vertical – and horizontal, determinations are. They are a bit of a key in the discussion of magma migration to follow.

We included here a statement on the uncertainty of centroid depths

(23) Line 119-121, no uncertainty given, just approximate numbers. Not sure 1 cm uplift is even significant.

Uncertainty are now mentioned

(24) Line 129. Again, it would be great if depth range of earthquakes (absolute) came with some uncertainty.

Absolute centroid depths and their uncertainties are now reported in the paper. We included references to the relevant figures here.

(25) And again, line 131. Is this 20 km depth source really supported/needed in the modelling of this limited dataset? Seems from method section that it cannot provide better fit (line 417-8) so why included here?

See reply above. The manuscript text was improved to make this clear.

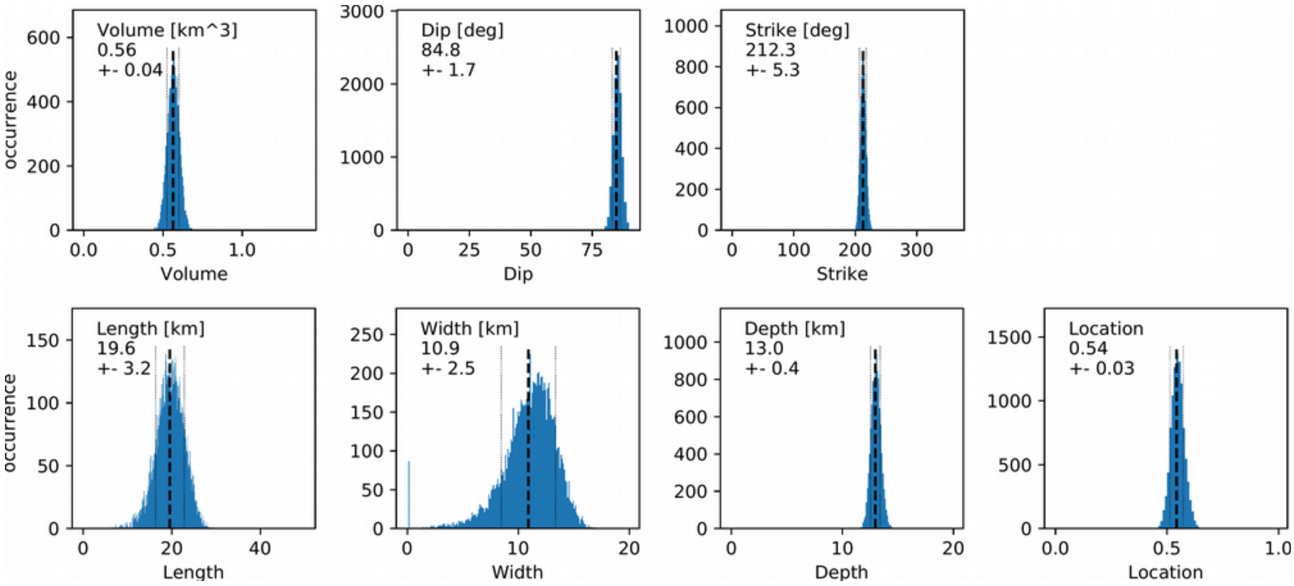
(26) Line 138. Maybe use km instead of m? (0.56 km^3). There is no indication given of how well this volume is constrained by data. What volumes fit the data within 2 sigma of the data? In Supplement table 2 there are different models given, showing volume from 0.26 km^3. Can you do some sort of monte carlo or something, using uncertainty of data etc to give some sort of bounds on this value and the length and width of the dike? These are big numbers.

We originally chose to use SI base units, now changing the text to km as suggested.

Concerning model uncertainties we followed the suggestion and report in the supplement:

"For our preferred model, we estimate uncertainties based on the GNSS measurement errors, as now given in the manuscript. We follow a bootstrap approach drawing from normal distributions with the measured component-wise displacements as means and the corresponding errors as the standard deviations. We avoid a full grid search with all parameters since it is computationally very expensive. Instead, for each of the 10000 disturbed GNNS displacements, we search for the minimum misfit between the data and the model within a 1D-Grid for each parameter, e.g., with only the depth parameter being free, while all other parameters are fixed following the preferred solution (see Suppl. Tab. 2). Consequently, we calculate the standard deviation from the resulting distribution of best-fitting depth estimates and repeat the procedure for each parameter. We find that the parameters volume (0.56±0.4 km^3), strike (±5°), dip (±2°), depth (13±1km), location (±1km along the AB profile) are well constrained, while the width (12±3km) and the length (20±3 km) show increased uncertainties."

The analysis is illustrated in the following figure



As mentioned above, the effect of a depleting source is partially counteracting the opening of the dike. The smallest volume we can estimate is thus the one obtained without depleting source (pure dike model), which is indeed 0.26 km³. Thus, we updated the main text to discuss the range 0.26-0.56 km³: these model uncertainties are much larger than those due to data only, which are now reported in the supplement.

(27) The estimated volume is comparable volume to the Bardarbunga dike intrusion which was also seismologically very intense but much better instrumented. I find it curious that there is no reference to that sequence in this paper. That 2014-5 rifiting event in Iceland showed mostly strike slip earthquakes and the dike was modelled to be above the observed seismicity (e.g., Agustsdottir et al., 2016, Sigmundsson et al., 2015).
We included these two reference to the Bardarunga case.

(28) Line 159. Not sure earthquake locations can be used like this. Normal fault earthquakes are occurring in the volume above, strike slip presumably below? How are you getting 3-4 km/day? (which would mean that you go through the whole crust in just 4-5 days). Again would be helpful if uncertainty on absolute and relative earthquake locations was presented in the paper.
Absolute centroid depth uncertainties are now included, with a dedicated figure in the supplement. A rough migration velocity estimate of 3-4 km was originally derived from the delay of the first (shallow) normal fault earthquake (~3 days) and the depth difference to the early deepest events (~10-12 km, considering ~15 km for the early deep ones and ~3-5 km for the shallow normal faulting). We still think that having some quantitative estimation for the vertical migration velocity is useful here. The velocity is indeed comparable to the vertical migration at Mayotte (Cesca et al. 2020).

(29) Line 175. I find it curious that the authors talk about the unloading of ice as affecting seismicity migration, but do not consider it as a source of deformation signal in GNSS and InSAR data.
We included mention to potential ice unloading effects to geodetic data.

(30) Line 196, Bardarbunga 2014-5 should be added as an example of magma migration and seismicity, really well documented example.
We added two references to the Bardarbunga magmatic migration.

(31) Line 213, again, please some sort of confidence regions on these model numbers.
We added confidence intervals

(32) Line 450-70. Why would you expect floating pumice? This is a rifting basin where you are creating ocean floor (according to text). In line 64 you talk about basalts. You have a fast moving dike from (well paper claims) 20 km mantle depth. Should this not be a basaltic dike and thus basaltic eruption? I am just very confused. I think this section works against the paper. It makes absolutely no sense. What is the water depth in this region? Are there any tremor signals suggesting eruption at the JUBA seismic station? Perhaps the most recent volcanic eruption in Iceland should be an example for discussions here as leading up to the eruption both seismicity and deformation slowed down, the eruption started aseismically with no significant deformation (there might be a abstract or a paper somewhere by now you could cite, not sure).

We moved this part of the text in the previous part of the manuscript, where we describe observations (see reply to previous question #5).

(33) Lastly, when reviewing the paper I had to google the activity to get some better information to better understand the paper. I came across a paper Olivet, Bettucci et al, “A seismic swarm at the Bransfield Rift, Antarctica”. The authors should cite that paper.
(https://doi.org/10.1016/j.jsames.2021.103412)
We now early reference this new paper, which was not published at the time of our first submission.

16th Feb 22

Dear Dr Cesca,

Your manuscript titled "Massive earthquake swarm driven by magmatic intrusion at the Bransfield Strait, Antarctica" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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

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

Best regards,

Luca Dal Zilio
Editorial Board Member
Communications Earth & Environment

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

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I noticed that the manuscript was improved and all comments and suggestions were done by the authors. In my opinion, the manuscript is suitable for publication.

Haruhisa Nakamichi

Reviewer #2 (Remarks to the Author):

I have now read the revised version of a paper by Cesca et al. on the earthquake swarm at the Bransfield Strait, Antarctica. The authors have addressed my concerns with the previous version, improved both figures and text and added uncertainties where appropriate.

This paper is a significant contribution to the field of volcanology and will be of interest to the larger scientific community. I therefore endorse its publication in the current form in Communications Earth & Environment.

Rebuttal letter

Dear editor, dear reviewers,

we are thankful for your appreciation to our work. We accompany our final submission with this short rebuttal letter to confirm we considered the last editorial requests. We report the original comments in black, and our reply in blue.

Simone Cesca (on behalf of all authors)

Dear Dr Cesca,

Your manuscript titled "Massive earthquake swarm driven by magmatic intrusion at the Bransfield Strait, Antarctica" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table". Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files.

We filled and uploaded the filled “Editorial Requests Table”. The manuscript has been improved with very minor edits to account for these requests.

SUBMISSION INFORMATION:

In order to accept your paper, we require the files listed at the end of the Editorial Requests Table; the list of required files is also available at https://www.nature.com/documents/commsj-file-checklist.pdf .

Requested files have been uploaded, except for those items which do not apply to our manuscript (Main table(s), Boxes, Supplementary Audio, Supplementary Movies, Supplementary Software, Life sciences reporting summary, Suggested feature image).

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be asked to declare that all required third party permissions have been obtained, and to provide billing information in order to pay the article-processing charge (APC). **Please note that that we will not be able to move forward without having first received your payment information and therefore we ask that you please have this information ready when submitting the final version of your manuscript. We will also need your third-party declarations.**

The information is ready at the time of submission.

Best regards,

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We are thankful to both reviewer for their accurate reading and constructive suggestions, which helped improving our manuscript.