First round of revision

Based on reviews I have received, your manuscript may be suitable for publication after some revisions. The reviewers made several comments that should be considered carefully. In particular, I would suggest to discuss carefully the methodology and results compared to studies about other regions.

The paper is interesting and describe a dataset acquired in a region difficult to access and to monitor. There are very few similar studies in this particular region of Gakkel Ridge and only a few passive seismic studies all along Gakkel Ridge. It does provide valuable informations to understand the volcanic processes at slow spreading ridges.

The processing to obtain the catalog is well explained but some informations are missing that could help the reader better assess the different catalogs and subset and the quality of locations and detections. For exemple, the number of events detected at each step or after each selection should be given. Other informations like Vp/Vs ratio seems to be implied and there are no direct references or explanations on how they were choosen or calculated. The author have certainly all the informations to answer those questions.

Some additionnal parameters of the catalog could be calculated and would help the discussion and the characterisation of the seismicity : apparent Vp/Vs from phase arrivals (Chatelain or Wadati diagram), b-value of both swarms, magnitude of completeness. Those values could add interesting arguments to assess the tectonic or volcanic origin of the seismicity in the discussion. Finally, there have been explosive volcanism clues at other place along Gakkel Ridge (doi:10.1038/nature07075). The setting in this particular place could be different, but the link with different volcanism detections along the ridge could be discussed to place this study in the overall Gakkel ridge litterature corpus.

We cordially thank the reviewer for taking the time to review our manuscript and for these mindful suggestions. We now discuss our results in the context of known Gakkel Ridge volcanism. In addition, we provide a b-value and completeness magnitudes. Wadati diagrams have been added to the Supplementary Material.

Here are some additional comments referenced by the line in the manuscript

89 - Why is it particularly challenging to search small events in oceanic environments ?

The sea surface is a strong noise source causing broadband inference leading to an increased noise level particularly below 5 Hz (Webb, 1998). At higher frequencies, at which local earthquakes can be detected, further ocean-specific noise sources may drastically deteriorate the detection threshold of small events. For example, ocean currents cause vibrations of different parts of the OBS structure (Essing et al., 2021; Stähler et al., 2016). Other sources of high-frequency unwanted signals are so-called short-duration events or whale calls, that all lead to increase noise levels and problems in signal detection and classification (e.g. Domel et al., 2023). In addition, OBS are deployed in free-fall mode, such that optimal ground coupling cannot be warranted. OBS networks are costly to deploy, such that a comparably small station spacing needed to lower the detection threshold is likewise not as easily to realize as on land. We added some of these aspects to the manuscript. We modified the text in line 93-97.

97 - Number of earthquake detected and located only appears line 166, but there is a number of selected events here. It would be helpfull to understand how selective are the parameters if we had the number of events for every sub-catalog in section 2. - it seems that two subcatalogs are used one with RMS < 0.1s and nphases > 8 for station corrections, one with RMS < 0.1s for templates construction.

Yes, this is correct. We added the specific numbers of the catalogs throughout the text. Since the earthquakes are mostly located outside of the seismic network, the selection criterion for events for the calculation of station corrections was strictest to avoid including poorly located events with large residuals. In the dataset provided as supplement and uploaded to Zenodo, we include all events that have 4 phase readings and are within 40 km as a minimum quality criterion ensuring that underdetermined or poorly constrained regional events are not misused in geological interpretation.

99 - It would be interesting to give and if needed discuss the station corrections computed. They can give insights into structures or crustal anomalies

Station correction terms are fairly small, but were inconclusive in terms of subsurface structures. We compared them also to parasound records, but the thinner sediment cover at GKD03 relative to the other stations did not entirely account for the observed station corrections. We therefore do not use or discuss them in a geological context, but now provide a Table with the numbers in the Supplementary Material.

100 - Which profile of Ding et al was used for the velocity model ? And with which Vp/Vs ratio ?

We used the profile at pos. 1 of Ding et al., assuming that a position on a pronounced volcanic ridge with high crustal thickness may approximate the subsurface at the location of our network best. We added this information to the text in line 108. We used a vp/vs ratio of 1.77 which seemed reasonable from interstation pairs of S versus P arrivals and our prior experience with seismic networks in geologically similar settings. Line 112.

107 - "depth poorly constrained" could you add an estimate of the uncertainty in km ?

The estimate of depth uncertainty is 4.5 km as indicated in the sentence above. We did some additional testing and located the earthquakes with different fixed depths from 15 km to 5 km. It clearly turned out that the northern cluster can best be located with fixed depths of about 15 km, while the southern cluster locates best at depths of about 5 km. Any geological interpretation should not go further than this estimate. The manuscript also does not go further in the interpretation.

108 - The equation and the units of measure are not completely clear. The Hutton and Boore relation assumes measurements in mm on a Wood Anderson recording and the manuscript talks about amplitudes in nanometers.

We used the implementation in SEISAN, that includes a version of Hutton and Boore with amplitude readings in nm. A Wood-Anderson recording is simulated before the amplitude reading. We clarified this in the text in line 119-120.

116 - It could be said more simply by "2s time window centered on the P and S phase arrival"

We changed the MS accordingly in line 126.

120 - Why use a Vp/Vs ratio of 1.7 ? Is this the Vp/Vs ratio used in the velocity model ?

We appreciate the opportunity to clarify this point. In our analysis, we indeed used the more precise value of 1.77 as input for the velocity model and the less precise value of 1.7 for predicting the arrival times of seismic phases during the template extraction. We realize that there was an oversight in keeping these values consistent.

Through a simple calculation, we here clarify that even with the less precise value of 1.7, the differences in estimated travel times for the phase arrivals are minimal compared to the window length used for template extraction.

For a p-wave velocity of 6.5 km/s (Ding et al., 2022) and a distance of 40 km between event and receiver (this is the upper threshold of distances we captured reasonable detections in this array) we retrieve a travel-time of 10.47 s for the s-wave assuming a vp/vs ratio of 1.7. A vp/vs ratio of 1.77 would lead to a travel-time of 10.90 s. The difference in approximated travel times from the two different vp/vs ratios is of 0.47s for events coming from this maximum distances. Comparing this time difference resulting from the two different vp/vs ratios to the overall window length of the template that is 2s demonstrate its minor impact and assures that the results of the template matching will not change, with changing the vp/vs ratio the more consistent and precise value of 1.77.

We therefore would prefer not to mention the less precise value in the main manuscript but indicate it in the supplementary material (Table S2) referenced in line 132.

129 - All events from the manual catalog are used for the templates, whatever is the RMS ? Or only the subset of events with RMS < 0.1

We clarified the part, now indicating that we only used high-quality templates having an RMS < 0.1 and include least 6 components having with an SNR>3. Line 140-141

137 - I assume it is weighted by the SNR calculated on the continuous signal tested - a supplementary figure of one event detected with the templates would help following the method

We realized that the explanations were not easy to follow. We reworded this paragraph (line 148-152) to make better accessible what we meant. We couldn't find a way to convey this in an intuitive figure and hope that the explanations are now sufficiently clear.

146 - I do not understand "the final TM", the previous paragraph does not seem to imply several runs of the TM, there seem to be only one run

Indeed, we changed the MS in order to make it better accessible. Line 159

166 - Does the template matching recovers all the manual events ?

Yes, this can also be seen from the catalogue. Templates have a CC of 1.

177 - A phrase should be added to explain why ambient noise is lower when there is less ice cover. Land stations in Antarctica are noisier during summer time when the iceshelf breaks away;

We have intensively studied the seasonal noise levels of the sea ice cover in a separate study in this data set, currently in review. One important difference to Antarctica is that the Arctic Ocean is fairly enclosed and sea ice is under compression during winter times – In Antarctica, the sea ice is surrounded by open water and hence not so much under compression. In the Arctic, freezing stops towards the end of May, melt ponds appear everywhere and the ice changes its physical properties. Therefore, we expect a difference to Antarctic sea ice. We added a more detailed explanation in the text in line 193-194.

205-204 - Would be interesting to also have a map like fig1c but color-coded by time to show the migrations of the templates. It could be added to figure 5 with a symbol or color to distinguish located event from TM detected events or it could be a supplementary figure. This could help to understand spatial cluster 3 migration and description.

We investigated how well the locations of the template reflect the migration that appears as changes in the S-P traveltime. For the start of the June sequence, a lateral migration is visible for the first events. For April, a downward migration. However, the location error is larger than the actual changes in location. Therefore, the figure below can only serve as some support to a trend that is more robustly seen in realibly determined S-P traveltime differences. We added the figure to the Supplement and referenced it in the text.

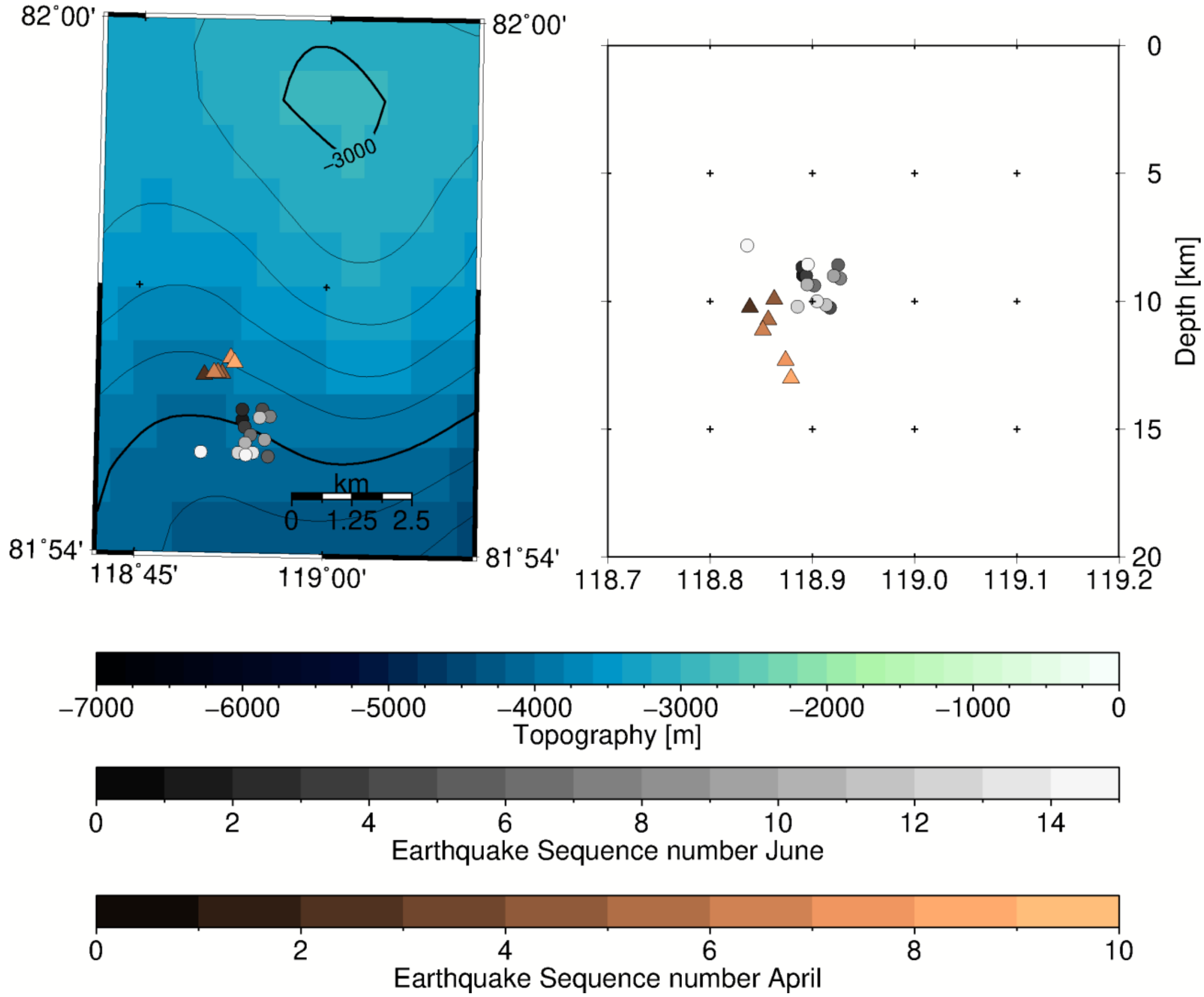

Figure 1: Earthquake location during the start of April and June bursts. A shift in hypocentres can be seen.

271-275 - This could be supported by observed Vp/Vs ratio from events seen by the network. Selecting events-station observation from ray-path travelling through the aseismic area could show some Vp/Vs ratio different from ray-path that do not travel through the aseismic area

We thoroughly investigated Wadati diagrams and vp/vs ratios. Indeed, one of the motivations of this study was the observation that Wadati diagrams looked very different depending on the position of the earthquake source relative to the network. Fig.2 shows Wadati diagrams for two example earthquakes in the northern cluster in a and b, and for two examples from the southern earthquake group in c and d. vp/vs ratios are very *different. If plotted for several earthquakes, the different gradients for the two group are likewise apparent (Fig. 3). However, when we looked at average station residuals, there*

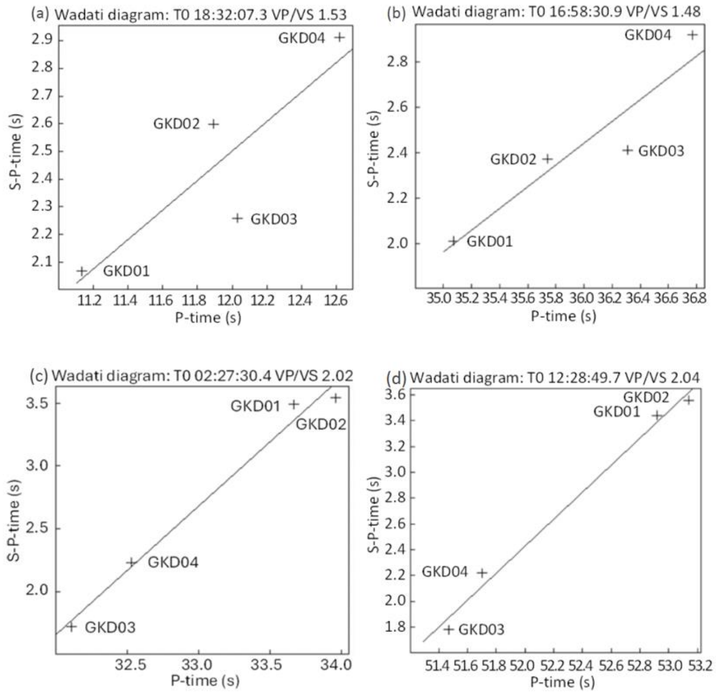

Figure 2: Wadati diagrams for 2 earthquakes from the northern (a,b) and southern (c,d) cluster, respectively.

were no differences for the southern and northern groups, both produced the same average station residuals. We therefore used the same station correction terms for all stations. As a result, the we now obtain reasonable Wadati diagrams without pronounced deviations (Fig. 4).

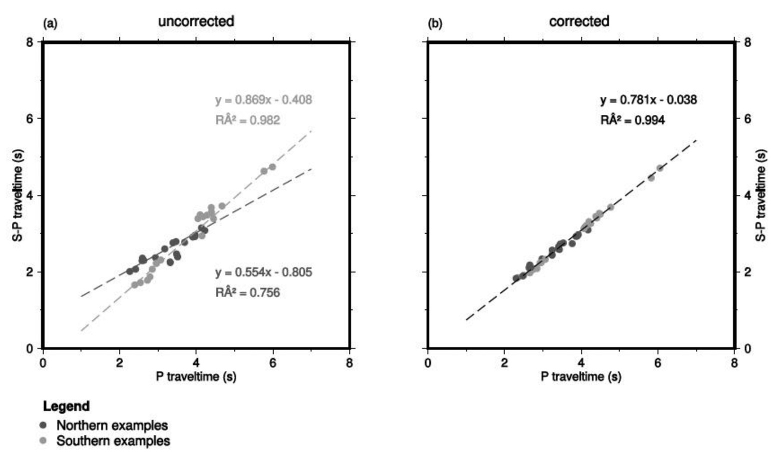

Figure 3 Pseudo-Wadati diagrams for earthquakes from the southern and northern cluster before and after the application of station correction terms.

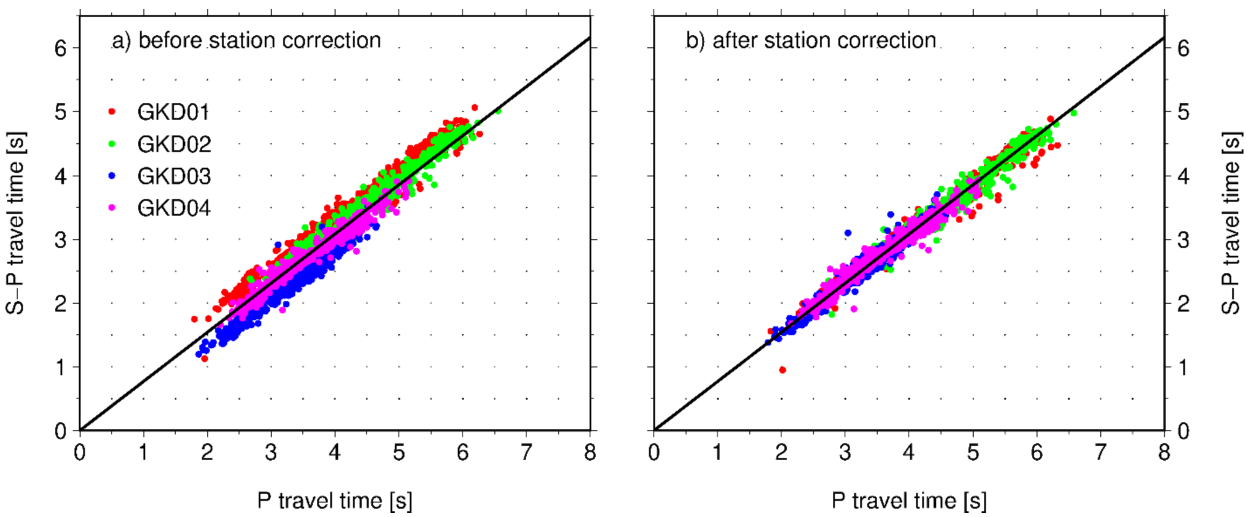

Figure 4. Pseudo-Wadati diagrams coloured station-wise for all earthquake regions. The constant shift of earthquakes from one station relative to the other stations is apparent. After station correction, all events align along one linear trend with a vp/vs ratio of about 1.77.

Fig. 5 shows the ray paths from the earthquake sources to the stations and marks an area, where there could still be increased vp/vs ratios without any indication for it at our network, since there are no rays crossing this critical area.

So, unfortunately, we do not get any conclusive additional information from vp/vs ratios at this location.

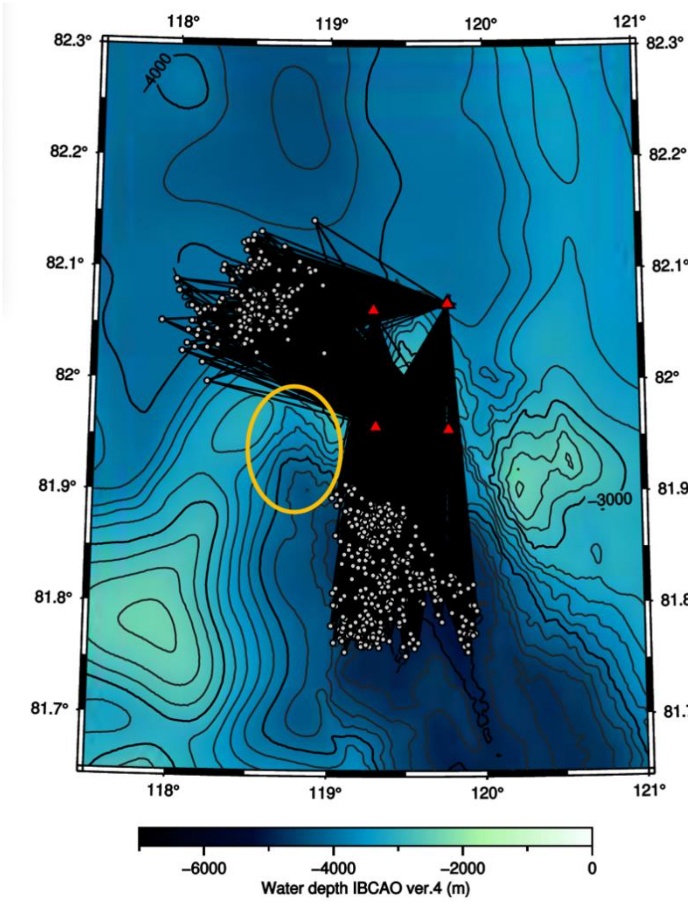

Figure 5: Ray paths from earthquake sources to the stations. The orange circle marks a region in the aseismic area that is not crossed by seismic rays. This area might contain melt, but this would go unnoticed with our small network that is placed outside the main rift valley due to the proximity of the Russian EEZ.

275 - Is there a bibliographic reference or source of this local tomography ?

Yes, this is Meier et al. and Schmid et al. referred to in the sentence above. We clarified this by rewording in line 294.

283-284 - Detection of hydro-acoustic events (if the used OBS have hydrophone recording) in the data could help confirm effusive activity during the deployment. At least this possibility could be mentionned and open up for future study of this dataset

The OBS were equipped with hydrophones. However, the records are not usable (Fig. 6). We used for the first time a new data logger which was supposed to have a higher preamplification on the hydrophones. However, this turned out to be not true, such that we hardly see any signals on the hydrophones, that could be reliably interpreted in terms of *volcanic activity.*

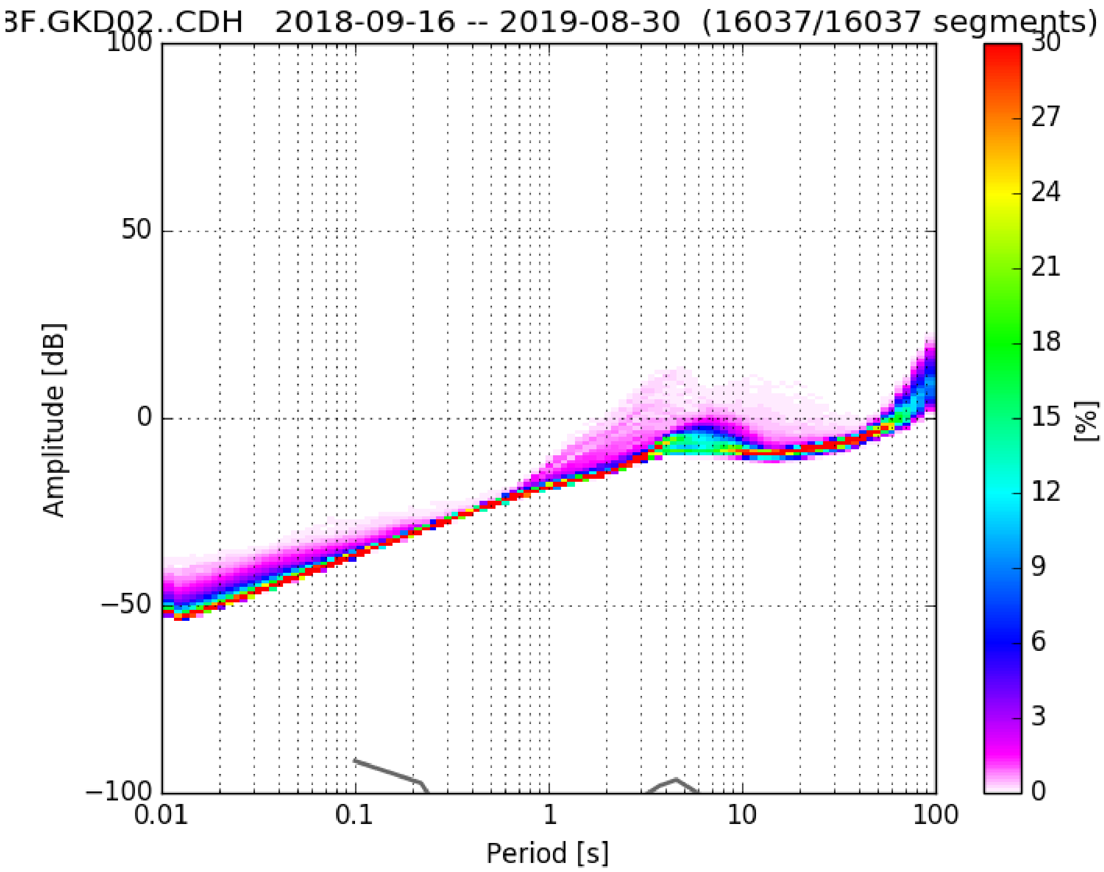

Figure 6. Probabilistic Power spectral density plot of the hydrophone channel of GKD02. There is hardly any variability seen because of insufficient preamplification.

290-300 - By comparing the results with White and McCausland (https://doi.org/10.1016/j.jvolgeores.2019.03.004), could this diffuse seismicity and aseismic gap be distal VT and magmatic center respectively ?

We believe that Gakkel Ridge is a different geological setting compared to the mostly subaerial stratovolcanoes described by White and McCausland. Magmatic activity typically occurs at elongate axial volcanic ridges rather than confined volcanic edifices like strato-volcanoes. In addition, the magmatic systems seem to be very long-lived. The volcano studied by Schmid et al. 2017 showed intense earthquake activity over 16 years prior to our OBS network deployment. Likewise, Logachev volcano studied by Meier et al. 2021 in 2016 showed the same aseismic area as observed already in 2009. The volcanic centres at Gakkel Ridge are clearly identifiable in bathymetric maps by elevated axial *volcanic ridges and pronounced chains of off-axis highs that testify to continuous crustal production at these sites. Therefore, we do not think that the volcano was necessarily dormant for a longer time period as in the study of White and McCausland. We likewise do not know, whether any of these volcanoes produced effusive activity or whether just dike intrusions occurred.*

We rather think that the organized seismicity is of VT nature, triggered by dike intrusions, and confined to a very narrow region adjacent to or within the central hot and aseismic area. The diffuse seismicity in contrast happens throughout the year with no signs of a temporal relation to the clustered seismicity, likely a "normal" tectonic activity.

Fig. 7 is from Meier et al. 2021 at the Logachev Seamount. Along the entire ridge, also in less magmatic areas, there is more or less continuous seismic activity unrelated to magmatic activity. This is likely comparable to our diffuse seismicity. The close-up in Fig. 8 shows the seismicity that is organized in time. It is at the margins or within the aseismic zone and occurs sporadically with a duration of few days, and occupies a narrow spatial area. We think that we see here either the advancing dike (although only few of these swarms even after relocation with HypoDD showed clear signs of migration), or the tectonic reaction to a dike intrusion below.

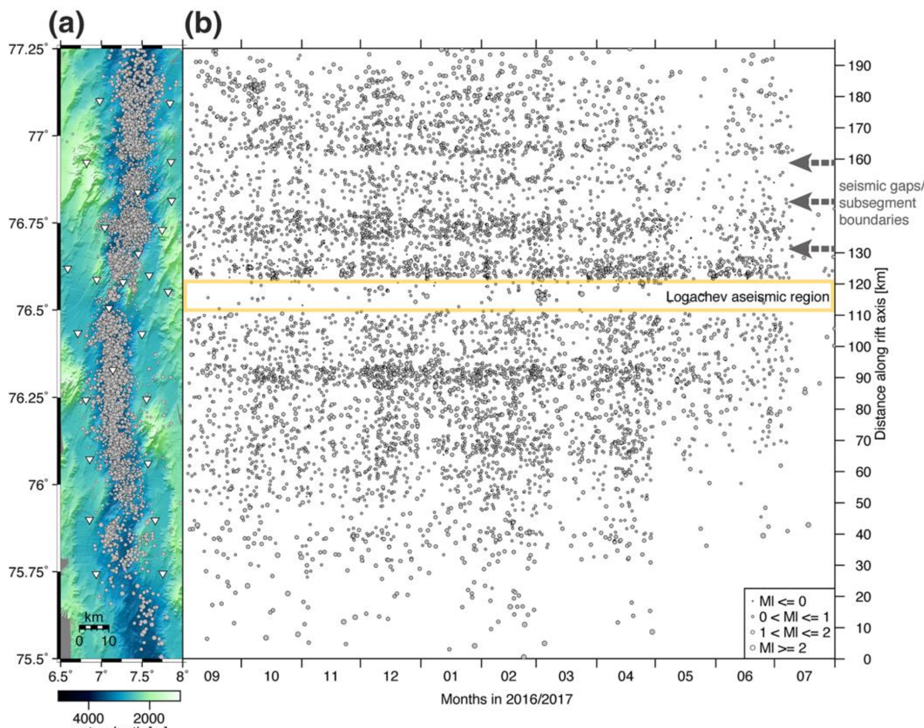

Figure 7. Seismicity along Knipovich Ridge. The aseismic area and diffuse seismic activity filling the entire rift valley are stable in time.(Meier et al., 2021)

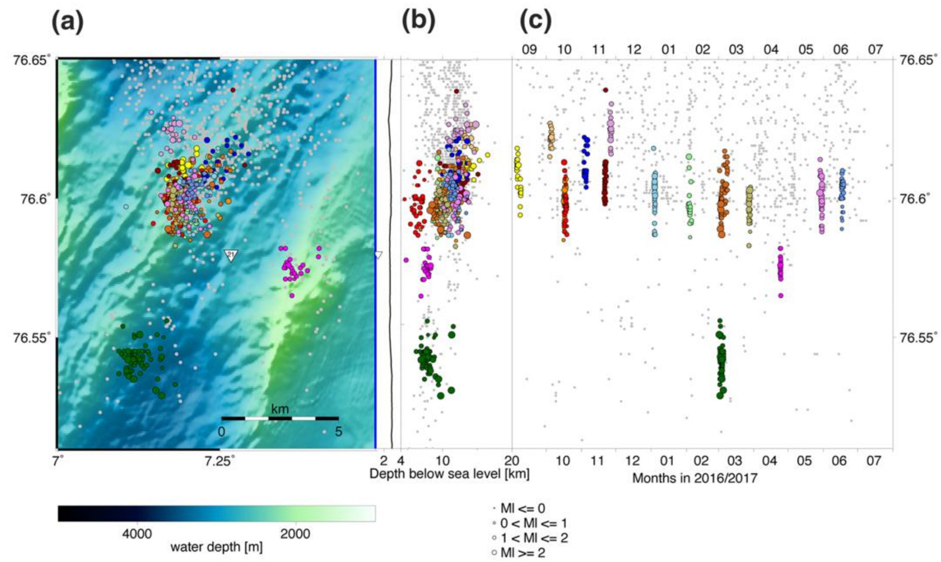

Figure 8: Swarms of earthquakes confined in time and space occur at the margin of the aseismic area. (Meier et al., 2021)

Since the setting in White and McCauseland is quite different, we would not discuss this in the manuscript but rather extended our discussion to other locations at Gakkel Ridge, although seismic records there are confined to seismometers on ice floes drifting for a short time period in the vicinity of the volcanoes. These records do not give the same coverage of seismicity in time and space.

Figure 1 - trace of the whole Gakkel ridge and experiment position would help position this study relative to other referred studies, maybe in a map similar to the globe but zoomed to the polar region only - in b, the bathymetry profile should be added, there are references later in the manuscript to the topography above the swarms and that would help the reader to follow - it is not clear whether the earthquakes are all the manual located, the one with RMS < 0.1s or only those with RMS < 0.1 and more than 8 phases - in b, the legend of the color dots should be ordered by date - a cross-section of fig 1c would help understand the description of results at line 234

We reorganized figure 1 according to the comments. We added an overview over the whole Gakkel ridge, indicating our and cited experiments. To the cross-section, we added bathymetry and sorted the legend by increasing dates.

Jean-Marie Saurel

Reviewer b

Essing et al. generated an earthquake catalog based on data from a one-year, four-station OBS deployment at the Gakkel Ridge Deep and interpreted the spatiotemporal evolution of the seismicity in relation to tectonic and magmatic processes. The manuscript is generally well-written and can be a valuable contribution considering the limited number of microearthquake studies in this region. However, I have a moderate amount of questions and comments that I hope the authors can address in detail, which I believe will help improve the manuscript:

We thank the reviewer for taking the time to review our manuscript and for the helpful suggestions. We have carefully addressed all questions and comments in detail below.

Lines 60-62: "…indicators of magmatic activity although not reflecting directly the movement of magma" – I'm not exactly sure what you mean with the second part of the sentence. Are you saying the swarms are not related to diking hence "not reflecting directly the movement of magma"? If so, what "magmatic activity" are they indicating?

We indicate here that the seismicity is not triggered directly by the migration of the dike via crack opening at the dike tip, but we rather think that the seismicity results from the related stress change around the newly formed dike, reflecting for example subsidence as the dike cools. We modified the sentence accordingly in line 61-65.

Lines 85: "…from the here presented experiment…" – maybe revise to "…from the experiment presented in this study…"?

We changed the phrase accordingly in line 89-90.

Figure 1c: x-axis label in the left corner should be 118.6 instead of 18.6?

In our reorganized figure 1, we changed the label accordingly.

Line 105: what do you mean by "expectation hypocenter"? Is it simply the best-fit hypocenter output by NonLinLoc?

Nonlinloc outputs a maximum-likelihood hypocentre and an expectation hypocentre (Fig. 9). The maximum-likelihood hypocentre may end up at the very margin of the scatter cloud of probable locations, while the expectation hypocentre is located at the centre of the error ellipsoid that encompasses the scatter cloud. Since focal depths are not very well *constrained in this network geometry, quite a few maximum likelihood hypocentres ended up at 0 km depth. We therefore preferred to use the centre of the error ellipsoid as best estimate of the earthquake location.*

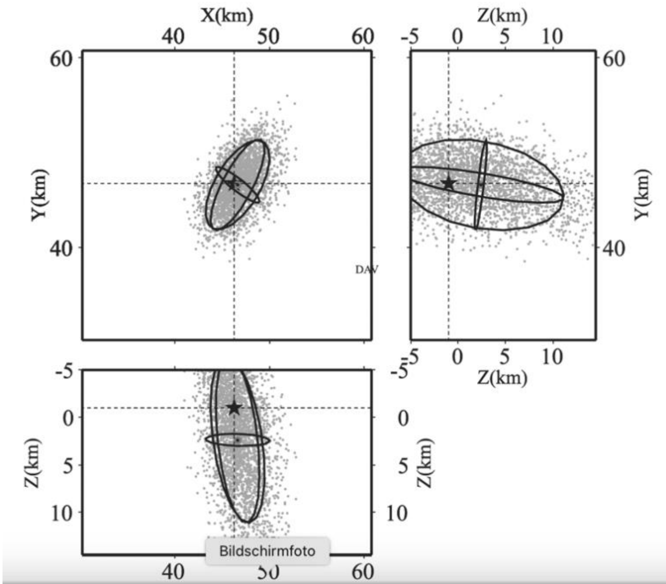

Figure 9. Scatterplot of nonlinloc with Gaussian error ellipsoid. Star marks the maximum likelihood hypocentre. The expectation hypocentre is marked by a circle at the centre of the error ellipsoid. From Husen et al. (2003).

Lines 104-105: “In cases where multi-detections contain the self-detection of a template, we keep the template…” – do you mean you only keep the template and remove all other detections? This is confusing because shouldn’t all templates detect themselves with perfect CC value, so I would imagine you always have to remove the self-detection and keep the next largest CC value detection instead.

In situations where multiple templates detect a single event that is itself already a template, we retain only the auto-detected event. In this case, the auto-detected template is already part of the manual catalog. We have revised the paragraph 159-169 for clarity.

Figure 3: what are the b-value and magnitude of completeness for the two catalogs?

We now added the values for the magnitude of completeness as well as the approach to estimate it to figure 3 in the manuscript. For the estimation, we were using the goodness-of-fit test (GFT) as well as the the maximum curvature (Maxc). Maxc systematically underestimates the values of Mc by ~0.2 relative to the GFT. We therefore report the values of GFT in the manuscript. B-values for the two catalogs are ~0.80.

Line 178: If indeed the TM detection capability shows seasonal variation, do you see such seasonal variation in the estimated magnitude of completeness e.g., using the maximum curvature or goodness-of-fit methods?

Indeed, the magnitude of completeness (Mc) changes with the variation of the noise level. In the case of this experiment, during winter (high noise level) the Mc is at ~0.5 (like the Mc for the entire experiment). During summer (lower noise level) it drops down to a value of ~0.2. This is a prominent feature found in many earthquake catalogs and discussed for example in Sanchez-Reyes et al. (2021). For this test, we were using the goodness-of-fit test (GFT) as well as the the maximum curvature (maxc). Maxc systematically underestimates the values of Mc by ~0.2 relative to the GFT. We therefore report the values of GFT in the manuscript.

Lines 196-197 and Fig. 2: I find it quite puzzling that the M>4 events are not associated with high COV values – the aftershock sequences should not be Poissonian/occurring independent of each other.

The observation of the reviewer is certainly puzzling. We double-checked the event catalog and can clearly say that also in the manually picked catalog there is hardly a pronounced aftershock activity related to the M>4 events. Daily event rates are fairly normal after these events and in large contrast to the activity during the swarms. For example, the event on May 30 is followed by 3 events on May 31 and June 1 in the vicinity of the main shock, all of them with magnitudes < 1. After that, there are no events close-by until 16 days later. Bohnenstiehl et al. describe aftershock sequences that are to some part related to transform faults or compressional ridges. Hence, the tectonic setting differs from the one shown here, where rare M>4 events occur in the vicinity of a likely warm area that potentially does not produce many aftershocks. We incorporated some of this discussion in the text in line 214-216.

Lines 200-204: “none of these periods of temporally organized seismicity yield increased moment release” – from Fig. 2d, I think there’s a perceivable increase in moment release associated with the April swarms (just not as large a moment release increase compared to when there’s an M>4 earthquake).

Indeed, there is a very minor increase in seismicity. We slightly rephrased the sentence in line 221-223.

Lines 233-238: Why did you choose to use only S-P time difference changes, which in one case is inferred to be related to depth changes while in another case is inferred to be related to activation of more distant area? Base on S-P time alone it's hard to differentiate the two possibilities. Did you try double-difference relative relocation e.g., using hypoDD to see if it's resolvable despite the relatively poor station coverage?

In this specific environment, obtaining double-difference relative locations is challenging due to the limited availability of only four stations all on one side of the seismicity. We refrained from a double difference relocation as we would likely also obtain a result that was prone to systematic errors due to the large observational gap. S-P time differences and their changes, however, are an observable that is independent from the location quality. Nevertheless, we checked whether the trend observed in the S-P time differences is also reflected in absolute hypocentre locations, despite the limitations in location accuracy. We selected templates from the onset of the bursts that had 7 or 8 phase readings and thus the best possible constraint. The lateral and depth changes are also seen in this plot. We added the plot to the supplement.

Lines 260-262: "seismic events scattered in time and space without obvious organization" – From Fig. 2b, there are many streaks that I assume are aftershock sequences (hence not "random"), which are expected for seismicity related to tectonic release on faults. I'm puzzled why they are not associated with high COV value since they are clearly not Poissonian? Shouldn't the COV analysis identify both aftershock sequences and swarms, and then further analysis is necessary to differentiate the two types of clustered seismicity?

We agree that there are more streaks than we discuss in this work. From our observation it seems that aftershock occurrence is not very pronounced in the study area. Another argument against aftershock triggering is the general lack of large events (only 3 events Ml>4) which is why we argue that streaks are mainly related to swarms more or less strong temporally organized (as the three with the strongest organization in April and June). And indeed, the COV does capture that exhibiting large values for most of the streaks in the IEs (see figure 10 below). Streaks that do not overlap with large COV are probably too short in time, therefore not the dominant mechanism during the time window of 1 day (time window we take the IEs from). For the first group of seismicity that we generally refer to time periods without streaks and temporal organization. We rephrased the sentence 280-281.

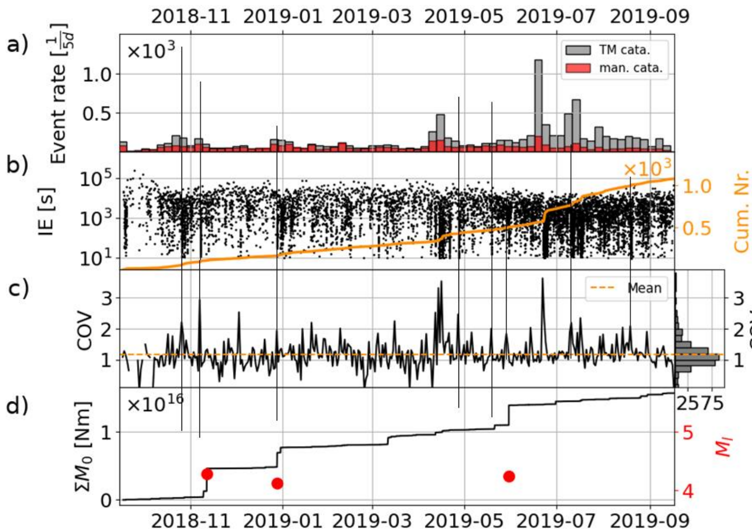

Figure 10 Connecting the streaks to the values of the COV

Lines 279-280: “The occurrence of similar-sized largest events late in the bursts suggests swarm behavior rather than mainshock-aftershock sequences” – I recommend having a figure to show this pattern.

The reference to figure 5 was missing and has now been added. Line 301

Lines 284-285: “ the character of the bursts is similar to Icelandic diking episodes” – however, swarms related to diking episodes are usually associated with lateral migration. Do you observe clear lateral migration in the seismicity? In addition, swarms can also be simply related to slow-slip/fluids instead of magmatic processes e.g., Vidale and Shearer, JGR 2006; Ross and Cochran, GRL 2021; Liu et al., Geology 2024. So why must these swarms be necessarily related to magmatism?

We can certainly provide no evidence for magmatism versus fluid migration. We do see some migration in hypocentres and have added Fig. S5 to illustrate this. S-P times are a more direct observable. They change, which is an indication that the earthquake source is moving. Our analogy to the Icelandic example comes also from the frequency of occurrence of such swarms and from their temporal duration. The earthquake swarms described by Ross and Cochran, for example, last for many months. Liu et al. describe a subaerial setting. We think that both of these locations are no good equivalents to our observations. Geologically the most comparable location is the Reykjanes Peninsula or the Krafla volcanic system in Iceland. We believe that the swarms we observe are similar to those currently happening on Reykjanes, Iceland. The area there shows a couple of swarms per year, each lasting on the order of few days, including occasional stronger earthquakes. Over the past year, 5 eruptions and three diking events took place. Spatially, *the Reykjanes earthquakes occur mostly in the area of the dike. In our case, we see that the swarms occupy repeatedly the same narrow area at the margin of a presumably hot area devoid of seismicity. We saw 3 major swarms each lasting max 2 days. However, the Reykjanes area also shows seismicity that has been related to fluid injections into an aquifer below the seismicity, producing likewise swarm activity of similar magnitudes and temporal pattern (Flóvenz et al., 2022). However, seismicity there is not adjacent to an aseismic, hot area but rather on top of it. Some magmatic diking is also needed in this setting to recharge the hot area. A migration of seismicity is not described in this context. While we cannot exclude fluids as source for the swarms, we believe that magma intrusions are more likely. It is also unclear, whether the gas rich fluids described in this publication would be present in a submarine setting with the overburden of 4 km of water column. However, earthquake activity in the context of submarine fluid flow in hydrothermal systems does also exist. Tolstoy et al. (2008) describe such seismicity at the East Pacific Rise. It is confined to the area above an axial magma chamber. Earthquakes are shallower than 1.5 km and a maximum magnitude of Ml=1.4 is reached. Our seismicity appears stronger and deeper and rather at the margin of the hot area than on top of it.*

For slow-slip events, we would expect that these might happen on any of the large bounding faults of the rift valley and not necessarily in the center of the rift valley close to a warm area.

We slightly expanded our discussion of a probable magmatic origin in the text to include some of these aspects. Paragraph 333-342.

Line 296: “indicates intrusion toward the GRD” – but I thought you didn’t identify any high COV swarms in the northwestern cluster? Do you actually observe any seismicity migration towards the GRD?

The northwestern cluster is not located near the GRD. The GRD starts at the southern tip of the southeastern cluster. We realize that the wording was misleading and accordingly modified the text. Line 319-320

Lines 294-295: “with diffuse seismicity extending to the depths of about 10 km” – from Fig. 1b, it appears more like there’s no seismicity at < 7 km (whereas that’s the depth range where seismicity for the south-eastern cluster is concentrated). If the deeper seismicity for the north-western cluster is related to a colder lithosphere, why isn’t there seismicity at < 7 km depths?

This is an observation that we made already at several locations along ultraslow spreading ridges. Fig. 11 is from Meier et al. 2021. Amagmatic areas seem to be devoid of shallow seismicity but display a deeper band of seismicity with sub-parallel upper and lower limits.

We suggest that these upper and lower limits are temperature-controlled. The lower boundary is the brittle-ductile boundary. The upper boundary could represent a temperature-controlled stability boundary for alteration of rocks. A potential explanation is that mantle rocks are present at shallow levels and are serpentinized to such an extent that deformation occurs aseismically. Predominantly magmatic areas in contrast, typically show shallow seismicity.

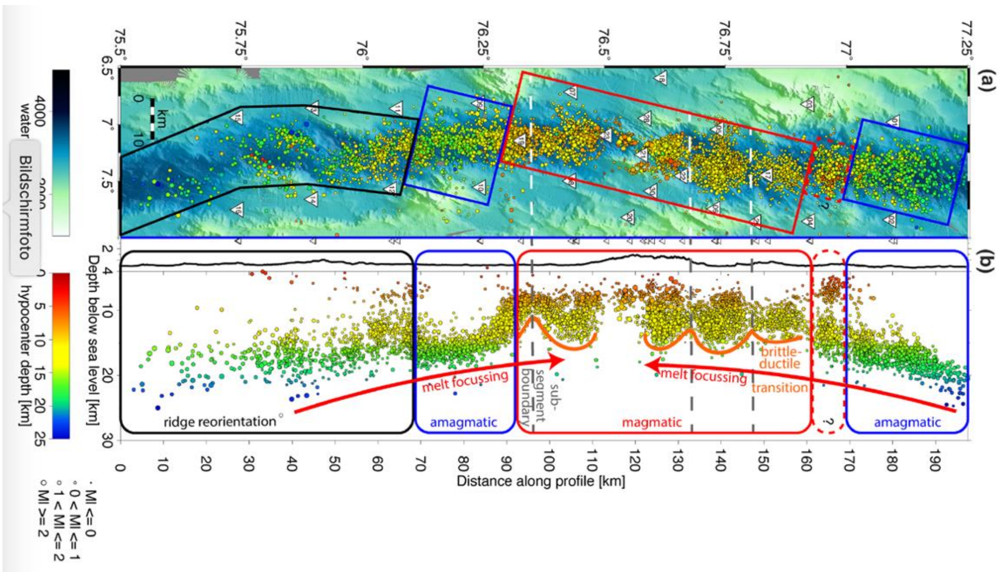

Figure 11. Interpretative figure of the seismicity along Knipovich Ridge (Meier et al., 2021). Shallow seismicity is typically absent in amagmatic areas and present in more magmatic sections of the ridge.

Line 312: “is characteristic of ultraslow spreading independently of the spreading rate.” – not sure what you mean by “independently of the spreading rate” when you are specifically referring to ultraslow spreading ridges?

Within the group of ultraslow spreading ridges, there is still quite a variability of spreading rates, varying from about 15 mm/y at the western end of Gakkel Ridge to about 5 mm/y at the location studied here, thus a variability of a factor of 3. One theory is that with decreasing spreading rates of ultraslow spreading ridges, magmatism should become even less. From our study, it looks like the same type of volcanic centres is encountered over quite a range of ultraslow spreading rates. We reworded the sentence to make clearer that we are talking about the range of different ultraslow spreading rates. Line 347-348

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Secound round of revision

We sincerely thank the two reviewers for their time and effort in evaluating the manuscript. Their valuable feedback allowed us to further improve the manuscript by addressing all their comments. Below, we provide their comments and requested changes along with our responses.

Reviewer A:

This revised version is largely improved and address carefully the comments of the reviewers. Their answers to the review and explanations are well documented and argumented, with many details that enlight their choices. The paper is now much easier to understand and the authors choices, interpretations and conclusions better supported. The figures were improved and the added supplements clearly help to understand the work of the authors.

Here are some minor typos and suggestions, both on the manuscript and the supplementary files.

69 - [...] interplay between tectonic "and" magmatic processes [...]. The "and" seems to be missing between "tectonic" and "magmatic".

We added the "and".

85 - Reference to "Figure 1" should probably precisely point to "Figure 1b", which is the only sub-figure with moment tensors illustrating the extensional tectonic context.

We added "b".

121 - [...] the amplitudes of the "Wood Anderson equivalent" horizontal components in nm [...]. The addition of the Wood Anderson mention may avoid confusing with real displacement measured by the OBS.

We added "Wood Anderson equivalent".

Figure S1 - It seems the "1594 events from dataset S2" are the same as the "1599 of 4503 events" represented in Figure 6a. If this is correct, there is a little discrepancy in the number of events that should be solved or corrected. The description of this subset in the Figure 6a legend is better than in the legend of Figure S1 and should replace the reference to "dataset S2" which is never defined.

We checked again the catalog. It contains 1594 events that meet the quality criteria. 1599 was a typo. We likewise modified the caption and establish a reference to the data set S2.

Figure S5 - The June and April legends should be swapped so that they are ordered by date from top to bottom.

Done.

Data Set S2 - Would it be possible to have this dataset in a format supported by Obspy (quakeML or NLLoc .obs for instance) ? That would certainly favour future valorisation of this very interesting dataset.

We modified the de data availability section, now indicating that other formats will be made available upon request. We chose this more universal data format as it contains also location results and not only phase input for NLLoc.

Recommendation: Revisions Required

Reviewer B:

Review of Magmatic activity at the slowest spreading rates: insights from a high-resolution earthquake catalog obtained from Gakkel Ridge Deep (Arctic Ocean) by D. Essing, A. Hellbrück, and V. Schlindwein.

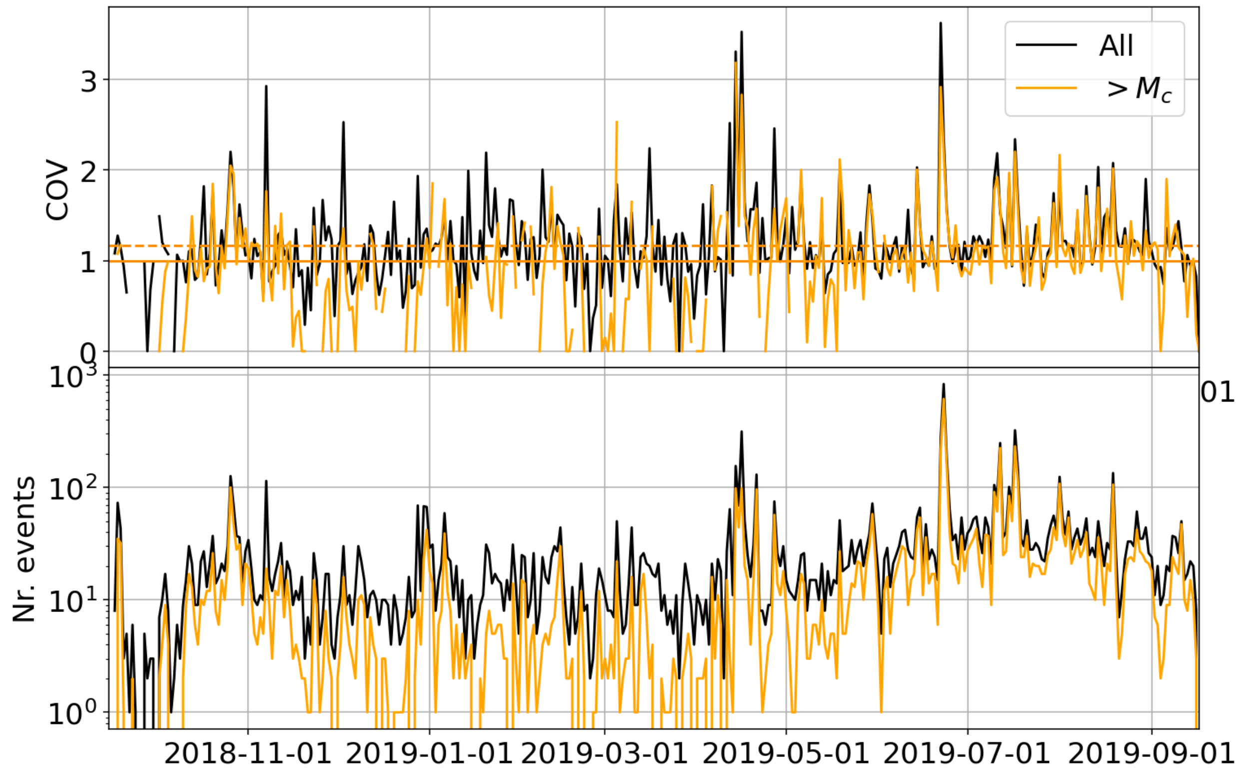
This article deals with the description of the seismicity on the remote Gakkel Ridge from a sparse OBS deployment. As this is already a second round of review and because the authors made noticeable changes on their manuscripts to comply with the recommendations of the reviewers I consider that the manuscript is now well presented and all results are clearly explained. I just listed below few minor remarks that the authors may want to take into consideration in order to improve the clarity of their manuscript.

L163 « decluster » : I am not sure this is the appropriate word. The apparent clustering here is the result of the multiple detections of the same event and is not caused by different events. This is mostly a bias of the detection algorithm and not related to a physical process based on earthquake triggering consideration. Maybe a different word would be more appropriate.

We modified the two involved sentences.

The COV Figurez 2 is estimated daily (it means the number of events in the window is changing every day). It may thus happened that very few events are actually present during a day resulting in low COV value for example (periodic seismicity). Maybe a sentence to explain this or a finding a way to represent the COV that are considered more robust could be helpful. Maybe also it should be reported if for this COV analysis and also for the IE times analysis of panel b if all the events are used or only those with m>m_c.

The small values of the COV are indeed often correlating with small number of daily detections. We now briefly mention this correlation. Regarding the comment on m>mc, we did the here presented analysis always considering m>mc as well as all events (see attached figure). While for m>mc the results of COV are slightly shifted to smaller values, the overall trend remains similar, and the three clusters (April/June) remain significant. As the temporal resolution is higher using all events, in the manuscript we present the analysis with all events.



L283-284 «Seismicity is not concentrated at distinct fault planes which could be related to the relatively low number of aftershocks observed in the region. » I have to admit that I do not understand exactly what the authors want to say with this sentence. Why is the number of aftershocks related to the concentration on distinct fault planes? it is not so obvious for me.

We modified the sentence to increase the accessibility

L333 « we cannot rule out that fluids instead of magma » : I think you could be more precise about what you mean here. I guess the magma is also a fluid. Do you mean hydrothermal fluids?

We modified this sentence for better accessibility.