



EGUsphere, referee comment RC3
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Comment on egusphere-2022-29

Fabian Walter (Referee)

Referee comment on "Cryo-seismicity triggered by ice mass discharge through the Antarctic subglacial hydrographic network" by Stefania Danesi et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-29-RC3>, 2022

This submission by Danesi et al. describes a study of cryoseismological sources associated with the flow of David Glacier, an ice stream draining the Eastern Antarctic Ice Sheet. The glacier has been subject to various studies, including seismological monitoring, which have suggested a relation to basal sliding modulated by tidal height at the ice-ocean interface. The present study may be a valuable contribution as it documents a shift in seismic source locations that may be related to water-enhanced sliding episodes.

In its current form, the manuscript is very difficult to digest and I have found an evaluation of the scientific quality nearly impossible. Key information seems to drown in varying numbers, poor figures and unclear explanations given in different parts of the manuscript. In the present form I cannot recommend this manuscript for publication unless it is extensively revised, explanations are improved and/or additional data analysis (though this may be brief) is given. Please see my comments below for further elaboration.

Fabian Walter.

MAJOR COMMENTS

The main problem I found with this manuscript is that I could not wrap my brain around the data set. The scientific interpretation seems to hinge on the "ice fall" and the "Cauldron area". Neither of these places is labeled in any of the figures (unless I missed this). In addition, almost all clusters in Figure 5 have the same label. Consequently, when

individual clusters or seismic events above or below the ice fall are described, the reader cannot verify or follow the explanations (see specific comments below). The cluster activity and lifetime can also not be verified or put into context. A range of dates specifying the data time window is given: 2003/04, 2005/06, 2015/16 (Line 85); 2003-presence (Lines 93-95); 2005-2017 (Line 96); "period 2003-2004" under study (Line 303), 2002-2016 (Figure 1) and perhaps some more. It is not clear which period ends up being the focus and of relevance to this study. This seems to produce some contradictory statements. For example, on Lines 194-196 the authors state that there existed clusters that were active between 2003 and 2016. On Lines 287ff the authors write that repeating seismicity was only observed between November and December 2003. In general, I was not able to understand which events have which characteristics (locations, waveform similarity, clustering behavior, correlation with forcing data, ...).

If I understand correctly then Zoet et al. (2012 in Nature Geoscience) studied the same glacier and found a forcing by tidal amplitudes. However, the authors of the present manuscript did not attempt to look for this correlation, in fact they purposefully ignore such a correlation against the tidal phase and only investigate the tidal amplitude (Lines 223 ff). Why? It would be interesting to see which events/clusters behave like the Zoet events and which do not. If the authors cannot reproduce the Zoet results, then there may be some problems with the catalogue completeness (even though different seismometers were used for the two studies and the data epochs do not overlap). This deserves discussion.

To be honest, the discussion/conclusion called into question the few take-home messages I thought I had understood when reading the manuscript. First of all, there is a reference to seasonal "behavior" inferred from the seismic data. How is this seasonality backed up in the manuscript? Moreover, the discussion rejects the hypothesis of surface melt affecting basal conditions. Whereas I agree with this, why did the authors come up with this hypothesis in the first place? The temperature data argue against any surface melt. In this regard, it would be interesting to compare the activity of individual clusters that overlapped with the Moon et al. (2021) study to see if there is a correlation with the seasonal speedup.

Source parameters: The authors mention source magnitudes. How were they obtained? This is an important piece of information, because it seems that there is a magnitude overlap with the events by Zoet et al. (2012). Moreover, the size of these events should be discussed and compared to other stick-slip events beneath polar ice streams. It would also be interesting to analyze the waveforms more. Are there different P-phases (refraction through the crust; multiple reflections within the ice column; P-S conversions at the ice-bed interface; ...)? See next comment. Furthermore, the authors seem to have access to a good azimuthal coverage of recording stations. From this, first motion polarities and focal mechanisms could be obtained that provide relevant information for comparison with ice flow.

Locations: Do the locations from the 1-D and 3-D velocity models differ substantially? If so, then this could be related to refracted phases. Moreover, the maps showing event locations have to include error bars. Note that the uncertainties for the relative location is rather low (100 m). How large is the area occupied by location maxima of events within

one cluster? Larger or smaller than the error bars? An analysis of the relative locations could give an estimate of asperity size and perhaps even indicate source migration, which would be extremely informative for basal conditions characterizing the material interface at the rupture area (see, e.g., discussion in Gräff et al., 2021, in GRL).

The text is generally easy to follow, but there are many grammatical errors and phrases, which do not conform to scientific writing standards. It seems that the manuscript was put together in haste, with many orphan paragraphs, false punctuation, single-sentence paragraphs and incorrect terminology. I suggest a thorough re-read and revision from native English speaker.

SPECIFIC COMMENTS

Lines 27ff: Not all the named studies talk about failure mechanisms (e.g., the tremor sources). This has to be reworded.

Lines 35-49: Pointing out the slope break in the respective figures is extremely important here.

Lines 50ff: The hydraulic system is indicated in some figures. How was it inferred? This should be discussed better and perhaps be plotted in other figures, too.

Line 65: "significant correlation with data" is too unspecific.

Line 109: "integrate the lower number of bedrock reflections" is unclear.

Lines 116-117: This information belongs in the figure caption, not the main text.

Line 123: Does the "9.5 km depth" refer to below the surface or below the ice sole?

Lines 125-126: "we merge mean velocity values" is unclear.

Line 131: Specify "many full years".

Line 142: What are "coherent P-picks"?

Lines 143-145: Why did you decide this way?

Line 147: "manual locations": From the explanations above it seemed that the locations were automatic.

Lines 150-151: Indicate this location on the map.

Lines 170ff: You mention what you did "initially", but what did you end up doing? What is the difference between the two correlation scans?

Lines 199ff and elsewhere: The authors tend to announce what they are about to do in upcoming sections. This is a waste of space.

Lines 211ff: The shift in inter-event times needs to be backed up with a figure.

Line 226: "by interpolation" is too unspecific.

Lines 233ff: This paragraph is unconnected from the rest of the text.

Lines 237ff: The correlation between trigger threshold changes and wind speed can be complicated. It can depend on how you parameterize wind speed, i.e. via the noise floor or peak. The latter would isolate gusts. If a noise floor (e.g., lower percentile), median or mean is chosen then you may miss the influence of gusts.

Line 247: "December 1st" of which year?

Line 267: "a resolution higher than the average value": Unclear.

Lines 273ff: Is Smith et al. (2015) the right reference? It talks about Rutford Ice Stream, not David Glacier. "soaring up" has to be quantified.

Lines 281ff: A waveform record supporting the fluid resonance is needed. There could be other explanations for dominant frequencies. I do not see how the last sentence in this paragraph follows.

Lines 297ff: The discussion about sliding physics seems oversimplified. First of all, I suggest not only referring to regelation and plastic flow. This holds for hard beds, but there are other mechanisms in the context of soft beds. Zoet and Iverson (2020 in Science) should be discussed. Finally, is it really valid to consider a simple velocity threshold for seismogenesis? After all, it seems that large changes in basal hydraulic pressures were involved.

Lines 310ff, including the next paragraph: Ideally, some ice flow velocities should be shown here. Is there absolutely no data from 2003? If so, this statement should be made more concrete (there certainly exist flow velocity estimates) and backed up with references.

FIGURES

In general, panels should be labeled a, b, c, ... or equivalently and not be referred to with "upper", "lower", "right", "left", ...

Figure 1: Mark ice fall and Cauldron. Give a date for "now" in caption.

Figure 2: Mark Cauldron. Where was the radar transect taken? Annotate/label the radar transect (e.g., fast flowing ice vs. ice sheet).

Figure 3: Not sure if showing the surface in grey shade has an added value.

Figure 4: Is this from the 1-D or 3-D inversion? Show error bars.

Figure 5: How were the grounding/hydrostatic lines inferred? I would explain them in the text, too. How was the subglacial water flux determined? This is important information, by the way! Show error bars. Where are the cluster events? Are they masked by the number circles? This would be unfortunate, actual locations would be informative.

Figure 6: Fonts are too small. What are the different grey shades in the top right panel?

Figure 7: Fonts are too small. Which clusters do the events shown in the top panel belong to? Vertical green line is missing or hard to see.

Figure 8: How was significance determined/defined?

Figure 9: Fonts are too small.

Figure 10: The horizontal bars should be shown against some along-flow-line quantity like ice thickness or bedrock elevation. A reference for the mass change data should be given in the caption (in view of GRACE).