

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-83

Thomas Rackow (Referee)

Referee comment on "A probabilistic model for fracture events of Petermann ice islands under the influence of atmospheric and oceanic conditions" by Reza Zeinali-Torbati et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-83-RC1>, 2021

Review of „A probabilistic model for fracture events of Petermann ice islands under the influence of atmospheric and oceanic conditions“ by Reza Zeinali-Torbati et al.

The present study tackles an important problem not only for real-world applications and offshore operations, but also for numerical modelling of icebergs. While there is some knowledge about melting and wave erosion of icebergs, the fracturing of icebergs is a process that is not well understood and therefore still usually missing from models, and only a handful of studies have mentioned or even tackled this issue. Zeinali-Torbati et al. present a timely paper with a probabilistic fracture model for ice islands as a function of the underlying oceanic and atmospheric conditions that could be of high interest to marine offshore activities in the Canadian Arctic, and conceptually it is also very interesting for the inclusion in general iceberg forecasting models.

The paper is generally well-written and presented in an understandable manner. The quality of the figures is okay. I think that the authors address a topic that is of considerable interest and there are only very few papers about that topic so far, so I would like to see the study published. Specifically, however, there are two studies that go into a very similar direction and that are not discussed. First of all, this is the 3yr-old study by Bouhier et al. (2018) that was published in the same journal (The Cryosphere), and secondly the high-impact study by England, Wagner and Eisenman (2020) in Science Advances. In my opinion, it is import that these two studies are appropriately discussed and cited, and some statements in the paper should be toned down accordingly. To give some examples:

- 106/107 "To date, there is no deterministic model to describe the large-scale fracture mechanisms as a function of the metocean conditions that govern these events"
- 113/114 "However, these models did not account for the relative role of metocean conditions in the fracture processes."
- 448 "Therefore, it is impossible to compare the methodologies and results of the presented Bayesian fracture model with an existing physical ice island fracture model"

Bouhier et al., in their section 5, analyse different environmental parameters (SST, current speed, relative velocity between iceberg and currents, wave height, wave peak frequency, wave energy). They note that fragmentation is a complex process and due to its stochastic nature, "individual calving events cannot be forecast. Yet, fragmentation can still be studied in terms of a probability distribution of a calving". They conclude that the highest correlations are found for the ocean temperature (and the iceberg velocity) while the wave-related quantities show no significant link with the volume loss. Ultimately, they present a simple (deterministic) bulk model based on some environmental parameters that somewhat mimics the effect of the fragmentation of large icebergs, and that could -to my understanding- serve as a comparision/benchmark for your work.

- 446 "To date, no probabilistic or deterministic models have been presented to investigate the atmospheric and oceanic conditions that lead to the highest probability of large-scale fracture event occurrence for ice islands."

- 115-117: "To date, no previous research has adopted probabilistic methods (e.g. Bayesian approach)"

While this might be true for the "Bayesian approach", England et al. add a stochastic/probabilistic representation of the "footloose mechanism" (cited in your paper) into an iceberg drift and decay model, with clear success (their Figures 3 and 4). They note, however, that the breakup scheme is still relatively idealized and based on assumptions. For example, in their study the probability of a child iceberg breaking from the parent iceberg is set as constant in time, while it should depend on SST, sea ice, the roughness of the sea etc.

The present paper will be much more compelling if the relationship to this previous work is appropriately discussed (in terms of advantages, disadvantages, similarities).

Alternatively, going even further than that, a version of the bulk formula by Bouhier et al. could in principle be used as a comparison, or you could discuss how to better choose the probability of a child iceberg breaking from the parent, which was chosen as a constant in time by England et al.. These latter changes would require work but are however not urgently needed for the present study, in my opinion.

Another slight weakness of the paper, as far as I understand it, is that you are considering only 328 fracture event days. If I understand correctly, you do not allow for a shift around that date. So any extreme conditions (in air temperature for example), even a single day before the calving event, are potentially missed and can only enter your model via the "lifetime" air temperature? Depending on the length of the iceberg's life, which can be months up to years, I am worried that short-lived extremes could thus be rather hidden in this long-term mean for the oldest bergs.

Instead of lifetime air temperature, average temperatures for the previous 7-day (or 14-day?) period might help in that regard. Furthermore, I have the impression that example timeseries for the days around fracture events (for your considered 7 main variables in Table 1) could help the reader to understand your choices better and illustrate the likely major (minor) role of some of them in causing iceberg fracture.

Another suggestion would be to say some words in the Discussion about how you plan to add this model for fracture events to an iceberg drift (forecasting) model? (l. 519, l. 532-534)

Say you determine a probability of 28% in the field, given your environmental conditions (as in l. 358-359). If your berg is still intact the next day under similar environmental conditions, how does this change the probability for fracture? What if you begin to check hourly, does this change the probability? I am probably wondering about the time-step dependence of your model (see also equation 4 in England et al.). If you can give some hints for what you are considering in your iceberg forecasting model that is in development, this would be greatly appreciated.

A last question in that regard is the following: Imagine you have hundreds of icebergs drifting through similar environmental conditions, what is the "expected number of days" an iceberg can drift through a 75% fracture corridor zone? In l.419 you state that one ice island drifted for about 14 days in the medium-high fracture probability zone. Intuitively, this seems rather unlikely, given that every day for two weeks the ice island was apparently more likely to fracture than to stay intact. Since we are dealing with probabilities, however, also unlikely trajectories can happen in reality. So could one maybe compute a theoretical upper limit of days of survival - and was this ice island close to it?

References:

Bouhier, N., Tournadre, J., Rémy, F., and Gourves-Cousin, R.: Melting and fragmentation laws from the evolution of two large Southern Ocean icebergs estimated from satellite data, *The Cryosphere*, 12, 2267–2285, <https://doi.org/10.5194/tc-12-2267-2018>, 2018.

England, M.R., Wagner, T. J. W., and Eisenman, I.: Modeling the breakup of tabular icebergs, *Science Advances*, Vol. 6, no. 51, <https://doi.org/10.1126/sciadv.abd1273>, 2020.

Line-by-line comments:

Abstract: „presented“ -> present tense maybe?

„Bayes theorem“ -> „Bayes' theorem“

l.75 surface area “of” ice islands

l.96 originated from "the" 2012 calving event

l.104 "convection caused by iceberg rolling" Is there a citation for this?

- 129 Barbat et al. (2019) also find a power law distribution for Antarctic near-coastal icebergs (their Fig. 5), <https://doi.org/10.1029/2019JC015205>

- 145 , l. 256 Most often you refer to the "parent-child" relationship, sometimes to "mother-daughter". Maybe you could use the former more consistently

- 149 Since the total lifespan is differently long for different icebergs, have you considered something like 7-day running means instead of "lifetime" ("the week before potential fracture")? (where "7 "can be replaced by any number that sounds reasonable to you)

- 175 Did you ever consider something very simple like "latitude"?

- 190 You mean you normalized by the number of days the ice island drifted? (because dividing by the timespan in seconds would result in a weird unit)

- 189-191 I think this is described in a very complicated manner. Don't you just take the mean of the daily values?

- 198 This could be a good line to mention that sea ice will play a role in other conditions or regions on Earth (e.g. England et al. 2020), so that your model would need to be extended for other applications

Figure 1: How do you determine the direction of the causality? Why does air temperature "cause" water temperature and not vice versa (they are tightly coupled)

- 207/208 Is this due to the (relative to CMEMS) lower spatial resolution of ERA-Interim? Are the ice islands coinciding with land boxes then?

- 218/219 Again, how do you decide on causality between, e.g., air and water

temperature? Also, it would be great to add the r values to the Figure.

- 220 "high metocean conditions" -> maybe "extreme metocean conditions"

l.223/224 No brackets around the reference

Table 1: The notation is not clear to me. Do you subtract the median value, or is $V_{(w-x)}$ just the median of all V_w values? Could you give numbers here as well?

- 280 "This indicates the important contribution of warm waters to faster deterioration of glacial ice features..." See also papers mentioned above, where SST is considered

Figure 3 and even more so, Figure 4, shows strong signs of bimodality. Is that why you split into two states in Table 1? This is unclear.

Furthermore, an immediate question is whether the two modes (in Figure 4) are potentially due to different seasons, or whether the fracture events for the two modes are maybe spatially clustered in specific areas? This could also potentially hint at different mechanisms involved, which is very difficult to assess from the histograms alone.

- 314 I was wondering whether instead of the mean (wave energy index), the maximum during a day could be more telling (I do not know whether that is available in the reanalysis). Same for winds etc

Figure 3 and 4: Could you add the median line for all observations in the right panels so that one can see the displacement for the median of the fracture events directly? In general, are the different medians significantly different from each other in a statistical sense (see e.g. l. 322 "slightly greater")?

Figure 5: Please add more ticks; add median line for all observations in the right panels

Figure 5 caption: "for a) all observations (n=3985) and for b) n=131 fracture events"

- 335: This is a much clearer definition for the lifetime mean variables that could be given in the beginning of the paper

- 346 No bracket in the end

