Aerial observations of sea ice break-up by ship waves
by Elie Dumas-Lefebvre & Dany Dumont

General comment

This is an enjoyable little paper which reports some unique results on a topic of meaningful contemporary interest that has been reinvigorated by the effects of global climate change on the polar and subpolar seas. The work is reported clearly and is mostly well-written. Although the observations and subsequent analysis are limited in extent, they are interesting and thought-provoking, as they intersect earlier theoretical conjectures about the way sea ice fractures under the action of ocean waves – namely in regard crack separation and the FSD configuration that is created as a consequence. Nonetheless, sometimes assertions are made in the paper that are not adequately justified by evidence. The authors should be careful of this and ensure that what they are saying is correct and that earlier papers are cited where applicable as conclusions have been reached in some cases without the benefit of results reported in a previously published paper that remains uncited. This notwithstanding, and appreciating that keeping track of every publication in a research field is impossible, in the reviewer's opinion a revision of the work to accommodate the specific comments and technical corrections below will improve the paper sufficiently for it to be published in Cryosphere. I am aware that my review will seem pernickety and possibly irksome at first sight but my intention has been to finesse the paper because I believe its results should ultimately be disseminated through publication. In sum, I feel that the authors are being too assertive in regard to the outcomes of their study. The methods they have used are novel, exciting and have tremendous potential. I congratulate them for collecting and compiling a manuscript to publish these observations. I am inclined to disagree with some of their unequivocal affirmations which, to me, are not always based on robust evidence and perhaps to some extent show a lack of deep understanding of this multifaceted research topic. I recommend that the manuscript should be published but only after the authors have acted on the comments below.

Comment

Thank you for considering our manuscript. At first, it has been a challenge to open our minds to the perspective you brought up in your review but, in the end, there is a clear benefit to include the ideas and suggestions presented in your comments to our paper. Key aspects of the review made us realize that the experiment we carried out are not representative of how wave-induced sea ice breakup universally behaves in the natural environment. Furthermore, even though x^* has been used in numerous studies, using this quantity derived from a quasi-static approach in order to understand the underlying physics of a inherently dynamic problem might be wrong. Quantities derived from scattering models might be better tools for that sake since they consider the dynamics of breakup. In the end, your review has brought our article at an higher level but has also helped us to broaden our theoretical comprehension of wave-induced sea ice breakup. For the answers to your specific comments and technical corrections, please see below.

Answer

Specific comments

Line 3–4. What does 'When represented as probability density functions weighted by the surface of ice floes' mean? That is, how can something be weighted by a surface?

Comment

Floe size distributions have traditionally been represented by frequency distributions, i.e. as if all floes are non-dimensional objects that have the same probability, irrespective of the surface they occupy in the space they live in. However, sea ice floes are arranged on a surface without overlapping such that the total area covered by floes can't be larger than the total area that is considered. The area-weighted distribution wishes to characterize *the probability of finding a floe of size d at a randomly chosen position in the image*. This definition is similar to the so-called ice thickness distribution (ITD) that is used in sea ice models. That being said, we acknowledge that labelling this approach with the words "area-based" or "area-weighted" can be confusing. Instead, similarly to the vocabulary used in the ice modeling community for the ITD, we propose using *partial concentrations*, which refers to the portion of the total surface that is covered by floes of a given size, rather than "area-weighted" or "area-based". Eq. (3) of the manuscript shows the mathematical definition of this FSD.

Answer

Line 9 the 'mass loading dispersion relation' won't be known to many readers. I am assuming a citation is not allowed in the abstract, so a definition or explanation should be provided here.

Comment

This will be changed in the abstract.

Answer

Line 9–10. 'Moreover, our experiments show that thicker ice can attenuate wave less than thinner ice'. This is counterintuitive so, if the authors still believe this to be the case after reading my comments, I would suggest adding a short statement explaining why, e.g. Moreover, our experiments show that thicker ice can attenuate waves less than thinner ice, because . . . '. Notice also that 'wave' has been altered to 'waves'.

Comment

Yes we will better justify that statement, if it is still a valid conclusion considering the changes to be expected with lower values of Y^*.

Answer

Line 18. What do the authors mean by 'larger' waves? Waves of greater amplitude or waves of longer wavelength, or both?

Comment

Good point, the text of line 18 has been changed to:
"... that generate a more energetic wave field in the Arctic basin ... "

Answer

Line 20. Why do waves ‘change the mechanical properties of the ice’? Are the authors hinting at fatigue or are they using the word ‘ice’ loosely, meaning the ice-cover as a whole?

Comment

What we want to highlight in line 20 is the fact that the broken up sea ice does not have the same rheology, and hence large scale mechanical properties, than a consolidated ice pack. This phrase says that wave-induced breakup changes mechanical properties of sea ice by altering the scale of the floes, not that wave directly impact the mechanical properties of the floes. From your comment, we understand that the term ”mechanical” is not useful here and the idea is adequately transmitted through the word ”dynamics”. We will thus remove the former.
The text will be modified to :
”The increasingly energetic waves may then have greater potential to break up sea ice thus generating a larger MIZ so that, on the large scale, changes in the dynamics of the ice cover and in ocean-atmosphere heat exchanges could be observed.”

Answer

Line 34. Don’t the usual definitions of anelastic and inelastic designate that anelasticity is a particular type of inelasticity; if so why do the authors list them separately suggesting that they represent independent material behaviors.

Comment

We distinguished the two because they are treated separately by certain authors, as reviewed for example by Boutin et al. (2018). Since we do not intend here to provide any explanation or validate any theory here, we are inclined to keep that discussion focused specifically on what could generate the counterintuitive results we obtain, namely that thicker ice attenuate waves less (see comment above).

Answer

Line 36. I find it hard to believe that ‘there are no observations that directly relate the FSD to a given process in the natural environment’, but accept what the authors intended in the sentences that follow in the text.

Comment

It is true that it may be a bold statement since some studies may not have been included in our literature review since we cannot truly know every studies that have been published. We will change this sentence to :
‘To our knowledge, there are no observational studies that directly relate the FSD to the processes that generated it in the natural environment.’

Answer

Line 67. ‘This means that wave-induced break-up leads to a bell-shaped FSD, a result that indicates that the morphology of floes resulting from breakup might not be well represented by a power law.’ Didn’t Montiel, F. and Squire, V. A. Modelling wave-induced sea ice break-up in the marginal ice zone. Proceedings of the Royal Society of London, Series A 473(2206): 1–25 (2017) deduce a similar result? This paper should be cited incidentally, as it concerns breakup by waves.

Comment

Right. The following sentence will be added : "Montiel & Squire (2017) also found out that either a modal or bimodal distribution was generated from wave-induced sea ice breakup, thus suggesting that the observed power law does not come out from this process alone."

Answer

Line 70. I personally think the paper Fox, C. and Squire, V.A. On the oblique reflexion and transmission of ocean waves from shore fast sea ice. Philosophical Transactions of the Royal Society A 347(1682): 185–218 (1994) is a better one to cite than Fox and Squire (1991), as it is more general and includes everything reported in the 1991 paper, but I accept that the authors are quoting a specific statement.

Comment

Thank you for your understanding and for the suggestion.

Answer

Line 87. The author state 'quantitative analysis is required to fully test these hypotheses' but isn't it obvious that, all things being equal and ignoring any small amplification arising due to the free edge boundary condition, long-crested propagating waves stress all parts of an ice plate similarly, so the specific wavelengths involved will be less important unless standing waves are created.

Comment

Indeed, the use of the words "quantitative analysis" alone is not adequate since both observational and modeling studies can be quantitative. The words "quantitative analysis of observational data" will thus be used for clarity.

Answer

Lines 100–108 are the nub of the paper yet I find the paragraph poorly expressed and somewhat blah. As the authors rightly say, it is incredibly difficult to get data on naturally-occurring wave-induced breakup of ice floes because it demands the stars to be aligned even when instrumentation is available. What this particular study does is to create the waves and then measure the outcome. The only thing missing is a measurement of the induced curvature or stress. Wouldn't that be nice! At minimum break the paragraph at line 104, i.e. start a new paragraph at 'In this study, we use . . . ' so the reader can immediately see the goals and any limitations of the paper.

Comment

This paragraph has been changed to the following :
"Few observational studies about natural break-up have been made yet mainly because the MIZ is an arduous area to sample directly from. It is indeed hard to be in the MIZ at the right place and at the right time, with good but not too harsh weather conditions for break-up to happen, and with the right apparatus to measure all relevant variables during a natural break-up event.
Rather than waiting for the stars to be aligned in the natural environment, we chose to create waves with a ship in order to simulate breakup events. With the help of an unmanned aerial vehicle (UAV or drone) and image processing, the breakup experiments conducted in the Gulf of Saint-Lawrence (GSL) and in the northern Baffin Bay (NBB) allowed us to measure the outcome of small period waves breaking naturally grown sea ice. While no apparatus to measure the strain of the ice or incident wave properties were successfully deployed, it was nonetheless possible to infer some key wave and ice properties and to fully characterize the FSD, the breakup speed and its extent. When compared to thin elastic plate theory, these results give insight on the underlying physics of wave-induced sea ice breakup."

Answer

Line 110–111. What does the word localized mean in 'First, a large level ice floe having a side exposed to open water is localized'?

Comment

Good point. The use of localized here is derived from french and a better word for this sentence would be identified. The sentence will now read : "First, a large level ice floe having a side exposed to open water is identified"

Answer

Line 116. In'... hands a better management of weather conditions ...', I do not know what 'hands' means in this context. Is this a technical word? Or is it bad English and means 'provides'.

Comment

It might be bad english. Thank you for providing us an alternative word for this sentence.

Answer

On Line 127 the authors state that the sea ice in the Gulf of St. Lawrence was grey and grey-white between 10 and 30 cm-thick'. This is an important observation that the reviewer will refer to later. No sensor was deployed on the ice itself.

Comment

It is for sure not as precise as a in situ ice thickness measurement but it is still a measure that has been made by an officer of the Canadian Ice Service. It would have been amazing to have data on the ITD and the wave spectrum as well as the FSD but since the experiment was opportunistically planned on the ship, we could not have such data.

Answer

Likewise lines 140–142 indicate that sea ice in Northern Baffin Bay during the experiment there was heavily rotted first year ice between about 40 and 60 cm. Two SKIb wave buoys deployed on the ice were capsized, unfortunately, so provided no data.

Comment

Yes.

Answer

Line 205. I note for future reference that the waves are relatively short period, as expected for a Kelvin wake, namely around 4 s.

Comment

Good.

Answer

Figures 5–8 are awesome!

Comment

Thanks ! If you want to see the video footage of the GSL breakup, it is accessible here : `https://www.researchgate.net/publication/356568634_Aerial_footage_of_wave-induced_sea_ice_breakup_in_the_Gulf_of_Saint-Lawrence/stats`

Answer

Line 255–258. I agree with the opinions expressed in this paragraph but the authors must also remind the reader here that the waves being discussed here are of modest period, i.e. around 4 s. Indeed, in regard to natural ocean waves they would be categorized as 'chop'. Furthermore, the sea ice in the experiment is somewhat distinctive, i.e. a subcategory of what is encountered in the polar oceans. It would be disingenuous to suggest that the data being presented are universally applicable to all wave-ice interaction scenarios. This is not intended to lessen the importance of the results being presented, which I regard highly, but rather to ensure that the facts are presented accurately.

Comment

With your comment and the one of the other reviewer, we have added information about ship wave at line 103 with the following sentence :
"Indeed it is possible to study wave-ice interactions in the laboratory, as Herman et al. (2018) did, but it is not clear if the results directly apply to the natural environment owing mostly to the complex life history of naturally-grown sea ice compared to the more homogeneous growth conditions of the laboratory. Even though ship-generated waves are different from wind-generated waves, their period and amplitude are nonetheless representative of natural waves generated in short fetch seas that impacting ice conditions."
It is true that our result do not apply universally to every type of ice and to every type of waves, thank you for underlining that detail.we will consider that when rewriting our discussion.

Answer

Eq. (4) assumes the ice floe is an Euler-Bernoulli beam, i.e. the sea ice is thin and homogeneous through its thickness. This should be stated, as sea ice in nature has a temperature and brine volume gradient that renders the latter assumption an approximation.

Comment

This will be added in the text.

Answer

While the statement in lines 278-283 is correct, I do wonder whether the small local peak that occurs near the ice edge is sufficient to explain why the ice progressively fractures from its margin to its interior, given the degree of approximation inherent in the hydroelastic model.

Comment

See our response to the comment below.

Answer

Line 283. I am not sure what ‘Unfortunately, there is no simple analytical solution we can use to scale our result’ is saying. There are a wealth of hydroelastic studies of wave-ice interaction dating back to papers published done in the 1950s and the first Weiner-Hopf analysis of Evans and Davies in 1968 which was generalized in Williams, T.D. and Squire, V.A. Scattering of flexural–gravity waves at the boundaries between three floating sheets with applications. Journal of Fluid Mechanics 569: 113–140 (2006). It is also worth noting that by ignoring the mass of the plate, a fully algebraic result was obtained in Tkacheva, L.A. Scattering of surface waves by the edge of a floating elastic plate. Journal of Applied Mechanics and Technical Physics 42(4): 638–646 (2001), which could presumably be used. It is disappointing to me that a more authoritative model has not been used when so much theory is available; the fragility of the wave-ice interaction topic generally is not situated in mathematical theory but in the paucity of data to validate theory. On the other hand, I am not suggesting that the authors can ameliorate this problem at this stage by reworking their analysis, I am simply conjecturing that a different approach could have changed the manuscript from where I began this review, i.e. ‘This is an enjoyable little paper’, to a something more substantive.

Comment

This work is the main outcome of a Master’s research project and you are right that a thorough literature review of hydroelastic studies was not carried out, one of the many aspects of the observational work forming the central part of the manuscript. Nonetheless, we are very interested to deepen the discussion of our results towards. We thus thank you for suggesting the article of L.A. Tkacheva. We will definitely consider this paper and references therein in our discussion. From comments received from two reviewers, we decided to emphasize the significance of the observation of a progressive break-up from the ice edge, which is an original contribution of this paper, without claiming too strongly that we know what is going on.

Answer

Line 284. I will point out the obvious. The Hétenyi (1946) model has no fluid dynamics, which I would perceive as fundamental to interpreting an ocean wave phenomenon.

Comment

We agree that the use of a value derived from the theory of Hétenyi (1946) might not be adequate in order to obtain information about the contribution of sea ice properties in determining floe size resulting from breakup. Nonetheless, it has been used in many studies since 2011 (e.g. Toyota et al; 2011, Williams et al.; 2013a-b, Hermann et al., 2017; Boutin et al.; 2018) so that we deemed necessary to compare it to our data and to show how x^* was derived in order to clarify this topic in the scientific community. In the revised version of the manuscript, we will specify that and use Hetenyi's formulation as a material-dependent length scale that represents in essence our strain develops in a semi-infinite plate. We will also better link with studies that explicitly consider waves.

Answer

In Eq. (5), the authors need to say that it is an Euler-Bernoulli beam that they are using as a model. Strictly, they would actually be better to use a Kirchhoff–Love plate, i.e. the plate equivalent of an Euler-Bernoulli beam, which would introduce a Poisson ratio effect. However, both suffer from the obvious approximation that the sea ice is assumed to be homogeneous through its thickness. (I note that most theory assumes a similar paradigm on the basis that the waves are long compared to the thickness, so I am not criticizing the authors for assuming homogeneity, I am simply advising them to tell the reader what they are assuming.)

Comment

The paragraph preceding eq. (5) as been modified to :
"[**?**] considers a semi-infinite Euler-Bernoulli beam having constant thickness h which extends along the x axis. When submitted to a load P acting downwards at its edge, a vertical deflection of the beam's edge is generated and imposes a bending moment M defined as ..."

Answer

Line 288–291. This is confusing. In consecutive sentences the authors refer to bending moment, moment of inertia and moment. The third occurrence is the bending moment. Why not just say M vanishes for large x. And to be pedantic, isn't I the second moment of area rather than the moment of inertia, i.e. no mass?

Comment

The text between eq. (5) and eq. (6) has been changed to :
"where E and I are respectively the elastic modulus and the second moment of area of the beam, i.e. its massless inertia (Hetényi, 1946). Considering a stress-free condition at the edge and a that M vanishes for large x, the general solution is"

Answer

Line 294. 'First-order derivative of Eq. (6)' with respect to x.

Comment

Corrected.

Answer

Ah. Eq. (8) suddenly introduces ν, which we are told later is Poisson's ration. This means that the authors are actually considering a Kirchhoff-Love plate after all, so my comment 23 is superseded. However, I think I could argue that this section needs a good tidy up to be consistent.

Comment

This section has been replaced by the following in order to clarify the mathematical approach as well as physical considerations and approximations :
"Considering a stress-free condition at the edge and a that M vanishes for large values of x, the general solution is

$$M = -\frac{P}{\mu} e^{-\mu x} \sin \mu x, \qquad \mu = \left(\frac{k_f}{4EI}\right)^{\frac{1}{4}}, \tag{1}$$

where k_f is the foundation modulus, which can be viewed as a Hooke's constant, and x is the axial direction of the beam (Hetényi, 1946). Setting the first-order derivative of Eq. (1) with respect to x to zero, we obtain the following algebraic equation

$$e^{-\mu x}(\cos \mu x - \sin \mu x) = 0, \tag{2}$$

which is satisfied when $x \to \infty$ or when $x = (4n+1)\pi/4\mu$ with $n = 0, 1, 2, ...$ This implies that the location of the maximum bending moment, and therefore of maximal deformation, is

$$x^* = \frac{\pi}{4}\left(\frac{4EI}{k_f}\right)^{1/4}. \tag{3}$$

Even though x^* is derived for an Euler-Bernoulli beam, we insert the second moment of area (I) of a Kirchoff-Love plate and use the Young's modulus Y as the elastic modulus E of the plate in order to use this framework in the context of the sea ice in our experiments. That way, we have

$$E = Y, \quad I = \frac{h^3}{12(1-\nu^2)} \tag{4}$$

which implies that

$$x^* = \frac{\pi}{4}\left(\frac{Y^* h^3}{3\rho_w g(1-\nu^2)}\right)^{1/4}. \tag{5}$$

with Y being the Young's modulus for sea ice, h the plate thickness, $\nu = 0.3$ the Poisson ratio, $\rho_w \simeq 1025$ kg m^{-3} the sea water density and g the gravitational acceleration. Another consideration made here is that, in order to take into account for the fact that the plate lies on water, the foundation modulus was set to $\rho_w g$."

Answer

Line 304. The authors write that 'Mellor (1983) used this framework for determining a flexure-induced fracture distance in the context of ice rafting, not for the case of wave-induced breakup.' This is important as Mellor is using his analysis for a quasi-static problem, while the authors are using it for a dynamic problem. This is why I dislike what they have done.

Comment

We understand your comment but as we said in an earlier comment, it is because x^* has been used by the scientific community in various situations that we felt the need to include it in our analysis. I although think that it should be important to bring up the limitation of this framework in the discussion.
The following is added at line 310 :
"Moreover, since the framework used to obtain x^* is quasi-static and wave-induced sea ice breakup is inherently dynamic, is x^* the right tool to gain insight on the underlying physics of this process ?"

Answer

Line 304–310. Rather a dubious argument for all the reasons I have just articulated, namely that the theory being used is inappropriate for the dynamical data set being modeled.

Comment

Following the answer above, it is true that using the bending moment in a quasi-static manner to approach a fundamentally dynamic problem might not be the best approach. Nonetheless, since x^* has been used wrongfully in some studies, we wanted to elaborate the origins of this quantity in order to rectify the assumptions made when using it. Your comment bring this rectification a step further in the sense that, even after rectifying the theory, it remains a quasi-static framework that could be inappropriate to describe wave-induced sea ice breakup. We will for sure make a point on that in our discussion. We thank you for pointing out that detail. It breaks up the paradigm a number of authors were trapped into and opens up the way for further discussion.

Answer

'The latter causes the ice to break at strains lower than its initial flexural rigidity.' How can this be? Strain is dimensionless. Flexural rigidity has units of Pa m3. This makes the sentence nonsense.

Comment

Strains will be replaced with stresses in the sentence.

Answer

I am rather worried by the arguments used starting at line 316 and 338. I simply don't believe the value of the effective modulus is nearly as high as the authors have estimated. Assuming the arithmetic is correct—and I haven't checked, the argument about the unmeasured salinity is flawed to my mind. The sea ice in question is relatively thin and warm, and the NBB floe is 'heavily rotten' according to the authors, so I would expect a much lower effective modulus. I believe the cited argument about the salinity being less later in the season relates to desalination mechanisms that are not present in warm, highly saline, sea ice of modest thickness (especially acknowledging the uncertainty around NBB thickness, apropos lines 344–349). as sea water at 30–35 ppt flushes a good proportion of the lower parts of the ice matrix. Sea ice is a mushy layer as reported by Feltham, D. L., Untersteiner, N., Wettlaufer, J. S., and Worster, M. G. Sea ice is a mushy layer, Geophysical Research Letters 33: L14501 (2006). My advice is to think through this section very carefully, as the value of effective modulus calculated is way too high in my opinion and, unfortunately, this has a bearing on the subsequent analyses and conclusions. Incidentally, while Cox and Weeks (1983) is undoubtedly the most comprehensive publication on brine volume, an easier analysis was completed earlier by Frankenstein, G. and Garner, R. Equations for determining the brine volume of sea ice from $-0.5°$ to $-22.9°$ C. Journal of Glaciology, 6(48): 943-944 (1967).

Comment

When analyzing the results, we were also worried that we might come to wrong conclusions with respect to the dispersion relation. The fact that we only have an uncertain estimation of ice thickness and that we did not measure sea ice salinity, temperature or porosity leads to large uncertainties for the elastic modulus. This is what we wanted to show with shaded areas in the figures 10 and 12. However, thanks to a thorough sensitivity analysis that includes much smaller values of Y, we found that our conclusions are robust to these uncertainties. We recall that we measured a decrease of the wavelength. However, flexural waves, irrespective of the value of Young's modulus are always faster and thus longer than open water waves or waves-in-ice following the mass loading. The break-up speed c_b is what we measured and according to Figure 12, it better matches the group speed if the mass loading applies. We admit that flexural waves might exist, but they would travel in the ice unnoticed from the footage. We will alter the discussion to reflect this possibility, as well as exploring the effect of lower values of Y^* on the results.

Answer

Line 371–379. The focus of my concern signalled in the previous item-30 targets the issue of whether the sea ice is behaving as a mass loading medium or as a flexible plate, as the latter depends strongly on the value of the flexural rigidity $Y^*h^3/12(1-\nu^2)$, primarily expressed via thickness h but also the effective modulus Y^*. If h or Y^* are over estimated then the dispersion relation will be incorrect. The somewhat uninspiring publication Squire, V.A. A comparison of the mass-loading and elastic plate models of an ice field. Cold Regions Science and Technology 21:219–229 (1993) points out why this is so, namely that the mass-loading dispersion relation is just the flexural plate one with zero flexural rigidity. The authors should take a look at Fig. 7 of that paper, which shows how wavelength is affected by a change of flexural rigidity at a fixed thickness, i.e. a change of Y^*. Getting Y^* or h right is crucial. My question is how does it affect the very strong argument for the mass-loading model made in lines 376–379, where the authors state categorically 'It shows quite clearly that at this frequency, waves that are responsible for the break-up and that are visible from the UAV propagate following the mass loading dispersion relation.

Comment

We understand your criticism regarding the conclusions made relative to the dispersion relation constraining wave propagation into the ice, such that waves obeying to mass loading are in fact flexural waves propagating in a low flexural rigidity medium. We will discuss this with more care about underlying concepts. please refer to the previous comment.

Answer

Line 381–382. I note here and on line 141 that the NBB floe was 540 m wide but I cannot find a statement about the GSL ice cover. Admittedly, it is rather late to be asking this question but is there any possibility that the far side of the NBB ice floe is affecting the breakup by creating a standing oscillation under the floe? (A similar question can be asked for the GSL experiment but I don't have the details and the question may be irrelevant.

Comment

We think that since breakup happens fast, there is no time for the development of a standing wave in the ice floe. From the video footage, it is clear that the incident wave crests propagating in the ice are responsible for the breakup.

Answer

Line 390. The authors write 'Based on the previous discussion, we use the mass loading dispersion relation when the wave propagates in unfractured ice.' So, given my comment 31 above in regard to Fig. 7 of Squire (1993) can the authors be sure that it is actually the mass-loading model or could it be a flexible thin plate with a lower value of Y^* or h? For example, page 225 of Squire (1993) states 'Fig. 7 shows that the choice of Y^* is critical in determining the relative magnitudes of the wave numbers in ice and water which, since the elastic plate analogy is a parameterization, suggests caution.' Because the mass-loading model has been dismissed so many times in the wave-ice interaction literature, even for propagation in frazil ice, I am afraid I favor a reduced flexural rigidity hypothesis articulated via Y^* or h.

Comment

Please refer to our previous response.

Answer

Line 391. 'It is unclear whether the same relation applies to fragmented ice or if it rather follows the deep water relation'. It is obvious that fractured ice will not have the same dispersion relation as a continuous ice plate if the dispersion relation is based upon a Kirchhoff-Love plate (Euler-Bernoulli beam), but now one enters the murky realm of parameterization. Essentially one could envision a reduction in Y^* when the ice breaks up. Or one could imagine a slightly more extreme version where after breakup $Y^* \to 0$, i.e. the medium becomes a mass-loading medium. If the ice floe starts out behaving as a mass-loading medium then physically I guess one could argue that the masses somehow change ... but they don't as far as I know!

Comment

What we meant is that we weren't able to assess it in our observations. We will make this clearer. We agree with the reviewer how different models of ice behaves differently. What we try to assess here is in essence what models would apply best to the ice we are observing.
We suggest changing the sentence to something like: "We were not able to measure waves in fragmented ice and thus it was not possible to characterize the dispersion relation in this part of the ice cover."

Answer

Line 395. I recommend adding 'open-water' as it is slightly unclear as it stands; so 'The largest open-water wave was slightly less than one meter high.'

Comment

Corrected.

Answer

Line 414. I am bothered by 'Even though the ice is thicker in the NBB experiment, the attenuation is almost one order of magnitude weaker than the attenuation in the GSL.' This actually suggests to me that Eq. (17) is producing the wrong ab, i.e. either Y^* or h is incorrect (which would also produce an inaccurate k2).

Comment

This is our intention to take into account uncertainties on all parameters before concluding anything. Based on your recommendations we will consider a greater range for Y^* in our calculation of the figures 12 and 13 in order to bring a bit of nuance to the idea proposed. Thank you for underlining the sensitivity of our calculations on Y^* and h. Anyhow, we are pretty confident that our measurement of breakup speed and in-ice wave phase speed are valuable so that our results stress how observations are critical to better constrain break-up models.

Answer

Line 420. There is no doubt that inelastic effects could cause a difference in attenuation between the two experiments because, as I understand it, the physical properties of the sea ice and hence its material properties were different between the two experiments. (There is no need to say anelastic, as noted elsewhere, as inelasticity covers a multitude of sins.) However, given what I have said above and the lack of amplitude measurement in the ice cover, perhaps any statements about inelastic constitutive laws are best avoided.

Comment

Thank you for pointing out that detail, we will rewrite the text accordingly. For sure it would be amazing to have buoys measuring the curvature of the ice in future experiments.

Answer

Technical corrections

The authors declare that they provide the first in situ observations of floe size distributions (FSD) resulting from wave-induced sea ice breakup. Being a pedant, I would aver that the statement is misleading as there are numerous observations of the FSD arising as a result of wave-induced sea ice breakup but that the observations have not segued into any interpretation which improves our understanding of the process. What the authors actually mean is that they are the first to have computed the FSD from observational data of wave-induced breakup. While I actually also find this statement hard to believe, I shall take the authors at their word and assume that they have perused Russian, Japanese and Chinese, as well as North American and European sea ice corpora. However, I also caution that if they haven't scrutinized the international scientific literature carefully, they should avoid making such a strong statement to avoid a rebuttal.

Comment

The abstract has been changed to :
"We provide high resolution in situ observations of wave-induced sea ice breakup in the natural environment. In order to obtain such data, an unmanned aerial vehicle was deployed from the Canadian Coast Guard Ship *Amundsen* as it sailed in the vicinity of large ice floes in Baffin Bay and in the St. Lawrence Estuary, Canada. The footage obtained from these experiment allows for the analysis of both the resulting floe size distribution (FSD) and the temporal evolution of the breakup. When expressed as probability density functions weighted by the floe area, FSDs exhibit a modal shape thus indicating that there is a preferential size associated to wave-induced break-up. Both FSDs are compared to a flexural rigidity length scale, which depends on ice properties, and with the wavelength scale. This comparison tends to show that the maximal distance between cracks is preferentially dictated by sea ice thickness and rigidity rather than by the wavelength. Temporal analysis of one fracture event shows that the break-up advances almost as fast as the wave energy and that waves responsible for the break-up propagate following the mass loading dispersion relation. Moreover, our experiments show that thicker ice can attenuate wave less than thinner ice. This novel dataset and thus provides key information on the wave-induced ice break-up, that has the potential to strengthen theoretical aspects of wave-ice interactions and their implementation in models."

Answer

There are various instances of poor English grammar, e.g. singular words that should be plural, poor syntax where the subject is missing, weak or confusing sentence structure, a missing (adjective or adverb) article 'the', etc. In lines 116–118, for instance, '... while still allowing to study break-up in the natural environment. Such a setup also allows to have no constraint on the location of deployments and to search for the right sea ice to break.' Or 'The error on its vertical and horizontal position are respectively of 0.5 and 1.5 m.' Blunders are scattered throughout the text and need to be copy-edited out by somebody as there are quite a few indiscretions, assuming that The Cryosphere expects sound English prose.

Comment

We will take a closer look at the grammar when editing the manuscript to be submitted in its final version.

Answer

The authors use the word 'further' consistently. In many cases, although not all, they actually mean 'farther'. Such occurrences should be corrected.

Comment

See comment above.

Answer

There is a mixture of US and English spelling, e.g. 'traveled' yet 'modelled'.

Comment

See comment above.

Answer

Line 65. 'They rather let it evolve ...' should be 'Rather they let it evolve ...'

Comment

Corrected.

Answer

Figure 4 caption should read Matlab not Maltab.

Comment

Corrected.

Answer

Line 154. 'consists in a series of steps' should be 'consists of a series of steps' or 'proceeds in a series of steps'.

Comment

Corrected.

Answer

Line 161–162. 'We refer the reader to (Zhang and Skjetne, 2018)' should be cited as 'Zhang and Skjetne (2018)'.

Comment

Corrected.

Answer

Line 220. ‘let’s compute’ is a little informal.

Comment

Line 220 has been changed to :
”To characterize the FSD, the floe size distribution (NFSD) is computed using the frequency of observation as it is done in most studies ...”

Answer

Line 278. The brackets in the citation are of the wrong type, i.e. it should be Fox and Squire (1991).

Comment

Corrected.

Answer

Line 310. ‘... lies on the same mathematical premises?’ is poor English. Replace with ‘... based upon the same mathematical premises?’

Comment

Corrected.

Answer