Aerial observations of sea ice break-up by ship waves

by Elie Dumas-Lefebvre and Dany Dumont

General comments

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.

Specific comments

1. 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?
2. 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.
3. 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'.

4. Line 18. What do the authors mean by 'larger' waves? Waves of greater amplitude or waves of longer wavelength, or both?
5. 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?
6. 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.
7. 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.
8. 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.
9. 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.
10. 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.
11. 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.
12. 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'?
13. 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'.
14. 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.
15. 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.

16. Line 205. I note for future reference that the waves are relatively short period, as expected for a Kelvin wake, namely around 4 s.
17. Figures 5–8 are awesome!
18. 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 than the facts are presented accurately.
19. 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.
20. 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.
21. 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.
22. 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.
23. 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.)

24. 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?
25. Line 294. 'First-order derivative of Eq. (6)' with respect to *x.*
26. 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.
27. 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.
28. 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.
29. Line 313. '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 m^3. This makes the sentence nonsense.
30. 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)**.
31. 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.'

32. 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.
33. 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.
34. 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!
35. 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.'
36. 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 a_b, i.e. either Y^* or h is incorrect (which would also produce an inaccurate k^2).
37. 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.
38. Conclusions. See the general comments section.

Technical corrections

1. 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.
2. 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.
3. The authors use the word 'further' consistently. In many cases, although not all, they actually mean 'farther'. Such occurrences should be corrected.
4. There is a mixture of US and English spelling, e.g. 'traveled' yet 'modelled'.
5. Line 65. 'They rather let it evolve …' should be 'Rather they let it evolve …'
6. Figure 4 caption should read Matlab not Maltab.
7. Line 154. 'consists in a series of steps' should be 'consists of a series of steps' or 'proceeds in a series of steps'.
8. Line 161–162. 'We refer the reader to (Zhang and Skjetne, 2018)' should be cited as 'Zhang and Skjetne (2018)'.
9. Line 220. 'let's compute' is a little informal.
10. Line 278. The brackets in the citation are of the wrong type, i.e. it should be Fox and Squire (1991).
11. Line 310. '… lies on the same mathematical premises?' is poor English. Replace with '… based upon the same mathematical premises?'