Review of "Aerial observations of sea ice break-up by ship waves" by Dumas-Lefebvre & Dumont (2021)

General appreciation

Comment

This is a challenging and valuable paper to attempt to clarify the sea ice break-up processes and resultant floe size distributions through wave-ice interaction from "artificial" field experiments. By inducing waves with a cruising ship, how waves broke sea ice floes was monitored by a drone. This experiment was conducted in winter and summer, and the results were assessed to improve the understanding of the break-up processes of sea ice and the generation of floe size distribution. The major findings are the importance of the ice rigidity as well as wavelength to determine the maximum floe size, much slower propagation speed of ice break-up compared with the expected wave propagation speed, and the influence of ice thickness on the wave attenuation. They also proposed a new method to represent the floe size distribution.

It is well known that the floe size distribution is one of the key parameters of sea ice and the ice break-up processes due to waves play an important role in its formation. However, due to logistical difficulties, it has been quite difficult to clarify the break-up processes from in-situ observations. To my knowledge, this is the first experiment conducted in the real sea ice area under the idealized conditions. Therefore, basically I support this work and recommend publication. Having said that, I feel some descriptions seem confusing and need to be reconsidered. For me, some conclusions are not necessarily clear. I would appreciate it if the authors address them before publication. If it comes from the lack of my knowledge or my misunderstanding, please forgive me.

Answer

Thank you for reviewing our paper. Your comments helped us to increase the quality of our manuscript. Please see below for the answers to your major and specific comments.

Major comments

Comment

In this experiment, ice thickness is one of the key parameters. Therefore, I think the measurement method and accuracy should be described clearly. To my understanding, it was obtained from the ice chart for GSL (L134) and from meter stick (L147-148). Since the description is only brief, I wonder if it is possible to use these data obtained from different sources equally for quantitative assessment. Some additional descriptions to guarantee it would be desirable. Besides, the horizontal scale seems to have been determined only from the FOV of the camera and the flight height of the drone (L127-128). Since the view angle and flight height could contain some ambiguities, it would be ideal to check the horizontal scale from the real scale such as the ship length if possible. It was regrettable that buoy data were not available (L154) because the information of incoming waves is quite important

The ice thickness and image resolution are indeed key parameters of the experiments that allow to compare observations against theory and to scale the majority of the breakup properties. Unfortunately, as the reviewer points out, we were unable to hop on the ice and measure its thickness at various places from drill holes. We were only able to measure the thickness during the Baffin Bay experiment directly by going out on a zodiac and using a meter stick with a hook at one end at the edge of broken up floe. We gave to this measurement a somewhat large uncertainty (10 cm) in our analyses in order to compensate for various possible sources of errors.
In the GSL experiment, the thickness of the unbroken ice floe was assessed visually, looking at the ice freeboard while standing on the ship's lower deck 3 m above the ice. The value was also given a large 10-cm uncertainty for analyses. We also put that information in the context of the Canadian Ice Service ice chart which is also coherent with what we saw.
We modified the text of section 2.1 and 2.2 as well as Figures 1 and 2 to describe in greater detail prevailing ice conditions and how we estimated ice thickness.
Like for the error on altitude, the error on horizontal dimensions due to the uncertainty on the camera field of view is very low. We appended the error assessment at L145 with details on the impact of the error on the FOV, which is conservatively estimated to $\pm 0.5°$. It leads to an error of 0.03 cm px^{-1} (0.9%), which is still two orders of magnitude smaller than the pixel size (3.1 cm). The total error associated with both paramters is 0.04 cm px^{-1}.

Answer

In section 3.1 they described "the minor axis length is the chosen floe length scale as it represents the characteristic break-up length scale" (L186-187). If they mean the ice break-up due to major propagating waves, the orientation of the minor axis should be aligned in the similar direction. But as far as looking at Fig. 4, the directions of the minor axis are variable. I guess various factors affected the ice break-up processes. I want the authors to explain what they meant by the characteristic break-up scale. Besides, I would like to know how the major axis length was determined and the relationship between the two parameters. I think this is important because the selection of floe size is relevant to the Area-based floe size distribution in Fig. 10.

Comment

Using the minor axis of ellipses fitted to ice floes is convenient to rapidly assess the horizontal scale that is relevant to flexural break-up, and other studies have used that metric (see for e.g. Herman et al. 2018). When waves break-up the ice, sometimes floes are very anisotropic, and the smallest dimension occurs along the wave propagation direction. For such elongated floes, the fitted ellipse is very well aligned with the main axes. Of course, when floes are nearly isotropic, the fitted ellipse may not be aligned with the wave directional axes, but since the two length scales are similar, the number that is computed is still representative. Of course, the shape of floes produced during a wave-induced break-up do not comply perfectly with ideal behavior. However, the advantage of this method is that it is fast and that all floes are taken into account in the FSD. Other methods would be possible, but all would have their limitations. Looking at Fig. 4, one sees that floes that deviate the most from the wave direction (minor axis along the wave direction) are nearly isotropic or are smaller than average. In the former case the error on the size is low, and on the latter case these floes do not contribute significantly to the shape of the AFSD.

Answer

Based on the ratio of Cg to Cp (Fig. 11), they inferred that the break-up speed was controlled by the mass loading effect. Although it might be true, I am a bit skeptical about the idea. This approach would apply to the waves propagating in the fractured ice area. But I consider the ice break-up mechanism would be involved to determine the break-up speed, and the situation is not necessarily the same as the mass loading effect. Some additional effect such as ice fatigue, as discussed by Langhorne et al.(1998), or the heterogeneous properties of sea ice should be involved. If they consider the break-up mechanism is not so important, please add some explanation.

Comment

In this study (GSL experiment) we measured three things: 1) the wavelength in unbroken ice, 2) the wave period in broken and unbroken ice and 3) the speed at which break-up occur in the direction of the wave, that we call c_b. We do not measure the group speed. The phase speed is defined as the wavelength (distance between two crests/troughs) divided by the period (time it takes for one crest/trough to travel at the position of the previous one). This is the speed we can estimate directly. What we try to do is to determine how does c_b relate to the wave group propagation. Figure 11 shows that c_b is slower than the group speed of open water waves and that mas-loaded wave speed is within the uncertainties of our measurements. After relflection, your comment made us realise that the breakup speed will be shifted from the group speed, either negatively due to attenuation or positively due to ice fatigue. The identification of the dispersion relation of wave propagating in unbroken ice can be made with the phase speed and that is why we added a subfigure to Figure 11. Section 5.2 was modified to clarify this interpretation, which in our opinion greatly improves the manuscript.

Answer

I think the [conclusion] can be more concise, focusing on the essence of the new findings. Regarding the novel way of computing the FSD, it might be better to add some general explanations about how effectively it represents the physical properties of FSD compared with the traditional way. I am wondering why they focus on d_{max} although they use d_{min} for drawing the AFSD.

Comment

We added a better description of the advantages of the AFSD over the NFSD and modified the text in the conclusion in order for it to be more concise.

We focus on $d_{\max}$ because the theoretical frameworks prescribe values of $d_{\max}$. We would have done otherwise if there were theories for the modal size or $d_{\min}$, but there is unfortunately none that focuses on the position of the critical strain. This would be helpful for parametrizing the FSD from sea ice and wave properties in larger scale models.

We do not use $d_{\min}$ for drawing the AFSD. I guess you refer to $x*$ which is close to the minimal sizes in the GSL and NBB AFSDs. We think that the value of $x*$ being close to the minimal size in both experiment exhibits the fact that this semi-static framework is not well suited for breakup since it should represent the maximal size and it does not.

Answer

Specific comments

* (Abstract, L11) ".. thicker ice can attenuate wave less than thinner ice." Is that true? To my understanding, in the thicker ice situation of NBB, the wave attenuation was less because of the less ice rigidity caused by more brine volume fraction.

Comment

It is. The ice was thicker in the NBB and the extent of breakup was larger. This highlights the fact that brine volume, and thus Young's modulus, are important values in determining if sea ice is going to be broken by waves or not.

Answer

*(Introduction) I think the background of this study was well researched. But if you agree, please consider adding the following papers for the observational studies that directly relate the FSD (L38): Kohout, A.L., et al. (2016): In situ observations of wave-induced sea ice breakup. Deep-Sea Research II, 131, 22-27. And for the analysis of the resulting FSD and its possible connection to sea ice flexural rigidity (L86): Toyota et al. (2011): Size distribution and shape properties of relatively small sea-ice floes in the Antarctic marginal ice zone in late winter. Deep-Sea Research II, 58, 1182-1193.

Comment

Thank you for your suggestions. After further examination of Kohout et al. (2016), there is indeed a thorough quantification of wave conditions during each breakup event and an attempt to predict the breakup extent with a strain model. Unfortunately, there is no FSD extracted from their dataset. They do mention that the "[evenly spaced cracks suggest] a relationship between wave induced ice breakup and floe size distribution" but do not proceed to further analysis. We will thus add add this reference at line 85 since this portion of the paper mentions the other wave-induced ice breakup already made.
We will add Toyota et al. (2011) at line 86.

Answer

*(Introduction, L55) Please add the definition of "WIMs".

Comment

Corrected, thank you.

Answer

*(Section 3.2, L192) "The wave phase speed is then obtained.." I am wondering that judging from the estimation method, this might correspond to the wave group velocity (Cg). It seems that they treated this value as the group velocity in section 5.2, didn't they? To my understanding, the cruising ship generated the fixed frequency, which induced the waves with various two-dimensional wave numbers satisfying the dispersion relation. The observed large wave amplitude evolution corresponds to the maximum group velocity produced by them.

Comment

As discussed in a response to a previous comment, what we measure is the phase speed, defined as $c_p = \omega/k = \lambda/T$. We did not measure the group speed, but rather inferred it using existing theories.
Note that the phase speed of ship waves is also given by $c_p = U\cos(\theta)$, which is consistent with our independent and more direct measurement in ice, within uncertainty limits.

Answer

*(Section 4.2, L228) ".. the minor axis d since.." I am wondering if the orientation of the minor axis might be important as well. I want to know how the length of the major axis was determined. Is that because of the heterogeneous properties of sea ice or any other reasons?

Comment

The orientation of the minor axis, which may have a slight offset relative to the *true* inter-crack distance, could contribute to the spread of the FSD. But, since the orientation distribution of the floes (result not shown) is narrow, it is likely not a major contributor to the spread of the FSD as the heterogeneity of the ice properties might be.
The length of the major axis is determined by MATLAB's Image Processing Toolbox and has the following definition : "Length (in pixels) of the major axis of the ellipse that has the same normalized second central moments as the region, returned as a scalar."
To support our choice of metric for the floe size, we added Herman et al. (2018) as reference who did the same choice for the same reasons.

Answer

*(Section 5.1, L270-271) "The shape of the AFSD also highlights the fact that this process alone does not explain the power law distribution..." This is interesting. To show it clearly, how about displaying the (traditional) cumulative FSD directly, if you agree?

Comment

Thank you for your suggestion. Nonetheless, we consider that the non-cumulative FSD used in the paper displays efficiently the fact that waves alone do not generated a power-law FSD and allows for an easier translation into models which already use binned distribution of ice thickness.

Answer

*(L277) "propose" should be "proposed".

Comment

Corrected, thank you.

Answer

*(Section 5.1, L319-320) "Toyota et al. (2011) to consider that.." Since they introduced this idea to explain the threshold of two regimes, I feel that their concept is not inconsistent with the authors' idea described in this paper.

Comment

Even if it was used by them for the threshold between two regimes, they still based their rationale on the fact x^* should represent a minimal fracture distance which turned out to be a wrong interpretation of Mellor (1983). That is why we mention it.

Answer

(Section 5.1, L350-354) Equation 14 & 15 According to the definition, it seems that β corresponds to the ratio of x to wavelength. Please add some description about the physical meaning of β, which would facilitate the readers' understanding.

Comment

It is indeed the ratio $(x^*/\lambda)^4$ and comes from the dimensional analysis of the boundary condition on η, eq. 1.3 in Tkacheva (2001). We added the following text between eq. 14 & 15 to make it clearer to the readers : "This quantity arises from the dimensional analysis of the the boundary conditions considered for solving the velocity potential of a plane wave being diffracted by a plate."

Answer

*(Section 5.1, L367) “an” should be “and”.

Comment

Corrected, thank you.

Answer

*(Section 5.2, L406-408) “this result rather suggest that…” This is an interesting result and I agree.

Comment

Great !

Answer