The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-121-RC1, 2019

Interactive comment on "Wave energy attenuation in fields of colliding ice floes. Part A: Discrete-element modelling of dissipation due to ice–water drag" *by* Agnieszka Herman et al.

Anonymous Referee #1

Received and published: 18 July 2019

1 General comments

This paper is concerned with how ocean waves attenuate as they propagate in fields of sea ice, specifically how dissipation due to ice-water drag contributes to the attenuation. It represents Part A of two papers, the first focussed primarily on modelling and the second (Part B) focussed on experiments in a wave flume. I have been asked to review both papers. Respecting the journal's page limits—which don't appear to be immutably adhered to based on other published papers—a single longer paper combining Parts A and B would have made my assignment easier and have been preferable, because it is difficult to review Parts A and B interdependently as the manuscripts intersect significantly. (I also conjecture that a synthesis of Parts A and B would likely have occupied less pages than Part A and Part B published independently because of repetition.) Be that as it may, I shall do my best.

Fortunately, I support publication of Part A (and Part B) subject to *very* minor amendments and remark that most of my specific comments below are intended to clarify and hopefully improve the manuscript rather than being obligatory.

Although the work considers a subset of the many potential dissipative mechanisms that are possible when waves traverse sea ice, which collectively depend on the multifaceted relationship between the incoming waves and the ice morphology, the paper should be read in the context of our poor overall understanding of the topic. While good progress has been made in accounting for the conservative redistribution of wave energy that arises because of scattering by and between ice floes, we know appreciably less about the physics of how energy is systematically removed from the waves as they travel through sea ice. Few models exist and historical data sets are perfunctory due to the sensitivity of the dissipative processes to the physical and mechanical properties of the ice cover and the attributes of the waves themselves. In sum, as the authors point out, “interpretation of the observed attenuation rates is extremely difficult, as it would require simultaneous measurement of several wave and ice characteristics over large distances.”

The authors use a one-dimensional discrete-element model (DEM) to simulate the wave-induced surge and collisions of ice floes, coupled to the wave energy transport equation via phase-averaged source terms. The modest size of the ice floes and the wavelengths are similar, as details about the sea ice (and waves) were chosen to emulate a series of wave flume experiments reported in Part B that took place at the Hamburg Ship Model Basin. For a compact, horizontally-confined ice cover, the authors report a seemingly surprising result but one that is not without precedent, namely that nonexponential attenuation of wave amplitude occurs. They find that wave amplitude a as a function of distance travelled x behaves as $a(x) = 1/(\alpha x + 1/a_0)$, with $a_0 = a(0)$; an

Printer-friendly version

Discussion paper

example of the more general (nonlinear) power law attenuation hypothesized by Shen and Squire (1998) for pancake ice collisions triggered by proportionately longer waves (see also Squire, 2018) and, in fact, by Wadhams (1973) using an alternative creep parametrization. (In passing, I note that existing field observations do not allow power law attenuation to be straightforwardly tested against the pervasive $a(x) = a_0 \exp(-\alpha x)$ relationship for particular circumstances, because the usual confidence intervals associated with in situ wave measurements in ice covers are too large due to unavoidable experimental design constraints. Indeed, the authors assert, “In many cases, large scatter in observational data and/or limited number of measurement locations make the usage of more complicated models unjustified.”) The attenuation rate, α, depends on frequency ω, i.e. how the waves disperse. The authors conclude that $\alpha \sim \omega^{2-2.5}$ for continuous ice, which does appear to be supported by observations. The DEM model predicts the existence of two zones, a narrow collisional zone with very strong attenuation near the ice edge and an interior zone where attenuation rates are less. While similar structures are frequently encountered in field data, e.g. see Squire and Moore (1980, doi: 10.1038/283365a0) and the explanation provided by the authors for the genesis of the outer zone seems plausible, other prospective causes exist.

2 Specific comments

1. While the theme of the paper is quite technical, the authors remind us that large scale sea ice models, e.g. Bateson et al. (2019)’s augmented CICE-based model coupled to a prognostic ocean mixed layer model, indicate that ice extent and volume are sensitive to ocean wave attenuation rates; waves break up the sea ice and move it around to a degree that is strongly dependent on how they are attenuated. Consequently, this paper is an important addition to the wave/ice interactions literature. To help the reader appreciate the significance of its outcomes, a little more could be said in regard to its relevance to climate change, as ocean

waves have unquestionably contributed to the demise of the Arctic summer sea ice by accelerating other effects such as ice-albedo feedback. It is unlikely that this is not also true in the Southern Ocean, given the intensity and uptrending of the wave climate there.

2. If, as the authors state, "[nonlinear] processes leading to the dissipation of wave energy take place within the ice itself as well as in the underlying water layer and include viscous deformation of the ice, overwash, vortex shedding and turbulence generation, friction between ice floes and between ice and water (form and skin drag), inelastic floe-floe collisions, breaking and rafting of floes, and many more," I personally believe it is unlikely that simple exponential attenuation via the linear differential equation $da/dx = -\alpha a$ will predominate. However, I also recognize that our ability to differentiate between it and a more sophisticated model such as $da/dx = -\alpha a^n$ may not be achievable because of the considerable uncertainty associated with most field data sets or remotely-collected data, e.g. satellite or airborne SAR, and that exponential decay may be perfectly reasonable for operational wave forecasting and large scale modelling projects. The authors could be a little more forthright in saying this, assuming that they agree with me.

3. In a similar vein, even if exponential attenuation is prevalent, it is very unlikely that a single attenuation coefficient α will hold, given the intense heterogeneity associated with most fields of sea ice floes. The authors could add words to this effect too.

4. While mostly saved for Part B, there is a statement on page 4 that even seemingly simple situations lead to wave propagation and attenuation that is shaped by several interrelated processes that are impossible to isolate from one another, and that several different model configurations can reproduce the observed attenuation rates. As the authors point out, this makes identification of processes actually responsible for dissipation a formidable task. This is a crucially important—

although a rather dispiriting and potentially controversial conclusion, which needs to come through much more strongly in the paper(s) than it currently does, as it potentially redirects the topic to a more empirical/statistical future. Again, I encourage the authors to be more assertive about this conclusion as, while it is mentioned early in the paper and is reiterated in Part B, it appears to be played down or absent in § 5 of Part A.

5. As noted above, the DEM model is one-dimensional. Not much is made of this but it may have serious implications that the authors need to discuss. While Part A includes only the transmitted mode, Part B assimilates more modes using the Kohout et al. (2007) analysis. In both cases all the energy is trapped in the wave vector direction. What is the effect of this?

6. One of the authors (Shen) has constructed an impressive, sophisticated model of sea ice to study wave propagation that includes a form of viscosity via a complex shear modulus. As such it is more general than the options provided by dispersion relation (6), which restricts us to open water, the mass-loading approximation and the elastic plate. There is also the simpler model of Robinson and Palmer (1990, doi: 10.1016/0022-460X(90)90661-I) that includes viscosity by means of a velocity-dependent damping term, and which seems to reproduce observations better in regard to the frequency dependence of $\alpha(\omega)$. Especially given that the mass-loading model tends not to be favoured by many of my colleagues, I ask the present authors to provide an explanation as to why they used dispersion relation (6). (Note that I am only asking for an explanation, not a reworking of the model.)

7. The authors point out the difference between the DEM in regard to the attenuation of wave energy and the Shen and Squire (1998) model, and they include source terms for both drag and overwash, the latter primarily targeted at the experiments reported in Part B and being especially important near the ice edge before waves have attenuated substantially. I understand the explanation for the difference but

what is the effect of choosing this particular formulation?

8. I really enjoyed the $c = 1$ analysis, especially the outcome that $da/dx = -\alpha_c a^2$, with its solution $a(x) = 1/(\alpha x + 1/a_0)$. This notwithstanding, I was bemused by why the limiting case of confined ice, i.e. with no collisions, should satisfy a particular exemplar of the more general differential equation predicted by Shen and Squire (1998) when collisions were modelled, viz. $da/dx = -\alpha a^n$ with $n = 2$. I guess this reinforces point 4 above, paraphrased that different physics can lead to the same outcome. I also enjoyed the subsequent analysis that investigated how α varied for the three different dispersion relations used, revealing ω^4 behaviour for open water, $\omega^{>4}$ behaviour for the mass-loading approximation and $\omega^{<4}$ for the continuous elastic plate. The asymptotic dependence of $\alpha(\omega)$ is important because of the consistency of in situ field measurements that suggest a power between 2 and 3.

9. As hinted at earlier, I would contest the statement made at the end of § 3 that the mass-loading model is a good approximation of waves propagating through small ice floes or, at least, ask the authors to provide a reference to that effect. However, I do agree with the sentiment expressed that, for this model, it is the dispersion relation (and c_g) that causes differences in how α behaves. Because, taken out of context, this is counter-intuitive as one expects dispersion relation (6) to provide information about how the principal propagating mode disperses rather than attenuates, the authors could explain this better.

10. The paper continues by using the model derived in earlier sections with some of the laboratory data from Part B, essentially embarking on a sensitivity study to see which parameters influence specific outcomes. Some results are not obvious, e.g. why an increased restitution coefficient should lead to lower wave amplitudes, but this is explained. A change of slope is very evident in many circumstances in Fig. 2, dividing the ice field into two zones of high and low at-

tenuation as is often observed in the field (see Squire and Moore, 1980, e.g.). Comparable curves in Fig. 2a and 2b show the effect of the mass-loading and elastic plate dispersion relations. The figure is well explained and the point is well made that smaller floes will always lead to stronger attenuation because of at least two mechanisms. Floe size is investigated in Fig. 5, again with the elastic plate dispersion relation showing a smaller reduction in $a(x)$ than the mass-loading one, while average floe to floe distance is considered in Fig. 6a, where similar behaviour is evident save for very short waves where the zone of intense attenuation is particularly narrow. How the initial amplitude $a_0 = a(0)$ affects $a(x)$ is shown in Fig. 6b. In sum, the properties and the outputs of the model are well tested and well explained by a number of figures, of which I have mentioned just a few. The authors are congratulated for their thorough analysis.

11. The authors begin § 5 on page 16 by reminding us that $a(x)$ and $\alpha(\omega)$ are signatures of the underlying dissipative physics; the question is can the behaviour of these parameters be used to shed light on that physics? We are told that "the DEM simulations predict a very distinctive pattern of wave attenuation resulting from ice-water drag and collisions," but, as the authors point out, conditions at a real ice edge may be dominated by compressive forces exerted by radiation stress, winds and currents compensating increased granular pressure within the ice cover sustained by the waves themselves. Furthermore, the wavelengths considered in the simulations are comparable to the size of the floes, which is different from the situation that is often seen in the winter Antarctic and more recently in the western Arctic where a pancake ice zone forms at the ice margin. What is particularly noteworthy—and deserving more comment to finish—is the result that the tail of $\alpha(\omega)$ follows a power law $\sim \omega^{2-2.5}$ within the ice interior, quite different from open water and close to what has been observed in situ.

3 Technical corrections

I have very few technical corrections that I ask the authors to address, as follows

1. Page 3, line 5. Squire (2018) is not really a review paper. Rather, that paper justifies and then fleshes out an explanation for the differential equation $da/dx = -\alpha a^n$ as a power-law fluid and tests the outcome of different values of n. The word *review* should be removed.

2. Page 3, line 13. The Part B paper is not in the bibliography.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-121, 2019.