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Interactive
comment

Interactive comment on "Wave energy attenuation in fields of colliding ice floes. Part B: A laboratory case study" *by* Agnieszka Herman et al.

Anonymous Referee #1

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1   General comments

This paper is about the attenuation of ocean waves as they pass through fields of sea ice. It is the second instalment of a two-part connected study, which follows a Part A focussed predominantly on modelling. I have been asked to review both papers. As expressed in my review of Part A, a single longer paper combining Parts A and B would certainly have made my job easier because of intersections between the two parts. Be that as it may, I focus here on Part B which reports on a set of experiments done in a wave flume interpreted using theory from Part A and the Kohout et al. (2007) model.

I support publication of Part B (and Part A, incidentally), subject to minor amendments. As with my review of Part A, many of my specific comments below are intended to

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clarify and hopefully improve the manuscript.

The laboratory experiments analyzed in this paper were performed in a large ice model basin at the Hamburg Ship Model Basin (HSVA). Initial analyses of these data with somewhat different orientations have already been completed and published by the authors and others. In essence, the purpose of the experiments was to replicate how long-crested waves interact with densely packed ice floes at the scale of a wave flume, the HSVA facility providing a unique infrastructure to achieve this expeditiously by allowing parallel experiments to be done contemporaneously because of its width. All experiments and the models used to analyze the data are inherently one-dimensional.

Somewhat predictably, the authors find that the Kohout et al. (2007) model (designated MEEM after the mathematical method used), which computes the conservative redistribution of wave energy arising from successive reflections at the edges of consecutive ice floes, cannot replicate the attenuation observed in the experimental data, but the discrete-element model (DEM) reported in the companion Part A paper can. However, as the authors state "more than one combination of the parameters of the coupled DEM–wave model (restitution coefficient, drag coefficient, overwash criteria) produces spatial attenuation patterns in good agreement with observed ones over a range of wave periods and floe sizes, making selection of optimal model settings difficult. The results demonstrate that experiments aimed at identifying dissipative processes accompanying wave propagation in sea ice and quantifying the contribution of those processes to the overall attenuation require simultaneous measurements of many processes over possibly large spatial domains." This is a crucial revelation, as the Part A DEM is necessarily limited in its quantification of the physical processes that act to dissipate wave energy—which are many and hard to model. It intimates a rather different, more empirical, statistical approach to simulating the effects of sea ice on ocean waves in the future, especially for wave forecasting and large scale modelling predictions where compromised sea ice covers and the uptrending of global winds and wave height consequent to a warming climate are now paramount. If, as the authors

point out "the main goal of [their] study is to demonstrate that the interpretation of the observed attenuation and validation of numerical models based on that type of data is problematic, as many mutually interrelated mechanisms contribute to the net attenuation," one has to ask whether there is any value at all in trying to parametrize the underlying physics or is it simply wiser to just work with features in the data themselves?

The experimental programme is impressive with respect to its thoroughness, covering a good range of floe lengths and wave periods commensurate with the size of the flume, with sophisticated measurement sensors including video to clarify features in the behavioural response of floes. Floes are always rectangular, which is appropriate given the one-dimensionality of the experiments. Overwashing of the floes is also considered. Relevance to Nature, i.e. whether results can be scaled up to wave-ice interactions in naturally occurring sea ice fields, goes notably unreported and this is really my only weighty criticism mentioned below under Specific Comments.

The Part A DEM predicts two interesting outcomes that I mention specifically here: nonexponential attenuation that depends strongly on the group velocity via the dispersion relation and, consequently, on the ice morphology; and wave amplitude profiles that identify the existence of two zones—a narrow zone near the ice edge where energetic collisions lead to high attenuation rates, and an interior zone with densely packed ice floes where attenuation is less. As foreshadowed above, I find the outcome that the MEEM model cannot reproduce the attenuation seen in the wave flume less interesting as I would not have expected a conservative model, based entirely on reflections occurring due to impedance changes at what are a relatively modest number of ice cakes in a wave flume, to predict much attenuation at all.

2 Specific comments

1. The authors do not mention at all how to scale up their results to ocean wave-lengths and sea ice floes in Nature. Is Froude scaling appropriate? Some discussion on scaling should be included in the final paper to move it from an interesting analysis of some laboratory data to work that has implications for the Earth's polar and subpolar seas. Specifically, can the authors assure the reader that Froude scaling is adequate to extrapolate model scale tank testing to full scale sea ice covers such as the marginal ice zone. While this sounds daunting, can I suggest that the DEM might be used to do this.

2. Can I encourage the authors to say something about the ramifications of their work to how the topic of wave-ice interactions might evolve in the future, given their strong statements about fitting models to data. If the authors agree with the (uncitable) comments I have made in my review, they should discuss in their paper what this implies with respect to (i) how the effects of ocean waves should be embedded in large scale sea ice models—as the way floes break up and move about depends in part on attenuation; and (ii) how wave forecasting models should parametrize sea ice covers with differing morphologies to provide realistic wave decay rates. As the authors write "many different models can be calibrated to reproduce observations with reasonable accuracy, especially considering large uncertainties in attenuation rates derived from measurements."

3. Unlike Part A, Part B includes a (finite) set of each of the possible modes in the MEEM theory. The explanation of the model is brief but, given it has been published before (see Kohout et al. 2007 and related papers), it is sufficient. What I am unsure about is how the model is applied in the context of the wave flume, which has a wave-maker, a forward and terminal ice edge, and a beach. Can the authors please explain this in more detail.

4. §2.2 is a summary of the DEM model, described more exhaustively in Part A. (I have already said that I would have preferred a single paper, so I am not going to labour this point here.)

5. The section on laboratory observations is well written and thorough, with Fig. 1 providing a good schematic overview of the set up and Table 1 listing the experimental parameters themselves. It is evident that the experiments were done with care and that data were rejected when unforeseen mishaps occurred. Nonetheless, I always seek reassurance that the beach is effective for these types of experiment as, without active control, returning wave fronts can rather mess up an otherwise well planned experimental design. Can the authors please provide percentage beach efficiencies as a function of wave period and also reassure me that no ice was present over the beach?

6. I was pleased to hear that strong overwash was limited to the zone near the frontmost ice edge and that it was mostly associated with steep waves. This is what I would expect, as the strong attenuation that is being observed in the outer zone would soon reduce the waves sufficiently to minimize overwash farther in, even with the small freeboard of the ice in the experiment.

7. The authors explain features of the dispersion in Fig. 2 convincingly.

8. Fig. 3 shows how the wave amplitude $a(x)$ varies with distance x measured down the wave flume, including data, MEEM results and least square fits to equations from Part A reproduced in Part B as equations (3) and (4) with attenuation coefficients α_{exp} and α_{pow} respectively. The lack of fit to MEEM is very apparent but I am not surprised by this because of the number of floes involved—even when $L_x = 0.5$ m, and their sizes relative to wavelength. A *resonance effect* seems to occur in companion tests when wavelengths are roughly twice the floe size; again this effect has been documented in other publications, e.g. those of Meylan. As hinted above, a flawless application of the Kohout et al. theory for ice

cakes in a physical wave flume, where there is also likely to be considerable dissipation due to several mostly-nonlinear processes, is also challenging, making MEEM an unworthy candidate to fit such experimental data. Incidentally, can I assume in Fig. 4 because there is open water on each side of the ice cover, that $|a_{\mathrm{MEEM},R_0}|^2 + |a_{\mathrm{MEEM},T_0}|^2 = 1$ for plots (b) and (c)? If so, say this, and the figure could be simplified.

9. In applying the DEM to a good proportion of the data to select the *best* C_{sd}, ε, s_{min} and c_{ow} using σ_{std} and δ_{m} (see Part B page 14–15 text), the authors wisely acknowledge the uncertainty around some of the parameters required to drive the model, e.g. floe separation. And, not knowing the actual dispersion relation, they have had to make an educated decision to get the group velocity c_g. Table 2 shows 4 Runs A–D meeting documented satisfactory quality criteria; see also Fig. 7. The supporting discussion is very detailed and sometimes a little confusing, unfortunately.

10. For example, starting at line 10 on page 15 we read that "simulations without overwash require very high values of C_{sd} and the optimal configuration is obtained for $C_{\mathrm{sd}} = 4.0$." OK, got that. Then we read that "runs A and B ... mostly differ from each other in the area close to the ice edge in tests with short, and thus steep waves (e.g., text 3360) in which overwash contributes significantly ..." But I thought overwash was minimal in Runs A and B. Having said this, the premise is fine, namely that different processes can produce similar net effects. I recommend page 15 and the first half of page 16 are rewritten to be clearer; even if I am misunderstanding, my example suggests the current prose is ambiguous.

11. I agree that the authors have shown "how difficult it is to quantitatively assess wave energy attenuation in sea ice (especially in laboratory conditions, with a limited number of floes and over short distances) and [crucially] to attribute the observed attenuation to individual physical processes, even in a highly idealized

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laboratory setting." As I have already said, this is a tremendously important out-
come of this work. Plagiarizing the authors' words, ... more generally this is a
consequence of a large number of poorly constrained coefficients, large uncer-
tainties in measured data, and the fact that the vertical deflection of the ice, being
a combined effect of many processes, is invariably the only validated quantity.

12. I reiterate that I am less excited by the fact that scattering alone doesn't explain
the observations, as I would not have expected it to. But I also remind the authors
that (i) they have used a one-dimensional model and one-dimensional data; and
(ii) they have not considered that in a real ice field there will be dissipation of the
scattered waves in the regions between ice floes whether these are ice-free or
filled with brash.

13. But I do agree with the authors statement that "The patterns of wave attenuation
observed in [the wave flume tests] are predominantly shaped by dissipative pro-
cesses, and that the effectiveness of those processes in attenuating wave energy
is frequency dependent."

14. While the arguments around using C_{sd} as an effective drag coefficient are con-
vincing and recalling that the authors do actually mention what happens when a
linear drag law is used instead of a quadratic one in Part A (viz. that exponen-
tial decay as opposed to power law decay eventuates), in general, i.e. for real
sea ice fields, this idea would need to be carefully validated. It is tantamount to
packaging a smorgasbord of dissipative mechanisms under the quadratic drag
law umbrella. Certainly nice if you can do it!

3 Technical corrections

1. Page 1, line 20. The Part A paper is not in the bibliography. In fact I had to source it from ResearchGate.

2. Page 15, line 12. Should $c_{\mathrm{ow}} = 0.3$ here to match Table 2? The wording is such that I am unsure whether you are comparing with the previous sentence, i.e. Run A, or are saying $c_{\mathrm{ow}} = 0.4$ gives the same result as in Table 2. Either way, it is ambiguous so should be clarified.

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