The Cryosphere Discussions

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Comment on tc-2021-210

Anonymous Referee #2

Referee comment on "Wave dispersion and dissipation in landfast ice: comparison of observations against models" by Joey J. Voermans et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-210-RC2, 2021

The manuscript „Wave dispersion and dissipation in landfast ice: comparison of observations against models" by Joey Voermans and colleagues is devoted to the analysis of wave propagation and dissipation in landfast sea ice. The study is based on data from field measurements (with IMUs placed on the ice) from two locations, one in the Arctic and one in the Antarctic. The observations are used to estimate the wave numbers and attenuation coefficients of waves within the frequency range of approx. 0.05–0.2 Hz. The obtained attenuation coefficients are compared with those predicted by several models of wave energy dissipation within sea ice and in the turbulent boundary layer under the ice.

Undoubtedly, the problems discussed in the study are important for the current research on sea ice–wave interactions. Our better understanding of the physical mechanisms underlying wave energy attenuation in sea ice is crucial for developing better parameterizations of those processes and thus for a better performance of local and larger-scale sea ice models. Moreover, in my opinion the manuscript is well written, the presented analysis thorough and convincing, and the discussion interesting. My recommendation is to accept the manuscript for publication in The Cryosphere after the Authors address the comments listed below.

Major comments:

- I really like the introduction: it is relatively short, but well formulated and contains all relevant information needed as a background for the results presented in the manuscript.
  Just one comment to that part: I'd suggest stating explicitly that equation (1) is based on an assumption that the dissipative processes leading to wave energy attenuation are linear. This assumption is not always true (see, e.g., Squire, Phil. Trans. A, 2018 or Herman, J. Phys. Oceanogr., 2021), and considering that the present study identifies turbulence as an important source of energy dissipation, and that turbulence is strongly

nonlinear, I think it is worth mentioning already in the introduction (the Authors state it later, in lines 105-106, when equation (3) is introduced).
Note that the models of Kohout et al. 2011 and Stopa et al. 2016 both produce non-exponential attenuation rates (see equation 12 in Kohout et al. 2011; analogously, the coefficient β in equation B1 of Stopa et al. is a function of wave energy – for high Reynold numbers, the model is analogous to the bottom friction formulation in spectral wave models, which is nonlinear).

- On page 5 the Authors write: "We note that for the Arctic experiment only those observations are used that were obtained from the buoy pair furthest apart as they were deemed most accurate." This statement implicitly means that the Authors assume that the attenuation coefficients computed from their data are distance dependent – buoys placed 600 m and 800 m apart are expected to produce different results than those placed 1400 m apart. But then why are 1400 m enough? Can we expect attenuation coefficients obtained for larger buoy distances to be different? How? Do they depend on buoy-buoy distance in a systematic way? Are the differences between attenuation coefficients computed for different distances wave-frequency dependent and do they thus affect the resulting slope of alpha(f) – which is the main result of this paper?
  Did the Authors compute α from buoys 1-2 and 2-3 and compare the results with those for buoys 1-3, presented in the paper?
  It must be also remembered that, for the longest waves considered in the analysis (f<0.1Hz), even buoys 1-3 are only ~3 wavelengths apart, i.e., the distance over which attenuation is measured can be regarded as very short. Do the Authors agree that this fact might have some influence on the results? (Please note that I'm not criticizing the fact that the buoys were placed the way they were – there might have been several practical reasons for that – but only that the Authors don't pay any attention to the possible role of buoy-buoy distance in their analysis).
  Overall, considering how limited the dataset is (9 and 2 data points for the Arctic and Antarctic), I'd say that data from 3 buoy pairs are better than from just one!

- Related to the previous comment: I'm wondering whether the Authors have any information on the open water wave heights corresponding to those measured within the ice. For the Antarctic experiment, ERA5 data are mentioned (line 112), but also the fact that ice pack was present between the open ocean and the fastice, which certainly modified the wave energy reaching the fastice edge. What about the Arctic experiment? What I mean is: It would be interesting to see how the open water spectra computed from those measured in the ice and from the computed attenuation rates compare with the corresponding open water spectra from spectral models or other sources.

- Discussion, lines 213-217: the model of Liu and Mollo-Christensen (1988) is suitable only for a laminar boundary layer! It is simply incorrect to try to make it suitable for turbulent boundary layers by increasing the viscosity as much as one finds it necessary in order to make the model fit the observations – although several numerical studies do exactly that. The viscosity in the model of Liu and Mollo-Christensen (1988) has a physical meaning and cannot be simply increased by a few orders of magnitude when that seems necessary. Crucially, in a laminar boundary layer the viscosity does not depend on wave energy, but in a turbulent boundary layer it does. Difficulty with calibrating the models to perform well in both calm and storm conditions is one of the consequences of the (mis)use of the Liu and Mollo-Christensen (1988) model for turbulent dissipation – as analyzed e.g. by Li et al. 2015.

Minor (mostly technical) comments:

- Page 3, line 54: "is never be"
- Page 3, line 57: Why "perhaps"?
- Page 4, line 91: omega has not been defined (and f is used instead of omega throughout the paper)
- Page 5, line 113: "indicating a relative bearing of approximately 15°". What does "relative bearing" exactly mean? The angle between wave propagation and the line connecting the two buoys? And were its value constant throughout the whole period of the experiment?
- Page 6, line 135: ρ hasn't been defined.
- Figure 2, measured data: what exactly do circles, bars and vertical lines mean? (i.e. standard deviations or percentiles, etc.?)
- Figure 3a: I'm not sure if I can see it correctly, but there are crosses inside of some of the circle symbols – please clarify.
- I find it inconsistent that the data in Figs. A2 and 2 (with the corresponding text in the first part of the Results section) are presented in terms of wave frequency, and then the data in Figs. 3–5 is plotted and discussed in the text in terms of wave periods. It's not wrong, of course, and the Authors may decide to leave it the way it is if they prefer to do so, but it makes comparisons between plots less easy.