The Cryosphere Discuss.,
doi:10.5194/tc-2017-24-RC1, 2017

The Cryosphere Discussions

Interactive comment on "Wave-ice interactions in the neXtSIM sea-ice model" *by* Timothy D. Williams et al.

Anonymous Referee #1

Received and published: 4 April 2017

In their manuscript "Wave-ice interactions in the neXtSIM sea-ice model", T. Williams and colleagues present a coupled wave–sea ice model, based on their neXt-generation Sea Ice Model (neXtSIM). The authors describe in detail the treatment of ice–wave interactions in the coupled model, including sea ice breaking by waves, wave attenuation by ice and calculation of the wave radiation stress (WRS). The model performance is illustrated with results of idealized simulations, in which waves approach sea ice with initially uniform properties, in situations with and without wind.

The manuscript is devoted to a subject which is extremely important for the performance of numerical sea ice models on both short (synoptic) and long (climate) time scales. As the authors point out in the introduction, recent climate change and the associated negative trends in sea ice extent, thickness and strength have produced conditions in which sea ice interactions with waves are becoming more and more im-

portant over larger and larger areas. However, our limited understanding of many aspects of these processes is a serious limitation for development of parameterizations of ice–wave interactions suitable for numerical sea ice models. In my opinion, the proposed manuscript is an important contribution to the subject, even though many solutions used in the coupled model and assumptions underlying it are oversimplified (or maybe even wrong). As the authors correctly remark, these simplifying assumptions are a direct result of the lack of observational data and/or theoretical understanding available. In this respect, the most important contribution of the manuscript is that it develops a framework in which future developments can be integrated, as new data and insights become available. In other words, in spite of some clear limitations of the solutions presented, I find this contribution very valuable, as it paves the way for further development.

My comments to the manuscript are listed below. Because addressing them does not require any substantial changes/additions to the content of the paper, my recommendation to the Editors is "minor revision".

1. I have problems with understanding the concept of the damage parameter d in relation to ice concentration c and maximum floe size Dmax. Equation (9c) states that (without thermodynamic effects) d can change (or, more precisely, increase) only if stress falls outside the prescribed envelope. That is, d is not in any direct way related to other quantities characterizing the ice (although, I suppose, in longer simulations the model would by itself evolve into a state in which d, c and Dmax are related). This makes d quite mysterious to me. For example, how should one imagine ice with c = 70% and d = 0, which is used as an initial condition in the simulations? We have relatively dispersed ice, with 30% open water, floes with power-law size distribution (which makes sense only if the number of floes per grid cell is large), but the ice is "undamaged" – how realistic is that? What motivated this choice of initial conditions and how does it influence the results? A more general question: What does "damaged" and "undamaged" mean in physical terms? The authors should include some

discussion/explanation before they proceed to describing details of their model, otherwise some parts of it seem rather obscure. d, c and Dmax are quantities describing the state of the ice cover, and by being shaped by wave–ice interactions they act as a signature of those interactions – so sufficient space should be given to relationships between them. Are all combinations of d, c and Dmax realistic? If not, does the model allow for those combinations or are there some mechanisms that relate one variable to the other?

2. The authors do not describe how Dmax is modified if the breaking criteria (section 3.4) are fulfilled. I know this information can be found in previous papers, but it should be given here for completeness (presumably in section 3.3, together with the description of FSD).

Some minor, technical comments:

1. I'd suggest to replace the word "movement" with "displacement" (or something similar) in the context of the changing position of the ice edge. Especially in the abstract, it is not clear what the sentence ". . . wind waves can produce noticeable movement in loose ice" really means, as no reference to ice edge is made. It wrongly suggests that some analysis of ice motion is made in the paper.

2. The authors should check if all symbols are explained in the text. In most cases (e.g., wave number and amplitude in section 3.4.1) it is obvious what the symbols mean, but still, they should be defined.

3. In the list of references, some papers have missing volume/page numbers, e.g., Meylan et al. 2014 or Rabatel et al. 2015.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2017-24, 2017.