Answers to the Editor's comments

The authors are most grateful for your comments. We have followed your suggestions and revised the manuscript accordingly in many places. Please, find our responses below.

1. The title of the paper is somewhat misleading as the vast majority of the simulations considered in the paper are on the transformation at the ice-edge boundary.

Answer: Thank you for the suggestion. We changed the title to "Transformation of internal solitary waves at the ice-open water boundary"

2. Following up on the previous point, dynamically an ISW propagating past the ice-edge boundary is equivalent to the setup in previous work which considered ISWs propagating past a step at the bottom. It should be made clear what is new here for this set of simulations. What is different about this set of simulations and what are the new conclusions? For example Figure 2 in Talipova et al. 2013 and the current manuscript are very similar.

Answer: In this study, we used the Reynolds equations and a turbulence model to describe field-scale processes, in contrast to Talipova et al (2013), which considered laboratory-scale processes and used the Navier-Stokes equations. Therefore, there is not a direct correspondence between our calculations and Talipova et al (2013) in Fig. 2, although they are qualitatively close. Text was changed accordingly:

L 189 "The differences in values of the energy losses from (Talipova et al., 2013) and from the present investigation can be explained by the fact that the field scale problem was studied in this work using the Reynolds averaged equation, while in Talipova et al. (2013) the propagation of ISWs in a laboratory-scale computational domain was studied by using the Navier-Stokes equations."

3. On page 4 it is stated that a zero pressure gradient boundary condition is used. This can not be correct as a non-zero pressure gradient exists at the boundaries above and below an ISW. This, after all, is what accelerates the fluid near the boundaries.

Answer: Thank you for the comment. The text was changed accordingly:

L 89 "The Neumann-type boundary condition for the nonhydrostatic pressure component was used at the solid boundaries. At the free surface and open boundaries, this component was set zero (Maderich et al., 2012)."

4. It is mentioned that this work considers the 'real scale' situation (e.g., page 11). What exactly is meant by this? The only way it can be field scale (I assume this is what is meant) is if the Reynolds number is appropriately large. The Reynolds number is never mentioned. Neither is the value of the viscosity used in these simulations.

Answer: Thank you for the suggestion. The field-scale Reynolds number based on ISW amplitude and velocity is an order 10^7 which forces the use of Reynolds equations closed by a turbulence model. We changed 'real scale' to 'field scale' and added text:

L. 192 "The eddy viscosity and diffusivity calculated from the turbulence model (Siegel, Domaradzky, 1994) vary in space and time with characteristic values 10^{-4}-$10^{-3} m^2 s^{-1}$."

5. For the simulations of an ISW propagating beneath multiple ice keels the results are discussed in terms of the distance between the keels. Wouldn't the primary dependence be on the number of ice keels?

Answer: Thank you for the suggestion. For a finite length of the ice layer L_{ice}, as in the calculations under consideration, the distance between the keels L_k is expressed through L_{ice} and the number of keels n. We indicated this in the text

L.131 "In turn, for a finite length of the ice layer L_{ice} the distance between keels depend on keel number n as $L_k = L_{ice}/n$".

When L_{ice} asymptotically increases then n also increases and the only governing parameter is L_k.

Also, I have provided a marked up copy to help with English corrections. The list is not complete but it should be a good start.

Answer: Thank you very much. We corrected the text in many places accordingly.